Motivating Collusion[*]

Sangeun Ha   Fangyuan Ma   Alminas Žaldokas

Dec 2023

Abstract

We examine how executive compensation can be designed to facilitate product market collusion. We look at the 2013 decision to close several regional offices of the U.S. Department of Justice, which lowered antitrust enforcement for firms located near these closed offices. We argue this made collusion more appealing to shareholders, and find that these firms increased the sensitivity of executive pay to local rivals' performance, consistent with rewarding the managers for colluding with them. The affected CEOs were also granted longer vesting periods, which provides long-term incentives that could foster collusive arrangements.

Keywords: Product Market Collusion; Corporate Governance; Managerial Compensation

JEL Classification: G34, G38, L22

1   Introduction

Firm shareholders adopt various corporate governance mechanisms to motivate the management to pursue strategies that increase firm value. As posited by the industrial organization literature, entering into collusive arrangements with product market peers might be profitable to the firm's shareholders, depending on their discount factors, market conditions, and antitrust enforcement. However, even when shareholders prefer the firm to cooperate with other firms, its managers likely have different intrinsic incentives. In this case, a compensation contract can be used to align incentives and motivate collusion.

Several factors can drive a wedge between the management's and the shareholders' preferences in whether the firm should engage in product market coordination. For instance, executives in the U.S. are subject to criminal charges if the firm is convicted of explicit horizontal agreement conspiracies such as price fixing, bid rigging, or geographic market allocation. Such criminal antitrust enforcement against individuals has been rising over time (e.g., Kades (2019)). Although firms often indemnify their employees for the monetary fines, financial reimbursement may be less effective in the case of imprisonment, which imposes a large personal cost. Moreover, even absent antitrust enforcement, managers have career considerations and reputational concerns that further discourage collusion with other market participants. Both of these factors suggest that managers could have lower discount factors than anonymous marginal investors and are thus less intrinsically motivated to enter into collusive agreements.

In this paper, we study how executive compensation can be designed to encourage collusion when antitrust enforcement is weakened and collusion becomes more appealing to the firm. Shareholders, and the board of directors on their behalf, may design compensation packages to discourage competition, thereby incentivizing managers to engage in collusion without explicitly instructing them to do so. This argument leads to the prediction that weakening antitrust enforcement could result in changes to the structure of managerial compensation in the direction that encourages (or tolerates) less competition.

We focus on U.S. firms during 2008-2017 and look at a recent regulatory change that weakened enforcement of competition law for some firms in the U.S. The event we study is the 2013 decision to close down four regional offices of the Department of Justice (DoJ) Antitrust Division in Cleveland, Dallas, Atlanta, and Philadelphia. Among other responsibilities, these field offices were in charge of collecting information on potential conspiracies in local product markets. In 2013, the DoJ made the decision to save costs and focus on larger firms in the economy by transferring the casework of these offices to the DoJ main headquarters in Washington, DC and the remaining regional offices.

We argue that this regulatory decision reduced in the monitoring of collusion in the local markets that were near the closed DoJ offices and farther away from the remaining DoJ offices. As a result, firms that were operating in these geographic markets experienced a sudden decrease in the probability of being detected in collusive arrangements with their local peers, making such collusive strategies more attractive to the shareholders. We study whether this regulatory decision has led to changes in CEO compensation that weaken incentives for competition or even foster explicit cartel arrangements.

In particular, we focus on the link between CEO compensation and the performance of product market peers. When a CEO is rewarded for outperforming these peers, the CEO's pay is negatively associated with their performance. However, when it benefits the shareholders to soften product market competition, the need to disincentivize competition tilts the optimal contract toward more positive loading on the peers' performance. As weaker antitrust enforcement makes collusion more appealing, shareholders can encourage softer competition by establishing a more positive link between CEO pay and peer performance.

We report empirical evidence supporting this prediction by testing the sensitivity of CEO's pay to the stock returns of their own firm and local peer firms. For firms located near the closed regional DoJ offices, the sensitivity of CEO pay to the performance of local industry rivals became more positive. We find that the weighted average causal response of the sensitivity to a 100-mile increase in the firm's distance to the DoJ office is 0.018,

while unaffected firms had a negative sensitivity of -0.053 before the reform. The change in pay sensitivity is likely driven by boards' active adjustments to compensation schemes. In particular, the effects are stronger for cash than for equity compensation, the former of which is more flexible and can be more quickly adjusted to the changing contracting environment.

We further explore the heterogeneous impact on CEO compensation induced by the regulatory reform. We first show that the effects are stronger among the firms that have better board governance. One possible interpretation of our main finding is that the weakened antitrust enforcement alleviates managers' intrinsic aversion to collusion, so that they proactively pursue it and influence the board to adjust compensation structures in a way that reassures the other cartel members and fosters collusive arrangements. If this is the case, we should find stronger effects among firms with captured boards. However, we find the opposite to be true, which suggests that the observed compensation changes are more likely to be driven by shareholder value maximization than by managerial entrenchment.

We also show stronger effects in the industries in which firms compete as strategic complements and thus are more likely to collude with each other. Moreover, the effects are stronger for firms in the concentrated industries, in which collusion is more likely to take place since it is more feasible to coordinate among a limited number of players. Also, the estimates are larger for the firms with more concentrated local operations that arguably are more affected by the decline in local market monitoring by the antitrust authorities. In addition, CEOs nearing retirement often have shorter-term perspectives and different preferences compared to long-term shareholders or those shareholders who plan to sell shares at prices corresponding to long-term intrinsic values. Indeed, the effects are larger for this group of CEOs. Also, the effects are stronger for firms in more flexible executive labor markets, where CEOs presumably have stronger reputation concerns. Finally, they are stronger in the industries with a larger fraction of public firms since in those cases peer performance can be better inferred from stock returns.

Importantly, managerial compensation arrangements at the time of the policy reform are

3

related to changes in firms' operating performance, which we capture by the gross profit margins. We find that after the reform, the firms affected by the reform started having higher gross profit margins than the unaffected firms. Moreover, the incremental wedge in profit margin was concentrated in the industries in which affected firms' compensation design was more responsive to the reform. In addition, these firms' stock returns started comoving more with the returns of their local product market peers, which is indicative of correlated operating performance. These trends are consistent with anti-competitive effects.

We also examine the adjustments to some other features of executive compensation. In particular, collusive strategies are unstable by nature even without antitrust enforcement, since a myopic player might deviate from the arrangement to capture a large short-term profit from aggressive competition. Thus, after the reform, shareholders may find it beneficial to lengthen the managerial incentive horizon and minimize such myopic strategies. Consistent with this argument, we find that the vesting horizon of stock grants to CEOs in the affected firms was significantly extended when compared to those in unaffected firms.

Overall, in this paper, we suggest that shareholders might be interested in creating incentives to induce managers to pursue collusive strategies with their peers and thus hurt consumer welfare. In doing so, shareholders as a group, or the board members who represent them, do not give direct instructions to collude; thus they have plausible deniability that the incentive schemes do not reflect this particular product market strategy to maximize profits. In this way, they are not subject to personal antitrust liability. Our findings raise a public policy dilemma. On the one hand, corporate governance standards require alignment between the incentives of investors and those of managers. On the other hand, if long-term investor behavior facilitates collusion, policies to promote consumer welfare might have to encourage manager short-termism and thereby exacerbate the principal-agent problem if that could have pro-competitive effects.

Our paper contributes to the literature on how incentive structures affect the strategic interaction of firms in the product markets. The theoretical literature has recognized that the

4

optimal incentive contract depends on assumptions about the competition environment as well as restrictions on the contracting space. Fershtman and Judd (1987) and Sklivas (1987) show that providing powerful incentives with a bonus scheme is optimal to achieve strategic advantages, while Reitman (1993) argues that stock options provide threats to rivals and thus can lead to higher profits for shareholders. We contribute by providing empirical evidence on how compensation design adapts to changing incentives in product markets (see, e.g., Cuñat and Guadalupe (2009); Cuñat and Guadalupe (2009)). In particular, our paper shows that CEO compensation structure changes in the direction of discouraging competition when there is an exogenous decrease in antitrust enforcement. We thus complement the finding of Antón et al. (2023) that CEOs are provided with weaker incentives when there is higher common ownership that favors less aggressive competition in the product markets.

In a related stream of studies on convicted cartels, González et al. (2019) and Bloomfield et al. (2023) find that the compensation structures of executives in convicted cartel firms differ from those of executives in other firms. We instead focus on the changes in compensation structure around antitrust policy reforms that lower the costs of collusion.

Our paper is also closely related to the literature on relative performance evaluation. The principal-agent theories (e.g., Holmström (1979), Holmström (1982), and Nalebuff and Stiglitz (1983)) suggest that managers should be rewarded based on their performance relative to that of their industry peers, which reflects the impact of common shocks to performance that are outside of managers' control. These predictions are largely supported by the empirical evidence (e.g., Aggarwal and Samwick (1999b), Albuquerque (2009), and Jayaraman et al. (2021)). However, while relative performance evaluation has been shown to create powerful incentives, it might also encourage over-aggressive competition. Aggarwal and Samwick (1999a) propose that when outputs are strategic complements, the optimal contract has a positive weight on the performance of both own firm and peer firms, and they find empirical evidence consistent with their predictions. This finding is also confirmed by Vrettos (2013). Consistently, Joh (1999) finds that executive pay became positively related

to peer performance in Japan when the government discouraged excessive competition. In contrast, Gong et al. (2011) find that firms are more likely to pay managers based on their out-performance of their peers in less concentrated industries.

Our paper adds to this literature by showing that the goal of motivating collusion, a specific means of weakening competition, shapes relative performance evaluation. By relying on a shock to antitrust enforcement, we identify the impact of the strategic weakening of competition on the sensitivity of CEO pay to the performance of peer firms.

More broadly, our paper relates to the literature on corporate misconduct such as money laundering, bribery, or breach of environmental standards (e.g., Agrawal et al. (1999); Zeume (2017)). Most of such misconduct likely benefits shareholders, as their firm's profits, at least in the short term, are higher.[1] While some corporate governance implications of these types of corporate misconduct are similar to those of antitrust infringement, the features of executive compensation contracts that we study (i.e., relative performance compensation and vesting terms) particularly relate to the trade-offs in the product markets.

2   Data

We study U.S. publicly listed firms over 2008-2017. We focus on the compensation of CEOs. As discussed by Harrington (2006), cartel decisions are typically taken by the top management to ensure the coordination at different layers of the organization (e.g., avoid "overzealous sales representatives" who might share information about the cartel with the firm's customers). Moreover, top executives' incentives are likely to trickle down to the incentives of middle management.

Our main data source for CEO compensation is Execucomp. We extract information such as total compensation, cash compensation, and the value of stock and option compensation awards. We obtain data on performance benchmarking and vesting schemes from Incentive

---

[1]That contrasts with insider trading, disclosure irregularities, and other managerial misconduct that executives perform at the expense of shareholders.

Lab. We complement the compensation data with stock returns from CRSP and financial data from Compustat. We define the product market peers based on Hoberg and Phillips (2016). The data of convicted cartel cases come from Connor (2014). State-level economic statistics are from the Census Bureau. Historical headquarter data come from SEC filings. Table 1 reports descriptive statistics of our main variables.

The information on the field offices comes from the DoJ Antitrust Division. In particular, we get the case coverage of all field offices before and after the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia).

3 Identification

We first describe and validate the regulatory change that arguably made collusion in some markets a more appealing strategy for firms to follow. To our knowledge, this paper is the first to provide empirical evidence on this regulatory change. In this section, we provide evidence that antitrust enforcement decreased in the areas that were more exposed to the regulatory change, and that the reform's passage was largely exogenous to the business environment faced by individual firms.

3.1 DoJ field office closures

In 2013, DoJ Antitrust Division closed down four of its seven regional offices (Atlanta, Cleveland, Dallas, and Philadelphia) that primarily monitored local markets and dealt with criminal antitrust enforcement. Some of the regional coverage was relocated to the three remaining field offices (Chicago, New York, and San Francisco) but most of it was moved to Washington, DC (Washington Criminal I and II). While this event came purely from the budget cuts, analysts considered that this made it harder for DoJ to police regional cases and instead, DoJ started focusing more on the big nationwide cases. Local politicians and antitrust lawyers raised arguments that the supervision of local firms was expected

to decrease, since one of the important duties of regional DoJ field offices was to source information on local market conspiracies. In Appendix A, we provide more details of the institutional background behind the office closures, as well as media quotes with the reactions from policymakers and the antitrust community. The change in coverage affected 23 states and territories. Table 2 provides details of this geographic breakdown.

We capture each firm's exposure to this reform by the change in distance between the firm and its covering field office under the identifying assumption that the geographic distance serves as a proxy for the intensity of antitrust inspection. Geographic proximity has been shown to reduce the cost of monitoring and information friction between regulators and firms, and the change in distance to the banking regulator or the SEC has been used to capture firms' exposure to regulations (e.g., Kedia and Rajgopal (2011), Wilson and Veuger (2017), Lim et al. (2017), Ganduri (2019), and Gopalan et al. (2021)). In the context of collusion, a successful case often involves granting leniency to those former cartel members who volunteer information on the collusion, therefore, local firms might be more willing to trust sharing such information in a more proximate office.

In this paper, we conduct a difference-in-differences analysis using the change in distance as a continuous treatment variable. This choice primarily stems from an observation about the absence of a clear distinction between treated and untreated firms in our context. Firms experiencing a positive but relatively small increase in the distance to covering field offices may remain in close proximity to the existing law enforcement agency and, consequently, subject to continued robust monitoring. Given this concern, it is difficult to establish a precise binary treatment. Nevertheless, we explore the robustness of our findings by creating binary treatment variables based on various definitions in Section 4.3.1.

Operationally, we measure firms' exposure to the DoJ reform by the change in distance ($\Delta Distance$) from a firm's headquarter to its covering DoJ field office, using the geographic coordinates based on ZIP codes. The mean (median) $\Delta Distance$ is 170.9 (0) miles in the full sample and 513.8 (394.1) miles among the firms that experienced an increase in distance.

We first present suggestive evidence about the impact of DoJ reform on the overall intensity of antitrust enforcement by counting the number of antitrust lawsuits in each state. We sort the states by whether an average firm headquartered in those states became farther away from the covering DoJ field office after the reform. In Figure 1, we show the number of criminal charges of violating the Sherman Act at the affected and unaffected states' courts. We find that such antitrust filings decreased sharply in the affected states after 2013, while there was no such trend in the unaffected states. This is likely to be attributed to the decreasing monitoring intensity from the field offices.

We next examine whether the reform has weakened the monitoring intensity of the *local* markets in particular. We manually sort the antitrust lawsuit cases by whether the alleged actions occurred locally, nationally, or internationally. Although international or national cases could possibly be reallocated to the remaining offices or Washington, DC, the local cases became unlikely to be discovered and processed since the closure of field offices. We find that the drop in antitrust filings of the affected states is driven by the local cases where the defendants are concentrated geographically rather than distributed across the nation or internationally. Table 3 reports the average proportion of local antitrust cases among all prosecuted criminal Sherman Act cases. In the affected states, the average proportion of local cases decreased from 0.26 before 2013 to 0.16 after 2013. In contrast, in the unaffected states, this proportion increased from 0.24 before 2013 to 0.3 after 2013. We observe a similar tendency if we consider convicted local cases.

This evidence coupled with media reports in Appendix A suggests that the 2013 reform weakened antitrust oversight over the firms nearby the original DoJ field offices, especially in terms of monitoring the business activities in the *local* markets. Thus, for the rest of our analysis, we mainly focus on the firms that have local peers as competitors. We define local peers as the close rivals in the product market (with product similarity score within top 70%[2] based on Hoberg and Phillips (2016)) and headquartered within 200-mile radius.

---

[2]The mean (median) number of local peers within the top 70% of product similarity score is 11 (4) which is comparable to the mean (median) number of cartel peers in the convicted collusion cases of 10 (5).

As shown in Table 1, Panel A, the median firm has four local peers and 35 non-local peers.

Table 1, Panel B, shows the median number of local and non-local peers for the top ten and the bottom ten of the Fama-French 48 industries in terms of the fraction of local peers. We can see that the geographic distribution varies a lot across industries. Industries such as tobacco, automobile, and computers are relatively concentrated geographically, while industries such as transportation, utilities, and banking are highly dispersed. By design, our estimates will be identified mainly by geographically concentrated industries.

Before proceeding further, we present the geographical distribution of firms' exposure to the DoJ reform in Internet Appendix Figure IA1. In Panel A, we depict each state's average change in distance ($\Delta Distance$) from firms' headquarters to their respective covering DoJ field office. A higher positive value of $\Delta Distance$ signifies a more substantial weakening of antitrust oversight. As shown in the figure, the states originally covered by the Dallas and Atlanta field offices appear to be the most exposed to the reform, while those originally covered by the Cleveland and Philadelphia field offices exhibit less exposure. This is consistent with the arguments by Robert Connolly, the former chief of the Philadelphia field office, that reopening the Dallas and Atlanta field offices has the potential benefits of enhancing the geographic diversity of the antitrust division and improving the monitoring efficiency (see 'A Call to Reopen the Atlanta and Dallas Field Offices', *cartelcapers.com* (May 27, 2021)).

Additionally, as the reform primarily impacts firm interactions within local markets, exposure to the reform is also determined by the presence of local competitors. In Panel B, we illustrate the proportion of firms associated with local peers in each state. A higher fraction of such firms is related to a state being more strongly exposed to this policy reform.

3.2 Conditions prior to regulatory change

We further argue that the reform is exogenous to the economic condition and business environment that the firms faced. First, we compare the observed economic conditions between the affected and unaffected states. In Internet Appendix Figure IA2, we plot the

average annual real GDP growth rate, unemployment rate, and the growth rate of the total number of firms for the two groups of states, and find that the trends of the two groups have been quite similar both before and after 2013. This alleviates the concern that affected and unaffected states have already been experiencing different business environments and economic conditions before 2013.

In addition, we study the trends in the competitive environment of the firms in the affected and unaffected states. In this regard, we construct several measures based on the Hoberg-Phillips similarity scores. In particular, we consider: (a) the average similarity score of each firm's ten geographically closest peers, (b) the number of peers with similarity scores exceeding 0.1, and (c) the number of local peers (headquartered within 200 miles) with similarity scores exceeding 0.1. These measures reflect how similar a firm's products are to their close peers. As shown in Internet Appendix Figure IA2, we find that none of the three measures presents divergence between the affected and unaffected states before 2013. In fact, if anything, these figures show that after 2013 the firms in the affected states experienced a decrease in the number of local peers and the average similarity of their products to the close peers. This pattern is consistent with the argument that firms in the affected states could have begun to adopt strategies that reduce competition, i.e., they differentiated themselves more from their close peers to avoid direct competition in the product market after 2013.[3]

Next, we examine firm characteristics to understand whether the reform might have targeted firms with certain features. Since we mainly rely on a continuous variable that captures each firm's exposure to the reform, we provide two sets of comparisons. We first compare the exposed (i.e., $\Delta Distance > 0$ mile) with the unexposed firms (i.e., $\Delta Distance = 0$ mile), and then also compare the highly exposed (i.e., $\Delta Distance > 400$ miles) with the firms exposed to a lower degree (i.e., $\Delta Distance \leq 400$ miles), where 400-mile cutoff definition is based on the median increase in distance. As shown in Internet Appendix Table IA1, columns (1)-(6), the firm characteristics in 2012 are balanced across both thresholds, except for firm

---

[3]Market division or customer allocation is one of the practices falling under common violations of the Sherman Act.

size and past stock return. We also correlate each characteristic in 2012 with $\Delta Distance$ in separate regressions. In column (7), the regression coefficients are not statistically significant except for sales growth. We control for these characteristics in our subsequent regressions, and moreover, in Section 4.3.2, we provide robustness tests in matched samples based on balanced firm characteristics.

Last, we study whether firm attributes show divergent trends based on their exposure to varying degrees of regulatory change. As shown in Internet Appendix Figure IA3, Panel A, the differences in total compensation, stock returns, firm size, and sales growth between firms with $\Delta Distance$ of zero miles or greater remained stable before 2013, while CEO tenure displayed a marginally declining trend in treated firms compared to their control counterparts. Similar patterns appear in Panel B, wherein firms are distinguished by $\Delta Distance$ being above or below 400 miles. Such divergence in CEO tenure disappears in the tests based on matched samples that we discuss in Section 4.3.2.

4   Main empirical findings

We focus on the role played by firms' product market peers in determining CEO pay after the DoJ policy change of closing regional offices. Even absent considerations of collusive incentives, the sensitivity to peer performance might be positive or negative depending on the relative strength of information effects and the strategic considerations. On the one hand, rewarding CEOs based on their performance relative to that of peers can have powerful incentives, since peer performance provides information on the common shocks that are outside of CEOs' control (e.g., Holmström (1982)). On the other hand, pay-to-peer-performance sensitivity can affect managers' competition strategies, depending on the competition mode in the industry (e.g., Aggarwal and Samwick (1999a), Vrettos (2013)). A positive sensitivity provides motivation to withhold competition or coordinate with product market peers, while a negative sensitivity encourages aggressive competition and out-performance, thus the op-

timal sensitivity is positive when firms compete as strategic complements but negative when they compete as strategic substitutes.

This is particularly true in the case of the local peer firms. On the one hand, local firms share the same market for labor inputs and real estate, and sometimes the same pool of local customers. In this case, local peers' performance could be quite informative about the common shocks faced by the company, and thus the optimal loading of executive pay on local peer performance is likely to be negative. On the other hand, cooperating with local peers could be more feasible than cooperating with non-local peers, and thus the compensation sensitivity to local peer performance might be less negative as compared to non-local peers.

We consider how this trade-off changes with the antitrust enforcement. If the closure of the regional DoJ offices reduced the affected firms' expected costs of collusion, it should have made it more attractive for firms to compete less aggressively in their product markets. Thus, the policy reform should have also affected shareholders' strategic considerations when designing CEO compensation, i.e., after the policy change, the affected firms' CEO compensation should have put a more positive loading on local peer performance, irrespective of whether the absolute sensitivity was ex-ante positive or negative. We discuss more details on the assumptions underlying our predictions in Internet Appendix IA1.

The loading on own firm performance was correspondingly expected to decrease. Given that collusive equilibrium displays strategic complementarities, under the optimal contract a more positive loading on peer firm performance should be associated with a lower positive loading on own firm performance.

4.1 Baseline effects

We follow the empirical specification which has been widely used in studies on relative performance evaluation (e.g., Albuquerque (2009)). We focus on the implicit benchmark firms used to determine CEO pay, rather than the explicit benchmark group disclosed in firms' proxy statements. We do so because firms do not always report the complete set of

peer firms for performance benchmarking. As pointed out by Jayaraman et al. (2021), even if firms do not disclose the peer groups explicitly, they may rely on implicit peer groups for performance evaluation. Firms with similar products are not only subject to the common shocks to performance but they are also the main competitors with each other. Therefore, we regard the firms with similar products as the potential performance benchmark peers and examine the correlation between their stock returns and CEO pay, irrespective of whether they were mentioned explicitly in the compensation benchmark groups. We focus on stock returns as the measure of firm performance since stock price, if it is efficient, should take into account all future returns to the shareholders from the collusive schemes.

Before proceeding with our baseline test, we first establish the validity of product market peers in effectively representing the benchmark group for implicit relative performance evaluation. As detailed in columns (1)-(2) of Table 4, during the sample period spanning from 1994 to 2018, we observe statistically significant and positive correlation between CEO compensation and the stock returns of their own firms, and negative correlation with the stock returns of the peer firms. Furthermore, as indicated in columns (3)-(6), we find a statistically significant and negative relationship between CEO compensation and the stock returns of local peer firms, underscoring the significance of local peers as important benchmarks in determining CEO pay.

In our baseline test, we are interested in whether the sensitivity of total compensation to the stock price performance changed after the closure of DoJ offices. We thus estimate the following difference-in-differences specification:

$$
\begin{aligned}
Ln(Compensation_{i,t}) = &\beta_1 \times \Delta Distance_i \times Post_t \times Ln(Return_{i,t}) + \\
&\beta_2 \times \Delta Distance_i \times Post_t \times Ln((Local)\,Peer\,Return_{i,t}) + \quad (1) \\
&K_{i,t} + X_{i,t} + \tau_t + \gamma_i + \epsilon_{i,t}
\end{aligned}
$$

$Ln(Compensation_{i,t})$ refers to the natural logarithm of total compensation or to a certain component of CEO compensation. $Post_t$ refers to the post dummy which is equal to one for

14

years on or after 2013. $\Delta Distance_i$ refers to the increase in geographical distance between headquarter of a firm and a governing antitrust field office after the DoJ office closures in 100-mile units.[4] This variable is zero for the firms for which covering field offices does not change or the distance to new field offices is shorter than before.

$K_{i,t}$ refers stock return variables, i.e., $Ln(Return_{i,t})$ and $Ln((Local)\,Peer\,Return_{i,t})$ and their interaction terms with $Post_t$ and $\Delta Distance_i$. Following the literature (e.g., Albuquerque (2009)), we measure firm stock performance using the natural logarithm of one plus annual stock return. $Return_{i,t}$ refers to firm i's own stock market return in year t, $Peer$ $Return_{i,t}$ refers to average stock market return of peers firm i in year t, i.e., the firms with Hoberg-Phillips product similarity score within top 70% of firm i in year t,and $Local\,Peer$ $Return_{i,t}$ refers to average stock market return of peers headquartered within 200 miles from firm i. $X_{i,t}$ refers to the control variables, including all individual components of interaction terms, and the firm and CEO characteristics including firm size, sales growth, and CEO tenure. τ_t and γ_i refer to the year- and firm-fixed effects. Standard errors are clustered at the state level, since although $\Delta Distance$ varies firm by firm, 98.7% of the total sum of squares in $\Delta Distance$ can be attributed to variations between states.

In this specification, the estimates of β_1 and β_2 reflect how the sensitivity of CEO pay to own-firm performance and to peer-firm performance, respectively, changed in response to the DoJ policy change. As discussed before, we expect β_2 to be positive as less aggressive competition should be preferred after the reform, and β_1 to be negative.

Table 5 shows the estimation results for total compensation. In columns (1)-(2), we focus on the sensitivities of CEO pay to own and peer firm returns. In column (1), we incorporate controls for firm and year fixed effects, while in column (2), we further control for the joint fixed effects for year and industry. In the latter specification, we are comparing the treated and control firms in the same year within the same industry, thereby accounting for common industry trends that might be related to the competition disincentives. In both columns, we

---

[4]All our findings are consistent if we use log transformation for the $\Delta Distance_i$ variable.

find a negative β_1 and a positive β_2, which implies that firms employed relative performance evaluation to a lesser extent after the diminished antitrust oversight.

In columns (3)-(4), we replace the average peer return with the average return of *local* peers. In cases where a firm did not have local peers in the year prior to the reform, we set *Local Peer Return* as zero. We further include a binary variable that denotes the firms with local peers, labeled as *Local Market*, along with its interaction with *Post* and $\Delta Distance$. With this specification, we are comparing more-exposed firms with less-exposed firms, where the latter either were less exposed to the policy reform or did not have local peers. We find β_2, which measures the change in the relationship with the local peer returns, to be positive and statistically significant and β_1, which measures the change in the relationship with the own returns, to be negative and statistically significant.

In columns (5)-(6), we narrow our focus to the sample of firms with local peers only and estimate the same regression specification. With this specification, we compare more-exposed firms with less-exposed firms, considering only those firms that had local peers. We exclude the dummy indicator of *Local Market* and its interaction terms. Once again, we find a statistically significant positive coefficient of β_2 and a statistically significant negative coefficient of β_1. We will consider the last two columns of Table 5 as our baseline specifications in the subsequent comparisons.

To estimate the economic magnitudes of our findings, we take column (6) of Table 5. As a benchmark, the coefficient on the level effect of $Ln(Local\ Peer\ Return_{i,t})$ is -0.053, which indicates that for untreated firms prior to the reform, a 1% lower local peer performance was associated with a 0.053% higher CEO compensation. The coefficient of β_2 is 0.018, which suggests that the weighted average causal response of the local peer-performance sensitivity to a 100-mile increase in the firm's distance to the DoJ field office is 0.018.

Similarly, the coefficient on $Ln(Return_{i,t})$ is 0.092, which translates to a 0.092% increase in CEO pay in response to a 1% increase in own stock return for the untreated firms. Moreover, the coefficient of β_1 is -0.016. This suggests that the DoJ reform has led to

a weighted average causal response of -0.016 of pay level change to the own performance sensitivity.

Overall, this evidence supports the argument that firms adjust managerial compensation schemes to accommodate shareholders' preference for softer competition. Lower expected antitrust enforcement against collusion reduced the incentives for the firms to outperform peer firms with whom they have a possibility of colluding in the local product markets.

4.2 Discretionary compensation

We further examine whether the changes in pay-to-performance sensitivities can be attributed to the board's compensation decisions. A possible interpretation of the results in Table 5 is that the board did not adjust compensation in response to the DoJ office closure, while the CEOs of treated firms found collusion more attractive under their original compensation contracts. Reflecting higher profits, firms' equity value rose and stock grants became more valuable. Since stock prices co-move more in collusive equilibrium, we might mechanically observe greater sensitivity of equity compensation value to peer performance even without boards actively adjusting compensation.

To rule out this possibility, we look at which component of compensation – cash or equity – drives the changes in pay-to-performance sensitivity. If the relationship appears mechanically, we expect to observe a greater increase in pay-to-performance sensitivity for the equity component of CEO pay.

We further note that the value of cash compensation is subject to higher discretion from the board. Although discretionary compensation can be paid both in cash and equity (De Angelis and Grinstein, 2015), the cash component is generally more flexible to adjust as it often involves the board's judgment (Ma, 2020). For instance, incentive plans often include qualitative performance measures such as "strategic goals" and the board has the discretion to determine whether the outcomes of such goals are met. In our sample, we see that 89.8% of the "strategic goals" appear as a performance measure in the cash incentive

17

plans rather than in the equity plans. Appendix B lists a few examples of such "strategic goals" from DEF14A filings and we further study them in Section 5.2. If the board actively adapts contract design in response to the policy reform, we should expect a larger effect on the cash component of CEO compensation.

In Table 6, columns (1)-(2), we report the same specification as in columns (5)-(6) in Table 5 but with cash compensation as the outcome variable. In addition, in columns (3)-(4), we report the same specifications where the outcome variable is the equity compensation. We find that the effect is indeed driven by the cash compensation, while the effect on equity compensation is not statistically significant. The finding highlights that boards' discretionary practice plays an important role in adapting compensation after the DoJ office closures.

4.3 Alternative specifications and robustness tests

We estimate alternative specifications and additional robustness tests to support our analysis as follows: (a) we use a binary treatment, (b) we provide estimates based on matched pairs, (c) we control for possible confounding economic trends, (d) we provide findings based on the alternative classifications of product market peers, (e) we separately look at the CEO compensation sensitivity to local and non-local peers, and (f) we investigate whether our findings are robust to alternative regression choices. Unless otherwise stated, we re-estimate our baseline model, i.e., columns (5)-(6) of Table 5. We briefly introduce the tests in this section and summarize the main findings in Figure 2 and Table 7 with more detailed descriptions reported in Internet Appendix Table IA4 and the full tables provided in Internet Appendix Table IA2, Table IA3, Table IA4, Table IA5, Table IA6, Table IA7, Table IA8, Table IA9, and Table IA10.

4.3.1 Binary treatment

We create binary variables that indicate whether $\Delta Distance$ exceeds specific threshold values. We anticipate that our baseline effect will persist across various positive thresholds but

will be stronger for larger values of $\Delta Distance$ as further located field offices are likely to engage in significantly lower oversight. We thus substitute $\Delta Distance$ in regression (1) with a binary variable indicating whether $\Delta Distance$ exceeds a given threshold value x within the range of 0 to 1000 miles. The upper limit of this range corresponds to approximately the 95th percentile of the distribution of $\Delta Distance$ within our regression sample. Figure 2 shows that the point estimates of β_1 and β_2 are negative and positive, respectively, across all cutoff values. However, smaller cutoff values are also associated with larger estimation errors, resulting in statistically insignificant estimates. The coefficient values increase and the standard errors drop as the threshold x rises, and they are consistently statistically significant at the conventional levels for relatively high cutoff values. Specifically, β_1 is statistically significant for any threshold $\Delta Distance \geq 500$, while β_2 is statistically significant for $\Delta Distance \geq 400$. Note that firms with $\Delta Distance$ above 400 miles are predominantly located within the original jurisdictions of the Dallas and Atlanta field offices. We report the coefficients based on the 400 mile split in Table 7, Panel A, and the full table in Internet Appendix Table IA2.

4.3.2 Matching

We address the possible ex-ante differences in firm characteristics between affected and un-affected firms with propensity score matching. Based on the previous analysis, we consider two separate thresholds of $\Delta Distance$ to define the treated and untreated firms, i.e., 0 and 400 miles. The firm characteristics that we consider include own and local peer returns, firm size, sales growth rate, CEO tenure, and industry dummies. As shown in Internet Appendix Table IA3, Panel A, Figure IA4, and Figure IA5, (a) firm characteristics are not statistically significantly different at the conventional levels between the treated and control groups; (b) the fitted densities of the estimated propensity score of the treated and control firms resemble each other; (c) the firm characteristics do not show divergence between the treated and matched control firms prior to the reform. In summary, the matched samples appear to be

19

well-balanced between the treated and control firms. As summarized in Table 7, Panel B, our baseline findings are robust when estimated based on these matched samples. The full table is reported in Internet Appendix Table IA3, Panel B.

We also adopt entropy balancing approach which essentially re-weights our sample by balancing the first moment of covariate distributions across the treated and control group in the year before the shock (see, e.g., Hainmueller (2012)). The covariates we consider are the same set of firm characteristics that we used in the propensity score matching. Internet Appendix Table IA4, Panel A, shows that the re-weighted samples are well-balanced and have a relatively larger sample size than the propensity-score-matching sample. As shown in Panel C of Table 7 and Internet Appendix Table IA4, Panel B, the main finding is robust to entropy balancing adjustments. The coefficients of the triple difference terms, if anything, are even larger than those based on our baseline estimates.

4.3.3   Confounding economic trends and omitted variables

A related concern is that the findings could be driven by diverging economic trends between states that started in the period preceding the 2013 regulatory change. Although Internet Appendix Figure IA2 does not suggest that the economic performance started diverging between states before 2013, we conduct placebo tests by defining the post-shock period as the years since 2006 and adjusting the sample period to 2001-2010, accordingly. We report these estimates in Panel D of Table 7 and Internet Appendix Table IA5. We do not find that the findings are statistically significant if we consider a placebo year instead of the actual year when the antitrust field office reform was implemented.

To assess the robustness of our estimates to unobservable omitted variables, we also employ the bounding method of Oster (2019). This method gauges the extent to which an omitted variable would need to account for variation to reverse the sign of an estimated coefficient. Internet Appendix Table IA6 presents this analysis for the coefficients β_1 and β_2 of columns (1)-(6) of Table 5 and indicates that the inclusion of covariates reinforces our

findings, implying that unobserved omitted variables are unlikely to drive them.

4.3.4 Alternative peer definitions

In the main analysis, we define local peers to be within the top 70% of Hoberg-Phillips similarity scores to focal firms. We provide tests using alternative definitions of peers. First, in Panel E of Table 7 and Panel A of Internet Appendix Table IA7, we show that the findings are consistent for the thresholds of the top 30% and 60%, while they are weaker when we do not impose any threshold and include all the Hoberg-Phillips peers. This suggests that only close peers are relevant for the strategic concerns in CEO compensation design.

Second, we provide robustness to peer classification choices. To begin with, we impose an additional constraint that local Hoberg-Phillips peers should also be similar in terms of firm size and valuation (Jayaraman et al., 2021). Next, we apply peer classifications other than the Hoberg-Philips method: (a) we define peers as the firms providing substitute goods estimated using the cross-price demand elasticity in Pellegrino (2023); (b) we use peer firms as recorded in Factset Supply Chain Relationships (formerly, Revere); (c) we define peers as the firms covered by common analysts (Kaustia and Rantala, 2021). The findings are reported in Panel F of Table 7 and in Panel B of Internet Appendix Table IA7.

Finally, we investigate different definitions of locality in Table 7, Panel G, and in Internet Appendix Table IA8. In our baseline tests, we define local firms as the ones headquartered within 200 miles. Alternatively, we define local peers to be the firms headquartered within 100, 300, and 400 miles, and find an increase in pay-to-peer-performance sensitivity following the reform, irrespective of how we set up the radius. Defining local peers by the smallest radius, 100 miles, provides the largest economic magnitudes.

4.3.5 Non-local peers

We now separately estimate the effect for local and non-local peers by adding the average return of non-local peers and its interaction terms to specification (1). The findings are

summarized in Table 7, Panel H, and full findings are in Internet Appendix Table IA9. The increase of pay-to-peer-performance sensitivity is concentrated among local peer firms whose incentive to collude became stronger after regional DoJ office closures, while the change in pay sensitivity to non-local peers is not statistically significant.

4.3.6   Other regression choices

We perform a number of other robustness tests regarding the regression setup. First, 57 out of our total sample of 1,400 firms became closer to a field office. These firms are mainly located in Maryland and Virginia. Our findings are robust to dropping them from our sample. Second, we winsorize the main variables at the 1 and 99% level instead of 0.5 and 99.5% as we do in our baseline specifications. Third, we estimate a Poisson regression. Fourth, we cluster the standard errors separately at the firm level, ZIP code, SIC 2-digit industry level, and the pre-shock DoJ region level. Our findings are robust to these choices, as reported in Panel I and J of Table 7 and Internet Appendix Table IA10. We further study if the imbalanced clusters distort our baseline findings by employing the Jackknife-based Cluster Robust Variance Estimator, following Hansen (2022), MacKinnon et al. (2023a) and MacKinnon et al. (2023b). The corresponding t-statistics for β_1 are -9.612, -1.788, and, -1.232 for state, ZIP code, and firm level clustering, respectively, and t-statistics for β_2 are 6.292, 2.009, and, 1.700, respectively, for the specification reported in column (6) of Table 5.

Finally, when defining our local peers, we remove those peers that have an overlap with focal firms in terms of top five blockholders, where blockholders are defined as having more than 5% of stakes in firm's equity. These findings, available at request, are consistent with Table 5, suggesting that we identify a different channel from common ownership which has also been shown to shape managerial incentives (Antón et al., 2023).

4.4 Heterogeneous effects

We next explore the heterogeneous impact of the DoJ regional office closures on compensation practices based on several firm characteristics and market conditions such as competition mode, geographical concentration of firm operations, industry concentration, board independence, CEO age, and executive labor market mobility. We present the summary of the sample splits for our main coefficients of interest in Table 8, and report the full tables in Internet Appendix Table IA11.

First, the optimal compensation structure depends on firms' competition mode. As noted by the industrial organization literature (e.g., Potters and Suetens (2009)), collusive arrangements are more likely to form among firms that compete as strategic complements, as in the Bertrand games. We expect that the weakening of antitrust enforcement encouraged less competition primarily among such firms. We thus partition firms into those whose decision variables are more likely to be strategic complements (e.g., Bertrand competition) and those whose decision variables are more likely to be strategic substitutes (e.g., Cournot competition). We follow Kedia (2006) and Bloomfield (2021) in constructing the measure of competition mode for each industry, and find a stronger effect among the industries in which firms compete as strategic complements.

Second, we investigate the industry concentration. Motta (2004) argues that a smaller number of players is the most important cross-sectional factor empirically predicting collusion, and Huck et al. (2004) provide supporting experimental evidence. We expect that firms in the concentrated industries are more likely to change CEO compensation and form collusive arrangements after the reform, since the coordination among a limited number of cartel members might be easier. To study this, we measure industry concentration by the revenue percentage of the eight largest firms in each NAICS industry, as reported by U.S. Census Bureau in 2012. Our sample is divided based on whether firm's industry was in the top or bottom quartile based on the 8-firms' revenue share in that industry. The effects are stronger for the firms in the more concentrated industries.

Third, we look at whether the effects are stronger in the industries with a higher prevalence of publicly listed (as opposed to privately held) firms. Presumably, more publicly listed peer firms and thus better observability of financial and stock performance of peer firms allow more precise contracting on peer firm performance than in the cases where such peer firms are privately held. We follow Badertscher et al. (2013) to measure the proportion of private firms in the NAICS industry, and we split the sample based on the median value of the proportion of private firms in 2012. The effects are stronger in the industries with a higher fraction of public firms.

Fourth, we expect the effect to be stronger for firms that have their operations more concentrated geographically, i.e., firms that operate primarily in the local markets. For such firms, the closures of the field offices should be more relevant as compared to the firms with operations that are spread across the U.S. In particular, since the antitrust field offices are monitoring with the help of local knowledge, their closure should be more relevant for the local-level collusion. Therefore, the firms with significant local operations should be more responsive to the change in the antitrust field offices.

We use two sources of data to estimate proxies for local operations. The first source is the Lexis Nexis Corporate Affiliations dataset on firm subsidiary locations, from which we estimate the geographic distribution of the firm's sales among the states where the subsidiaries and headquarters are located. In particular, for each firm we calculate the overall distribution of the firms' operations across states and estimate the sales concentration index (HHI) within firm. In this case, the firms are assigned as "concentrated" ("dispersed") if such an HHI index falls into the top (bottom) 30% across all firms' HHI indices.

Given that subsidiaries might not fully represent firm operations, our second source is the number of states mentioned in 10Ks. We take the last year of data in Garcia and Norli (2012), which for different firms is either 2007 or 2008. Again, for each firm, we estimate the concentration index (HHI) of state mentions and assign firms as "concentrated" ("dispersed") if such HHI index falls into top (bottom) 30% across all firms' HHI indices. Across both

data sources, the effects are larger for the firms with more concentrated operations.

Fifth, we investigate board governance. Our interpretation is that after the antitrust policy reform, the board of directors adjusted CEO compensation on behalf of shareholders to discourage competition in the product market. Alternatively, the CEOs might proactively choose to collude and influence the board to adjust their compensation contracts (González et al., 2019). The former argument would predict stronger effects among firms with the board that is more loyal to the shareholders as opposed to the CEO. We follow Coles et al. (2014) and consider proportion of directors who were appointed after the CEO assumed his or her job (so-called "co-opted directors") and split the sample based on the median fraction of co-opted directors for the post-reform period. We find that our findings are stronger among the firms with a lower fraction of co-opted directors, i.e., those that that are likely less disloyal to shareholders. That is consistent with the interpretation that the boards proactively adjusted CEO compensation in response to the reform on behalf of the shareholders.

Sixth, we examine whether the changes in contract structure vary depending on the CEO's intrinsic motivation. We investigate two dimensions. The first one is based on whether CEOs are approaching retirement age. The retiring CEOs are likely to be more short-term oriented and less willing to participate in collusion at personal legal risks, especially since the maximum imprisonment sentence has been raised from three to up to ten years by Antitrust Criminal Penalty Enhancement and Reform Act (ACPERA) in 2004. To provide incentives for collusion, the board of directors needs to adjust their compensation contracts. We split the sample by whether in 2012 the firm's CEO is older or younger than the median age, by which we assume that older CEOs are closer to retirement than younger ones. The effects are concentrated in the subsample of CEOs closer to retirement.

The second personal dimension is based on whether the CEO faces a more flexible executive labor market. We posit that more flexible executive labor markets create higher reputational concerns for CEOs since they might be poached by other corporations. As such reputation concerns could generate a larger wedge between managerial and shareholders'

intrinsic incentives, we expect that compensation designs of CEOs in more flexible labor markets are more sensitive to the shareholders' expected payoff of motivating collusion. As a source of variation in the labor market flexibility, we look at whether the firms are located in states with the Inevitable Disclosure Doctrine (IDD). IDD prevents the firm's workers who have knowledge of its trade secrets from working for a rival firm (Klasa et al., 2018). As IDD limits the outside job opportunities of CEOs, we expect CEOs in the firms headquartered in the IDD states to have fewer reputational concerns in the external labor market, which should reduce the intention of the shareholders to adjust compensation scheme to motivate collusive behavior. To test this, we split the sample based on whether the courts of the state of firms' headquarters recognized IDD or not in 2012. We find that the antitrust reform had a larger impact on CEO compensation schemes for the firms that are located in the states that did not recognize IDD in 2012.

4.5   Incentive alignment and firm outcomes

Successful coordination in product markets should be associated with larger markups and higher comovement in competitors' performance. We proxy for the firm markups by gross profit margins and for the comovement in rivals' performance by their stock price correlation.

We first investigate whether firms' profitability changed in response to the DoJ office closures. In Figure 3, we plot the annual differences in gross profit margin between the treated and untreated firms based on two separate thresholds.[5] Treated firms are defined as the ones that experienced either more than 0 or more than 400 miles of increase in distance to the covering DoJ office. For both definitions, we find that while the difference was close to zero and constant before the reform, it increased significantly right after 2013. The evidence suggests that the reform might have had an anti-competitive effect and increased the treated

---

[5]We focus on gross margins rather than markups as in Deng (2023), who also examines DoJ office closures. Deng (2023) applies the production function methodology (De Loecker et al., 2020) in markup estimation, assuming a certain cost minimization function. However, this function does not consider potential legal costs from collusion and regulatory detection, which might result in the markup mis-estimation in the pre-reform period. As revising markup estimation involves untested assumptions, we opt for observable gross profit margin measures, which are also more likely used by antitrust agencies or courts.

firms' profitability relative to that of the control firms.

To understand whether anti-competitive effects were associated with our identified change in pay-performance sensitivity, we now examine changes in gross profit margins in a triple difference-in-difference setting. We expect that the increase in profitability should be stronger in the industries in which the treated firms were more responsive to the reform in terms of changing their CEO pay. We thus construct an industry-level measure of the reform-induced change in sensitivity of CEO pay to local peer performance. Specifically, we estimate the β_{2j} coefficient from separate regressions (1) for each SIC 2-digit industry, j, and refer to it as change in pay-to-local-peer-performance sensitivity, namely $\Delta PLPS_j$. We then estimate a regression with the following specification using the sample of firms with local peers:

$$
\begin{aligned}
Profit\ Margin_{i,j,t} =& \beta_1 \times Post_t \times \Delta Distance_i \times \Delta PLPS_j \\
&+ \beta_2 \times Post_t \times \Delta Distance_i + X_{i,t} + \tau_{j,t} + \gamma_i + \epsilon_{i,t}
\end{aligned}
\tag{2}
$$

where for each firm i of industry j and year t, $Profit\ Margin_{i,j,t}$ corresponds to the gross profit margin, i.e., the gross profit over revenue. $Post_t$ is the dummy indicator for the years on and after 2013. $\Delta Distance_i$ refers to the increase in geographical distance between firm's headquarter and a governing antitrust field office after the reform in 100-mile units. We control for firm characteristics, firm fixed effects, and the joint year and industry fixed effects at the SIC 2-digit level.

This triple difference methodology allows us to control for the general industry-wide trends as we rely on the incremental post-reform change in the peer performance sensitivity by the treated group as compared to the same-industry control group. Thus, the estimate of β_1 in specification (2) captures the heterogeneity in reform-induced change in gross profit margins across industries associated with different degrees of CEO compensation adjustments. Since a more positive $\Delta PLPS_j$ reflects a greater anti-competitive incentive provision, we expect β_1 to be positive. We find supporting findings in column (1) of Table 9: the increase in profitability induced by the reform was concentrated among the industries in

which after the reform treated firms' CEO pay became more positively sensitive to the local peer performance as compared to the control firms' CEO pay. This finding is consistent with the argument that a more positive peer performance sensitivity discourages competition.

Next, we examine whether this finding is robust to individual firm level change in compensation that we estimate from the single-difference estimations comparing the local peer performance sensitivity after the shock to that before the shock. In particular, we estimate the incremental sensitivity of CEO pay to local peer performance after the reform based on separate regressions for each firm. The dependent variable is the natural logarithm of total compensation and independent variables include stock return, local peer return, the post-reform period dummy, and the interactions of the post dummy with the firm's own return and the local peers' return with the post dummy. These variables are measured in the same way as those used in the regression (1). We construct a dummy indicator for the coefficient estimate for the interaction term between the post dummy and local peer return to be positive and statistically significant at the 90% level, and we refer to it as $D(\Delta PLPS_i > 0)$. This indicator reflects whether a firm significantly increased the sensitivity of CEO pay to the local peers' performance after the reform. We then use $D(\Delta PLPS_i > 0)$ instead of $\Delta PLPS_j$ in the specification (2) above. As shown in column (2) of Table 9, the triple interaction of $\Delta Distance \times Post \times D(\Delta PLPS_i > 0)$ is positive and statistically significant.

Moreover, coordinated actions are likely to result in product prices changing in tandem, predicting greater comovement among the peer firms. We next study whether treated firms' performance started moving together more with the performance of their peers. We do not have access to the product price data across different firms and industries, so we cannot capture high-frequency comovement of firm operating performance. Thus, we use stock return comovement as its proxy. We calculate annual correlation of the firm's weekly stock price returns with each of its local peers, and take an average across the local peers.

Following the same specification as in regression (2), but with stock return comovement as the outcome variable, in columns (3)-(4), we show that after the regulatory reform,

28

stock return comovement increased more for the exposed firms or industries that had more positively adjusted peer performance sensitivity. This is consistent with the lower cost of collusion leading to more coordinated performance with the local product market peers.

Finally, we provide descriptive evidence on whether the improvement in profitability of the treated firms is likely to be an outcome of local market interactions. In Internet Appendix Table IA12, we report the number of local peers that experienced an increase (or decrease) in profitability during the period of 2013 to 2017 relative to the period of 2007 to 2012. First, more of the exposed firms increased their gross profit margins (482) than reduced them (347), consistent with the overall improvement of profitability. More importantly, we find that among the exposed firms that increased gross profit margins since 2013, on average 14 local peers had increased margins, but on average only 4 local peers had decreased margins. On the other hand, for the exposed firms that experienced a decrease in gross profit margin, on average 8 of their local peers also had decreased margins, but 6 local peers had increased margins. The evidence supports the interpretation that local firms, when exposed to the DoJ office closures, have experienced changes in the gross profit margins that were congruous within the industry.

5   Other features of executive compensation

In this section, we examine the changes of other features of CEO compensation, including the use of explicit relative performance evaluation, the choice of performance metrics, the vesting horizon of equity compensation, and the structure of compensation package.

5.1   Explicit peer groups

First, in addition to implicit relative performance evaluation, we look at firms' *explicit* disclosure of the relative performance evaluation (RPE) provisions in CEOs' incentive plans. If such disclosures (or lack thereof) are based on how the board's assesses CEO performance,

we would anticipate a reduced adoption of explicit RPE following the decline of antitrust enforcement. On the contrary, Bloomfield et al. (2023) argue that the costly incentive for sabotage associated with RPE could be curbed by cartel arrangements, potentially resulting in a more widespread adoption of RPE in a low-antitrust environment.

Our first observation is that only 37% of the firms in our sample explicitly reported RPE provisions in 2012, the year before the DoJ office closures. Second, we zoom into the firms with explicit performance peer groups and examine their unconditional inclusion of local peers. On average, 23.98% (17.79%) of the local peers are included as the benchmark firms of equity (cash) RPE plans, while 9.35% (10.34%) of the equity (cash) RPE benchmark firms are local firms.[6] These relatively modest percentages suggest that firms may have deliberately excluded certain local peers from their RPE benchmarks, potentially to avoid engaging in aggressive competition with them.

We then examine whether there was a shift in the likelihood of employing RPE in CEOs' compensation packages following the DoJ reform. In columns (1)-(2) of Table 10, Panel A, we do not find statistically significant evidence of such changes. Next, in columns (3)-(6), we delve deeper into the design of relative performance benchmarks, specifically examining the use of an index or a specified peer group.[7] Our findings reveal that conditional on granting incentive plans with explicit RPE provisions, there was no significant change in the design of the benchmark in response to the reform. The absence of changes in predefined groups is in line with our previous findings in Section 4.2 that boards follow ex post discretion when building compensation arrangements that facilitate product market coordination.

However, when we study the composition of explicit performance peer groups in more detail, we find that the proportion of product market peers included in the peer group decreased to a larger extent for the more exposed firms after the reform. We report these

---

[6]Please see Internet Appendix IA13 for more detailed analysis.

[7]We obtain the information from Incentive Lab. We categorize a benchmark as an index if the plan details mention "S&P500" or "Index" and as a specified peer group if the identities of peer firms are disclosed. The use of an index or specified peer group is not mutually exclusive, as some firms may refine their peer group from an index in the market. Thus, we analyze these two indicators separately as dependent variables.

findings in Panel B of Table 10, columns (1)-(2). Also, in column (3), we find a larger reduction in the proportion of local peers for the more exposed firms, although this effect lacks statistical significance after we control for the year-industry joint fixed effects in column (4) possibly due to the small sample size.

Further, we investigate peer group overlap, i.e., the extent to which two firms select each other as relative performance evaluation benchmarks. This overlap can have a pro-competitive effect, as proposed by Feichter et al. (2022). We expect that firms exposed to the reform may adjust their benchmark groups to achieve a lower overlap following the DoJ reform. In columns (5)-(6) of Table 10, Panel B, we examine whether the exposed firms were more likely to select reciprocal or non-reciprocal peers, when they added new firms to the performance benchmark group. Our finding in column (6) suggests that the selection of new peers became more tilted toward non-reciprocal firms in response to the DoJ reform, which would lead to a reduced overlap of the peer group. However, when we examine the removal of peer firms, in the untabulated tests, we do not observe a statistically significant change in the tendency to choose reciprocal versus non-reciprocal peers.

Collectively, our findings indicate that while the inclination to adopt explicit RPE provisions remained largely unaffected by the decrease in antitrust enforcement, those firms that did incorporate RPE provisions adjusted the composition of peer groups to mitigate aggressive competition. This aligns with the arguments put forward by Karpoff (2023) that it is highly improbable for firms to engage in costly sabotage to gain an RPE advantage.

5.2 Performance metric changes

We next investigate whether other features of incentive plans were actively adjusted in response to the DoJ office closures. In particular, we study the changes in performance metrics used to set own performance targets (i.e., not targets used to benchmark to the explicit peer groups) for incentive plans. We focus on three types of targets: strategic goals, profit margin, and sales. We expect an increase in the use of less precisely measurable and thus more

discretionary "strategic goals" metric when determining pay amounts. We also expect that if company goals shifted away from aggressive competition, "profit margin" target should become more important in assessing CEO performance, while expanding output should become less encouraged. Few firms directly provide targets for output but we might expect some reduction in the "sales" targets that take into account both product prices and output.

Table 11 shows that the performance measures of "profit margin" and "strategic goals" were more frequently adopted in determining the CEO compensation for the treated firms after 2012, while "sales" was used less frequently. Managers who experienced such changes in their performance evaluation functions were thus likely to focus more on retaining high profit margins or achieving strategic goals instead of expanding the firm's production. Moreover, we find that the design of both cash and equity incentive plans changed. The frequency of "strategic goals" and "profit margin" changed for the cash plans, while the frequency of "sales" changed for the equity plans. Overall, this evidence complements the findings on pay-to-performance sensitivity by suggesting that boards made active adjustments to incentive contracts and these adjustments were consistent with higher incentives for collusion.

5.3   Vesting horizon

To delve deeper into the compensation arrangements, we next study the vesting horizon of CEOs' long-term incentive plans. Collusive strategies are unstable by nature even without antitrust enforcement, since a myopic player might deviate from the arrangement and gain a large short-term profit by undercutting the cartel peers. Compared to shareholders that might be concerned with the long-term value, managers could behave more myopically, partly because they have stronger career and reputational concerns. Outperforming market rivals in the short run may increase the manager's outside options in the labor market. Therefore, after the antitrust policy reform, shareholders with increased preference for collusion may find it beneficial to align the manager by providing more long-term incentives. In particular, since stock and option compensations are usually associated with restrictive periods that

tie managerial payoff to the firm's long-term stock returns, longer vesting terms effectively reduce managers' discount rate in their strategic interactions with competitors. This can effectively reduce managers' myopic incentives to engage in overly aggressive competition that is not in the shareholders' long-term interest.

We obtain the vesting schemes of time-vesting equity incentive plans from Incentive Lab. We measure the expected vesting period of each incentive plan as the number of months until the last vesting date for cliff vesting plans, and the average number of months between the first and last vesting dates for ratable vesting plans. If multiple plans were granted within one year, we take the maximum value of the expected vesting period.[8]

We study whether this vesting horizon is affected by the DoJ reform by regressing the expected vesting period on the interaction of $\Delta Distance$ and the post-reform period dummy and controlling for lagged firm size, sales growth, and CEO tenure, as well as firm fixed effects and joint year and industry fixed effects. As reported in Table 12, column (1), the interaction term is positive and statistically significant, which suggests that after the reform, affected firms lengthened the vesting horizon compared to the unaffected firms. Columns (2)-(3) report that this finding is robust to replacing $\Delta Distance$ with a binary variable and clustering the standard errors at the firm level. Finally, in columns (4)-(5), we report a cross-sectional test from Section 4.4 related to CEO reputation concerns in labor markets. The effect is indeed concentrated in the states in which courts did not recognize IDD, i.e. where the CEOs were facing a more mobile labor market and potentially cared more for their shorter-term reputations relative to long-term shareholder value. These findings suggest that the reform induced the affected firms to provide long-term incentives that could potentially foster collusion, especially if the CEO had more flexibility in the labor market.

---

[8]We focus on time-vesting equity plans due to their role in stabilizing employment relationship, which is a crucial factor in sustaining collusive arrangements in the product market. Our findings remain robust when using the value-weighted duration of time-vesting equity plans. However, we do not observe that the overall duration, when all plans are considered, changed in response to the reform. This is because equity plans with the RPE provisions experienced a reduction in duration. Building on our earlier findings in Section 5.1, this implies that, as collusion became more attractive, product market peers were likely to be excluded from the RPE peer groups, and the remaining RPE plans shortened their horizon, emphasizing incentive provision over competitive concerns.

5.4 Compensation structure

Finally, we examine the level and structure of total compensation. As noted by Antón et al. (2023), powerful incentive plans induce managerial effort to improve productivity, which helps firms gain advantages in product markets. In other words, greater incentive provisions through the CEO compensation package could result in more aggressive competition, which might or might not align with the shareholders' interest. As we have discussed, following the decrease of antitrust enforcement, shareholders could have begun to prefer softer competition, and thus reduced high-powered incentive provisions through CEO compensation. For a risk-averse CEO, less incentive provision would require less compensation for risk and thus a lower level of total pay. Thus, the overall compensation level may also decrease.

We test this argument by estimating the regression of total CEO compensation on the interaction of $\Delta Distance$ and $Post$ dummy. We start with the total compensation. Columns (1)-(2) of Internet Appendix Table IA14, Panel A, show that the coefficient of $\Delta Distance$ x $Post$ is negative and statistically significant, suggesting a decreased level of total CEO compensation for the more exposed firms after the policy reform.

Next, we construct the incentive measures of delta for newly granted stocks and options. Columns (3)-(4) of Internet Appendix Table IA14, Panel A, report a decrease in the delta of new stock grants, although this effect becomes not statistically significant after we control for joint year and industry fixed effects. We do not observe a statistically significant change in delta of new options in columns (5)-(6). The columns (7)-(8) report a statistically significant decrease in the delta of CEOs' total equity holdings measured following Core and Guay (2002) and Coles et al. (2006). Collectively, this evidence is partially consistent with the notion that both the provision of incentives and the overall level of pay decreased once aggressive competition was no longer preferred.

Last, in Panel B of Internet Appendix Table IA14, we study the composition of CEOs' compensation packages, as divided into four parts: salary, other cash incentive plans, equity incentive plans, and other compensation. We find that the importance of the incentive com-

ponents, including the cash and equity incentive plans, decreased in response to the reform. In contrast, the fixed components, including salary and other compensation, increased as proportions of total compensation. Taken together with our former findings, this tells that shareholders not only employed implicit relative performance evaluation to a lesser extent but also constrained incentive-based compensation. These findings are in line with the interpretation that compensation design changed toward mitigating product market competition.

6   Convicted cartels

In this section, we present additional evidence on the relationship between CEO compensation and collusive activities using a sample of convicted cartel cases. We sourced information on U.S. cartel members and the initiation year of their collusive activities from Connor (2014) and manually matched this data with stock returns. For each cartel-affiliated firm, we calculated the average stock return of the other public members of the cartel, as well as the return of the Hoberg-Phillips peers who were not involved in the cartel. Our analysis is centered on firms that participated in cartels and were convicted by 2012, which is the final year covered by Connor (2014) data. We specifically focus on the total CEO compensation of these firms for the period from 2000 to 2012.

Columns (1)-(2) of Table 13, Panel A, show that for these firms with a history of convicted collusive arrangements, the unconditional average sensitivity of CEO pay to their own firm's return is 0.3, while the sensitivity to cartel peer return is -0.06. These figures are comparable in magnitude to the sensitivities observed in the main sample in Table 4, columns (1)-(2).

Next, we investigate whether the start of collusion has impacted pay-performance sensitivities. To address this, we introduce a binary variable, "post dummy", which is equal to one for the years following the initiation of a cartel arrangement. In columns (3)-(4), we find that the interaction term between the post dummy and own firm return has a negative and statistically significant coefficient, while the interaction term between the post dummy and

cartel peer firm return has a positive and statistically significant coefficient. On the other hand, in columns (5)-(6), where we further include the average return of non-cartel peers in the product market and its interaction with the post dummy to the regressions, we do not find the coefficient on this interaction term to be statistically significant. These observations suggest that as a firm becomes involved in a cartel arrangement, there is a reduced likelihood of incorporating the cartel peers as benchmark firms for relative performance evaluation.

Further, we investigate this phenomenon by examining the compensation peer groups explicitly disclosed in firms' proxy statements, which we obtain from Incentive Lab data. Panel B of Table 13 shows the overlap between the peers that were convicted in the same collusion case and the peers mentioned in two compensation benchmark schemes: general compensation and relative performance evaluation. General compensation benchmark refers to the peer group companies that are picked to benchmark the overall compensation level for CEO and the other executives.[9] On the other hand, the relative performance evaluation benchmark refers to the peer firms whom the firm needs to outperform for the executives to be eligible for the relative performance awards. If the firm intends to collude in the product markets with the peer firms, such peer firms should not be included in the relative performance group, so that the executives do not have incentives to outperform them.

We manually name-match 416 firm-cartel-year observations from Connor (2014) to Incentive Lab database. Private firms and non-U.S. firms might also be included in the benchmark peer sets. Table 13, Panel B, shows that 20.67% of cartel cases have at least one cartel peer in the general compensation peer benchmark, but only 10.1% cases have at least one peer in the relative performance benchmark. This suggests that cartel peers overlap more with the benchmark firms in general compensation scheme than in relative performance scheme. If we consider the fraction of the cartel peers appearing in the benchmark sets, the respective averages are 7.8% for general compensation benchmark and 3.7% for relative performance

---

[9]Faulkender and Yang (2010) find that this compensation benchmark group appears to be comprised of highly paid peers that can be used to justify the general level of the CEO compensation. Bizjak et al. (2008) show that the general compensation group provides a benchmark for the overall pay level, which plays an important role in retaining valuable human capital.

benchmark. These differences between the benchmark groups are statistically significant.

This finding is consistent when instead of matching the firms in the actual period when cartel was active, we match in the entire period of sample. In this way we are able to match 497 cases, suggesting that in 81 cases (=497-416), the relative performance evaluation benchmarks are not available during the years when cartel is active but they are available in the other years. This is consistent with firms having less explicit performance evaluation with respect to the cartel peers during the active cartel years. In this entire sample, in over half of the cases, at least one cartel peer is included in the compensation peer benchmark and 20.25% of peers are mentioned at least in some year. In terms of relative performance benchmark, the numbers are lower, and a quarter of cartel members mention at least one of their peers, and just 8.09% of peers are mentioned at least once.

7   Conclusion

In this paper we study the relationship between managerial incentives schemes and collusion in the product markets. We explore the decision by the DoJ to close down its four regional offices in 2013, which affected antitrust enforcement of regional collusion cases and thus made collusion a more attractive strategic choice to the affected firms' shareholders.

We study whether executive compensation contracts changed accordingly. We look at the relative performance evaluation, which is typically used to provide incentives for executives to outperform industry rivals. We find that affected firms reduced such incentives by tying CEO pay, especially the cash compensation, more positively to peer firm performance after this regulatory change. Importantly, we see that these compensation changes were associated with changes in firm outcomes. Firms in industries that lowered peer performance sensitivity experienced better product market outcomes in terms of profitability, and also higher stock return comovement with the local product market peers.

Our findings raise cautionary corporate governance implications. In the absence of an-

titrust enforcement, aligning investor and manager incentives might reduce consumer welfare. In addition, our findings contribute to the debate on the optimal enforcement of competition law (Kaplow, 2011). The practice of sanctioning individuals in addition to the corporations exacerbates the principal-agent problem, increases the wedge between the shareholder interests and managerial incentives, and thus makes it more costly to close this wedge with extrinsic incentives. Finally, given that we document an increase in the gross profit margins of the firms exposed to the DoJ office closures, our findings suggest the importance of having "boots on the ground" to provide local antitrust enforcement.

References

Aggarwal, Rajesh K, and Andrew A Samwick, 1999a, Executive compensation, strategic competition, and relative performance evaluation: Theory and evidence, *Journal of Finance* 54, 1999–2043.

Aggarwal, Rajesh K, and Andrew A Samwick, 1999b, The other side of the trade-off: The impact of risk on executive compensation, *Journal of Political Economy* 107, 65–105.

Agrawal, Anup, Jeffrey F Jaffe, and Jonathan M Karpoff, 1999, Management turnover and governance changes following the revelation of fraud, *Journal of Law & Economics* 42, 309–342.

Albuquerque, Ana, 2009, Peer firms in relative performance evaluation, *Journal of Accounting and Economics* 48, 69–89.

Antón, Miguel, Florian Ederer, Mireia Giné, and Martin Schmalz, 2023, Common ownership, competition, and top management incentives, *Journal of Political Economy* 131, forthcoming.

Badertscher, Brad, Nemit Shroff, and Hal D. White, 2013, Externalities of public firm presence: Evidence from private firms' investment decisions, *Journal of Financial Economics* 109, 682–706.

Bizjak, John M, Michael L Lemmon, and Lalitha Naveen, 2008, Does the use of peer groups contribute to higher pay and less efficient compensation?, *Journal of Financial Economics* 90, 152–168.

Bloomfield, Matthew J, 2021, Compensation disclosures and strategic commitment: Evidence from revenue-based pay, *Journal of Financial Economics* 141, 620–643.

Bloomfield, Matthew J, Catarina Marvão, and Giancarlo Spagnolo, 2023, Relative performance evaluation, sabotage and collusion, *Journal of Accounting and Economics* 101608.

Coles, Jeffrey L, Naveen D Daniel, and Lalitha Naveen, 2006, Managerial incentives and risk-taking, *Journal of Financial Economics* 79, 431–468.

Coles, Jeffrey L, Naveen D Daniel, and Lalitha Naveen, 2014, Co-opted boards, *The Review of Financial Studies* 27, 1751–1796.

Connor, John, 2014, The private international cartels (PIC) data set: Guide and summary statistics, 1990-2013, Unpublished working paper.

Core, John, and Wayne Guay, 2002, Estimating the value of employee stock option portfolios and their sensitivities to price and volatility, *Journal of Accounting Research* 40, 613–630.

Cuñat, Vicente, and Maria Guadalupe, 2009, Globalization and the provision of incentives inside the firm: The effect of foreign competition, *Journal of Labor Economics* 27, 179–212.

Cuñat, Vicente, and Maria Guadalupe, 2009, Executive compensation and competition in the banking and financial sectors, *Journal of Banking & Finance* 33, 495–504.

De Angelis, David, and Yaniv Grinstein, 2015, Performance terms in CEO compensation contracts, *Review of Finance* 19, 619–651.

De Loecker, Jan, Jan Eeckhout, and Gabriel Unger, 2020, The rise of market power and the macroeconomic implications, *The Quarterly Journal of Economics* 135, 561–644.

Deng, Yongning, 2023, Market power and antitrust law enforcement: Does the local presence of antitrust forces matter?, Unpublished working paper.

Faulkender, Michael, and Jun Yang, 2010, Inside the black box: The role and composition of compensation peer groups, *Journal of Financial Economics* 96, 257–270.

Feichter, Christoph, Frank Moers, and Oscar Timmermans, 2022, Relative performance evaluation and competitive aggressiveness, *Journal of Accounting Research* 60, 1859–1913.

Fershtman, Chaim, and Kenneth L Judd, 1987, Equilibrium incentives in oligopoly, *The American Economic Review* 77, 927–940.

Ganduri, Rohan, 2019, Too far to regulate?, Unpublished working paper.

Garcia, Diego, and Øyvind Norli, 2012, Geographic dispersion and stock returns, *Journal of Financial Economics* 106, 547–565.

Gong, Guojin, Laura Yue Li, and Jae Yong Shin, 2011, Relative performance evaluation and related peer groups in executive compensation contracts, *The Accounting Review* 86, 1007–1043.

González, Tanja Artiga, Markus Schmid, and David Yermack, 2019, Does price fixing benefit corporate managers?, *Management Science* 65, 4813–4840.

Gopalan, Yadav, Ankit Kalda, and Asaf Manela, 2021, Hub-and-spoke regulation and bank leverage, *Review of Finance* 25, 1499–1545.

Hainmueller, Jens, 2012, Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies, *Political Analysis* 20, 25–46.

Hansen, Bruce E, 2022, Jackknife standard errors for clustered regression, Unpublished working paper.

Harrington, Joseph E, 2006, How do cartels operate?, *Foundations and Trends in Microeconomics* 2, 1–105.

Hoberg, Gerard, and Gordon Phillips, 2016, Text-based network industries and endogenous product differentiation, *Journal of Political Economy* 124, 1423–1465.

Holmström, Bengt, 1979, Moral hazard and observability, *The Bell Journal of Economics* 10, 74–91.

Holmström, Bengt, 1982, Moral hazard in teams, *The Bell Journal of Economics* 13, 324–340.

Huck, Steffen, Hans-Theo Normann, and Jörg Oechssler, 2004, Two are few and four are many: Number effects in experimental oligopolies, *Journal of Economic Behavior & Organization* 53, 435–446.

Jayaraman, Sudarshan, Todd Milbourn, Florian Peters, and Hojun Seo, 2021, Product market peers and relative performance evaluation, *The Accounting Review* 96, 341–366.

Joh, Sung Wook, 1999, Strategic managerial incentive compensation in Japan: Relative perfor-

mance evaluation and product market collusion, *The Review of Economics and Statistics* 81, 303–313.

Kades, Michael, 2019, The state of U.S. federal antitrust enforcement, `https://equitablegrowth.org/research-paper/the-state-of-u-s-federal-antitrust-enforcement/`.

Kaplow, Louis, 2011, An economic approach to price fixing, *Antitrust Law Journal* 77, 343–449.

Karpoff, Jonathan M, 2023, Unlikely sabotage: Comment on Bloomfield, Marvão, and Spagnolo, *Journal of Accounting and Economics* 101606.

Kaustia, Markku, and Ville Rantala, 2021, Common analysts: Method for defining peer firms, *Journal of Financial and Quantitative Analysis* 56, 1505–1536.

Kedia, Simi, 2006, Estimating product market competition: Methodology and application, *Journal of Banking & Finance* 30, 875–894.

Kedia, Simi, and Shiva Rajgopal, 2011, Do the SEC's enforcement preferences affect corporate misconduct?, *Journal of Accounting and Economics* 51, 259–278.

Klasa, Sandy, Hernán Ortiz-Molina, Matthew Serfling, and Shweta Srinivasan, 2018, Protection of trade secrets and capital structure decisions, *Journal of Financial Economics* 128, 266–286.

Lim, Ivan, Jens Hagendorff, and Seth Armitage, 2017, Does distance impede regulatory monitoring? Evidence from the banking industry, Unpublished working paper.

Ma, Fangyuan, 2020, Subjective CEO pay and long-term incentives, Unpublished working paper.

MacKinnon, James G, Morten Ørregaard Nielsen, and Matthew D Webb, 2023a, Cluster-robust inference: A guide to empirical practice, *Journal of Econometrics* 232, 272–299.

MacKinnon, James G, Morten Ørregaard Nielsen, and Matthew D Webb, 2023b, Fast and reliable jackknife and bootstrap methods for cluster-robust inference, *Journal of Applied Econometrics* 38, 671–694.

Motta, Massimo, 2004, *Competition Policy: Theory and Practice* (Cambridge University Press).

Nalebuff, Barry J, and Joseph E Stiglitz, 1983, Prizes and incentives: Towards a general theory of compensation and competition, *The Bell Journal of Economics* 14, 21–43.

Oster, Emily, 2019, Unobservable selection and coefficient stability: Theory and evidence, *Journal of Business & Economic Statistics* 37, 187–204.

Pellegrino, Bruno, 2023, Product differentiation and oligopoly: A network approach, Unpublished working paper.

Potters, Jan, and Sigrid Suetens, 2009, Cooperation in experimental games of strategic complements and substitutes, *The Review of Economic Studies* 76, 1125–1147.

Reitman, David, 1993, Stock options and the strategic use of managerial incentives, *The American Economic Review* 83, 513–524.

Sklivas, Steven D, 1987, The strategic choice of managerial incentives, *The RAND Journal of Economics* 18, 452–458.

Vrettos, Dimitris, 2013, Are relative performance measures in CEO incentive contracts used for risk reduction and/or for strategic interaction?, *The Accounting Review* 88, 2179–2212.

Wilson, Kristin, and Stan Veuger, 2017, Information frictions in uncertain regulatory environments: Evidence from U.S. commercial banks, *Oxford Bulletin of Economics & Statistics* 79, 205–233.

Zeume, Stefan, 2017, Bribes and firm value, *The Review of Financial Studies* 30, 1457–1489.

Appendices

A   Institutional background

In this section, we briefly summarize the institutional background of the closure of four antitrust field offices based on the media reports and commentary from antitrust community.

The decision to close the field offices can be traced back to the efforts by President Barack Obama to make the Federal Government more efficient and effective. On June 10, 2010, President Obama signed a memorandum to dispose of unneeded federal real estate. The memorandum required government agents to cut no less than $3 billion worth of building costs by the end of the 2012 fiscal year. It also pointed out that the federal government was managing more real estate than it needed and thus wasting taxpayer dollars, energy, and water resources.

In July 2010, under the leadership of the former Attorney General Eric H. Holder Jr, the DoJ launched Advisory Council for Savings and Efficiencies (SAVE Council). As part of the cost reduction plan, on October 5, 2011, DoJ announced that it will consolidate Antitrust Division field office space in Atlanta, Cleveland, Dallas, and Philadelphia into the Chicago, New York, and San Francisco field offices as well as the division's Washington, D.C.-based section, expecting annual savings of nearly $8 million.

DoJ planned to reassign 94 lawyers to the remaining offices. It has put an argument that consolidating resources will also allow it to focus on large investigations. Assistant Attorney General Ronald Weich said that DoJ "wants larger concentrations of lawyers in fewer locations so it can investigate more sophisticated bid-rigging and price-fixing crimes" ('Cleveland antitrust office should be preserved, lawmakers and lawyers say', *cleveland.com* (January 24, 2012)).

However, the decision was controversial as the government was criticized that it might not be able to keep its human capital with deep legal experience. According to Washington Post, "career antitrust lawyers affected by the plans said they were caught off guard, and they think the plans will result in de facto layoffs as colleagues decide to quit because they are unable or unwilling to move to another city" ('Justice department lawyers irked by plans to close offices', *Washington Post* (October 17, 2011)).

The decision has also met resistance among local politicians and antitrust lawyers. Cleveland mayor Frank Jackson argued that "Closing Cleveland's field office will ... impair the Department of Justice's ability to pursue effective criminal enforcement of antitrust laws" ('Cleveland antitrust office should be preserved, lawmakers and lawyers say', *cleveland.com* (January 24, 2012)). One affected DoJ employee was quoted saying that "by closing both of the Southern offices, that population in the South has been abandoned, and much of the criminal enforcement program eroded" ('Justice plans to cut four antitrust division field offices, employee relocation time', *Government Executive* (October 18, 2011)).

Similarly, on December 8, 2016, in his call to reopen field offices, Robert Connolly, the former chief of the Philadelphia field office, has stated that "the strength of the field offices had always been

their ability to network with investigative agencies from the FBI, the gamut of federal IG's offices, state and local prosecutors and public procurement officials. These local contacts were crucial to educating agents and purchasers about antitrust violations, and giving them the information needed (and motivation) to spot and report possible collusion" ('Time to reopen some antitrust division field offices?', *AntitrustConnect Blog* (December 8, 2016)).

Even the lawyers from a private bar opposed the decision. An Atlanta-based attorney suggested that "antitrust lawyers based in the New York office are not going to go down to Alabama, and San Francisco isn't going to go down to Texas to work on a case, and someone from New York can't go down to a grand jury in Dallas and prevail. They just don't know the people and know how to do the cases" ('Justice department lawyers irked by plans to close offices', *Washington Post* (October 17,2011)). Antitrust Attorney Joseph A. Tate said that many of his business clients were willing to speak openly with a well-known and trusted prosecutor, such as those in Philadelphia, than some "anonymous" person in Washington who "has not established a reputation for credibility" ('The plan to shutter DOJ offices gets stranger', *FTCWatch* (October 31, 2011)).

Further, the originally intended goal of achieving the cost effectiveness was also questioned. For example, on December 7, 2011, Senator Bob Casey sent letter to Attorney General Eric H. Holder Jr urging to reconsider the decision. According to his letter, the DoJ was closing its offices "for an estimated savings of $8 million. However, the Philadelphia office alone has collected hundreds of millions of dollars through antitrust enforcement. In addition, the proposed relocation ... carries additional costs and, ... means higher locality pay." Rep. Hank Johnson Jr sent letter to the same attorney on February 1, 2012, mentioning that the plan "puts nearly 100 jobs at risk in Atlanta and saves only $500,000 in fiscal year 2013". Also, Rep. Dennis Kucinich said that the Cleveland office generated criminal fines of $140.1 million, "[y]et the Cleveland Field Office's annual operating budget is just $3.2 million." Kucinich concluded that "it makes absolutely no sense to eviscerate this office's criminal enforcement efforts by disbanding the office for the mere perception of saving money on rent" ('The plan to shutter DOJ offices gets stranger', *FTCWatch* (October 31, 2011)).

Finally, this decision has been discussed to have had long-term consequences. In 2019, MLex, a media organization specializing on regulatory risk, discussed possible reasons for the limited number of recent prosecutions in criminal collusion cases by DoJ: "Some longtime criminal antitrust defense attorneys also suggested the string of closures could be a result of the relative inexperience of many of the antitrust division's criminal staff. In 2013, the antitrust division closed four of its so-called field offices ... Staff in those offices were offered positions in the remaining criminal units ... But over the past five years, the number of senior criminal antitrust prosecutors has continued to dwindle through retirements or individuals moving to private practice. ... As more senior criminal prosecutors have left, the division has hired a raft of new attorneys for the criminal program who don't have the same experience building and investigating cartel cases" ('Under Delrahim, DOJ wary about filing criminal antitrust charges, indictments', *MLex* (March 21, 2019)).

B Examples of "strategic goals"

Firms often specify "strategic goals" as part of the subjective performance metrics. We collect a few examples of such "strategic goals" from firms' DEF14A filings:

- *strategic initiative goals are primarily related to key planned strategic actions, such as portfolio expansion, key R&D milestones, gross margin expansion, and entry into new markets.*

- *strategic planning to position us for long-term growth.*

- *enter into long-term contractual arrangements to secure revenue optimizing our balance sheet and capital allocation and managing risk.*

- *18 strategic goals in the following categories: (i) service excellence; (ii) safety and risk management; (iii) value pricing; (iv) profitable growth; (v) resource utilization; (vi) new energy environment; and (vii) employee engagement. // enterprise, legal, compliance and fraud risk assessments. // operating income, operating ratio, return on assets, safety, customer service, operating efficiency and other strategic goals.*

- *(xvi) the formation of joint ventures, research and development collaborations, marketing or customer service collaborations, or the completion of other corporate transactions intended to enhance the Company's revenue or profitability or expand its customer base.*

- *inventory management, growth in the channel market, gross margin and business velocity.*

- *focus executives on achieving results that contribute to continued long-term growth in stockholder value.*

- *capture pricing opportunities and improve financial position and prospects; improve succession planning and employee development; improve financial strength rating; enhance technology platforms.*

- *the Individual Strategic Goals identified for each executive officer included a mix of financial and operational, quantitative and qualitative factors. For fiscal 2015 these included but were not limited to: execution of the strategic plan; cost management, expense and pricing related goals; product development goals.*

- *the strategic objectives included but were not limited to emerging market growth, organic growth, and improving gross margins.*

- *produce long-term growth in revenue and earnings: A top priority is sustained profitable growth.*

- *the specific numbers used with regard to these goals (other than NOPAT less a capital charge) involve confidential trade secrets or confidential commercial or financial information, the disclosure of which would result in competitive harm.*

- *our compensation program is designed to align the interest of our executive officers with those of our stockholders through execution in three areas of strategic focus: growth and scale, operational excellence, and high performing organization.*

Panel A: Summary statistics

	N	Mean	S.D.	Q1	Median	Q3
ΔDistance	10,181	1.709	3.410	0	0	1.026
Ln(Return)	10,181	0.046	0.471	-0.126	0.102	0.299
Ln(Local peer return)	10,181	0.053	0.389	-0.116	0.098	0.277
Ln(Total compensation)	10,181	8.222	1.035	7.550	8.303	8.957
Ln(Cash compensation)	10,181	7.300	1.088	6.804	7.354	7.909
Ln(Equity compensation)	10,181	6.609	2.941	6.328	7.602	8.464
Vesting horizon	4,584	32.657	10.601	24	30	36
Strategic performance metric (cash)	5,249	0.079	0.270	0	0	0
Strategic performance metric (equity)	5,249	0.009	0.094	0	0	0
Profit margin performance metric (cash)	5,249	0.055	0.228	0	0	0
Profit margin performance metric (equity)	5,249	0.024	0.153	0	0	0
Sales performance metric (cash)	5,249	0.355	0.478	0	0	1
Sales performance metric (equity)	5,249	0.117	0.322	0	0	0
Explicit RPE	7,743	0.376	0.484	0	0	1
Index benchmark	2,917	0.383	0.486	0	0	1
Peer group benchmark	2,917	0.513	0.500	0	1	1
%HP peers in explicit benchmark	1,490	0.176	0.186	0.048	0.107	0.241
%Local peers in explicit benchmark	1,410	0.223	0.295	0	0.100	0.300
Add reciprocal	1,141	0.025	0.157	0	0	0
Gross profit margin	10,172	0.280	4.339	0.252	0.408	0.643
Return comovement	10,180	0.365	0.190	0.233	0.354	0.495
Size	10,179	7.984	1.823	6.653	7.896	9.166
Sales growth	10,176	0.084	0.389	-0.033	0.050	0.147
Ln(Tenure)	10,181	1.805	0.898	1.099	1.946	2.485
Number of local peers	10,181	11.992	17.861	2	4	14
Number of non-local peers	10,181	73.756	97.030	11	32	90
Similarity score of local peers	354,673	0.100	0.069	0.045	0.090	0.139

Panel B: Industry median number of local and non-local peers

FF48	Description	Number of peers	Number of local peers	% Local peers
5	Tobacco Products	8	2	50.00%
15	Rubber and Plastic Products	3	1	50.00%
16	Textiles	2	1	50.00%
23	Automobiles and Trucks	6	2	50.00%
38	Business Supplies	6.5	1.5	41.88%
2	Food Products	4	1	40.00%
21	Machinery	11	4	39.22%
35	Computers	37	10	38.42%
26	Defense	13	4	34.62%
1	Agriculture	3	1	33.33%
	...			
32	Communication	28	3	13.24%
43	Restaurants	25	3	12.25%
45	Insurance	67	7	11.73%
11	Healthcare	84.5	8	11.69%
42	Retail	33	3	11.11%
28	Non-Met. and Indust. Mining	29	1	10.32%
40	Transportation	15	1	10.00%
31	Utilities	61	4	9.83%
44	Banking	332	19	7.02%
29	Coal	119	2	2.78%

Notes: Panel A shows the summary statistics of main variables in the sample. ΔDistance is the increase in geographical distance between a firm's headquarter and its governing antitrust office after the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia) in 100 miles. Return refers to the annual stock return of the focal firm, measured as the compounded monthly returns. Local peer return refers to the average annual stock returns of firms that are defined as local peers, i.e., those that are headquartered within a 200-mile radius and have product similarity score within top 70% based on Hoberg and Phillips (2016). Total compensation is the total of salary, bonus, non-equity incentive plan compensation, grant-date fair value of option awards, grant-date fair value of stock awards, deferred compensation earnings reported as compensation, and other compensation from Execucomp. Cash compensation is total amount of salary, bonus and non-equity incentive compensation. Equity compensation is the total grant-date fair value of option awards and stock awards. The compensation variables are measured in thousand U.S. dollars. Vesting horizon is the expected vesting period measured using the number of months until the last vesting date for cliff vesting plans and using the average number of months between the first and last vesting dates for ratable vesting plans. Strategic performance metric (cash) refers to a dummy indicator that shows whether strategic performance measures are included in the CEO's cash incentive plan. Strategic performance metric (equity) refers to a dummy indicator that shows whether strategic performance measures are included in the CEO's equity incentive plan. Profit performance metric (cash) refers to a dummy indicator that shows whether profit-related performance measures are included in the CEO's equity incentive plan. Profit performance metric (equity) refers to a dummy indicator that shows whether profit-related performance measures are included in the CEO's equity incentive plan. Sales performance metric (cash) refers to a dummy indicator that shows whether sales-related performance measures are included in the CEO's cash incentive plan. Sales performance metric (equity) refers to a dummy indicator that shows whether sales-related performance measures are included in the CEO's equity incentive plan. These indicators of performance measures are only available for the firms covered by Incentive Lab and for the years when the CEO receives incentive plans with absolute performance evaluation provisions. Explicit RPE is a dummy indicator that incentive plans with explicit relative performance evaluation (RPE) provisions are granted. Index benchmark is a dummy indicator of using an index as the performance benchmark, when incentive plans explicit RPE are granted. Peer group benchmark is a dummy indicator of using a specified peer group as the performance benchmark, when incentive plans explicit RPE provisions are granted. %HP peers is the proportion of product market peers included as the explicit peer group benchmark, where product market peers refer to the Hoberg-Phillips peers with product similarity score within the top 70%. %Local peers is the proportion of local peers included as the explicit peer group benchmark, where local peers refer to the Hoberg-Phillips peers with product similarity score within the top 70% and headquartered within 200 miles. Add reciprocal is a dummy indicator of a net increase of reciprocal peers, i.e., the number of additional reciprocal peers is larger than the number of additional non-reciprocal peers, in the explicit peer group benchmark. %HP peers, %Local peers, and Add reciprocal are only available when the CEO receives incentive plans with explicit RPE provisions and specified peer group as the performance benchmark. Gross profit margin refers to the gross profit divided by sales. Return comovement is the average annual correlation of weekly stock market returns between the firm and its local peers. Size is natural logarithm of one plus total assets (in million U.S. dollars). Sales growth is the annual percentage change in sales. Tenure is the number of years since the executive assumed their CEO position. Number of local (non-local) peers refers to the number of firms that are headquartered within (beyond) 200 miles from the focal firm and have product similarity score within top 70% according to Hoberg and Phillips (2016). Similarity score of local peers is Hoberg Philips similarity score for local peers at firm-pair level, conditional on product similarity score being within top 70% according to Hoberg and Phillips (2016). Panel B reports the median value of number of peers and local peers, as well as the percent of local peers for twenty of the Fama-French 48 industries, ranked highest to lowest based on their local peer percentage. In all cases, "Ln" refers to natural logarithm of one plus the variable in parentheses. All the variables are winsorized at the 0.5% and 99.5% levels. Data spans from 2008 to 2017 and covers the firms with local peers in ExecuComp.

Table 2: States covered by antitrust division field offices

Field office	States and territories covered by the field offices
Atlanta	Alabama, Florida, Georgia, Mississippi, North Carolina, South Carolina, Tennessee, Puerto Rico, U.S. Virgin Islands
Chicago	Colorado, Illinois, Indiana, Iowa, Kansas, West District of Michigan, Minnesota, Missouri, Nebraska, North Dakota, South Dakota, Wisconsin
Cleveland	Kentucky, Eastern District of Michigan, Ohio, West Virginia
Dallas	Texas, Oklahoma, Louisiana, New Mexico, Arkansas
New York	Connecticut, Maine, Massachusetts, New Hampshire, Northern New Jersey, New York, Rhode Island, Vermont
Philadelphia	Delaware, Maryland, Southern New Jersey, Pennsylvania, Virginia
San Francisco	Alaska, Arizona, California, Hawaii, Idaho, Montana, Nevada, Oregon, Utah, Washington, Wyoming

Notes: This table shows the state coverage of field offices in the U.S. Department of Justice Antitrust Division before the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia) in 2013. This closure affected 23 states and territories. The data comes from Antitrust Division's April 2001 Report to the Chairman, Subcommittee on Administrative Oversight and the Courts, Committee on the Judiciary United States Senate, available at https://www.gao.gov/assets/240/231337.pdf.

Table 3: Trends in local antitrust cases

	(1)	(2)	(3)	(4)	(5)	(6)
	Affected states			Unaffected states		
	2008-12	2013-17	Diff.	2008-12	2013-17	Diff.
Average proportion of						
1. Local cases	0.258	0.163	-0.096	0.241	0.303	0.063
2. Convicted local cases	0.101	0.080	-0.021	0.209	0.190	-0.019

Notes: This table shows the average proportion of local criminal charges related to violations of the Sherman Act, both before and after the closure of four field offices in 2013. Row (1) reports filed cases while row (2) reports convicted cases. Local cases are defined as cases where convicted or suspected antitrust activities are limited to a particular U.S. region. We consider non-local cases to be those that national nor international in scope. Data spans from 2008 to 2017. Affected states refer to 19 states where firms experienced an increase in distance from their headquarters to antitrust offices: Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Michigan, Mississippi, New Jersey, New Mexico, North Carolina, Ohio, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, and West Virginia. Unaffected states include those whose covering offices were not removed (i.e., Alaska, Arizona, California, Colorado, Connecticut, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Massachusetts, Minnesota, Missouri, Montana, Nebraska, Nevada, New Hampshire, New York, North Dakota, Oregon, Rhode Island, South Dakota, Utah, Vermont, Washington, Wisconsin, and Wyoming) and those that experienced removal but the new office was not farther away (i.e., Maryland and Virginia).

Table 4: Unconditional sensitivity of pay to performance

	(1)	(2)	(3)	(4)	(5)	(6)
			Ln(Total compensation)			
Sample:		*full sample*			*with local peer*	
Ln(Return)	0.212***	0.206***	0.202***	0.195***	0.204***	0.193***
	(18.964)	(17.797)	(18.482)	(16.823)	(14.987)	(13.279)
Ln(Peer return)	-0.046***	-0.054***				
	(-3.014)	(-3.076)				
Ln(Local peer return)			-0.035***	-0.031**	-0.044***	-0.045***
			(-2.685)	(-2.251)	(-3.019)	(-2.800)
Local market			0.001	-0.004		
			(0.097)	(-0.273)		
Size$_{t-1}$	0.277***	0.270***	0.273***	0.265***	0.278***	0.267***
	(20.844)	(19.445)	(19.666)	(18.389)	(16.210)	(14.966)
Sales growth$_{t-1}$	0.153***	0.118***	0.157***	0.125***	0.142***	0.113***
	(8.094)	(5.942)	(8.030)	(6.089)	(6.120)	(4.583)
Ln(Tenure)$_t$	0.019**	0.019**	0.018**	0.018**	0.008	0.008
	(2.571)	(2.519)	(2.288)	(2.278)	(0.801)	(0.785)
Constant	5.834***	5.892***	5.860***	5.930***	5.856***	5.945***
	(57.004)	(55.089)	(54.429)	(53.041)	(43.763)	(42.785)
Year FE	YES	NO	YES	NO	YES	NO
Firm FE	YES	YES	YES	YES	YES	YES
SIC2 x Year FE	NO	YES	NO	YES	NO	YES
Adjusted R^2	0.691	0.701	0.688	0.699	0.685	0.697
N	38,526	38,367	36,638	36,491	24,668	24,436

Notes: This table shows the test on unconditiontal performance sensitivity of total compensation on own and peer returns. The dependent variable is the natural logarithm of one plus total compensation. Ln(Return) refers to the natural logarithm of one plus annual stock market return of focal firm. Ln(Peer return) refers to the natural logarithm of one plus annual stock market return of Hoberg-Phillips peer firms that are associated with the product similarity score within the top 70%. Ln(Local peer return) refers to the natural logarithm of one plus annual stock market return of local peer firms. Local peers are defined as the firms with product similarity scores among the top 70% and headquartered within 200 miles. Local market is an indicator of the presence of local peer firms. Size is natural logarithm of one plus total assets (in million U.S. dollars). Sales growth is the annual percentage change in sales. Ln(Tenure) is the natural logarithm of one plus the years since the executive assumes their CEO position. SIC2 x Year FE is joint fixed effect between year and SIC 2-digit industry. All the variables are winsorized at the 1% and 99% levels. The data spans from 1994 to 2018. The sample for columns (1)-(4) covers all the firms in Execucomp, and the sample for columns (5)-(6) covers the firms with local peers. Robust t-statistics based on standard errors clustered at the firm level are reported in parentheses.

Table 5: DoJ office closures and changes to peer performance sensitivity

	(1)	(2)	(3)	(4)	(5)	(6)
			Ln(Total compensation)			
Sample:			*full sample*		*with local peers*	
ΔDistance x Post x Ln(Return)	-0.018***	-0.016***	-0.014***	-0.011***	-0.015***	-0.016**
	(-5.075)	(-3.260)	(-4.907)	(-2.705)	(-2.985)	(-2.637)
ΔDistance x Post x Ln(Peer return)	0.022***	0.023***				
	(3.802)	(3.240)				
ΔDistance x Post x Ln(Local peer return)			0.015***	0.014**	0.017***	0.018**
			(3.336)	(2.027)	(3.131)	(2.351)
ΔDistance x Post	-0.001	-0.002	0.013***	0.011***	-0.005	-0.008**
	(-0.289)	(-0.660)	(3.307)	(2.868)	(-1.445)	(-2.064)
Ln(Return)	0.085***	0.088***	0.097***	0.094***	0.100***	0.092***
	(6.371)	(5.859)	(6.840)	(6.279)	(5.571)	(5.429)
ΔDistance x Ln(Return)	0.005*	0.003	0.002	0.001	0.000	0.001
	(1.838)	(0.768)	(0.724)	(0.154)	(0.024)	(0.308)
Post x Ln(Return)	0.166***	0.138***	0.143***	0.124***	0.137***	0.126***
	(7.471)	(5.438)	(5.741)	(4.541)	(3.738)	(3.649)
Ln(Peer return)	0.042	0.003				
	(1.253)	(0.081)				
ΔDistance x Ln(Peer return)	-0.011**	-0.007				
	(-2.313)	(-1.233)				
Post x Ln(Peer return)	-0.117*	-0.103				
	(-1.929)	(-1.501)				
Ln(Local peer return)			-0.008	-0.031	-0.022	-0.053*
			(-0.347)	(-1.327)	(-0.696)	(-1.831)
ΔDistance x Ln(Local peer return)			-0.007	-0.004	-0.006	-0.005
			(-1.515)	(-0.657)	(-1.412)	(-0.804)
Post x Ln(Local peer return)			-0.011	0.002	-0.017	0.012
			(-0.361)	(0.057)	(-0.417)	(0.298)
Local market			-0.054**	-0.048*		
			(-2.188)	(-1.850)		
ΔDistance x Local market			0.017***	0.018***		
			(3.632)	(2.878)		
Post x Local market			0.029	0.019		
			(0.816)	(0.513)		
ΔDistance x Post x Local market			-0.017***	-0.017**		
			(-3.007)	(-2.373)		
$Size_{t-1}$	0.271***	0.270***	0.271***	0.271***	0.297***	0.290***
	(12.680)	(11.973)	(13.107)	(12.380)	(11.576)	(9.578)
Sales growth$_{t-1}$	0.028*	0.017	0.026*	0.016	0.013	0.006
	(1.729)	(1.121)	(1.698)	(1.107)	(0.687)	(0.331)
Ln(Tenure)	0.045***	0.043***	0.044***	0.043***	0.037***	0.033**
	(3.956)	(3.750)	(3.921)	(3.756)	(2.847)	(2.556)
Constant	5.999***	6.012***	5.998***	6.008***	5.797***	5.862***
	(36.181)	(35.053)	(36.527)	(35.713)	(29.094)	(25.018)
Year FE	YES	NO	YES	NO	YES	NO
Firm FE	YES	YES	YES	YES	YES	YES
SIC2 x Year FE	NO	YES	NO	YES	NO	YES
Adjusted R^2	0.755	0.757	0.755	0.757	0.760	0.761
N	13,946	13,894	13,946	13,894	10,181	10,109

Notes: This table reports the regression results on changes in pay-performance sensitivity in response to the DoJ field office closures. The dependent variable is the natural logarithm of one plus total compensation. Post is a dummy variable that takes the value of one if the year is on or after 2013, and zero otherwise. ΔDistance is the increase in geographical distance between a firm's headquarter and its governing antitrust office after the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia) in 100 miles. Ln(Return) refers to the natural logarithm of one plus annual stock market return of focal firm. Ln(Peer return) is the natural logarithm of one plus average annual stock return of product market peers that are the Hoberg-Phillips peers with similarity score within the top 70%. Ln(Local peer return) is the natural logarithm of one plus average annual stock return of local product market peers that are headquartered within 200 miles of the focal firm. Local market is an indicator of the presence of local peer firms. Size is the natural logarithm of one plus total assets (in million U.S. dollars). Sales growth is the annual percentage change in sales. Ln(Tenure) is the natural logarithm of one plus the years since the executive assumes their CEO position. SIC2 x Year FE is the joint fixed effect between year and SIC 2-digit industry. All the variables are winsorized at the 0.5% and 99.5% levels. The data spans from 2008 to 2017. The sample for columns (1)-(4) covers all firms in ExecuComp, and the sample for columns (5)-(6) covers the firms with local peers. Robust t-statistics based on standard errors clustered at the state level are reported in parentheses.

Table 6: Compensation components

	(1)	(2)	(3)	(4)
	Ln(Cash compensation)		Ln(Equity compensation)	
Sample:	*with local peers*			
ΔDistance x Post x Ln(Return)	-0.013*	-0.014**	-0.006	0.000
	(-1.981)	(-2.273)	(-0.199)	(0.013)
ΔDistance x Post x Ln(Local peer return)	0.011**	0.014***	0.008	0.019
	(2.556)	(4.434)	(0.427)	(0.547)
ΔDistance x Post	-0.005	-0.012***	-0.022	-0.031*
	(-1.443)	(-2.688)	(-1.466)	(-1.913)
Ln(Return)	0.186***	0.181***	0.112	0.062
	(5.680)	(6.038)	(1.609)	(0.977)
ΔDistance x Ln(Return)	-0.001	-0.002	-0.042*	-0.037*
	(-0.115)	(-0.669)	(-1.846)	(-1.713)
Post x Ln(Return)	0.174***	0.170***	0.224**	0.260***
	(4.323)	(4.254)	(2.145)	(2.978)
Ln(Local peer return)	0.048*	0.023	-0.205	-0.291**
	(1.749)	(0.734)	(-1.570)	(-2.665)
ΔDistance x Ln(Local peer return)	-0.008**	-0.006	0.018	0.024
	(-2.583)	(-1.676)	(0.788)	(0.809)
Post x Ln(Local peer return)	0.026	0.048	0.027	0.056
	(0.894)	(1.341)	(0.131)	(0.254)
$Size_{t-1}$	0.154***	0.161***	0.574***	0.547***
	(3.299)	(3.946)	(5.564)	(4.665)
Sales growth$_{t-1}$	0.030	0.024	0.049	0.051
	(1.634)	(1.335)	(0.758)	(0.760)
Ln(Tenure)	0.060***	0.057***	-0.105*	-0.114**
	(6.416)	(4.789)	(-1.985)	(-2.223)
Constant	5.953***	5.911***	2.261***	2.498***
	(15.972)	(18.042)	(2.785)	(2.717)
Year FE	YES	NO	YES	NO
Firm FE	YES	YES	YES	YES
SIC2 x Year FE	NO	YES	NO	YES
Adjusted R^2	0.681	0.690	0.511	0.510
N	10,183	10,111	10,181	10,109

Notes: This table reports the regression results on changes in performance sensitivity of cash and equity compensation in response to the DoJ field office closures. The dependent variables are the natural logarithm of one plus cash compensation or equity compensation. Post is a dummy variable that takes the value of one if the year is on or after 2013, and zero otherwise. ΔDistance is the increase in geographical distance between a firm's headquarter and its governing antitrust office after the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia) in 100 miles. Ln(Return) refers to the natural logarithm of one plus annual stock market return of focal firm. Ln(Local peer return) is the natural logarithm of one plus average annual stock return of local product market peers that are the Hoberg-Phillips peers with similarity score within the top 70% and headquartered within 200 miles of the focal firm. Size is natural logarithm of one plus total assets (in million U.S. dollars). Sales growth is the annual percentage change in sales. Ln(Tenure) is the natural logarithm of one plus the years since the executive assumes their CEO position. SIC2 x Year FE is joint fixed effect between year and SIC 2-digit industry. All the variables are winsorized at the 0.5% and 99.5% levels. The data spans from 2008 to 2017 and covers the firms with local peers in Execucomp. Robust t-statistics based on standard errors clustered at the state level are reported in parentheses.

Table 7: Alternative specifications and robustness tests

	(1)	(2)
	ΔDistance x Post x Ln(Local peer return)	
	Year FE	SIC2 x Year FE
Panel A: Binary treatment variable		
1. D(ΔDistance > 400))	0.164***	0.171**
	(2.678)	(2.019)
Panel B: Propensity score matching		
2. ΔDistance> 400 vs. ≤ 400	0.027***	0.028*
	(2.978)	(1.737)
3. ΔDistance> 0 vs. = 0	0.037***	0.041***
	(3.729)	(3.009)
Panel C: Entropy balancing		
4. ΔDistance> 400 vs. ≤ 400	0.021***	0.027***
	(4.517)	(3.196)
5. ΔDistance> 0 vs. = 0	0.021***	0.024***
	(3.993)	(2.715)
Panel D: Placebo test		
6. Pseudo shock in 2006	-0.008	-0.006
	(-0.748)	(-0.475)
Panel E: Alternative definition of peers - Hoberg-Phillips similarity score		
7. Within top 30 %	0.015**	0.014
	(2.228)	(1.444)
8. Within top 60 %	0.019***	0.021**
	(2.876)	(2.341)
9. Within top 100 %	0.011	0.011
	(1.621)	(0.907)
Panel F: Alternative definitions of peers - Other classifications		
10. Similar size and book-to-market	0.015**	0.012
	(2.141)	(1.039)
11. Cross-price demand elasticity	0.021**	0.023*
	(2.578)	(1.838)
12. Factset Revere peers	0.065***	0.039*
	(3.978)	(1.907)
13. Analyst-based peer group	0.088*	0.225***
	(1.771)	(2.735)
Panel G: Alternative definition of locality		
14. Local peers within 100 miles	0.023***	0.025***
	(4.809)	(3.375)
15. Local peers within 300 miles	0.016**	0.016*
	(2.443)	(1.897)
16. Local peers within 400 miles	0.019***	0.018**
	(2.979)	(2.548)
Panel H: Control for non-local peers		
17. ΔDistance x Post x Ln(Local peer return)	0.020***	0.026**
	(3.006)	(2.452)
ΔDistance x Post x Ln(Non-local peer return)	-0.003	-0.014
	(-0.501)	(-1.353)

	ΔDistance x Post x Ln(Local peer return)	
	Year FE	SIC2 x Year FE
Panel I: Other regression choices		
18. Without firms who got closer	0.015**	0.017**
	(2.684)	(2.086)
19. Winsorize at 1% and 99% level	0.017***	0.018**
	(2.966)	(2.213)
20. Poisson regression	0.023***	0.018*
	(3.340)	(1.752)
Panel J: Alternative standard error clusters		
21. Firm level	0.017*	0.018*
	(1.743)	(1.809)
22. ZIP code level	0.017**	0.018**
	(2.110)	(2.113)
23. SIC2 level	0.017**	0.018*
	(2.206)	(1.780)
24. DoJ region (pre-shock) level	0.017**	0.018**
	(2.929)	(2.537)

Notes: This table shows ten different sets of robustness tests using alternative specifications to estimate the coefficient of β_2 reported in Table 5, columns (5)-(6). In Panel A, row 1, we replace ΔDistance with a binary variable that equals one if the change in distance to the field office is more than or equal to 400 miles, and zero otherwise. In Panel B, we match the treated and control firms with the closest propensity of being treated based on logistic regression of the treated dummy on own and local peer returns, firm size, sales growth rate, CEO tenure, and industry dummies using the 2012 sample. In row 2, the treated firms are those with $\Delta Distance$ above 400 miles; in row 3, the treated firms are those with $\Delta Distance$ above 0 miles. In Panel C, rows 4-5, we apply the entropy balancing method to reweigh these firm characteristics when estimating the treatment effect. In Panel D, row 6, we conduct the placebo test by taking 2006 as the pseudo event year. In Panel E, rows 7-9, local peers are defined as the firms within the top 30, 60, and 100% of the Hoberg-Phillips similarity scores. In Panel F, we use alternative classifications to define local peers. In row 10, we use size and book-to-market screening, choosing the closest half of local peers based on the closeness of the Mahalanobis distance using the market capitalization and book-to-market. In row 11, we use cross-price demand elasticities with focal firms (Pellegrino (2023)), defining local peers are the ones within the top tertile of cross-price demand elasticities with focal firms each year. In row 12, we use the Factset Revere classification. In row 13, we use an analysts-based peer group (Kaustia and Rantala (2021)), where local peer return is defined as the average stock performance of local peers who are followed by common analysts weighted on the number of analysts each year. In Panel G, rows 14-16, we define local peers as the firms headquartered within 100, 300, and 400 miles of the focal firm. In Panel H, row 17, we additionally control for average non-local peer return and their interaction terms. In Panel I, row 18, we drop the sample who got closer to the governing DoJ offices after 2013, in row 19, all variables are winsorized at the 1% and 99% level, in row 20, we estimate Poisson regression. In Panel J, rows 21-24, standard errors are clustered at the levels of firm, ZIP code, SIC 2-digit, and pre-shock covering DoJ office region levels. The dependent variables are the natural logarithm of one plus total compensation. Post is a dummy variable that equals one if the year is on or after 2013, and zero otherwise. ΔDistance is the increase in geographical distance between a firm's headquarter and its governing antitrust office after the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia) in 100 miles. Ln(Local peer return) refers to the natural logarithm of one plus the annual stock market return of local peer firms. SIC2 x Year FE is the joint fixed effect between year and SIC 2-digit industry. The data includes firms with local peers in Execucomp and spans from 2008 to 2017 (except for the sample for Panel D spans from 2001 to 2010). All the variables are winsorized at the 0.5% and 99.5% levels (except for row 19). Except for Panel J, robust t-statistics based on standard errors clustered at the state level are reported in parentheses. The full tables are reported in Internet Appendix Table IA2, Table IA3, Table IA4, Table IA5, Table IA6, Table IA7, Table IA8, Table IA9, and Table IA10.

Table 8: Heterogeneity

	(1)	(2)	(3)
	ΔDistance x Post x Ln(Local peer return)		
1. Competition mode	Strategic substitutes	Strategic complements	Difference
	0.007	0.040***	-0.033***
	(0.731)	(3.339)	(3.289)
2. Revenue of largest 8 firms in the NAICS	Low	High	Difference
	0.016	0.046***	-0.030
	(0.914)	(2.907)	(-1.231)
3. Fraction of public firms	Low	High	Difference
	0.020	0.038***	-0.017*
	(1.646)	(4.297)	(-1.713)
4. Concentration of states mentioned in 10K	Low	High	Diff
	0.002	0.043***	-0.041***
	(0.166)	(3.234)	(-2.962)
5. Concentration of sales across the states	Low	High	Difference
	0.010	0.081**	-0.070**
	(0.652)	(2.240)	(-2.525)
6. Co-opted board	High	Low	Difference
	-0.004	0.038***	-0.042***
	(-0.49)	(4.370)	(-4.083)
7. CEO age	Young	Old	Difference
	0.011	0.032***	-0.021***
	(1.493)	(3.251)	(-2.774)
8. Inevitable Disclosure Doctrine	IDD	Non-IDD	Difference
	-0.030*	0.028***	-0.058***
	(-1.941)	(5.086)	(-3.597)

Notes: This table presents ten different heterogeneity tests. In row 1, we partition firms into those who operate in an industry in which firms compete as strategic complements or strategic substitutes following Kedia (2006). In row 2, we split the sample based on the revenue percentage of the largest 8 firms over all firms for each NAICS 4-digit industry in 2012. Firms in the "High" ("Low") group operate in NAICS industry where the percentage of revenue by 8 largest firms is in the top (bottom) quartile. In row 3, we divide sample into firms with a proportion of public firms in NAICS industry in 2012 that is higher or lower than the median. In row 4, we divide the sample based on the concentration of the states mentioned in annual reports in 2007 or 2008. Firms falling in top (bottom) 30% of the concentration are assigned as "Concentrated" ("Dispersed"). In row 5, we split the sample based on the concentration of sales across states where the subsidiaries are located. Firms are assigned as "Concentrated" ("Dispersed") if the geographic concentration of firms' sales falls in top (bottom) 30% among all firms. In row 6, we denote the firms to have a high (low) proportion of co-opted board members based on whether the tenure-weighted fraction of co-opted directors is above (below) than the median value for the post-reform period within the same tenure group. In row 7, we divide the sample into firms with CEOs who are younger or older than the median of CEO ages in 2012. In row 8, we divide sample into the firms located in states with or without Inevitable Disclosure Doctrine (IDD) recognized by courts in 2012. The dependent variable is the natural logarithm of one plus total compensation. We summarize the coefficients on ΔDistance x Post x Ln(Local peer return) and also report the test statistics for the difference between the coefficients in two subsamples in column (3). The regression specification is the same as in Table 5, column (6). All the variables are winsorized at the 0.5% and 99.5% levels. The data includes firms with local peers in Execucomp and spans from 2008 to 2017. Robust t-statistics based on standard errors clustered at the state level are reported in parentheses. The full tables are reported in Internet Appendix Table IA11.

Table 9: Incentive alignment and firm outcomes

	(1)	(2)	(3)	(4)
	Gross profit margin		Return comovement	
Post x ΔDistance	-0.002*	-0.002*	0.001	0.000
	(-1.853)	(-1.856)	(1.296)	(0.461)
Post x ΔDistance x ΔPLPS$_{sic2}$	0.001***		0.001***	
	(5.937)		(2.706)	
Post x D(ΔPLPS$_{firm} > 0$)		0.000		-0.023
		(0.006)		(-1.524)
Post x ΔDistance x D(ΔPLPS$_{firm} > 0$)		0.002*		0.005***
		(1.937)		(3.248)
Size$_{t-1}$	-0.005	-0.006	0.020***	0.02***
	(-1.131)	(-1.156)	(4.428)	(4.508)
Sales growth$_{t-1}$	0.007	0.009	0.003	0.002
	(1.309)	(1.296)	(0.717)	(0.487)
Ln(Tenure)	0.002	0.002	0.003	0.004
	(1.273)	(0.854)	(1.075)	(1.32)
Constant	0.509***	0.517***	0.193***	0.193***
	(13.393)	(12.592)	(5.322)	(5.123)
Firm FE	YES	YES	YES	YES
SIC2 x Year FE	YES	YES	YES	YES
Adjusted R^2	0.931	0.93	0.664	0.668
N	7,985	7,195	8,167	7,362

Notes: This table demonstrates the variation in firm outcome resulting from the DoJ reform as firms adjust their pay-performance sensitivity in response to the reform. The dependent variables in the regressions are the gross profit margin and the return comovement. The gross margin is defined as the gross profit divided by sales. The return comovement is the average annual correlation of weekly stock market returns between the firm and its local peers. The local peer firms are defined as the firms with Hoberg-Phillips product similarity score within the top 70% and are headquartered within 200 miles of the focal firm. Post is a dummy indicator that takes the value of one for the years on and after 2013, and zero otherwise. ΔDistance is the increase in geographical distance between a firm's headquarter and its governing antitrust office after the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia) in 100 miles. $\Delta PLPS_{sic2}$ is the change in pay sensitivity to local peer return estimated for each SIC 2-digit industry following regression (1). D(ΔPLPS$_{firm} > 0$) is a dummy variable that equals one if the pay sensitivity to local peer return estimated for each firm is positive and statistically significant positive, and zero otherwise. Size is the natural logarithm of one plus total assets. Sales growth is the ratio of current year sales minus previous year sales and previous year sales. Ln(Tenure) is the natural logarithm of one plus the years since the executive assumes their CEO position. SIC2 x Year FE is the joint fixed effect between year and SIC 2-digit industry. All the variables are winsorized at the 0.5% and 99.5% levels. The data includes firms with local peers in Execucomp and spans from 2008 to 2017. Robust t-statistics based on standard errors clustered at the state level are reported in parentheses.

Table 10: Explicit relative performance evaluation

Panel A: Explicit RPE and benchmark choice

	(1)	(2)	(3)	(4)	(5)	(6)
	Explicit RPE		Index benchmark		Peer group benchmark	
Sample:	with local peers		with local peer & explicit RPE			
ΔDistance x Post	0.002	0.001	0.003	0.002	-0.003	-0.003
	(0.993)	(0.341)	(0.775)	(0.348)	(-0.619)	(-0.273)
$Size_{t-1}$	0.006	-0.008	0.100***	0.059	-0.034	-0.020
	(0.211)	(-0.352)	(3.013)	(1.405)	(-0.849)	(-0.490)
Sales $growth_{t-1}$	-0.021*	-0.024**	-0.005	0.021	-0.046	-0.049
	(-1.614)	(-1.938)	(-0.138)	(0.383)	(-0.931)	(-0.792)
$Ln(Tenure)_t$	-0.014	-0.017*	-0.020*	-0.022*	0.014	0.021
	(-1.452)	(-1.792)	(-1.849)	(-1.878)	(1.407)	(1.503)
Constant	0.407**	0.535**	-0.520*	-0.139	0.827**	0.687*
	(2.302)	(2.225)	(-1.697)	(-0.349)	(2.185)	(1.734)
Year FE	YES	NO	YES	NO	YES	NO
Firm FE	YES	YES	YES	YES	YES	YES
SIC2 x Year FE	NO	YES	NO	YES	NO	YES
Adjusted R^2	0.608	0.605	0.682	0.687	0.638	0.640
N	6,050	5,949	2,479	2,349	2,479	2,349

Panel B: Peer group composition

	(1)	(2)	(3)	(4)	(5)	(6)
	%HP peers		%Local peers		Add reciprocal	
Sample:	with local peer & explicit peer group benchmark					
ΔDistance x Post	-0.003**	-0.004**	-0.005**	0.001	-0.001	-0.006**
	(-2.208)	(-2.542)	(-2.218)	(0.189)	(-0.494)	(-2.109)
$Size_{t-1}$	-0.001	-0.009	-0.010	-0.024	-0.022	-0.043
	(-0.041)	(-0.553)	(-0.424)	(-0.821)	(-1.188)	(-1.420)
Sales $growth_{t-1}$	-0.003	0.018	-0.007	0.006	-0.008	-0.001
	(-0.285)	(1.337)	(-0.273)	(0.281)	(-0.397)	(-0.065)
$Ln(Tenure)_t$	-0.003	-0.002	0.021*	0.011	0.008	0.009*
	(-0.520)	(-0.338)	(2.009)	(0.947)	(1.503)	(1.814)
Constant	0.200	0.274	0.296	0.420	0.222	0.443
	(0.991)	(1.651)	(1.305)	(1.550)	(1.262)	(1.494)
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	NO	YES	NO	YES	NO
SIC2 x Year FE	NO	YES	NO	YES	NO	YES
Adjusted R^2	0.753	0.783	0.670	0.689	-0.051	-0.206
N	1,266	1,123	1,206	1,075	967	849

Notes: This table shows the test results on the likelihood of granting CEOs incentive plans with explicit relative performance evaluation (RPE) provisions, the choice of using an index or specifying a peer group as the relative performance benchmark, and the composition of the specified peer group. The dependent variables of Panel A include the dummy indicator of having explicit RPE provision and the dummy indicators of using an index or specified peer group as the performance benchmark (conditional on having explicit RPE plans). In Panel B, the first dependent variable is the proportion of product market peers included as the explicit peer group benchmark, where product market peers refer to the Hoberg-Phillips peers with product similarity score within the top 70%; the second dependent variable is the proportion of local peers included as the explicit peer group benchmark, where local peers refer to the Hoberg-Phillips peers with product similarity score within the top 70% and headquartered with 200 miles; the third dependent variable is a dummy indicator of a net increase of reciprocal peers, i.e., the number of additional reciprocal peers is larger than the number of additional non-reciprocal peers, in the explicit peer group benchmark. ΔDistance is the increase in geographical distance between a firm's headquarter and its governing antitrust office after the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia) in 100 miles. Size is the natural logarithm of one plus total assets. Sales growth is the ratio of current year sales minus previous year sales and previous year sales. Ln(Tenure) is the natural logarithm of one plus the years since the executive assumes their CEO position. SIC2 x Year FE is the joint fixed effect between year and SIC 2-digit industry. All the variables are winsorized at the 0.5% and 99.5% levels. The sample for Panel A, columns (1)-(2), includes firms with local peers and are covered by Incentive Lab, the sample for Panel A, columns (3)-(6), further requires the firms to have explicit RPE grants, the sample for Panel B, columns (1)-(4), further requires the use of specified peer group, and the sample for Panel B, columns (5)-(6), further requires the existence of explicit RPE plans with specified peer group in the previous. The sample spans from 2008-2017 for Panel A and columns (1)-(4) of Panel B. The sample for columns (5)-(6) of Panel B spans from 2009-2017. Robust t-statistics based on standard errors clustered at the state level are reported in parentheses.

Table 11: Performance metrics in incentive plans

	(1)	(2)	(3)	(4)	(5)	(6)
	Strategic		Profit margin		Sales	
	Cash	Stock	Cash	Stock	Cash	Stock
ΔDistance x Post	0.004*	-0.000	0.002**	-0.000	0.000	-0.006***
	(1.865)	(-0.087)	(2.137)	(-0.350)	(0.055)	(-3.714)
$Size_{t-1}$	-0.022**	-0.001	-0.016*	-0.012	-0.026	0.033*
	(-2.122)	(-0.243)	(-1.693)	(-1.010)	(-1.623)	(1.926)
Sales growth$_{t-1}$	0.006	0.008**	0.004	0.002	0.011	-0.016*
	(0.307)	(2.340)	(1.077)	(0.682)	(0.508)	(-1.820)
Ln(Tenure)	0.003	0.001	-0.005	-0.004	-0.002	0.006
	(0.515)	(0.283)	(-1.269)	(-1.484)	(-0.227)	(0.851)
Constant	0.263***	0.019	0.207**	0.139	0.583***	-0.177
	(2.892)	(0.384)	(2.413)	(1.272)	(4.111)	(-1.191)
Year FE	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Adjusted R^2	0.390	0.306	0.559	0.339	0.725	0.501
N	5,442	5,429	5,442	5,429	5,442	5,429

Notes: This table presents the changes in the use of performance metrics in CEO incentive plans after the relocation of DoJ field offices. The three dependent variables are the dummy indicators of whether a firm mentions the following terms as the performance metrics of its CEO's cash or stock incentive plans: "strateg*" for columns (1)-(2), "profit margin" for columns (3)-(4), and "sales", "gross revenue", "same store sales", or "revenue related" for column (5)-(6). Post is a dummy variable indicating whether the year is on or after 2013. ΔDistance is the increase in geographical distance between a firm's headquarter and its governing antitrust office after the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia) in 100 miles. Local peer firms are defined as the ones with Hoberg-Phillips product similarity score within the top 70% and headquartered within 200 miles of the focal firm. Size is natural logarithm of one plus total assets. Sales growth is the ratio of current year sales minus previous year sales and previous year sales. Ln(Tenure) is the natural logarithm of one plus the years since the executive assumes their CEO position. All the variables are winsorized at the 0.5% and 99.5% levels. The data includes firms with local peers that are covered by Incentive Lab database and spans from 2008 to 2017. Robust t-statistics based on standard errors clustered at the state level are reported in parentheses.

Table 12: Vesting horizon

	(1)	(2)	(3)	(4)	(5)
			Vesting horizon		
Sample:		*full sample*		*without IDD*	*with IDD*
Post x ΔDistance	0.178**		0.178**	0.172***	0.287
	(2.496)		(1.997)	(3.808)	(0.923)
Post x D(ΔDistance> 400)		2.306***			
		(2.798)			
Size$_{t-1}$	-0.463	-0.471	-0.463	-1.070	-0.209
	(-0.981)	(-1.005)	(-0.807)	(-1.733)	(-0.266)
Sales growth$_{t-1}$	0.163	0.155	0.163	0.471	-0.058
	(0.285)	(0.271)	(0.322)	(0.509)	(-0.229)
Ln(Tenure)	-0.988***	-0.986***	-0.988***	-0.995***	-1.346**
	(-4.399)	(-4.423)	(-3.905)	(-3.520)	(-2.468)
Constant	38.157***	38.190***	38.157***	43.594***	36.278***
	(9.042)	(9.144)	(7.482)	(8.215)	(5.095)
Firm FE	YES	YES	YES	YES	YES
SIC2 x Year FE	YES	YES	YES	YES	YES
Cluster	State	State	Firm	State	State
Adjusted R^2	0.560	0.561	0.560	0.540	0.599
N	4,560	4,560	4,560	2,992	1,392

Notes: This table presents the effect of DoJ field office relocation on the vesting horizon of CEO's time-vesting equity plans. The dependent variable is determined as follows: For each incentive plan, we calculate the duration in months until the last vesting date for cliff vesting plans, and we compute the average duration in months between the first and last vesting dates for ratable vesting plans. In cases where multiple plans are granted in a single year, the vesting horizon is defined as the maximum number of months across all plans. We define local peer firms as the ones with Hoberg-Phillips product similarity score within the top 70% and headquartered within 200 miles from the focal firm. Post is a dummy variable that equals one if the year is on or after 2013, and zero otherwise. ΔDistance is the increase in geographical distance between a firm's headquarter and its governing antitrust office after the closure of four field offices (Atlanta, Cleveland, Dallas, and Philadelphia) in 100 miles. D(ΔDistance> 400) is a dummy variable that takes the value of one if a firm experienced more than 400-mile increase in geographic distance. "With IDD" refers to the sample of firms who were located in states where courts had recognized the Inevitable Disclosure Doctrine (IDD) in 2012. "Without IDD" refers to the sample of firms that were not located in states where courts had recognized the IDD in 2012. Sales growth is the ratio of current year sales minus previous year sales and previous year sales. Ln(Tenure) is the natural logarithm of one plus the years since the executive assumes their CEO position. SIC2 x Year FE is the joint fixed effect between year and SIC 2-digit industry. All the variables are winsorized at the 0.5% and 99.5% levels. The data includes firms with local peers covered by Incentive Lab database that have granted new equity awards both before and after 2013. Sample period spans from 2008 to 2017. Robust t-statistics based on standard errors clustered at the state level (columns (1)-(2), (4)-(6)) and firm level (column (3)) are reported in parentheses.

Table 13: Convicted cartel peers and compensation schemes

Panel A: Sensitivity of CEO compensation to cartel peer performance

	(1)	(2)	(3)	(4)	(5)	(6)
			Ln(Total compensation)			
Ln(Return)	0.356***	0.339***	0.694***	0.669***	0.152	0.137
	(3.238)	(3.046)	(5.170)	(5.027)	(0.827)	(0.702)
Ln(Cartel peer return)	-0.059***	-0.057***	-0.158***	-0.156***	-0.111***	-0.111***
	(-2.775)	(-2.703)	(-2.864)	(-2.836)	(-2.927)	(-2.864)
Post x Ln(Return)			-0.444**	-0.425**	-0.405*	-0.393
			(-2.233)	(-2.196)	(-1.759)	(-1.620)
Post x Ln(Cartel peer return)			0.136**	0.138**	0.093**	0.096**
			(2.137)	(2.256)	(2.115)	(2.269)
Post x Ln(Non-cartel peer return)					-0.077	-0.075
					(-1.392)	(-1.432)
Post			-0.001	0.215	-0.308	-0.015
			(-0.006)	(1.513)	(-1.513)	(-0.129)
Ln(Non-cartel peer return)					0.096**	0.102**
					(2.324)	(2.422)
Size$_{t-1}$	-0.331***	-0.330***	-0.312***	-0.314***	-0.324***	-0.320***
	(-3.834)	(-3.674)	(-3.662)	(-3.508)	(-3.957)	(-3.632)
Sales growth$_{t-1}$	0.087	0.089	0.088*	0.092	0.050**	0.054**
	(1.613)	(1.562)	(1.683)	(1.627)	(2.192)	(2.226)
Ln(Tenure)$_t$	0.073*	0.074*	0.076*	0.075*	0.125***	0.122***
	(1.839)	(1.810)	(1.973)	(1.867)	(3.004)	(2.702)
Constant	12.141***	12.142***	11.935***	11.816***	12.535***	12.300***
	(13.365)	(12.799)	(13.049)	(12.379)	(14.099)	(12.996)
Year FE	YES	YES	YES	YES	YES	YES
Firm x Cartel FE	YES	NO	YES	NO	YES	NO
Firm FE	NO	YES	NO	YES	NO	YES
Adjusted R^2	0.472	0.500	0.478	0.504	0.516	0.547
N	1,642	1,643	1,642	1,643	1,264	1,269

Panel B: Overlap of cartel peers with compensation peers

	(1)	(2)	(3)	(4)
	At least one peer	Fraction peers		N
	Overlap at the time of cartel			
General compensation benchmark	86	20.67%	7.81%	416
Relative performance evaluation benchmark	42	10.10%	3.67%	416
Difference		10.58%***	4.14%***	
		(5.803)	(4.838)	
	Overlap over the entire sample period			
General compensation benchmark	252	50.70%	20.35%	497
Relative performance evaluation benchmark	117	23.54%	8.09%	497
Difference		27.16%***	12.26%***	
		(12.095)	(10.317)	

Notes: Panel A shows the sensitivity of CEO compensation to the performance of own firm and the other cartel members in the convicted cartels as collected from Connor (2014). The sample contains public firms with a history of cartel participation and available CEO compensation information from 2000 to 2012 in Execucomp. Observations are at the firm-cartel-year level. Post is a dummy indicator that equals one since the start year of each cartel. Ln(Cartel peer return) refers to the average annual stock return of the other public members of the cartel and Ln(Non-cartel return) refers to the average return of Hoberg-Phillips peers that were not engaged in the cartel. The other variables have the same definition as other tables. All variables are winsorized in 0.5%. Standard errors are clustered at the cartel level. Robust t-statistics are reported in the parentheses. Panel B shows the number and percent of compensation benchmark firms that overlap with the cartel peers covered by Incentive Lab.
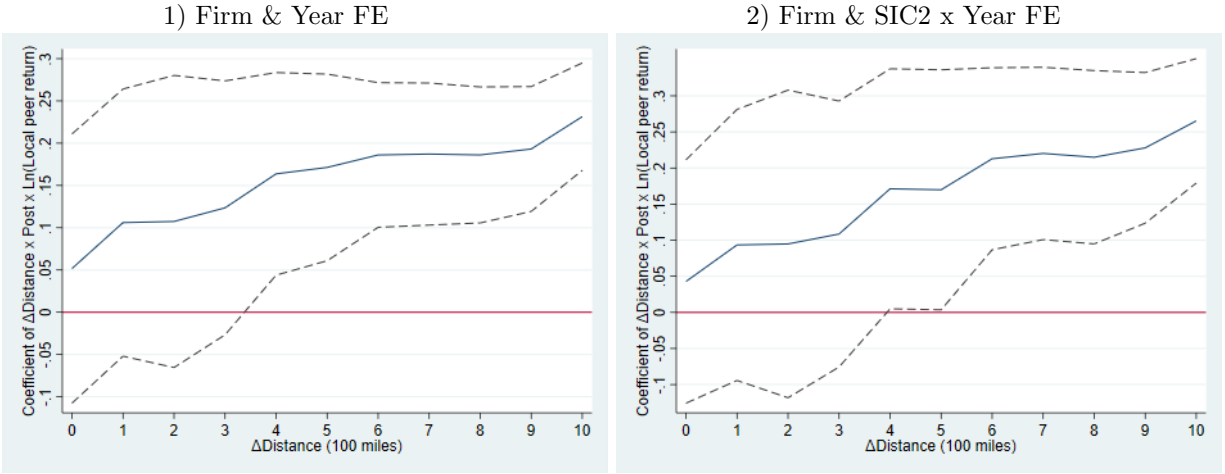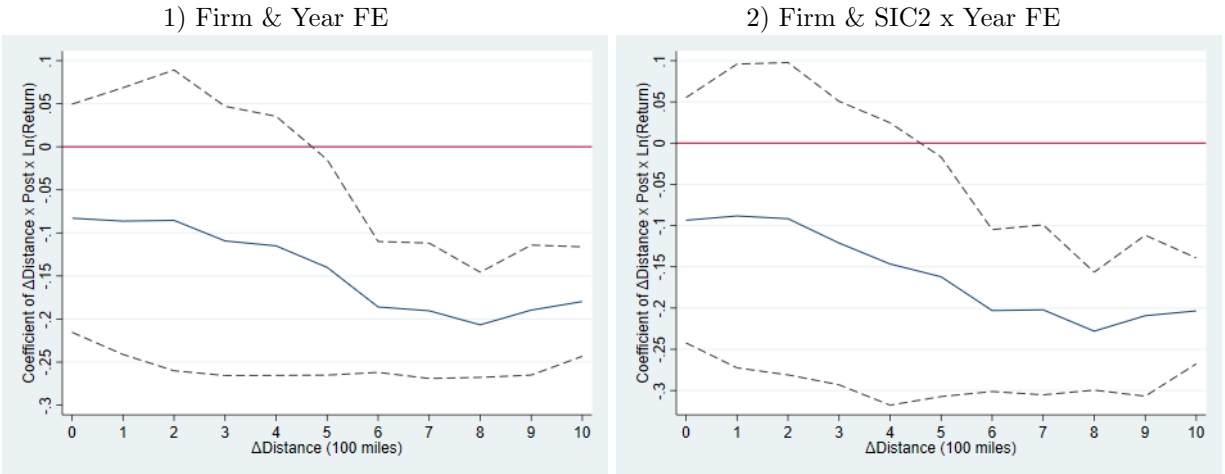
Figure 1: Trends in antitrust cases
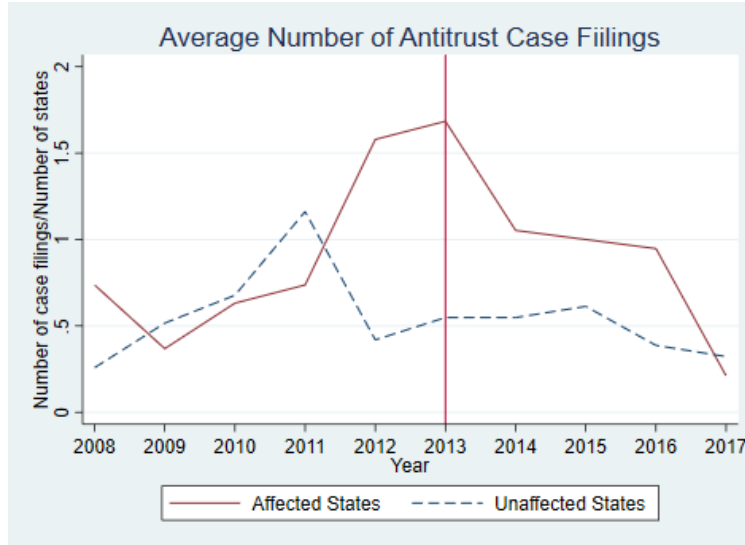
Notes: This figure shows the average number of antitrust case filings per state at the courts of the states that were affected and unaffected by the reform. A state is denoted as "affected" if the firms headquartered in the state experienced an increase in distance from their headquarters to the governing DoJ field offices on average after the reform. Affected states include Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Michigan, Mississippi, New Jersey, New Mexico, North Carolina, Ohio, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, and West Virginia. Unaffected states include those whose covering offices were not removed, i.e., Alaska, Arizona, California, Colorado, Connecticut, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Massachusetts, Minnesota, Missouri, Montana, Nebraska, Nevada, New Hampshire, New York, North Dakota, Oregon, Rhode Island, South Dakota, Utah, Vermont, Washington, Wisconsin, and Wyoming, and those that experienced removal but the new office was not farther away, i.e., Maryland and Virginia. For each year, we count the total number of criminal antitrust charges of violating Sherman Act, and scale it by the number of states in the group. The red line shows the average number of case filings among the affected states, while the dashed blue line shows that of the unaffected states.

Figure 2: Binary Treatment

Panel A: $D(\Delta Distance > x)$ x Post x Local Peer Return

1) Firm & Year FE   2) Firm & SIC2 x Year FE

Panel B: $D(\Delta Distance > x)$ x Post x Own Return

1) Firm & Year FE   2) Firm & SIC2 x Year FE

Notes: These figures show the coefficient estimates from the robustness of baseline regression using binary treatment variables. The regression specification follows columns (5)-(6) of Table 5 with $\Delta Distance$ replaced by a dummy indicator that $\Delta Distance$ is above a threshold x as in the horizontal axis. x is within the range of 0 to 1000 miles. Panel A plots the coefficient on the triple interaction of the logged local peer return, post-reform dummy, and the binary treatment. Panel B plots the coefficient on the triple interaction of the logged own firm return, post-reform dummy, and the binary treatment. In each panel, we present findings using two different sets of fixed effects: 1) firm and year FE, and 2) firm and joint fixed effect between year and SIC 2-digit industry. The solid lines plot the point estimation of coefficients, and the dashed lines plot the 95% confidence intervals. All variables are winsorized at 0.5% and 99.5% levels. The sample spans from 2008 to 2017 and contains Execucomp firms with local peers.

Figure 3: Gross profit margins

Panel A: $\Delta Distance > 0$ vs. $= 0$
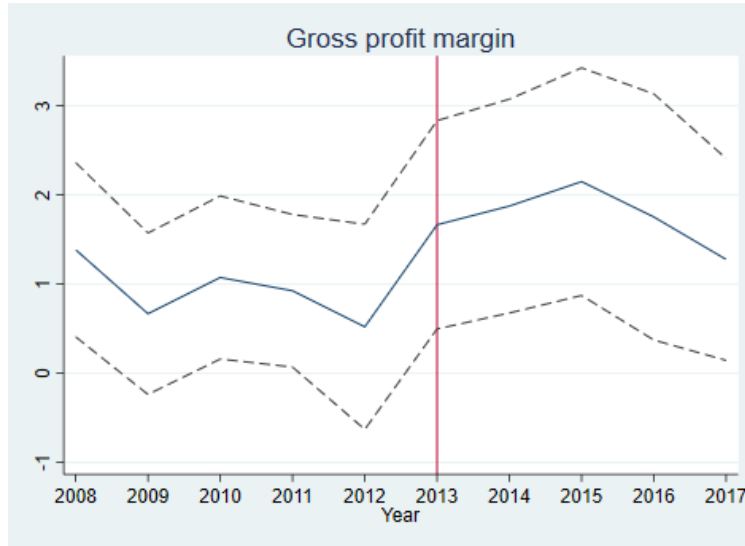
Panel B: $\Delta Distance > 400$ vs. ≤ 400
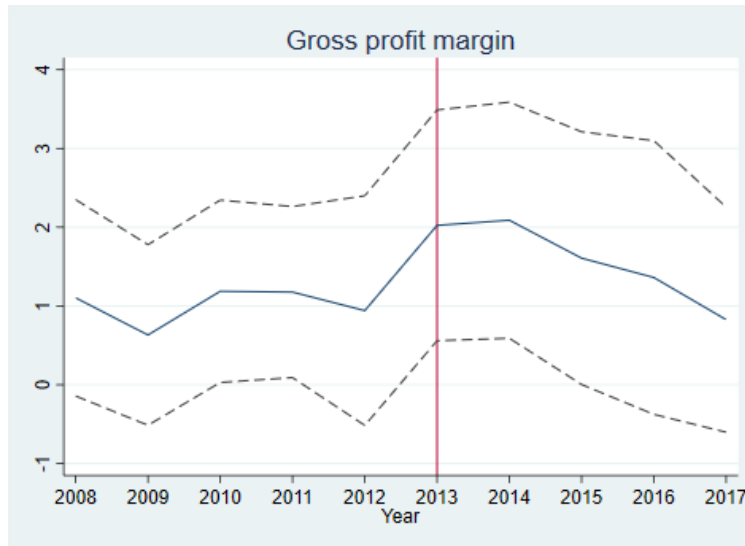
Notes: The figures show the differences in average gross profit margin between the treated and control firms in each year from 2008 to 2017. In Panel A, we define firms as treated if they experienced an increase in distance to the covering DoJ field office, and the other firms as the control group. In Panel B, we define firms as treated if they experienced an increase of distance to the covering DoJ field office more than 400 miles, and the other firms as the control group. The solid line plots the difference in the annual mean values of gross profit margin between the treated and control firms, and the dashed lines plot the 95% confidence intervals of the t-tests on group differences. The vertical solid line indicates the year of field office closure in 2013. All variables are winsorized at 0.5% and 99.5% levels. The sample spans from 2008 to 2017 and contains Execucomp firms with local peers.