Say on Pay Laws and Insider Trading

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Abstract: We examine whether mandatory adoption of say on pay increases executives' incentives to engage in insider trading to compensate for the negative impact of say on pay on the value of their explicit compensation packages. Our empirical design exploits the staggered adoption of say on pay laws across 14 countries over the 2000-2015 period. We find that mandatory adoption of say on pay is associated with a material increase in insider trading profits, especially in firms where executive pay is most affected by say on pay. The increase in insider trading profits is driven mostly by more frequent and larger profitable insider sales, consistent with executives' desire to reduce their greater exposure to firm-specific risk while increasing their trading profits. Overall, our results highlight the importance of considering potential effects on insider trading incentives when designing compensation reforms and when assessing their effectiveness.

Keywords*:* say on pay, insider trading, executive compensation, shareholder voting, shareholder activism, corporate governance, regulation

JEL Classification*:* G30, G34, G38, J33, K22, M12, M16

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I. INTRODUCTION

We examine whether the adoption of a shareholder vote on executive pay (commonly known as "say on pay") is associated with an increase in insider trading profits. Over the last two decades, numerous countries have introduced a say on pay (SoP) regime, generally with the stated goal of increasing pay-for-performance sensitivity (PPS), limiting the growth rate in pay levels (or reducing "excess" pay), and improving the transparency of the pay-setting process.

Prior studies document that PPS and pay riskiness increase around the adoption of SoP laws, while nominal pay levels are generally unaffected (Ertimur, Ferri, and Oesch 2013; Ferri and Maber 2013; Larcker, McCall, and Ormazabal 2015; Correa and Lel 2016). A key implication of these findings is that SoP led to riskier pay without a commensurate risk premium, i.e., *risk-adjusted* pay levels, and thus the value of compensation packages to executives declined. Furthermore, Correa and Lel (2016) find a reduction in the growth rate of pay, implying that post-SoP pay levels increased less than *expected*, absent an SoP regime.

In brief, the evidence suggests that the adoption of SoP reduced the value of compensation packages to executives by making pay riskier (without a corresponding risk premium) and by reducing its growth rate relative to what would be observed absent an SoP regime.[1]

While these studies focus on the effect of SoP on explicit compensation, little attention has been paid to its potential effect on "implicit" compensation. Prior research shows that insider trading serves as an implicit compensation mechanism and thus may substitute for explicit compensation (Manne 1966; Roulstone 2003; Denis and Xu 2013). Combining this idea with the above evidence on the effect of SoP, we predict that after the introduction of SoP executives will

[1] The adoption of SoP also made compensation "riskier" by introducing the risk of ex post shareholder intervention. Executives could negotiate and agree to a certain pay package and then see its terms adversely modified via an SoP vote, reducing the compensation contract's value (see Section II for a discussion of this point.)

seek greater insider trading profits to compensate for the reduction in the value of their compensation packages. This unintended effect of SoP could partially offset any positive effect in terms of aligning managers' and shareholders' interests. Thus, examining the impact of SoP on insider trading is important for a full assessment of the effectiveness of SoP laws.

However, the prediction that SoP can result in higher insider trading profits is not without tension. Executives trade off costs and benefits when engaging in insider trading. The evidence about the effect of SoP on the value of compensation packages implies that the benefits increase after the adoption of SoP, but the costs may also increase. In particular, the adoption of SoP may lead shareholders and reputation-sensitive boards to increase their scrutiny of insider trading activities and/or introduce tighter formal restrictions (e.g., blackout periods). Relatedly, the adoption of SoP may cause, or be symptomatic of, increased monitoring of all executives' activities by various outside parties in the country (media, regulators, governance agencies, etc.), in which case insider trading activity would result in higher reputation and/or litigation costs. Overall, whether the adoption of SoP affects insider trading profits is an empirical question.

To address this question, we follow Correa and Lel (2016) and exploit the staggered adoption of SoP across countries. We employ a difference-in-differences design, estimating changes in firm-level insider trading profits around the adoption of SoP laws. We compute insider trading profits for each firm-year by aggregating estimated capital gains and losses avoided (using size-adjusted returns over 6 and 12 months) from open-market purchases and sales by the firm's top 3 executives during the year (Skaife, Veenman, and Wangerin 2013). Using a sample of over 88,000 firm-year observations from 25 countries (including 14 SoP-adopting countries) over the 2000-2015 period, we find a statistically significant increase in insider trading profits around the adoption of SoP. In terms of economic significance, the increase translates, on average, to about $150,000 of additional implicit annual pay per executive which represents 7.6% of the mean executive pay in SoP-adopting countries prior to SoP adoption. We also find that the effect is most pronounced in firms where the CEO experiences a larger decrease in nominal pay after the SoP adoption, especially when such decrease is accompanied by an increase in PPS, consistent with the increase in insider trading profits substituting for a reduction in risk-adjusted pay, i.e., in the value of explicit compensation.[2]

Recent studies suggest that staggered difference-in-differences estimates may be biased in the presence of heterogeneous treatment effects (Baker, Larcker, and Wang 2022; Barrios 2021). Thus, we also perform two robustness tests suggested in the literature, i.e., the Goodman-Bacon decomposition (Goodman-Bacon 2021) and the stacked difference-in-difference method (Cengiz, Dube, Lindner, and Zipperer 2019). From these tests, we conclude that our estimates are not significantly affected by heterogeneous treatment effects.

The firm-year level measure of insider trading profits combines three distinct components: trade informativeness (i.e., the ability to predict future stock returns), trade size, and trade frequency (Skaife et al. 2013). It also combines profits from purchases and sales. Thus, in our last set of tests, we try to identify the source of the documented change in insider trading profits by investigating the three components separately for insider purchases and insider sales. We find that the increase in insider trading profits is driven primarily by more frequent and larger profitable insider sales, consistent with executives' desire to reduce their greater exposure to firm-specific risk (due to greater use of risky pay under SoP) while increasing their trading profits. We also find

[2] Specifically, in the cross-country sample, where we do not have sufficient data to measure PPS and risk-adjusted pay, we partition on nominal CEO pay and find stronger effects among CEOs experiencing a decline in nominal pay. While nominal pay does not change on average in response to SoP, decreases in nominal pay in the cross-section still capture CEOs likely to experience a decrease in risk-adjusted pay (given the evidence of an average increase in PPS) and thus most affected by SoP. Among U.S. firms, for which we have more granular data, we find the results to be stronger for CEOs that experience a decrease in nominal pay *and* an increase in PPS, i.e., CEOs more likely to experience a decline in risk-adjusted pay.

some evidence that post-SoP insider trades – especially insider sales – are more likely to take place during information-sensitive trading windows (i.e., before earnings announcements).

A common concern with cross-country studies exploiting the staggered adoption of specific governance mandates is that such mandates are often part of broader governance reforms. Hence, it is difficult to attribute the documented effects to the specific mandate. For example, in the US, SoP was part of the Dodd-Frank Act, a comprehensive piece of legislation introducing numerous major reforms. However, this concern is not as pronounced in our setting because any governance reform accompanying the adoption of SoP should, if anything, strengthen monitoring mechanisms and thus directly or indirectly *restrict* insider trading opportunities (e.g., Jagolinzer, Larcker, and Taylor 2011; Skaife et al. 2013; Dai, Fu, Kang, and Lee 2016; Hong, Li, and Zhu 2019; Davidson and Pirinsky 2021; Yost and Shu 2022). Thus, in our setting, the presence of concurrent governance reforms should bias against finding an increase in insider trading profits. On the other hand, if the adoption of other governance reforms were negatively correlated with the adoption of SoP laws, our results could be spurious (that is, they could be driven by reduced insider trading profits at control firms rather than increased insider trading profits at treatment firms). To address this concern, we control for other governance reforms that plausibly affect insider trading activity during our sample period in untabulated analyses. Our results are unchanged.

Our study contributes to the research on executive compensation and the research on insider trading. With respect to the former, we add to a recent stream of studies examining the effect of SoP, one of the most debated governance reforms of the last two decades (e.g., Ertimur et al. 2013; Ferri and Maber 2013; Larcker et al. 2015; Cuñat, Gine, and Guadalupe 2016; Malenko and Shen 2016). While prior studies focus on the drivers of SoP votes and the impact of SoP laws on explicit compensation, our study is the first to examine its impact on a form of implicit compensation –

insider trading profits. Understanding such unintended effects is important for investors and policymakers evaluating the effectiveness of SoP laws.

In this respect, it is important to note that our study does not assume or imply that compensation practices were sub-optimal prior to the adoption of SoP laws. If one views the introduction of SoP as correcting sub-optimal pay practices and reducing excess pay, then an increase in insider trading profits would suggest that executives make up for the decline in the value of their compensation packaging via insider trading, and boards and shareholders are unable (or perhaps unwilling, in the case of boards) to prevent or detect such behavior. If so, the positive effects of SoP laws may have been overstated. For example, the documented increase in pay-for-poor-performance sensitivity may have been partially offset by larger and more frequent profitable insider sales. In this scenario, the implication for regulators considering compensation-related reforms is to contemporaneously strengthen restrictions and controls on insider trading activities, while the implication for investors (and proxy advisors) is to include insider trading in their analyses of SoP proposals.

Alternatively, if one believes that compensation packages were generally optimal prior to SoP, an increase in insider trading profits may be consistent with an efficient contracting story: executives require a risk premium to compensate for "riskier" pay, and boards – anticipating shareholder and media scrutiny over an increase in explicit pay levels – respond by allowing executives to capture this premium via higher implicit pay (insider trading profits), which is harder for outsiders to detect. In this scenario, investors and regulators should still be aware that the mix of explicit and implicit pay (and the related incentives) will be affected. Under either interpretation, our study highlights that it is important to account for the impact of compensation reforms such as SoP on implicit compensation when designing these reforms and assessing their impact.

As for the literature on insider trading, we build on a limited number of studies examining the role of insider trading as a form of implicit compensation. Roulstone (2003) finds that firms imposing tighter insider trading restrictions pay a premium in total compensation levels and use higher incentive-based pay, consistent with insider trading playing a role in both rewarding and motivating executives. Denis and Xu (2013) document similar associations in a cross-country setting. In addition, they report significant increases in executive pay levels and in the use of equity-based incentives following the initial enforcement of insider trading laws. Overall, these studies suggest that boards optimally view insider trading profits as part of the total compensation package and design compensation awards and insider trading restrictions accordingly, i.e., they trade off explicit and implicit compensation.[3]

Similar to Roulstone (2003) and Denis and Xu (2013), we rely on the notion that an equilibrium exists between explicit and implicit executive compensation. However, while Denis and Xu (2013) examine how executive pay changes in response to greater restrictions to insider trading (insider trading laws and enforcement), we examine how insider trading profits change in response to a form of restriction on executive pay (the SoP regime). This difference is important because the forces and frictions potentially preventing a shift back toward the equilibrium are different. Restrictions to insider trading, such as insider trading laws, usually aim to protect markets' integrity, rather than reduce executive pay. Thus, to the extent that boards understand that insider trading is a form of implicit pay, it is not surprising that they would be able to optimally "adjust" (explicit) executive pay. In contrast, as discussed earlier, the adoption of SoP laws is often motivated by the perception of excesses in executive pay and thus could also result in greater

[3] In theory, one could study how executives' insider trading behavior changes as a function of yearly changes in explicit compensation. However, doing so would be subject to the usual endogeneity concerns. The advantage of the SoP setting is that it alleviates such concerns whilst allowing us to speak to the consequences of SoP laws.

monitoring of insider trading profits (a source of implicit compensation) by shareholders, boards, and other parties. Thus, it is less obvious that executives would be able to "adjust" their insider trading profits to make up for the loss in explicit pay.

Relatedly, while those studies show that *boards* consider insider trading restrictions in designing optimal pay packages from the perspective of shareholders, we focus on how *executives* change their insider trading behavior to compensate for a change in their explicit compensation based on their personal utility function. In doing so, we provide evidence that executives view insider trading profits as part of their total pay package. In this respect, we complement two concurrent studies: Gao (2022), who finds that executives use insider trading to make up for the loss in expected compensation due to missing relative performance goals, and Goldman and Ozel (2023), who find that executives partially offset increases in individual-level tax rates by seeking greater insider trading profits.

Finally, we contribute to a stream of research on the unintended consequences of different types of regulation. In the context of executive pay, Murphy (2013) reviews the unintended effects of various tax, accounting, and disclosure regulations in the US over the last century. Kleymenova and Tuna (2021) examine a regulation introduced in the UK to change bank executives' risk-taking incentives and find that it achieved its objective but also resulted in higher unforced CEO turnover. Bae, Gong, and Tong (2023) find that in response to pay cuts imposed on CEOs of centrally administered state-owned enterprises in China in 2009, these CEOs increased their consumption of perks and siphoned off firm resources for their own benefit. We extend this research by focusing on insider trading behavior as a potential side effect of compensation reform.[4]

[4] If executives resort to other forms of implicit compensation (e.g., greater consumption of perks; Bae et al. 2023), focusing on disclosed insider trading may lead us to understate the effect of SoP on implicit compensation, especially in certain countries. At the same time, since SoP reforms are often accompanied by requirements of more detailed disclosures of all pay elements, it is unlikely that perks would be the main form of implicit pay affected by SoP.

II. CONCEPTUAL DEVELOPMENT AND PREDICTIONS

Insider Trading as an Implicit Compensation Mechanism

The notion that insider trading may serve as an implicit compensation mechanism – and thus as a substitute for explicit compensation – dates back to Manne's (1966) seminal work. It follows from this notion that in equilibrium we should observe a positive association between insider trading restrictions and compensation *levels*. The reason is that such restrictions limit the ability of insiders to exploit their private information; thus, if wages are set competitively, executives need to be compensated via higher explicit pay (Baiman and Verrecchia, 1995, 1996).

Prior studies also posit a positive association between insider trading restrictions and *incentive pay*. One argument is that since unrestricted insider trading provides greater incentives to increase rather than decrease firm value (due to restrictions on short-selling and reputation and litigation concerns), any restriction will require firms to provide greater incentive pay (Manne 1966). Also, by preventing executives from rebalancing their equity portfolios at optimal times, restrictions on insider trading can reduce the value of equity awards (Core and Guay 2001); thus, larger equity grants are required to maintain optimal incentive levels. Finally, restricting insider trading can reduce the information about insider actions impounded into price (Damodaran and Liu 1993), thereby requiring a corresponding increase in incentive pay to solve moral hazard problems.[5]

Consistent with the above arguments, empirical studies document a positive association between (firm-level or country-level) insider trading restrictions on one hand, and both

Nonetheless, it remains possible that executives resort to other forms of rent extraction and private benefits of control unobservable to the researcher (or harder to detect in a large-sample study).

[5] Another argument is that insider trading restrictions make executives less willing to choose risky projects (Bebchuk and Fershtman 1994), and thus optimal contracts will require larger grants of risk-taking incentives (e.g., stock options). In all of the above arguments, the causation runs from the insider trading restriction to equilibrium compensation. Another argument is that higher insider trading restrictions may be required to mitigate trading-related agency costs associated with higher equity incentives. In this view, the causation runs from incentive pay to insider trading restrictions. However, Denis and Xue (2013) present evidence inconsistent with this argument.

compensation levels and incentive pay on the other (Roulstone 2003; Denis and Xu 2013). These findings suggest that insider trading plays a role in both rewarding and motivating executives and thus acts as an implicit compensation mechanism.

If insider trading serves as implicit compensation, it is plausible that executives will seek greater insider trading profits after the adoption of SoP laws as long as they perceive that (i) SoP negatively affects their explicit compensation, and (ii) the benefits from an increase in insider trading profits exceed the costs associated with the change in insider trading behavior required to generate those additional profits. We discuss these two conditions next.

Effect of Say on Pay on Compensation

Numerous studies have examined the effect of SoP laws (see Ferri and Göx 2018 for a review). One group examines changes to specific contractual features induced by the SoP regime. These studies conclude that the introduction of SoP resulted in the removal of controversial provisions viewed as rewards for failure and/or the inclusion of provisions tightening the link between pay and performance;[6] thus, SoP resulted in higher PPS (e.g., Ertimur et al. 2013, Ferri and Maber 2013; Larcker et al. 2015). Another group examines the effect of SoP on PPS, pay levels and growth rates in pay using a "pre" vs. "post" analysis (e.g., Ertimur et al. 2013; Ferri and Maber 2013, Correa and Lel 2016). In general, these studies find that PPS increases and the growth rate in pay levels decreases around the adoption of SoP, but there is little evidence of a decline in pay levels.

[6] For example, Ferri and Maber (2013) find that in the United Kingdom the introduction of SoP in 2003 resulted in the removal of severance payments greater than one year of compensation and the elimination of retesting provisions in equity grants. Similarly, Ertimur et al. (2013) find that in the United States the adoption of SoP led to the introduction of performance-based vesting conditions in equity grants (in lieu of time-based vesting), the use of tougher performance targets, and the removal of controversial provisions from severance contracts.

If SoP laws did not result in a decline in compensation levels, why would executives seek greater implicit pay? Two observations support such prediction. First, the above studies document an increase in PPS. The combination of higher PPS and unchanged pay levels is puzzling, since optimal contract theory predicts that an increase in "risky" pay (higher PPS) should be accompanied by an increase in the level of pay as "risk premium" (e.g., Pratt 1964). We conjecture that under an SoP regime, reputation-sensitive boards cannot offer an increase in explicit pay levels to compensate for riskier pay because doing so, even if optimal, would be interpreted as an attempt to circumvent the increase in PPS demanded by shareholders and would trigger a backlash.[7] Whatever its reason, the combination of riskier pay without an increase in the *nominal* level of pay (i.e., without a commensurate risk premium) implies that post-SoP executives are receiving *lower risk-adjusted pay* and thus will have an incentive to seek greater implicit compensation.[8] Furthermore, under an SoP regime executive pay is also "riskier" because of the risk of ex post shareholder intervention. That is, agreed-upon terms can be subsequently modified via SoP votes (an example is the removal of excise tax gross-ups; Ertimur et al. 2013), resulting in a reduction of the value of the compensation contract. In these situations, shareholders' scrutiny would make it difficult for executives to renegotiate the rest of the package or to receive compensation for those modifications, creating further incentive to resort to implicit compensation.

[7] In some cases, boards may not want to increase pay levels because they agree with shareholders that pre-SoP pay levels are too high relative to the riskiness of pay (i.e., pre-SoP contracts were sub-optimal), and thus the optimal action is to increase PPS without increasing the level of pay. This raises the question of why such boards did not increase PPS themselves prior to the adoption of SoP. Bebchuk and Fried (2004) argue that directors hesitate to question CEO pay out of loyalty or to avoid negative effects on collegiality and teamwork in the boardroom.

[8] This idea goes back to the certainty-equivalent framework introduced by Lambert, Larcker and Verrecchia (1991) and further developed by Hall and Murphy (2002). Hall and Murphy (2002) show that most of the increase in total pay levels in the 1990s is explained by a risk premium for "risky" equity pay (i.e., on a risk-adjusted basis, the rise in compensation levels was fairly small), and that most of the cross-industry differences in nominal pay levels can be explained by a risk premium for risky pay. Along the same lines, Conyon, Core, and Guay (2011) and Fernandes, Ferreira, Matos, and Murphy (2013) show that most of the difference in nominal pay levels between US CEOs and CEOs in other countries can be attributed to a risk premium for greater use of performance-based pay.

Second, Correa and Lel (2016) show that SoP laws result in *slower growth* in pay levels. That is, while nominal pay levels do not decline (in absolute terms) relative to pre-SoP pay levels, they are lower *relative* to what they would have been absent SoP, i.e., relative to their *expected* level. Consistent with executives caring about the *expected* level of pay, Gao (2022) finds an increase in subsequent insider trading profits among executives who barely miss a compensation target (i.e., executives who receive less compensation than expected). Thus, even setting aside the decline in risk-adjusted pay, if executives perceive their post-SoP nominal pay to be lower than it should be (absent the SoP regime), they will have an incentive to seek an increase in implicit compensation.

Combined, the SoP literature suggests that the adoption of SoP laws had a negative impact on the value of compensation packages to executives. Thus, we predict that after the adoption of SoP laws, executives will have an incentive to seek greater implicit pay via insider trading.

Costs and Benefits from Insider Trading: The Effect of SoP Laws

In the pre-SoP regime, executives presumably trade off the costs and benefits from insider trading. The benefits include the profits generated or losses avoided from trading, while the costs include litigation risk, reputation costs, potential signaling effects, and the risk of violating mandatory minimum holding requirements. The evidence in the previous subsection implies that after the adoption of SoP laws, the benefits from insider trading profits are higher. But it is conceivable that SoP also increases the costs associated with insider trading. For example, scrutiny of executives' activities (including insider trading) by various parties (boards, shareholders, media, regulators) may increase and thus result in higher reputation costs and litigation risk. In this case, the benefits from an increase in insider trading profits may not exceed the associated costs. Therefore, whether the adoption of SoP affects insider trading profits is an empirical question.

III. SAMPLE SELECTION AND RESEARCH DESIGN

Sample selection

Our key data source is the 2iQ Research database, which contains historical records of insider transactions of publicly traded firms in 50 countries between 2000 and 2015. Since we require at least two years of insider trading data under a SoP regime, our sample includes 14 countries that adopted a mandatory SoP regime starting in 2014 or earlier and 11 countries that did not adopt such a regime during the same period. To identify SoP adopters, we update the list in Correa and Lel (2016) – which covers SoP adoption up to 2012 – using information from various web sources. After requiring data on both insider trading and the standard controls of insider trading profits, we obtain a final sample of 88,640 firm-year observations comprised of 9,925 unique firms. Overall, 34.3% of the firm-year observations are classified as treated, i.e., as occurring under a mandatory SoP regime.

Table 1 lists for each country the number of firms and firm-year observations, the year of the SoP adoption (defined as the first full year under SoP) and the type of SoP regime for adopting countries. With respect to the latter, SoP regulations display substantial heterogeneity around the world. Of the 14 SoP countries, eight adopted an advisory SoP regime, where shareholders cast an advisory (non-binding) vote on the remuneration *report* detailing the pay packages awarded during the *prior* fiscal year. The board may decide to ignore the vote or consider it in determining pay packages going forward, but past payments to the executives are not affected (the US is an example of this regime). While non-binding, dissent votes often lead to substantial changes in pay practices (Ertimur et al. 2013; Ferri and Maber 2013).[9] The other six countries adopted a binding SoP

[9] Ferri and Maber (2013) report that a high fraction of UK firms shortened notice periods (and thus severance payments) and eliminated re-testing provisions (viewed as a form of reward for failure) from equity grants with performance-based vesting, either in response to or in anticipation of a high dissent SoP vote. Ertimur et al. (2013)

regime, where shareholders cast a vote on the *forward-looking* compensation *policy* (i.e., a general framework about how compensation packages will be awarded, rather than the specific awards). If the vote is negative, the policy is not approved and thus cannot be implemented by the board, who will need to present a new proposal. One country (the UK) adopted an advisory vote on the compensation report first and then added a binding vote on the remuneration policy.[10]

Research Design

To examine the effect of SoP laws on insider trading profits, we estimate the following generalized difference-in-differences (DiD) regression:

$$PROFIT\%_{i,t} = \beta_1 SoP_{i,t} + \sum X_{i,t-1} + \alpha_i + \gamma_t + \varepsilon_{it} \qquad (1)$$

where $PROFIT\%_{i,t}$ is our estimate of insider trading profits at the firm-year level, and $SoP_{i,t}$ is an indicator variable that equals one for firm-years after the adoption of mandatory SoP (including the adoption year), zero otherwise. The coefficient β_1 captures the average change in insider trading profits after the adoption of mandatory SoP relative to the control group. For each adopting country, the control group includes pre-SoP observations from countries that will adopt SoP later, post-SoP observations from countries that adopted SoP earlier, and firm-year observations from countries that did not introduce a mandatory SoP regime during the sample period.

One could test Eq. (1) only within countries adopting SoP during the sample period. This approach yields a more homogenous control sample (only countries adopting at some point in

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report that over half of US firms made compensation changes explicitly in response to negative ISS recommendations on SoP votes.

[10] Importantly, the distinction between advisory and binding votes does not fully capture the large amount of variation in the specific terms of SoP regimes across countries. For example, while the vote is held annually in most advisory SoP regimes, the US allows shareholders to opt for a biennial or triennial frequency (Ferri and Oesch 2016; Kronlund and Sandy 2018). In contrast, binding SoP regimes usually require a vote only every three years or when there are significant changes to the compensation policy. Countries also differ in the extent to which shareholders are allowed to vote on other compensation-related matters (e.g., approval of equity compensation plans). For a detailed review of different SoP regimes, see Thomas and Van der Elst (2015). Ferri and Göx (2018, Ch. 3) analyze the effectiveness of advisory versus binding votes (as well as retroactive versus prospective votes).

time), while preserving the benefits of the staggered design. However, it can introduce bias if the treatment effect is heterogeneous and time-varying (Baker et al. 2022; Barrios 2021). For example, if the treatment effect for early SoP adopters takes place gradually over time, the corresponding observations would not be a "good" control sample for late SoP adopters. In contrast, the inclusion of never-adopting countries allows for a control sample closer in spirit to a traditional DiD around a single event, where the control sample is truly untreated both before and after the event of interest, but the downside is a less homogenous control sample.[11] The implication of our design is that our estimate for β_1 in Eq. (1) captures a weighted average of many different comparisons. In Section IV, we will perform some tests to disentangle the relative weight of each comparison in our estimate and ensure that potential heterogeneous treatment effects do not affect our inferences.

$X_{i,t-1}$ represents a vector of time-varying firm-level covariates. We include firm fixed effects (α_i) to control for time-invariant factors at the firm level and year fixed effects (γ_t) to account for time-varying macroeconomic conditions. Standard errors are clustered by firm.

Measuring Insider Trading Profits

Following prior studies (e.g., Huddart and Ke 2007; Skaife et al. 2013; Chung, Goh, Lee, and Shevlin 2018), we define the profit of insiders' trades as the total (unrealized) capital gains after open-market purchases and the total losses avoided by open-market sales from the insiders. We focus on CEOs, CFOs, and COOs (hereinafter "insiders") because the number of top executives covered by SoP laws differs across countries, and this choice allows for a comparable measure across countries (in robustness tests we repeat the analysis including only the CEO).

To calculate insider trading profits at the firm level, we obtain data on open-market buy and sale transactions by the firm's insiders from the 2iQ Research database. We then sum the trading

[11] Untabulated tests indicate that firms in never-adopting countries are smaller, have lower analyst coverage and institutional ownership, and have higher insider ownership.

value (number of shares multiplied by stock price) of buy and sale transactions by each insider occurring on the same day, thereby obtaining a firm-day level measure of trading value separately for buy and sale transactions. Next, for buy transactions, we multiply the trading value at the firm-day level by the 6-month (12-month) buy-and-hold abnormal return after the trade, yielding an estimate of the insiders' unrealized capital gain over a 6-month (12-month) period for each transaction day.[12] For sale transactions, we take the negative of the product of the trading value at the firm-day level and the 6-month (12-month) buy-and-hold abnormal return, yielding an estimate of the insiders' avoided loss over a 6-month (12-month) period for each transaction day.

Finally, we sum the capital gains and avoided losses from all transaction days during the fiscal year. This provides a firm-year level estimate of insider trading profits in dollar terms, which we then scale by the market value at the end of the previous fiscal year. The following equation summarizes our computation:

$$PROFIT\%_{it} = \frac{\sum_{j=1}^{n} BHAR_{itj} \times VALUE\_BUY_{itj} - BHAR_{itj} \times VALUE\_SALE_{itj}}{MV_{it-1}} \times 100$$

where $BHAR_{itj}$ is the buy-and-hold abnormal return over 6 or 12 months after transaction day *j* (depending on whether we compute $PROFIT_{6m}\%_{it}$ or $PROFIT_{12m}\%_{it}$); $VALUE\_BUY_{itj}$ ($VALUE\_SALE_{itj}$) is the total traded value in dollars for buy (sale) transactions by firm *i*'s top 3 executives on day *j*; *n* equals the total number of days with insider trades during the firm-year (*it*); and MV_{it-1} is the firm's market capitalization at the end of fiscal year *t-1*. For *BHAR*, we compute

[12] The literature on insider trading documents abnormal returns following insider trades over various windows from a few days (e.g., Lakonishok and Lee 2001) to 12 months or more (e.g., Seyhun 1986). We choose six months as a lower bound because in the US the short swing rule requires insiders to disburse to the firm profits on round-trip transactions within that window. Hence, if insiders wish to increase their compensation, their information advantage should last at least six months. Even if the short swing rule has no equivalent outside of the US, non-U.S. studies also document abnormal returns over similar windows (e.g., Gregory et al. 1997 for UK insiders). We choose 12 months as the upper limit for our tests because we expect returns over longer windows to be noisier and to coincide with the next pay cycle, and thus with insiders' next round of trading decisions.

the abnormal return as the difference between the raw return and the value-weighted return on the portfolio that consists of stocks in the same quintile by market capitalization within the stock exchange where the firm is listed.[13] We refer to this measure as "size-adjusted returns."

Note that the above *PROFIT* measure combines both the size and value of the trade (capturing trade materiality) and the returns from trading (capturing the trades' predictive ability of future stock returns), consistent with our objective of measuring the change in implicit compensation from insider trading activity. In Section V we will examine the different components separately.

Control Variables

In Eq. (1) we control for firm-level variables known to affect insider trading incentives and profits, including firm characteristics, information environment, price informativeness, and total beneficial ownership of the top 3 executives (Seyhun 1986; Ofek and Yermack 2000; Frankel and Li 2004; Piotroski and Roulstone 2005; Skaife et al. 2013; Ryan, Tucker, and Zhou 2016). In terms of firm characteristics, we control for firm size (logged market capitalization, $Ln(MV_{t-1})$), growth (book-to-market ratio, BTM_{t-1}) and momentum (annual buy-and-hold abnormal returns, $BHAR_{t-1}$). These variables correlate with insider trading patterns, as insiders tend to buy (sell) relatively more shares in smaller (larger) firms, in value (growth) firms, and following poor (high) performance (e.g., Lakonishok and Lee 2001). The variables also capture risk factors that explain variation in post-transaction abnormal returns and profits (e.g., Fidrmuc et al. 2013).

To capture the firm's information environment, we control for analyst coverage (the natural logarithm of one plus the number of analysts following the firm, $Coverage_{t-1}$), which has been shown to be negatively associated with insider trading profits (Frankel and Li 2004). We also include total institutional ownership ($INST_{t-1}$), an indicator for non-zero reported R&D expense

[13] For each stock exchange, we assign each stock to a size quintile based on the market capitalization at the calendar year end and require that each quintile has at least five stocks.

(*$R\&D_{t-1}$*), and return volatility (*$RetVol_{t-1}$*), to further control for differences in information asymmetry across firms. Insiders trade less profitably when institutional ownership is higher (Hillegeist and Weng 2021), more often and more profitably in R&D firms (Aboody and Lev 2000; Huddart and Ke 2007) and more often when return volatility is higher (Frankel and Li 2004). To proxy for price informativeness, we follow Chen, Goldstein, and Jiang (2006) in using price non-synchronicity (*$Non\text{-}Synch_{t-1}$*), a measure of the extent to which the firm's stock price does not follow the price of the overall equity market and thus is informative about the firm's fundamentals. Insiders of firms whose stock exhibits more idiosyncratic movement have more (profitable) trading opportunities in connection with their private information (Piotroski and Roulstone 2004). Finally, we include *$InsiderOwn_{t-1}$* to control for the total beneficial ownership of insiders (i.e., the top 3 executives: CEO, CFO, and COO). This allows us to capture the potentially higher propensity of top executives with higher stock ownership to trade to reduce their exposure to firm risk.

Stock (flow) variables are measured at the end of (during) fiscal year t-1. Market capitalization is converted from local currency to US dollars based on the historical exchange rate. Detailed definitions and data sources for the above variables are in the Appendix. To mitigate the potential effect of outliers, we winsorize all continuous variables at the 1% and 99% levels.

IV. EMPIRICAL ANALYSES: INSIDER TRADING PROFITS AROUND SAY ON PAY

Summary Statistics

Table 2, Panel A reports the summary statistics of the variables used in our tests of Eq. (1), all at the firm-year level. The mean insider trading profit over a 6-month (12-month) period is 0.010% (0.014%) of market value. The average observation in the full sample has a market value of equity of $2.82 billion, a book-to-market ratio of 0.74 and past year's buy-and-hold abnormal returns of 9.2%. About 33% of the observations have non-zero reported R&D, the mean number of analysts covering a firm is 15.5 (33.0% of the observations have no analyst coverage; untabulated), and mean institutional and insider ownership are, respectively, 28.2% and 3.0%. Table A1 of the Online Appendix (hereinafter OA) reports mean differences in the above variables between pre- and post-SoP observations for firms in countries that adopt mandatory SoP laws.

Panel B of Table 2 displays the correlations between insider profits and our control variables for the full sample In line with prior studies, insider trading profits are negatively correlated with firm size and information environment proxies (analyst coverage and institutional ownership) and positively correlated with the book-to-market ratio and insiders' ownership (e.g., Seyhun 1986).

Baseline Results: SoP Laws and Insider Trading Profits

Table 3 presents our baseline results on the effect of SoP adoption on insider trading profits, based on the DiD specification in Equation (1). We first report the results with firm and year fixed effects but without any firm-level controls. We do so to prevent "bad controls" (firm characteristics that may also be impacted by the treatment) from affecting our ability to draw causal inferences (Gormley and Matsa 2014). The coefficient of $SoP_{i,t}$ is positive and significant at the 1% level for both insider trading profits measures, i.e., $PROFIT_{6m}\%_{it}$ (column 1) and $PROFIT_{12m}\%_{it}$ (column 2) The coefficients remain similar in magnitude and significance when we include the firm-level controls (columns 3 and 4). The stability of the coefficient across models with and without firm-level controls, combined with the stability of the R-squared reduces concerns with spurious effects from potential correlated omitted variables (Oster 2019).

In terms of economic significance, our estimate in column 4 (using $PROFIT_{12m}\%$) implies that the dollar value of insider trading profits increases by 0.016% of the firm's market capitalization. Given the average market capitalization of about \$2.825 billion (Table 2 Panel A), this translates to an approximately \$452,000 increase in annual implicit compensation, i.e., an average of over

$150,000 per executive. This increase represents about 7.6% of the mean executive pay in SoP-adopting countries the year prior to SoP adoption ($1,988,000; untabulated). As a comparison, Gao (2022) examines the effect of missing a compensation target on subsequent insider trading profits and reports an increase in insider trading profits equal to 8% of total pay.

Ideally, one would also want to compare our estimate to the potential loss in explicit compensation under an SoP regime. Using a similar staggered DiD design, Correa and Lel (2016) estimate that after SoP adoption, annual total CEO pay decreases by 9.1% (3.3%) for firms in the bottom (top) quartile of industry-adjusted stock performance, relative to CEO pay in non-SoP-adopting countries. However, the estimate in Correa and Lel (2016) is likely to be a lower bound for two reasons. First, as discussed in Section II, the adoption of SoP laws resulted in higher PPS without a commensurate risk premium in pay levels. Thus, any estimate of the change in nominal pay understates the change in *risk-adjusted* pay, which better measures the value of compensation to executives (Lambert et al. 1991; Hall and Murphy 2002). Second, the estimate in Correa and Lel (2016) – or any study based on annual total direct pay reported in standard databases – does not capture many changes in pay contracts documented by prior studies. Moreover, even if one could precisely quantify the effect of SoP on pay packages, a comparison with the increase in insider trading profits might not be accurate if these two effects have different persistence. Overall, we can only conclude that the increase in insider trading profits is material (relative to executive pay) and may partially offset the negative impact of SoP laws on explicit compensation packages.

Finally, we also benchmark the documented effect in terms of the standard deviation of insider trading profitability. Our coefficient estimates in columns 3 and 4 of Table 3 (0.009 and 0.016, respectively) imply that the change in insider trading profits is about 5.2 to 5.5% of the standard deviation of insider trading profits for the corresponding sample (0.174 for $PROFIT_{6m}\%$ and 0.292

for $PROFIT_{12m}\%$; untabulated). The small effect implies that even a relatively small change in insider trading profits relative to its standard deviation is sufficient to generate a significant increase in implicit pay. This finding is consistent with the notion that over time the widespread use of equity grants has resulted in equity holdings that can yield large trading profits. It also adds plausibility to our estimate, because – as noted in Section II – there are various constraints to insider trading activity (black-out windows, reputation and ligation risk, liquidity concerns, etc.); thus, a large change in insider trading profits relative to its standard deviation seems unlikely.

As for the control variables, insider trading profits are positively associated with asynchronicity (Piotroski and Roulstone 2004), R&D (Aboody and Lev 2000), insider ownership, and return volatility (Frankel and Li 2004), and negatively so with firm size (Lakonishok and Lee 2001), consistent with insiders trading more profitably in stocks with more idiosyncratic price movements and firm characteristics indicative of greater information asymmetry. Furthermore, the positive coefficient on insider ownership suggests that insiders' overall trading profits are higher when their stock holdings are greater in percentage.

To sum up, our baseline tests suggest an economically significant increase in insider trading profits around the adoption of SoP, consistent with executives relying on insider trading to compensate for the reduction in the value of their explicit compensation.

Assumptions behind Difference-in-Differences Estimations

Our research design differs from a simple DiD model with a single treatment event and instead exploits the staggered adoption of SoP laws across countries over two decades. This empirical approach is also known as two-way fixed effects estimation because staggered DiD estimations in panel data typically rely on unit (e.g., firm) and time (e.g., year) fixed effects. Recent studies in econometrics highlight that two-way fixed effect DiD can produce severely biased estimates (e.g., de Chaisemartin and D’Hautfeuille 2020; Abraham and Sun 2021). Biases arise because two-way fixed effects estimates capture a weighted average of many different comparisons, such as using firms that received the treatment early (late) as a control for firms receiving the treatment late (early). These studies show that such comparisons can introduce bias if the treatment effect is heterogeneous and varies over time, possibly leading to improper inferences. For example, if the treatment effect for early adopters takes place gradually over time, they might still be experiencing a change due to the treatment while serving as a control for later adopters, which would compromise their ability to act as a “good” control sample for these late adopters. Also, the treatment effect itself may differ between late and early adopters, which could artificially bias the overall estimate upwards or downwards.

Thus, following recent methodological reviews in accounting and finance (e.g., Baker et al. 2022; Barrios 2021), we perform two additional tests to correct for the potential biases due to heterogeneity and time variation in treatment effects discussed above and thus ensure the validity of our findings. First, we adopt the methodology developed by Goodman-Bacon (2021), which classifies the observations into the following four categories: Early Adopters (firms in countries that adopt an SoP law towards the beginning of the sample period), Late Adopters (firms in countries that adopt an SoP law towards the end of the sample period), Treated firms (firms in countries that adopt an SoP law at any point during the sample period), Never Treated firms (firms in countries that never adopt an SoP law during the sample period), and Already Treated firms (firms in countries that adopt an SoP law before the beginning of our sample period). Whereas the baseline DiD estimate is a weighted average of various comparisons, the Goodman-Bacon decomposition identifies the weight of each of the potential comparisons as well as the treatment effect estimate attributed to each comparison among the groups above.

It is important to stress that the Goodman-Bacon (2021) decomposition requires a balanced sample. This poses a specific challenge in our setting because insider trading data are not available for all countries at the beginning of our sample period. To account for this challenge, we restrict our sample period to 2006-2015, which allows us to use insider trading data for 23 out of the 25 countries used in our baseline results in Table 3 (i.e., all except Finland and Portugal). The cost of this choice is that four countries received the treatment by 2006 and thus do not exhibit any variation during this time period (thus they belong to the Already Treated group).

Table 4 Panel A reports the results of the Goodman-Bacon decomposition. The top row shows our DiD estimate using the balanced sample for 2006-2015, confirming our positive and significant baseline estimates in Table 3 (note that the Goodman-Bacon decomposition is estimated without covariates; thus, the proper benchmarks are columns 1 and 2 in Table 3). Next, it shows the weight and treatment effect estimate (Beta) for each of the underlying comparisons. The treatment effect estimates are largest for Treated vs. Never Treated (0.012 and 0.019 for 6- and 12-month profit, respectively) followed by Treated vs. Already Treated. The comparison Early Adopters vs. Late Adopters also yields a positive coefficient, but its magnitude is close to zero for 6-month profits, while the comparison Late Adopters vs. Early Adopters generates a negative coefficient.[14]

Furthermore, most of the weights in our DiD estimate come from comparing the groups of Treated vs. Never Treated (56.2% weight) and Treated vs. Already Treated (31.5%). That is, most of the treatment effect is driven by comparing treated firms to control firms that are stable in their status around the event of interest, similar to a traditional DiD around a single event. In contrast, and importantly, the weights associated with the timing comparisons – Early Adopters vs. Late

[14] The negative coefficient suggests that a time-varying treatment effect for Early Adopters makes firms in that group a "bad" control for firms in the group of Late Adopters, leading to an underestimation of the true effect of SoP on insider trading profits.

Adopters (with Late Adopters used as controls) and Late Adopters vs. Early Adopters (with Early Adopters used as controls) – are low (7.6% and 4.7%, respectively).[15] Since these timing comparisons can introduce significant bias if the treatment effects are time-varying and heterogeneous, it is comforting that they contribute only marginally to our DiD estimate.

Overall, the Goodman-Bacon (2021) decomposition suggests that the effect is derived mostly from comparing treated firms to the most stable control group (especially the Never Treated firms) and that any bias due to heterogeneous treatment effects should be modest in our sample.

Notwithstanding the reassuring results described above, we follow the recommendations in Barrios (2021) and report the stacked methodology proposed by Cengiz et al. (2019). Specifically, we construct a separate dataset for each treatment event (i.e., the adoption of SoP by a given country) such that a given firm can only appear either in the treatment group or in the control group with respect to the event. That is, for each event we restrict the control group to firms that do not receive the treatment (i.e., do not adopt SoP) in the period ranging from five years before to five years after the event. Hence, the control sample includes all firms that do not adopt SoP during the window, whether because they adopted it prior to the window, because they adopted it subsequent to the window, or because they never adopted it during our sample period. The benefit of this approach is that it removes from the control sample any firm that receives the treatment at a point in time close to the treatment event of interest, because such a firm could be a "bad" control in the presence of heterogeneous, time-varying treatment effects.

[15] The Early Adopters vs. Late Adopters comparison captures the portion of the overall DiD estimate that comes from comparing Early Adopters pre and post treatment using Late Adopters as a control group (before observations in this group receive the treatment). The Late Adopters vs. Early Adopters comparison captures the portion of the overall DiD estimate that comes from comparing Late Adopters pre and post treatment using Early Adopters as a control group (but after observations in this group received the treatment).

All the event-specific datasets are then stacked to calculate an average treatment effect, which we tabulate in Panel B of Table 4 (the creation of multiple stacked datasets explains the large increase in sample size). When we use this approach, our coefficient of interest remains positive and statistically significant at conventional levels across all four specifications (two trading profits measures, with and without additional controls).

Finally, we perform the dynamic decomposition to assess the existence of a pre-event trend and thus the validity of the parallel trends assumption that underlies DiD estimates. We do it using the stacked approach from Cengiz et al. (2019) to correct for potential bias in our baseline estimates. Specifically, for each treatment event we create a separate dataset, and we replace our $SoP_{i,t}$ indicator with a series of indicators that take a value of one for each year in the window comprised of five years before and five years after each regulatory event. We use $SoP_{i,t=-1}$ as our benchmark in these regressions. We then stack the datasets and report the results using both our $PROFIT_{6m}\%_{it}$ measure (Figure 1a) and our $PROFIT_{12m}\%_{it}$ measure (Figure 1b). The figures reveal that our coefficients of interest do not exhibit a pre-trend in the pre-regulation period. Indeed, the estimates are economically very close to zero and statistically insignificant in the pre-period. In contrast, our estimates are positive and statistically significant for all post-SoP years across both specifications. Thus, this test suggests that the parallel trends assumption is not violated. More generally, all the tests described in this subsection and motivated by recent methodological advances (Baker et al. 2022; Barrios 2021) support our inferences.

Exploratory Evidence on the Mechanism: Insider Trading as Substitute for Executive Pay

The findings in Table 3 and 4 are consistent with executives resorting to insider trading to compensate for the expected loss in compensation under a SoP regime. To test more directly this mechanism, we examine whether the increase in insider trading profit is concentrated among executives who ex post are most affected by SoP. In particular, in Table 5 we add an interaction term between *SoP* and an indicator equal to one if the average total pay across the top 3 executives after the SoP adoption is lower than in the pre-SoP period (*PAYdown*). The sample size, at N=60,142, is lower than in Table 3 because we restrict the sample to firm-years with data on total pay level in CapitalIQ. As shown in column 1 and 2, the coefficient on the interaction term is positive and significant (both statistically and economically), suggesting a greater increase in insider trading profits at firms where executives experience a decline in nominal pay levels.

Furthermore, we examine whether this effect is more pronounced when executives also experience an increase in PPS (riskier pay). To perform such test, we restrict our sample to U.S. firms in ExecuComp, which meet the data requirements for computing the time-series of PPS. Before proceeding with the test, we first ensure in columns 3 and 4 that the results in columns 1 and 2 hold for US firms with available pay level data in CapitalIQ (N=21,238; note that the main effect of *SoP* is subsumed by the year fixed effects). Next, we verify that the results continue to hold when limiting the sample to US firms with available data in Execucomp (N=8,576; untabulated). Finally, using the Execucomp sample, in column 5 and 6 we interact SoP with an indicator (*PAYdown_PPSup*) equal to one if *PAYdown* is equal to one *and* the average PPS across the top 3 executives after the adoption of SoP is higher than in the pre-SoP period. PPS is measured following Core and Guay (2002) and Core, Daniel, and Naveen (2006). The coefficient of the interaction term is positive and significant, suggesting a greater increase in insider trading profits among executives experiencing a pay decline combined with an increase in PPS.[16]

[16] As noted in Section 2, the standard measures of compensation used in Table 5 may not capture all the potential effect of SoP laws on compensation practices. Thus, the increase in insider trading profit is not expected to occur only among executives experiencing a decline in pay level or an increase in PPS.

Overall, while admittedly subject to endogeneity, these tests support the interpretation of our findings as evidence of a substitution of insider trading profit for explicit compensation.[17]

The Role of Concurrent Governance Reforms

As discussed in the Introduction, a common concern with studies examining the staggered adoption of specific governance mandates is that such mandates are often part of broader governance reforms. In our setting, concurrent governance reforms should bias against finding an increase in insider trading profits, since prior studies show that governance generally strengthens monitoring mechanisms and thus *restricts* insider trading opportunities (Jagolinzer et al. 2011; Skaife et al. 2013; Dai et al. 2016; Hong et al. 2019; Davidson and Pirinsky 2021; Yost and Shu 2022).[18] However, it is conceivable that countries viewed the adoption of SoP laws and other governance reforms as substitute mechanisms. If the adoption of such reforms were negatively correlated with the adoption of SoP laws, our results could be spuriously driven by reduced insider trading profits at control firms rather than increased insider trading profits at treatment firms.

Thus, we repeat the analysis in Table 3 including separate control variables for the adoption of three governance reforms implemented via a staggered rollout during our sample period that plausibly could affect insider trading activity: the Market Abuse Directive (MAD), the Transparency Directive (TPD) and International Financial Reporting Standards (IFRS). The MAD is especially relevant since it aims to address insider trading and market manipulation. However,

[17] Along the same lines, in cross-sectional tests we find that the increase in insider trading profits is more pronounced among firms that prior studies (Correa and Lel 2016) identify as most affected by the adoption of SoP, namely, firms with higher excess pay and weaker governance *prior* to the passage of SoP laws (see OA Table A2). In brief, using proxies for both ex post (Table 5) and ex ante (OA Table A2) effects of SoP on executive pay, we find evidence consistent with a substitution of insider trading profit for explicit compensation.

[18] A positive correlation between adoption of SoP and other governance reforms might lead us to understate the true magnitude of the economic effect we document. However, in untabulated analyses we find that, on average, the absolute distance in adoption time between SoP laws and the three regulations examined below ranges from 3.1 years to 4.7 years, suggesting that these reforms were generally not bundled with SoP and thus are unlikely to substantially affect our estimates.

as shown in OA Table A3, the coefficient of *SoP* remains positive, significant, and of similar magnitude. Thus, it does not appear that other governance reforms introduced during our sample period significantly affect our findings and inferences.[19]

Robustness and Placebo Tests

We perform three sets of robustness tests. First, we examine the robustness of our baseline results to variation in the sample. As shown in OA Table A4, our results hold if we (i) exclude the SoP adoption year, (ii) only focus on CEO trades, (iii) exclude firms from the United States (though the significance levels drop from 1% to 5% and 10%) – representing about one-third of the sample, (iv) exclude from the control sample firm-year observations corresponding to voluntary adoption of SoP, and (v) restrict our sample to firm-year observations with insider trades. The results are also robust to excluding each country from our sample one by one and to restricting our sample to countries that eventually adopt SoP (this test ensures that the findings in Table 3 do not reflect systematic differences between adopting and never adopting countries).

Second, we vary the measurement of insider trading profitability. As shown in the OA Table A5, our results in Table 3 are robust to using market-adjusted or risk-adjusted returns instead of size-based returns. In terms of economic significance, when using market-adjusted returns (risk-adjusted returns) the increase in insider trading profits translates to an increase of about 5.1% (12.7%) in mean executive pay, versus 7.6% when using size-adjusted returns (as reported earlier).

[19] Interestingly, none of the coefficients on the other governance reforms are significant. This may be surprising, especially for the MAD regulation, but it is consistent with findings in Gębka, Korczak, Korczak, and Traczykowski (2017). A potential explanation is that in many countries insider trading data were subject to mandatory disclosure only after (and as a result of) the adoption of MAD. But these are precisely the countries (countries with weaker securities regulations) where MAD most likely affected insider trading activity, and such effects would not be captured in empirical tests because of the lack of pre-MAD insider trading disclosures.

Third we consider an alternative approach to clustering standard errors.[20] As shown in OA Table A6 (Panel A), our results are robust to clustering by country rather than by firm.

Finally, we perform a placebo test where we examine the changes in trades' profits for independent (i.e., non-executive) directors, whose compensation is not covered by SoP. We find no change in insider trading profits for this group, consistent with the notion that the adoption of SoP laws primarily affects the trading behavior of those directly covered by such laws (i.e., top executives). This result is tabulated in OA Table A7.

V. DECOMPOSINGTHE CHANGE IN INSIDER TRADING PROFITS

Our analyses suggest an increase in firm-level insider trading profits around the adoption of SoP. In this section we investigate the potential sources of this increase, focusing on the three components of insider trading profits (trade informativeness, size, and frequency; Skaife et al. 2013) and the timing of insider trades. Ex ante, we do not have a prediction as to whether executives will try to increase their trading profits by engaging in more profitable (i.e., better timed) trades, larger profitable trades, and/or more frequent profitable trades.[21] It likely depends on each executive's unique circumstances in terms of endowment of private information, current

[20] In theory, clustering the standard errors by country seems more appropriate since the treatment (the introduction of SoP) is at the country level. However, the relatively small number of countries in our sample (25) leads to the "few clusters" problem, which may bias our tests and overestimate the precision of our effect (Bertrand, Duflo and Mullainathan 2004). Recent advances in the theory of econometrics suggest that the minimum required number of clusters to obtain asymptotic results significantly increases when there is an imbalance of observations within clusters (Imbens and Kolesár 2016; Carter, Schnepel, and Steigerwald 2017), leading Cameron and Miller (2015, p. 25) to conclude: "To repeat a key message, there is no clear-cut definition of 'few'. Depending on the situation 'few' may range from less than 20 clusters to less than 50 clusters in the balanced case, and even more clusters in the unbalanced case." As reported in Table 1, our sample is extremely unbalanced, with the number of observations ranging from only 151 (Portugal) to 30,986 (United States), which casts doubts on whether 25 clusters are 'enough'. Thus, in our main analyses standard errors are clustered by firm.

[21] With respect to the trades' profitability, the effect of SoP laws is ambiguous. On one hand, as explained in Section II, executives will have an incentive to seek higher implicit pay, and more profitable trades can be one of the channels. On the other hand, SoP laws may increase the transparency of pay and allow investors to better estimate unexpected pay. If there is information in unexpected pay (Hayes and Schaefer 2000), this might reduce any information advantage the executive has, thereby reducing the profitability of his trades. We thank an anonymous reviewer for pointing out this possibility.

equity portfolio, diversification and liquidity needs, etc. Nonetheless, it is useful to understand what aspects of insider trading behavior were most affected by the adoption of SoP laws.

Insider Trades' Informativeness, Size and Frequency

To examine the change in the three components of insider trading profits (informativeness, size, and frequency) around the adoption of SoP laws, in Table 6, Panel A, we replace insider trading profits with each of the three components as dependent variable.

We start by examining changes in trade informativeness, i.e., the extent to which top executives trade on private information, as proxied by the excess return following individual trades. To do so, we modify Equation (1) by regressing future excess returns on the $SoP_{i,t}$ indicator using individual *transaction-level* data (over 211,000 transactions by 20,044 unique executives). We measure future excess returns as the buy-and-hold abnormal returns over either 6 ($BHAR_{6m}$) or 12 months ($BHAR_{12m}$) after the transaction date (see columns 1 and 2). We include firm, year-month, and insider fixed effects. Next, in column 3, we examine the second component of insider trading profits, namely, the size of each *profitable* trade ($VALUE\%_{i,t}$), computed as the dollar value of each profitable trade (defined as a transaction with positive $BHAR_{12m}$), scaled by the market value of equity. Finally, to examine changes in the frequency of trades, in column 4 we go back to the firm-year level unit of analysis and use as dependent variable the natural log of the number of *profitable* insider trades at the firm-year level (i.e., across all top 3 executives; $Ln(NUM)_{i,t}$).

In columns 3 and 4, we focus on *profitable* trades because we aim to decompose the change in insider trading profit into its components. In other words, we are not interested in whether SoP laws resulted in larger and more frequent trades, but whether they resulted in larger and more frequent *profitable* trades as a mechanism used by insiders to achieve higher profits.

Table 6, Panel A shows that all three components are positive and significant, and thus they all contribute to the increase in insider trading profits. In other words, post-SoP insiders engage in more profitable trades (column 1 and 2), and their profitable trades are larger and more frequent (column 3 and 4). Next, we perform the analysis separately for buy (Panel B) and sale (Panel C) transactions.[22] Panel B suggests that the size and frequency of profitable insider purchases do not change significantly around the adoption of SoP, but insider purchases become more profitable on average (columns 1 and 2). This is consistent with insiders seeking higher profits by exploiting their private information, rather than by making their profitable trades larger or more frequent, which would increase their exposure to firm-specific risk. Panel C shows that insider sales become more profitable (though not significantly over 12 months) and profitable sales become more frequent and larger (columns 3 and 4). The results are generally robust to clustering standard errors by country, although the precision of our estimates is lower (See OA Table A6, Panel B).

This finding is consistent with executives trying to reduce their greater exposure to firm-specific risk induced by SoP (via higher PPS and higher use of equity pay) while increasing their trading profits. It also suggests that some of the policy objectives of SoP votes (e.g., increased pay-for-poor-performance sensitivity, increased equity holdings) may be partially offset by insiders' trading behavior. Besides, since SoP increases PPS especially when performance is poor (Correa and Lel 2016), larger and more frequent profitable sales allow insiders to increase their implicit pay right when they expect a drop in performance and thus in explicit pay.

As for the control variables, in Panel B (C), firm size exhibits a significantly negative (positive) association in columns 1, 2, and 3 (columns 1, 2, and 4), suggesting that insiders buy (sell) stock

[22] Note that the number of observations of the separate buy (Panel B) and sell (Panel C) analyses do not add up to the number of observations of the pooled analyses (Panel A) because of the use of fixed effects. By partitioning the sample, some firms with multiple observations in the pooled analyses are now automatically dropped as singletons.

more profitably and more often when firm size is relatively smaller (larger). These results are largely consistent with prior literature. The negative coefficient on firm size in column 3 of both panels is mechanical, as insider trades account for a lower percentage of firm value in larger firms. It also appears that insiders: (i) buy (sell) more frequently when past returns are low (high) and sell more when book-to-market of equity is low, again consistent with prior literature; (ii) they buy (sell) more frequently when asynchronicity is high (low) and return volatility low (high), suggesting that the information environment affects their propensity to trade; and (iii) insiders sell more often when firms cut R&D spending (Aboody and Lev 2000). While other firm characteristics are sometimes significant, the fixed effect structure strips out most of the cross-sectional variation; hence we do not interpret their coefficients for brevity.

Timing of Insider Trades

Finally, we consider whether insiders are more likely to time their trades during information-sensitive periods after SoP adoption. In particular, we examine whether $FracVALUE_{i,t}(FracNUM_{i,t})$, i.e., the fraction of the total value (number) of all insider trades taking place during the one-month period prior to the annual earnings announcement date – a proxy for the information-sensitive window – increases around the SoP adoption.

The results are reported in Table 7 (the sample size drops to 36,529 firm-year observations because the annual earnings announcement dates are missing for some firm-years). The positive and significant coefficients of $SoP_{i,t}$ in columns 1 and 2 indicate an increase of 1.9% (2.5%), respectively, in the fraction of insider trades' value (number) taking place every year during the one-month window prior to annual earnings announcements. As a benchmark, the mean fraction of insider trades' value (number) within this window in the entire sample is 7.1% (7.4%). In columns 3 and 4, we repeat the test by redefining $FracVALUE_{i,t}(FracNUM_{i,t})$ as the fraction of the total value (number) of all insider trades taking place during any of the one-month periods prior to quarterly earnings announcements during the year. The sample size further drops to 24,164 firm-year observations because we require four quarterly earnings announcement dates to make the figures comparable across firm-years, and such dates are missing for many firms, especially those with lower reporting frequency mandates. Nonetheless, we continue to find a positive and significant coefficient on $SoP_{i,t}$, consistent with a greater concentration of insider trades during information-sensitive windows after the adoption of SoP.

In Panels B and C, we repeat the analysis for buys and sales separately, and we find that the result in Panel A is most pronounced for sales transactions (both in terms of magnitude of the economic effect and significance levels). This suggests that the larger size and number of profitable sales documented in Table 6 may be partly due to insider sales taking place during information-sensitive windows.[23] As for the control variables, to the extent that many firms establish those windows on a permanent basis, it is not surprising that few of our controls load significantly in the presence of firm fixed effects. Finally, our results are generally similar when standard errors are clustered at the country level (See OA Table A6, Panel C).

VI. CONCLUSIONS

Over the last two decades, numerous countries have allowed shareholders to vote on executive compensation matters by adopting some form of a "say on pay" regime. Prior studies show that say on pay is associated with higher pay-for-performance sensitivity (i.e., riskier pay), a decline in the growth rate of pay levels and the removal of controversial features of compensation contracts. We posit that insiders may respond to the corresponding reduction in the value of their explicit

[23] We cannot conclude whether post-SoP trades are more likely to take place during restricted windows since (to the best of our knowledge) no database tracks the existence of restricted insider trading windows at the firm level.

compensation packages by seeking higher implicit pay via greater insider trading activity. Using the staggered adoption of say on pay across countries, we indeed find an increase in insider trading profits. When we decompose insider trading profits, we find that the increase in insider trading profits is driven mostly by larger and more frequent profitable sales, consistent with the executives' desire to reduce their greater exposure to firm-specific risk while increasing their trading profits. Also, after the adoption of say on pay insider sale transactions are more likely to occur during information-sensitive windows.

Our findings highlight an important unintended consequence of say on pay and have implications for policy makers and investors. For instance, if regulators' objective is to rein in perceived excessive executive compensation, then they should also consider tightening insider trading regulation and enforcement. As for investors, they should monitor insider trading disclosures and factor insider trading profitability into their assessment of compensation plans and their SoP voting decisions. That said, readers should not necessarily interpret our results as a cost or downside of SoP laws. More informed insider trading can improve stock price efficiency, especially when promptly disclosed. Therefore, investors could benefit from insiders' increased trading activity. Future research can build on our findings in two ways. First, by examining other consequences of SoP laws induced by underlying changes in compensation. Second, by considering executive compensation and insider trading as potential substitutes in evaluating the effect of other regulations that target either.

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Appendix: Variable Definitions

Variable	Definition
Independent Variable	
$SoP_{i,t}$	Indicator variable that equals one for the firm-years after the adoption of SoP laws (including the adoption year), zero otherwise.
PAYdown	Indicator variable equal to one if the average total pay across the top 3 executives is lower after the adoption of SoP relative to the pre-SoP period (variable used in Table 5 only). Data are obtained from CapitalIQ.
PAYdown_PPSup	Indicator variable equal to one if *PAYdown* is equal to one and the average pay-for-performance sensitivity (PPS) across the top 3 executives is higher after the adoption of SoP (variable used in Table 5 only). PPS is measured following Core and Guay (2002) and Core, Daniel and Naveen (2006). This variable is only defined for firms in the United States (where SoP was adopted in 2011) with available data in ExecuComp (required to compute PPS).
Dependent Variables	
$PROFIT_{6m}\%_{i,t}$	Aggregate profits from the open-market insider trades by firm *i*'s top 3 executives (CEO, CFO and COO) during fiscal year *t*, scaled by market value of equity at the end of fiscal year *t-1*. It is computed as follows (based on Skaife, Veenman and Wangerin (2013)): $$PROFIT_{6m}\%_{it} = \frac{\sum_{j=1}^{n} BHAR_{itj} \times VALUE\_BUY_{itj} - BHAR_{itj} \times VALUE\_SALE_{itj}}{MV_{it-1}} \times 100$$ where $BHAR_{itj}$ is the buy-and-hold abnormal return over 6 months after the trading day. The abnormal return is computed as the raw return minus the value weighted return for stocks in the same size quintile within same stock exchange (size adjusted return). For each stock exchange, we assign each stock to a size quintile based on the market capitalization at the calendar year end and require that each quintile has at least five stocks. $VALUE\_BUY_{itj}$ ($VALUE\_SALE_{itj}$) is the total traded value for buy (sale) transactions by firm *i*'s top 3 executives on day *j*; *n* equals the total number of insider trading days during the firm year (*it*); MV_{it-1} is the firm's market capitalization at the end of fiscal year *t-1*. Return data sources: Center for Research in Security Prices (CRSP) for US firms, Compustat/North America for Canadian firms, and Compustat/Global for firms in other countries. To identify the top 3 executives (CEO, CFO and CFO) in the 2iQ database, we examine the Insider Level A category (which includes the top five executives) and code the observation as CEO if the title in the Insider Relation field is CEO (the most common case), Executive Director, Executive Chairman, Managing Director or Chairman of the Management Board; we code the observation as CFO if the title is CFO or Finance Director; we code the observation as COO if the title is COO. To identity buy and sale transactions, in the 2iQ database we select trading data with Transaction Type equal to Buy or Sell. We filter out private placement and OTC trades (identified with a Transaction Label equal to PP and PR), trades that took place outside the home country where the firm's headquarters are located, and trades whose trading value exceeds the firm's market value (suggesting a reporting mistake in the database or the original insider trading form).
$PROFIT_{12m}\%_{i,t}$	Similar to $PROFIT_{6m}\%$, except that $BHAR_{itj}$ is computed over 12 months after the trading day *j*.
$BHAR_{6m\ (12m)\ i,t,j}$	This variable is used in Table 6. Buy-and-hold abnormal return over 6 (12) months after the trading day. The abnormal return is computed as the raw return minus the value weighted return for stocks in the same size quintile within same stock exchange (size adjusted return). For each stock exchange, we assign each stock to a size quintile based on the market capitalization at the calendar year end and require that each quintile has at least five stocks.
$VALUE\%_{i,t,j}$	In Table 6, Panel A, the value of profitable insider trades *j* by the top 3 executives of firm *i* during fiscal year *t*, scaled by the market value of equity at the end of fiscal year *t-1*. In Table 6, Panel B (Panel C), the value of profitable insider purchases (sales) *j* by the top 3 executives of

	firm *i* during fiscal year *t*, scaled by the market value of equity at the end of fiscal year *t-1*. A trade is defined as profitable if the subsequent $BHAR_{12m}$ is greater than zero.
$Ln(NUM)_{i,t}$	In Table 6, Panel A, the natural logarithm of the total number of profitable insider trades by the top 3 executives of firm *i* during fiscal year *t*. In Table 6, Panel B (Panel C), the natural logarithm of the total number of profitable insider purchases (sales) by the top 3 executives of firm *i* during fiscal year *t*. A trade is defined as profitable if the subsequent $BHAR_{12m}$ is greater than zero.
$FracVALUE_{i,t}$	In Table 7, column 1, the fraction of top 3 executives' total trades' value from trades taking place during the one-month period prior to the annual earnings announcement date of firm *i* during fiscal year *t*. In Table 7, column 3, the fraction of the top 3 executives' total trades' value from trades taking place during any of the one-month periods prior to the four quarterly earnings announcement dates of firm *i* during fiscal year *t*.
$FracNUM_{i,t}$	In Table 7, column 2, the fraction of the top 3 executives' total number of trades taking place during the one-month period prior to the annual earnings announcement date of firm *i* during fiscal year *t*. In Table 7, column 4, the fraction of the top 3 executives' total number of trades taking place during any of the one-month periods prior to the four quarterly earnings announcement dates of firm *i* during fiscal year *t*.
Control Variables	
$Ln(MV_{i,t-1})$	Firm size, measured as the natural logarithm of market capitalization (in millions) at the end of fiscal year *t-1*, converted from local currency to US dollars according to the historical exchange rate. Data sources: Center for Research in Security Prices (CRSP) for US firms, Compustat/North America for Canadian firms, and Compustat/Global for firms in other countries.
$BTM_{i,t-1}$	Book-to-market ratio, calculated as the ratio of book value of equity (item CEQ) to market value of equity (item MV) at the end of fiscal year *t-1*. Data source: Compustat/North America for firms in the US and Canada, Compustat/Global for firms in other countries.
$BHAR_{i,t-1}$	Buy-and-hold abnormal returns over the one-year period ending at the end of fiscal year *t-1*, where the calculation of abnormal return is consistent with that for *PROFIT6m%* and $PROFIT_{12m}\%$. Return data sources: Center for Research in Security Prices (CRSP) for US firms, Compustat/North America for Canadian firms, Compustat/Global for firms in other countries.
$LnCoverage_{i,t-1}$	The natural logarithm of one plus the total number of analysts following firm *i* during fiscal year *t-1* (based on one-year-ahead Earnings Per Share forecast). Data source: I/B/E/S.
$Non\text{-}Synch_{i,t-1}$	Price non-synchronicity, defined as one minus the R-squared from the following regression over the past 36 months (we require at least 15 months of available data), following Chen, Goldstein, and Jiang (2006): $RET_{i,k,t} = \alpha + \beta_1 MKTRET_{k,t} + \beta_2 MKTRET_{US,t} + \varepsilon_{i,k,t}$ where $RET_{i,k,t}$ is the return in month t of firm *i* belonging to country k, and $MKTRET_{k,t}$ and $MKTRET_{US,t}$ are the value-weighted return in country *k* and the value-weighted return in the US, respectively. Data sources: Center for Research in Security Prices (CRSP) for US firms, Compustat/North America for Canadian firms, and Compustat/Global for firms in other countries.
$R\&D_{i,t-1}$	Indicator variable that equals one if a company reports non-zero R&D expenditures at the end of fiscal year *t-1*, zero otherwise. Data source: Compustat/North America for firms in the US and Canada, and Compustat/Global for firms in other countries (item XRD).
$INST_{i,t-1}$	Institutional ownership, computed as shares owned by institutional investors divided by firm *i*'s total shares outstanding, both at the end of fiscal year *t-1*. Data source: FactSet.
$InsiderOwn_{i,t-1}$	Insider ownership, computed as the average shares owned by the top 3 executives (CEO, CFO, COO) divided by firm *i*'s total shares outstanding, both at the end of fiscal year *t-1*. Data source: 2iQ.
$RETVOL_{i,t-1}$	Return volatility, measured as the standard deviation of firm *i*'s daily stock returns during fiscal year *t-1*, with a minimum of 100 daily observations to calculate (following Frankel and Li 2004). Data sources: Center for Research in Security Prices (CRSP) for US firms, Compustat/North America for Canada firms, and Compustat/Global for firms in other countries.

Figure 1. Stacked Difference-in-Differences Parallel Trends

Figure 1a (1b) plots time-varying treatment effects on the insider trading profits of the top 3 executives captured by *PROFIT*$_{6m}$*%* (*PROFIT*$_{12m}$*%*) by applying the stacking approach of Cengiz et al. (2019), where we use *PROFIT*$_{6m}$*%* (*PROFIT*$_{12m}$*%*) in t=-1 as the benchmark. The X-axis represents the years relative to the year in which a country mandatorily adopts SoP laws (t=0). The dotted lines represent the coefficient on *PROFIT*$_{6m}$*%* (*PROFIT*$_{12m}$*%*) at the 95% confidence interval in Figure 1a (Figure 1b).

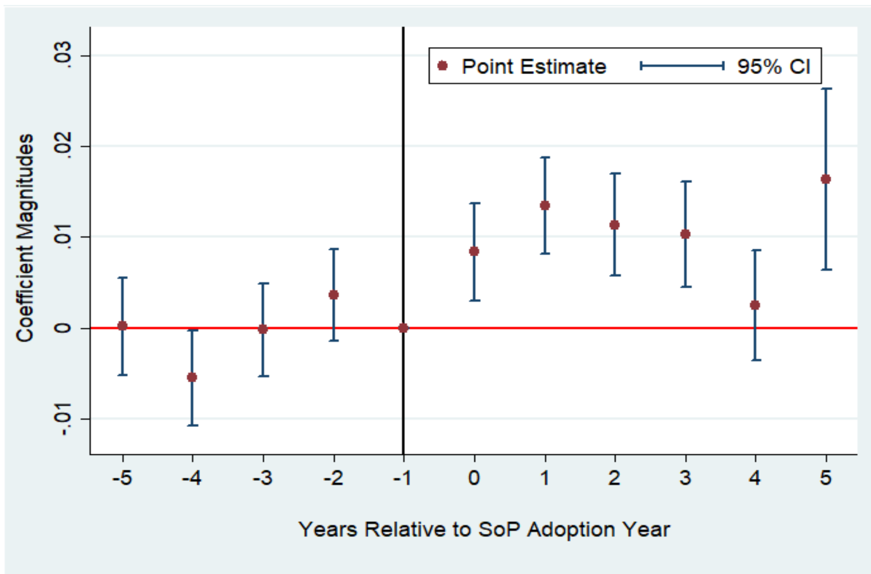

Figure 1a

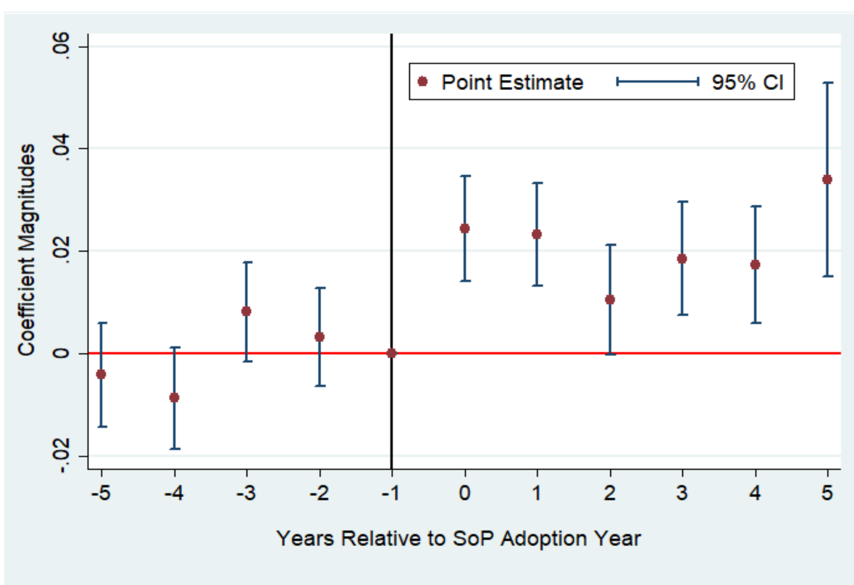

Figure 1b

Table 1: Say on Pay (SoP) by Country: Year of Adoption and Type of SoP Regime

This table lists all the countries in our sample, including 14 countries that introduced a mandatory say on pay (SoP) regime between 2003 (when the UK became the first country to introduce SoP) and 2014, and 11 countries that did not adopt such regime during the same period. For each country, the table reports the number of unique firms and the number of firm-year observations, the SoP adoption year, and whether it was an Advisory or Binding SoP regime. The information on SoP adoption is based on Thomas et al. (2015), Correa and Lel (2016) and web searches.

Country	SoP adoption year	Advisory vs binding	# of firms	# of firm-years
Australia	2005	A	805	6757
Austria	-	-	33	263
Belgium	2012	A	50	368
Canada	-	-	637	6071
China	-	-	1136	7060
Denmark	2007	B	33	297
Finland	-	-	87	961
France	2014	A	383	3452
Germany	-	-	304	3295
Hong Kong	-	-	503	4935
India	-	-	389	2977
Italy	2012	A	169	1662
Malaysia	-	-	39	386
Netherlands	2004	B	96	1067
New Zealand	-	-	34	335
Norway	2008	B	128	1035
Portugal	2010	A	21	151
Singapore	-	-	63	640
South Africa	2011	B	180	1440
Spain	2011	A	59	669
Sweden	2006	B	258	2490
Switzerland	2013	B	140	1178
Thailand	-	-	150	1527
United Kingdom	2003	A(B)	995	8638
USA	2011	A	3233	30986

Table 2. Descriptive Statistics

This table reports the summary statistics of key variables in our sample. Panel B reports the Pearson correlations between the key variables. (See the Appendix for variable definitions.)

Panel A: Summary Statistics

Variable	MEAN	P5	P25	P50	P75	P95	STD	N
SoP	0.343	0.000	0.000	0.000	1.000	1.000	0.475	88,640
$PROFIT_{6m}\%$	0.010	-0.063	0.000	0.000	0.000	0.096	0.174	88,640
$PROFIT_{12m}\%$	0.014	-0.110	0.000	0.000	0.001	0.159	0.292	88,640
MV_{t-1}	2824.773	12.425	81.652	348.109	1454.327	13892.290	8441.070	88,640
BTM_{t-1}	0.735	0.130	0.322	0.545	0.894	2.014	0.689	88,640
$BHAR_{t-1}$	0.092	-0.608	-0.234	-0.006	0.268	1.132	0.600	88,640
$Coverage_{t-1}$	15.503	0.000	0.000	6.000	23.000	62.000	20.836	88,640
$Non\text{-}Synch_{t-1}$	0.806	0.369	0.679	0.876	0.987	1.000	0.204	88,640
$R\&D_{t-1}$	0.332	0.000	0.000	0.000	1.000	1.000	0.471	88,640
$INST_{t-1}$	0.282	0.000	0.009	0.142	0.480	0.943	0.320	88,640
$InsiderOwn_{t-1}$	0.030	0.000	0.000	0.002	0.010	0.183	0.090	88,640
$RETVOL_{t-1}$	0.032	0.012	0.018	0.025	0.037	0.070	0.025	88,640

Panel B: Correlation Table

	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	*MV*	*BTM*	*BHAR*	*Coverage*	*Non-Synch*	*R&D*	*INST*	*InsiderOwn*	*RETVOL*
$PROFIT_{6m}\%$	1.000										
$PROFIT_{12m}\%$	0.734	1.000									
MV	-0.017	-0.014	1.000								
BTM	0.019	0.016	-0.131	1.000							
BHAR	0.017	0.011	-0.006	-0.240	1.000						
Coverage	-0.025	-0.022	0.561	-0.178	-0.043	1.000					
Non-Synch	0.022	0.020	-0.250	0.112	0.031	-0.409	1.000				
R&D	0.008	0.009	0.089	-0.103	-0.007	0.128	0.017	1.000			
INST	-0.022	-0.020	0.183	-0.168	0.003	0.425	-0.432	0.148	1.000		
InsiderOwn	0.055	0.051	-0.090	0.133	0.016	-0.152	0.132	-0.044	-0.194	1.000	
RETVOL	0.027	0.022	-0.097	0.177	0.032	-0.163	0.157	0.002	-0.183	0.089	1.000

Table 3. The Effect of SoP Laws' Adoption on Insider Trading Profits

This table reports the results of the effect of SoP laws' adoption on the firm-level insider trading profits of top 3 executives (CEO, CFO and COO) using the difference-in-differences specification in Equation (1). Insider trading (*PROFIT*) is computed using all open-market buy and sale transactions. The buy-and-hold abnormal return used in computing the returns subsequent to each transaction (and thus *PROFIT*) is a size-adjusted return. See the Appendix for details. Firm and year fixed effects are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

	(1)	(2)	(3)	(4)
	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$
$SoP_{i,t}$	0.010***	0.017***	0.009***	0.016***
	(2.95)	(3.03)	(2.78)	(2.84)
$ln(MV_{i,t-1})$			-0.005*	-0.009**
			(-1.74)	(-1.99)
$BTM_{i,t-1}$			0.002	0.003
			(0.57)	(0.46)
$BHAR_{i,t-1}$			0.001	-0.000
			(0.41)	(-0.08)
$LnCoverage_{i,t-1}$			0.003	0.002
			(1.63)	(0.89)
$Non\text{-}Synch_{i,t-1}$			0.010*	0.025**
			(1.66)	(2.48)
$R\&D_{i,t-1}$			0.010**	0.013*
			(2.08)	(1.66)
$INST_{i,t-1}$			0.008	0.018
			(0.83)	(1.00)
$InsiderOwn_{i,t-1}$			0.150**	0.306***
			(2.54)	(3.14)
$RETVOL_{i,t-1}$			0.174**	0.231*
			(2.23)	(1.75)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.152	0.160	0.153	0.161
N. of Obs.	88,640	88,640	88,640	88,640

Table 4. Goodman-Bacon Decomposition and Stacked Difference-in-Differences

Panel A of this table reports the results of the Goodman-Bacon decomposition of our treatment effects for $PROFIT_{6m}\%$ and $PROFIT_{12m}\%$ in a balanced sample (the decomposition is estimated without covariates). Panel B reports the results of the effect of mandatory SoP adoption on the insider trading profits of top 3 executives by applying the stacking events approach of Cengiz et al. (2019) to address the potential heterogeneity of treatment effects in the context of a staggered difference-in-differences model. We compute insider trading profits using all open-market buy and sale transactions. The buy-and-hold abnormal return used in computing the returns subsequent to each transaction (and thus *PROFIT*) is a size-adjusted return, as in Table 3. Firm and year fixed effects are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

Panel A: Goodman-Bacon Decomposition

Overall Estimation on Treatment Effect		$PROFIT_{6m}\%$	$PROFIT_{12m}\%$
$SoP_{i,t}$		0.009**	0.014**
Treatment Effect Decomposition			
	Total weights	Beta	Beta
Early Adopters vs. Late Adopters (Control)	0.076	0.001	0.008
Late Adopters vs. Early Adopters (Control)	0.047	0.001	-0.010
Treated vs. Never Treated (Control)	0.562	0.012	0.019
Treated vs. Already Treated (Control)	0.315	0.008	0.010

Panel B: Stacked Events Approach

	(1)	(2)	(3)	(4)
	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$
$SoP_{i,t}$	0.013***	0.024***	0.012***	0.021***
	(3.25)	(3.50)	(3.02)	(3.17)
$ln(MV_{i,t-1})$			-0.002	-0.005
			(-0.64)	(-0.82)
$BTM_{i,t-1}$			-0.016***	-0.021**
			(-2.85)	(-2.24)
$BHAR_{i,t-1}$			0.002	0.009*
			(0.63)	(1.85)
$LnCoverage_{i,t-1}$			0.002	0.001
			(1.08)	(0.41)
$Non\text{-}Synch_{i,t-1}$			0.015**	0.020*
			(2.38)	(1.86)
$R\&D_{i,t-1}$			0.014**	0.015
			(2.43)	(1.54)
$INST_{i,t-1}$			0.009	0.032
			(0.71)	(1.41)
$InsiderOwn_{i,t-1}$			0.055*	0.158*
			(1.91)	(1.66)
$RETVOL_{i,t-1}$			0.564***	0.777***
			(4.54)	(3.66)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.316	0.328	0.318	0.330
N. of Obs.	392,976	392,976	392,976	392,976

Table 5: The Effect of SoP Laws on Insider Trading Profits Conditional on post-SoP Changes in Executive Pay

This table examines whether the effect of SoP laws' adoption on insider trading profits is stronger among firms whose executives were most affected by SoP laws. In column 1 to 4, *PAYdown* is an indicator equal to one if the average total pay across the top 3 executives after the SoP adoption is lower than in the pre-SoP period. The sample is limited to firms with compensation data in CapitalIQ. In column 5 and 6, *PAYdown_PPSup* is an indicator equal to one if *PAYdown* is equal to one and the average PPS across the top 3 executives after the SoP adoption is higher than in the pre-SoP period. Due to data requirements to compute PPS, in column 5 the sample is limited to United States (US) firms with available compensation data in Execucomp. All the other variables are defined as in Table 3, except that in column 3-6 (US firms) *SoP* equals one (zero) during the period after (before) the SoP adoption in 2011. See the Appendix for more details. Firm and year fixed effects are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

	All Countries		Only USA			
	(1)	(2)	(3)	(4)	(5)	(6)
	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$
*SoP*PAYdown*	0.006***	0.008***	0.007***	0.009***		
	(4.28)	(2.60)	(3.51)	(2.69)		
SoP	0.004**	0.005*				
	(2.21)	(1.86)				
*SoP*PAYdown_PPSup*					0.013**	0.017*
					(2.04)	(1.86)
$ln(MV_{i,t-1})$	0.004*	0.010***	0.009***	0.019***	0.030***	0.049***
	(2.07)	(3.04)	(6.63)	(7.86)	(7.27)	(7.32)
$BTM_{i,t-1}$	0.003**	0.001	0.002	-0.001	0.009	-0.003
	(2.00)	(0.45)	(0.95)	(-0.23)	(0.80)	(-0.19)
$BHAR_{i,t-1}$	0.001	-0.001	0.002	0.002	0.000	-0.001
	(0.43)	(-0.36)	(1.03)	(0.54)	(0.07)	(-0.08)
$LnCoverage_{i,t-1}$	0.002*	0.002*	-0.001	-0.001	-0.009	-0.002
	(1.84)	(1.95)	(-0.72)	(-0.33)	(-1.61)	(-0.20)
$Non\text{-}Synch_{i,t-1}$	-0.001	0.005**	-0.000	0.012**	-0.008	0.018
	(-0.98)	(2.00)	(-0.15)	(2.46)	(-1.11)	(1.56)
$R\&D_{i,t-1}$	0.004	0.003	-0.004	-0.014*	-0.021	-0.019
	(1.00)	(0.40)	(-0.96)	(-1.73)	(-1.60)	(-0.74)
$INST_{i,t-1}$	-0.001	-0.001	0.003	-0.001	0.024	0.009
	(-0.15)	(-0.21)	(0.48)	(-0.06)	(1.25)	(0.31)

InsiderOwn $_{i,t-1}$	0.034	0.060	0.034	0.071	-0.376	-0.151
	(1.43)	(1.51)	(0.77)	(0.84)	(-1.26)	(-0.39)
RETVOL $_{i,t-1}$	0.129**	0.245*	0.215***	0.548***	0.756***	1.050***
	(2.33)	(1.80)	(3.21)	(4.89)	(3.22)	(3.05)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj.R-squared	0.139	0.148	0.144	0.160	0.194	0.213
N. of Obs.	60,142	60,142	21,238	21,238	8,576	8,576
N. of Obs.(PAYdown=1)	12,243		6,583			
N. of Obs.(PAYdown_PPSup=1)					756	

Table 6. Decomposing the Change in Insider Trading Profits around Say on Pay laws

This table reports the results of the effect of SoP laws' adoption on the components of insider trading profits using the difference-in-differences specification in Equation (1). Panels A-C report the results of the effect of SoP laws' adoption on insider trades' informativeness, size and frequency, respectively, for all insider transactions (Panel A), insider buy transactions (Panel B) and insider sales transactions (Panel C). In columns 1 and 2, the dependent variable is the future excess return associated with each insider trade (a proxy for the trade's informativeness), measured, alternatively, as the buy-and-hold abnormal return over the 6-month ($BHAR_{6m}$) or 12-month period subsequent to the trading date ($BHAR_{12m}$). The abnormal return is size adjusted (see the Appendix for details), as in Table 3. In column 3, the dependent variable is the dollar value of each profitable insider trade (*VALUE%*), expressed as a percentage of the firm's market value of equity at the end of fiscal year *t-1*. In column 4, the dependent variable is *Ln(NUM)*, where *NUM* is the total number of profitable trades during the year for a given firm. In columns 3 and 4, a profitable trade is defined as a trade with $BHAR_{12m}>0$. In columns 1 to 3, the unit of analysis is at the individual transaction (trade) level, and firm, year-month and insider fixed effects are included. In column 4, the unit of analysis is at the firm-year level, and firm and year fixed effects are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

Panel A: Components of Insider Trading Profits: All Transactions

	(1)	(2)	(3)	(4)
	$BHAR_{6m}$	$BHAR_{12m}$	*VALUE%*	*Ln(NUM)*
$SoP_{i,t}$	0.026***	0.018**	0.043***	0.271***
	(2.87)	(2.14)	(3.70)	(16.32)
$ln(MV_{i,t-1})$	0.035***	0.044***	-0.078***	0.034***
	(3.55)	(8.17)	(-7.60)	(3.70)
$BTM_{i,t-1}$	0.043***	0.075***	-0.021*	-0.014**
	(2.95)	(11.93)	(-1.70)	(-2.41)
$BHAR_{i,t-1}$	-0.002	-0.007*	-0.004	0.045***
	(-0.37)	(-1.76)	(-0.65)	(6.44)
$LnCoverage_{i,t-1}$	-0.000	0.020	0.008	-0.009
	(-0.02)	(1.45)	(0.53)	(-1.40)
$Non\text{-}Synch_{i,t-1}$	0.008	0.005	0.005	-0.033
	(1.26)	(1.51)	(0.71)	(-1.30)
$R\&D_{i,t-1}$	0.014	0.005	-0.034	-0.016**
	(0.74)	(0.49)	(-1.10)	(-2.38)
$INST_{i,t-1}$	0.032	0.017	-0.030	0.119**
	(0.98)	(0.70)	(-1.10)	(2.43)
$InsiderOwn_{i,t-1}$	-0.021	0.092**	0.395*	0.141
	(-0.22)	(2.04)	(1.94)	(1.59)
$RETVOL_{i,t-1}$	0.044	0.063	0.726	0.209
	(0.11)	(0.33)	(1.27)	(0.71)
Year-Month FE	Yes	Yes	Yes	No
Year FE	No	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Insider FE	Yes	Yes	Yes	No
Adj.R-squared	0.269	0.264	0.492	0.487
N. of Obs.	211,613	211,613	132,002	43,566

Panel B: Components of Insider Trading Profits: Buy Transactions

	(1)	(2)	(3)	(4)
	$BHAR_{6m}$	*$BHAR_{12m}$*	*VALUE%*	*Ln(NUM)*
$SoP_{i,t}$	0.048**	0.078*	0.043	0.037
	(2.23)	(1.84)	(1.21)	(0.16)
$ln(MV_{i,t-1})$	-0.095***	-0.197***	-0.133***	0.007
	(-6.36)	(-6.07)	(-4.95)	(0.61)
$BTM_{i,t-1}$	0.029**	0.020	-0.035	0.023*
	(1.97)	(0.71)	(-1.44)	(1.88)
$BHAR_{i,t-1}$	-0.013	-0.051**	-0.028	-0.006
	(-1.15)	(-2.36)	(-1.45)	(-0.68)
$LnCoverage_{i,t-1}$	0.023	0.044	0.041	-0.002
	(0.51)	(0.56)	(0.68)	(-0.17)
$Non\text{-}Synch_{i,t-1}$	-0.009	-0.028*	-0.017	0.125***
	(-1.22)	(-1.85)	(-1.49)	(2.84)
$R\&D_{i,t-1}$	0.009	0.013	-0.043	-0.029
	(0.41)	(0.22)	(-0.87)	(-1.20)
$INST_{i,t-1}$	-0.110	-0.274*	0.085	0.061
	(-1.33)	(-1.95)	(0.64)	(0.99)
$InsiderOwn_{i,t-1}$	0.004	0.079	-0.144	0.053
	(0.03)	(0.22)	(-0.27)	(0.48)
$RETVOL_{i,t-1}$	-0.349	-0.335	1.087	0.007
	(-1.05)	(-0.64)	(1.30)	(0.02)
Year-Month FE	Yes	Yes	Yes	No
Year FE	No	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Insider FE	Yes	Yes	Yes	No
Adj.R-squared	0.464	0.549	0.318	0.504
N. of Obs.	59,343	59,343	30,714	17,581

Panel C: Components of Insider Trading Profits: Sale Transactions

	(1)	(2)	(3)	(4)
	$BHAR_{6m}$	*$BHAR_{12m}$*	*VALUE%*	*Ln(NUM)*
$SoP_{i,t}$	0.030***	0.026	0.039***	0.275***
	(2.67)	(1.33)	(2.70)	(14.25)
ln($MV_{i,t-1}$)	0.148***	0.278***	-0.060***	0.034***
	(12.34)	(16.02)	(-4.84)	(2.84)
$BTM_{i,t-1}$	-0.030	-0.034	0.041	-0.036**
	(-1.19)	(-1.02)	(1.40)	(-2.46)
$BHAR_{i,t-1}$	0.008	0.023**	-0.006	0.068***
	(1.15)	(2.09)	(-1.13)	(7.98)
$LnCoverage_{i,t-1}$	-0.008	0.011	0.005	-0.011
	(-0.53)	(0.48)	(0.39)	(-1.37)
$Non\text{-}Synch_{i,t-1}$	0.018***	0.025***	-0.007	-0.065**
	(3.28)	(3.08)	(-0.74)	(-2.28)
$R\&D_{i,t-1}$	-0.005	-0.001	-0.017	-0.011***
	(-0.29)	(-0.02)	(-0.42)	(-3.03)
$INST_{i,t-1}$	0.054	0.045	-0.071**	0.094
	(1.46)	(0.86)	(-2.28)	(1.58)
$InsiderOwn_{i,t-1}$	-0.130	-0.040	2.591***	-0.014
	(-1.11)	(-0.24)	(7.39)	(-0.13)
$RETVOL_{i,t-1}$	0.519	0.870	1.403	0.780**
	(1.10)	(1.30)	(1.32)	(1.98)
Year-Month FE	Yes	Yes	Yes	No
Year FE	No	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Insider FE	Yes	Yes	Yes	No
Adj.R-squared	0.326	0.432	0.652	0.514
N. of Obs.	145,888	145,888	97,384	31,035

Table 7: The Effect of SoP Laws' Adoption on the Timing of Insider Trading

This table reports the results of the effect of SoP laws' adoption on the timing of insider trading for all transactions (Panel A), for buy transactions (Panel B) and for sale transactions (Panel C). In column 1 (2), for each firm-year observation, the dependent variable $FracVALUE_{i,t}$($FracNUM_{i,t}$) is defined as the fraction of the total value (number) of insider trades taking place during the one-month period prior to the annual earnings announcement date – a proxy for the information-sensitive window. In column 3 (4), for each firm-year observation, $FracVALUE_{i,t}$($FracNUM_{i,t}$) is redefined as the fraction of the total value (number) of insider trades taking place during any of the one-month periods prior to quarterly earnings announcement dates during the year. Both firm and year fixed effects are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

Panel A: All Transactions

	Annual EA		Quarterly EA	
	(1)	(2)	(3)	(4)
	FracVALUE	*FracNUM*	*FracVALUE*	*FracNUM*
$SoP_{i,t}$	0.019***	0.025***	0.045***	0.052***
	(3.52)	(4.83)	(3.89)	(4.69)
$ln(MV_{i,t-1})$	0.008**	0.006*	0.004	0.005
	(2.07)	(1.82)	(0.70)	(0.88)
$BTM_{i,t-1}$	0.002	0.005	-0.010	-0.007
	(0.55)	(1.14)	(-1.05)	(-0.78)
$BHAR_{i,t-1}$	0.002	0.001	-0.005	-0.004
	(0.61)	(0.48)	(-0.98)	(-0.95)
$LnCoverage_{i,t-1}$	0.001	0.001	-0.010*	-0.008
	(0.31)	(0.43)	(-1.83)	(-1.64)
$Non\text{-}Synch_{i,t-1}$	0.003	0.004	0.019	0.016
	(0.28)	(0.41)	(1.29)	(1.17)
$R\&D_{i,t-1}$	0.022***	0.018***	0.033**	0.042***
	(2.96)	(2.69)	(2.52)	(3.44)
$INST_{i,t-1}$	0.000	0.002	0.027	0.033
	(0.03)	(0.14)	(1.09)	(1.39)
$InsiderOwn_{i,t-1}$	0.054	0.047	0.000	0.033
	(1.59)	(1.51)	(0.00)	(0.35)
$RETVOL_{i,t-1}$	0.076	0.031	0.199	0.288
	(0.66)	(0.27)	(0.76)	(1.19)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.119	0.151	0.146	0.186
N. of Obs.	36,529	36,529	24,164	24,164

Panel B: Buy Transactions

	Annual EA		Quarterly EA	
	(1)	(2)	(3)	(4)
	FracVALUE	*FracNUM*	*FracVALUE*	*FracNUM*
$SoP_{i,t}$	0.005	0.006*	0.014*	0.016**
	(1.46)	(1.94)	(1.82)	(2.11)
$ln(MV_{i,t-1})$	-0.003	-0.002	-0.000	-0.000
	(-1.30)	(-1.07)	(-0.04)	(-0.09)
$BTM_{i,t-1}$	0.006*	0.007**	0.002	0.004
	(1.83)	(2.26)	(0.34)	(0.62)
$BHAR_{i,t-1}$	-0.001	-0.001	-0.008***	-0.007***
	(-0.41)	(-0.32)	(-3.47)	(-3.26)
$LnCoverage_{i,t-1}$	0.003*	0.003*	-0.003	-0.003
	(1.87)	(1.70)	(-1.04)	(-0.87)
$Non\text{-}Synch_{i,t-1}$	0.001	0.002	0.001	0.002
	(0.16)	(0.40)	(0.21)	(0.37)
$R\&D_{i,t-1}$	0.016***	0.015***	0.028***	0.041***
	(3.28)	(3.21)	(3.24)	(4.75)
$INST_{i,t-1}$	-0.018**	-0.019**	-0.019*	-0.021*
	(-2.15)	(-2.24)	(-1.71)	(-1.87)
$InsiderOwn_{i,t-1}$	0.023	0.018	-0.055	-0.015
	(0.86)	(0.77)	(-0.93)	(-0.27)
$RETVOL_{i,t-1}$	0.033	0.041	0.017	-0.020
	(0.41)	(0.51)	(0.11)	(-0.15)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.150	0.151	0.225	0.241
N. of Obs.	36,529	36,529	24,164	24,164

Panel C: Sale Transactions

	Annual EA		Quarterly EA	
	(1)	(2)	(3)	(4)
	FracVALUE	*FracNUM*	*FracVALUE*	*FracNUM*
$SoP_{i,t}$	0.014***	0.018***	0.031***	0.036***
	(3.26)	(4.55)	(3.45)	(4.17)
$ln(MV_{i,t-1})$	0.010***	0.009***	0.005	0.006
	(3.53)	(3.14)	(0.79)	(1.02)
$BTM_{i,t-1}$	-0.003	-0.002	-0.012	-0.010
	(-1.14)	(-0.89)	(-1.59)	(-1.50)
$BHAR_{i,t-1}$	0.002	0.002	0.004	0.003
	(1.08)	(0.86)	(0.86)	(0.80)
$LnCoverage_{i,t-1}$	-0.002	-0.002	-0.006	-0.006
	(-1.05)	(-0.78)	(-1.43)	(-1.32)
$Non\text{-}Synch_{i,t-1}$	0.002	0.002	0.017	0.013
	(0.23)	(0.24)	(1.31)	(1.09)
$R\&D_{i,t-1}$	0.006	0.003	0.005	0.001
	(1.05)	(0.61)	(0.47)	(0.11)
$INST_{i,t-1}$	0.019	0.021	0.046**	0.054**
	(1.32)	(1.51)	(2.05)	(2.48)
$InsiderOwn_{i,t-1}$	0.032	0.029	0.055	0.048
	(1.38)	(1.31)	(0.63)	(0.56)
$RETVOL_{i,t-1}$	0.043	-0.010	0.183	0.308
	(0.51)	(-0.13)	(0.82)	(1.46)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.127	0.179	0.149	0.205
N. of Obs.	36,529	36,529	24,164	24,164