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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 as a way to offset the regulatory-induced increase in compensation risk. Our empirical design exploits the staggered adoption of say on pay laws across fourteen countries over the 2000-2015 period. We find that mandatory adoption of say on pay is associated with a material increase in insider trading profitability, especially in firms where executive pay is most affected by say on pay (e.g., firms with excess pay and weaker governance). The increase in insider trading profits is mostly driven by more frequent and larger insider sales, consistent with executives' desire to reduce their exposure to firm-specific risk and rebalance their portfolio. We also find some evidence that after the adoption of say on pay insider sales become more predictive of future returns and are more likely to take place during information-sensitive trading windows (i.e., before earnings announcements). Overall, our results highlight the importance of taking into account potential effects on insider trading incentives when designing compensation reforms and when assessing their impact on executives' incentives.

Keywords*:* say on pay, insider trading, executive compensation, shareholder voting, shareholder activism, corporate governance, regulation
JEL Classification*:* G30, G34, G38, J33, K22, M12, M16
Data Availability: All data used in the paper are available from the public sources cited in the text.

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1. Introduction

We examine whether the adoption of a shareholder vote on executive compensation (commonly known as 'say on pay') is associated with an increase in insider trading profitability. Over the last two decades numerous countries have introduced a say on pay (SoP) regime, generally with the stated goal to increase pay-for-performance sensitivity (PPS), limit the growth rate in pay levels (or reduce "excess" pay) and improve the transparency of the pay setting process. Studying its staggered adoption across countries, Correa and Lel (2016) find that SoP is associated with an increase in PPS and a reduction in pay growth rates, especially in firms with excess pay and weak governance. Single-country studies further show that firms respond to adverse votes by removing controversial pay features (especially those reducing the link between pay and poor performance), and generally find an increase in PPS and in the quality of disclosures, while the effect on pay levels is less clear (e.g., Ertimur, Ferri and Oesch 2013; Ferri and Maber 2013). Overall, this evidence suggests that under a SoP regime executive pay is subject to greater risk and scrutiny.[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. Building on Manne (1966), prior studies show that insider trading can serve as such implicit executive compensation mechanism (Roulstone 2003; Denis and Xu 2013). We conjecture that top executives may engage in more profitable insider trading to compensate for the (perceived or actual) additional risk and potential loss of explicit compensation associated with the adoption of SoP. This unintended effect of SoP could reduce or even 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.[2]

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[1] We use the term "compensation risk" quite broadly to include a number of possibilities. One is the risk of a decrease in target pay levels as a result of SoP votes. Another one is the risk induced by an increase in PPS (and especially in pay-for-poor performance sensitivity), which leads to higher pay volatility and thus "riskier" pay. Under a SoP regime, executives may struggle to receive explicit compensation for this risk premium, due to the potential backlash associated with increases in explicit pay. Another source of risk is the risk of ex post intervention: executives may negotiate a certain pay package and then have some of the terms subsequently modified via SoP vote (e.g. removal of controversial provisions, such as excise tax gross-ups) without the ability to renegotiate the rest of the package or receive compensation for the change. For all these reasons, we argue that executive pay under SoP is subject to greater risk.

[2] 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 lost compensation by engaging in more opportunistic trading, with boards and shareholders unable (or, perhaps unwilling, in the case of boards) to prevent

The prediction that SoP can result in higher insider trading profitability is not without tension. First, executives may conclude that reputation concerns and higher litigation risk associated with greater insider trading activity offset any negative effects of SoP on their explicit compensation. Second, if shareholders monitor insider trading activity and, importantly, take it into account in casting SoP votes, executives will be deterred from seeking greater insider trading profits. Third, the very adoption of SoP may cause, or be symptomatic of, an increase in monitoring of all executives' activities by various outside parties in that country (media, regulators, governance agencies, etc.), resulting in higher reputation and/or litigation costs from engaging in insider trading. Importantly, reputation-sensitive boards may increase their scrutiny of insider trading activities, and/or introduce tighter formal restrictions (e.g., blackout periods). Overall, in view of the above arguments, whether the adoption of SoP affects insider trading is an empirical question.

To address this question, similar to Correa and Lel (2016) we exploit the staggered adoption of SoP across countries and employ a difference-in-differences design, estimating changes in firm-level insider trading profitability after the adoption of SoP relative to a large holdout sample which includes (pre-SoP) observations from countries that eventually mandate SoP and observations from countries that never mandate SoP during our sample period. Insider trading profitability is computed for each firm-year by aggregating estimated capital gains and avoided losses (computed, alternatively, 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 90,000 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 profitability around the adoption of SoP. In terms of economic significance, the increase translates, on average, to about $100,000 of additional implicit annual compensation per executive, which represents 10.1% of the mean executive pay in our sample ($990,000). The findings are robust to a number of design variations such as focusing only on CEO trades, excluding the SoP adoption year, excluding firm-year observations from the Unites States (representing one-third of

such behavior. If so, the positive effects of SoP laws may have been overstated. Alternatively, if compensation packages were optimal prior to the SoP intervention, 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 backlash over an increase in explicit pay levels - respond by effectively 'allowing' executives to capture this premium via higher implicit pay (insider trading profits), which is harder for outsiders to detect. Under either interpretation, our study highlights that in designing compensation reforms such as SoP (and in assessing their overall effect on managerial incentives) it is important to take into account their impact on implicit compensation.

the sample), re-defining the control sample to only include countries eventually adopting SOP or to exclude countries with a voluntary SoP regime, using only firm-year observations with non-zero insider trading activity, and alternative clustering approaches. Also, a placebo test suggests that there is no change in insider trading profits among independent directors, who are not affected by the SoP votes which only cover the pay of executives.

Recent studies suggest that staggered difference-in-differences estimates may be biased in the presence of heterogeneous treatment effects (Baker, Larcker and Wang, 2021; Barrios 2021). Thus, we also perform a series of tests suggested in the literature (e.g., the Goodman-Bacon decomposition, the 'stacked' difference-in-difference method) and conclude that our estimates are not significantly affected by heterogeneous treatment effects.

Our baseline finding is consistent with our prediction that executives respond to the additional compensation risk and potential losses expected under the SoP regime by engaging in more profitable insider trading activity. To corroborate this interpretation, we perform a number of cross-sectional tests. Using a similar sample of staggered adoptions of SoP across countries, Correa and Lel (2016) find that SoP affects executive pay more in firms with higher levels of excess pay and weaker governance (e.g., less independent boards, more 'busy' directors, lower institutional ownership) prior to SoP adoptions. Based on this evidence, we predict larger increases in insider trading profitability in these sub-samples, since executives will have stronger motives to engage in more insider trading to compensate for the effect of SoP. Our findings are consistent with these predictions.

We perform two additional sets of tests to identify the source of the change in insider trading profitability. First, we repeat our main analysis by separating insider buying profits from insider selling profits. Because SoP has been shown to increase the sensitivity of pay to performance especially when performance is poor (Ferri and Maber 2013; Correa and Lel 2016), one may conjecture that executives will be more inclined to engage in insider sales when they expect a drop in performance (and, thus, in their explicit pay) to compensate for the expected loss. More generally, insider sales allow executives to reduce their increased exposure to firm-specific risk under SoP (which has been shown to increase pay-performance sensitivity and equity pay). On the other hand, because it was driven by concerns with "rewards for failure", the adoption of SoP laws may lead to greater monitoring of insider sales, which reduce the alignment between executives and shareholders upon poor performance. Insider purchases opportunistically timed before price run-ups are also likely to trigger negative criticism. Besides, executives may be reluctant to further raise their exposure to firm-specific risk by increasing their purchases. Thus, overall, ex ante it is not clear whether one would expect the increase in insider trading profitability to be driven by buy or sale transactions. Our empirical tests show an increase in both insider buying profits and insider selling profits, with a stronger increase in the latter.

Second, our firm-year level insider trading profitability measure combines trade informativeness (i.e., its ability to predict future stock returns) and trade intensity (which in turn reflects trade size and trade frequency (Skaife et al. 2013)). Thus, we investigate each of these components, and we do so for purchases and sales separately. For insider buys, the three components do no change significantly around the adoption of SoP. In other words, the increase in the aggregate profitability of insider buys is not driven by any specific component, but, rather, is the result of their joint effect. In contrast, after the adoption of SoP insider sales become more frequent and larger in magnitude. We also find some evidence of an increase in their predictive ability (but only when using 6-month profits). Along these lines, additional tests indicate that after the adoption of SoP a higher fraction of insider sales takes place during informative-sensitive periods (i.e. the one-month period prior to annual and quarterly earnings announcement dates).

Overall, combined with our baseline tests, these analyses suggest that the increase in insider trading profitability around the passage of SoP laws is mostly driven by an increase in frequency and magnitude of insider sales, consistent with executives trying to reduce their greater exposure to firm-specific risk and re-balance their portfolio. This finding suggests that some of the policy objectives of SoP votes (e.g., increase pay-for-poor-performance sensitivity, increase equity holdings) may be partly neutralized by insider's trading behaviour. As for the predictive ability of insider trades, we find some evidence of an increase only for insider sales, in some specifications. A potential explanation for the limited effect of SoP on the timing of insider trades is that insiders may refrain from using more private information because they expect higher monitoring under a SoP regime.

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 (usually some governance improvement) to the specific mandate. For example, in the U.S. say on pay was part of the Dodd-Frank Act, a comprehensive piece of legislation introducing numerous, major reforms. However, in our setting this concern is not as pronounced because any other governance reform accompanying the adoption of SoP should, if anything, strengthen monitoring mechanisms and thus, directly or indirectly, *restrict* insider trading opportunities. An example would be a concurrent governance reform strengthening internal controls, which have been shown to be associated with lower insider trading profitability (Skaife et al. 2013). Also, broad governance reforms often attract foreign institutional investors, who have been shown to deter opportunistic insider trading (Hong, Li and Zhu 2019). In other words, in our setting the presence of concurrent governance reforms should bias against finding the hypothesized effect.[3]

Our study combines and contributes to two areas of research, namely, on executive compensation and 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, McCall and Ormazabal 2015; Cuñat, Gine and Guadalupe 2016; Malenko and Shen 2016). While these studies focus on drivers of SoP votes and/or their impact on explicit compensation, we examine the effect of SoP on a form of implicit compensation - insider trading profits. As discussed earlier, our findings suggest that such unintended effects are important in evaluating the effectiveness of SoP laws, and thus have implications for both investors and policy makers. In particular, regulators may need to strengthen restrictions and control on insider trading activities when introducing compensation-related reforms. Investors (and proxy advisors who advise them) need to account for insider trading activities in conjunction with explicit compensation packages when voting on SoP proposals.

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 and equity incentives, consistent with insider trading playing a role in both rewarding and motivating executives. Similarly, in a cross-country setting, Denis and Xu (2013) find that in countries with stronger insider trading restrictions, equity incentives and total pay are higher. They also document significant increases in executive pay and the use of equity-

[3] The literature generally documents a negative association between governance and monitoring quality, and insider trading profitability (Dai, Fu, Kang and Lee 2016; Yost and Shu 2020; Davidson and Pirinsky 2021). Along these lines, Jagolinzer, Larcker and Taylor (2011) find that when general counsel approval is required to execute a trade, insiders' trading profits and the predictive ability of insider trades for future operating performance are substantially lower.

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.[4] We contribute to this research in two ways. First, we complement Denis and Xu (2103)—who examine how executive pay changes in response to greater restrictions to insider trading (insider trading laws and enforcement)—by examining how insider trading profitability changes in response to a form of "restriction" to executive pay (the SoP regime). Second, and relatedly, while those studies highlight how *boards* consider insider trading restrictions in designing optimal pay packages from the perspective of shareholders, we focus on how *executives* change their insider trading behaviour to compensate for a change in their explicit compensation, based on their personal utility function. In doing so, we provide direct evidence that executives view insider trading profits as part of their total pay package. In this respect, we complement two concurrent studies: Gao (2021), who finds that executives use insider trading to make up for the loss in compensation due to missing relative performance goals; and Goldman and Ozel (2020), who find that executives partially offset increases in individual-level tax rates by engaging in more profitable insider trades.

Finally, we contribute to a stream of research which highlights unintended consequences of different types of regulation. In the context of executive pay, Murphy (2012) presents a comprehensive review of the unintended effects (e.g., growth in stock options, rise of perks and golden parachutes) of various tax, accounting, and disclosure regulations in the U.S. over the last century. Kleymenova and Tuna (2020) examine a regulation introduced in the U.K. to change bank

[4] Regarding the association between insider trading restrictions and compensation *levels*, the argument is that insider trading restrictions limit the ability of insiders to exploit their private information when trading, and thus, if wages are set competitively, executives need to be compensated via higher explicit pay (Baiman and Verrecchia, 1995, 1996). As for the association between insider trading restrictions and *incentive pay* (especially equity incentives), five arguments have been offered. First, to the extent that unrestricted insider trading provides greater incentives to increase than decrease firm value (due to restrictions on short-selling and reputation and litigation concerns), any restriction will require to provide greater incentive pay (Manne 1966). Second, by preventing executives from rebalancing their equity portfolios at optimal times, restrictions on insider trading can reduce the value of equity grants (Core and Guay 2001). As a result, firms using such restrictions need to make larger equity grants to maintain optimal incentive levels. Third, if insider trading restrictions make executives less willing to choose risky projects (Bebchuk and Fershtman 1994), then optimal contracts will require larger grants of risk-taking incentives (e.g., stock options). Fourth, restricting insider trading can reduce the information about insider actions impounded into price (Manne 1966, Damodoran and Liu 1993), requiring a corresponding increase in incentive pay to solve moral hazard problems. In all the above arguments the causation runs from the insider trading restriction to equilibrium compensation. A fifth argument is that higher insider trading restrictions may be required to mitigate trading-related agency costs associated with higher equity incentives. Under this argument, the causation runs from compensation to insider trading restrictions. However, Denis and Xue (2013) present evidence inconsistent with the last argument.

executives' risk-taking incentives and find that the regulation achieved its objective but also resulted in higher unforced CEO turnover. Bae et al. (2021) find that in response to pay cuts imposed on CEOs of centrally administered state-owned enterprises in China in 2009, these CEOs increased the consumption of perks and siphoned off firm resources for their own benefit. We extend this line of research by focusing on insider trading behavior as a potential side effect of compensation reform.[5]

2. Sample Selection and Research Design

2.1 Sample selection

Our key data source is the 2iQ Research database, which contains detailed 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 fourteen countries which adopted a mandatory SoP regime starting in 2014 or earlier and eleven countries which during the same period did not adopt such a regime. To identify the sample of 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 profitability (detailed in Section 2.4) we obtain a final sample of 91,692 firm-year observations comprised of 10,085 unique firms. Overall, 34.2% of the firm-year observations are classified as 'treated', i.e., as occurring under a mandatory SoP regime.

Table 1 lists the number of firms and firm-year observations by country, and (for the subset of countries adopting SoP) the year of the SoP adoption (defined as the first full year under SoP) and the type of SoP regime. 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 take it into account in determining pay packages going forward, but past payments to the executives are not affected (the U.S. is an example of this regime). While non-binding, these votes

[5] To the extent that, especially in certain countries, executives resort to other forms of implicit compensation, e.g. greater consumption of perks (Bae et al. 2021), by focusing on insider trading we may be understating the effect of SoP on implicit compensation. 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. That said, executives may 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).

often lead to substantial changes in pay practices when voting dissent is significant (Ertimur et al. 2013; Ferri and Maber 2013).[6] The other six countries adopted a binding SoP 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 U.K.) adopted an advisory vote on the compensation report first and then added a binding vote on the remuneration policy.

It is important to keep in mind that 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 in most advisory SoP regimes the vote is held annually, the U.S. 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), while Ferri and Göx (2018, ch.3) examine analytically the effectiveness of advisory versus binding votes (as well as retroactive versus prospective votes).

2.2 Research Design

To examine the effect of SoP laws on insider trading profitability, we estimate the following generalized difference-in-differences 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 profitability at the firm-year level (detailed in Section 2.3), and $SoP_{i,t}$ is an indicator variable that equals one for firm-years after the adoption of mandatory SoP (including the adoption year), and zero otherwise. Thus, the coefficient β_1

[6] 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 or in anticipation of a high dissent SoP vote. Ertimur et al. (2013) report that over half of the US firms made compensation changes explicitly in response to negative ISS recommendations on SoP votes. Other studies show that firms respond to nonbinding shareholder votes on director elections (Del Guercio, Seery and Woidtke 2008; Fischer, Gramlich, Miller and White 2009; Ertimur, Ferri and Oesch 2018, Aggarwal, Dahiya and Prabhala 2019), on non-SOP compensation proposals (Morgan and Poulsen 2001; Ferri and Sandino 2009; Ertimur Ferri and Muslu 2011), and on other types of shareholder proposals (Ertimur et al. 2010; Cuñat, Gine and Guadalupe 2012; Bach and Metzger 2017).

captures the average change in insider trading profitability subsequent to the adoption of mandatory SoP relative to the control group. The control group includes pre-SoP firm-year observations from mandatory SoP countries as well as firm-year observations from countries that period did not introduce a mandatory SOP regime during our sample period. $X_{i,t-1}$represents a vector of time-varying firm-level covariates, detailed in Section 3.4. To alleviate concerns with correlated omitted variables, we include both 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 macroeconomics conditions. Standard errors are clustered by firm. Ideally, we would cluster our standard errors by country since our treatment (the introduction of SoP) takes place at the country level. However, the relatively small number of countries in our sample (25), and the small number of observations for some countries, may bias our tests and overestimate the precision of our effect (Bertrand, Duflo, and Mullainathan 2004). In robustness tests (Section 3.5) we examine the sensitivity of the results to alternative clustering.

2.3 Measuring insider trading profitability

Following prior studies (e.g., Huddart and Ke 2007, Skaife et al. 2013, Chung, Goh, Lee and Shevlin 2018), we define the profitability of insiders' trades as the total (unrealized) capital gains after open market purchases and the total loss avoided by open market sales from the insiders. In particular, we focus on CEO, CFO, and COO (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).[7]

To calculate insider trading profitability at the firm-level, we first obtain data on open market buy and sale transactions by the firm's insiders from the 2iQ Research database.[8] We then sum the trading value (number of shares multiplied by stock price) of, respectively, buy and sale transactions by each insider occurring on the same day, thus obtaining a firm-day level measure of trading value separately for buy and sale transactions. Next, for buy transactions, we multiply the

[7] We effectively assume that CEO, CFO, and COO are covered by SoP laws. To identify these executives in 2iQ, 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'; as CFO if the title is 'CFO' or ' Finance Director'; and as COO if the title is 'COO'.

[8] 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' in the 2iQ database), trades which 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).

trading value at the firm-day level by the six-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.[9] For sale transactions, we take the negative of the product of the trading value at the firm-day level and the six-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, so as to obtain a firm-year level estimate of insider trading profitability in dollar terms, scaled 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 the 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 dollar 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*.

This measure combines both the size and value of the trade (capturing trade materiality) and the returns from trading (capturing the predictive ability of the trade with respect to future stock returns), consistent with our objective to measure the change in 'implicit compensation' from insider trading activity around the adoption of SoP. In Section 4 we will examine the different components separately.

2.4 Control Variables

In Eq. (1) we control for a number of firm-level variables shown by previous studies to affect insider trading incentives and profitability, including firm characteristics, information environment, price informativeness, and total beneficial ownership of top-3 executives (Frankel

[9] 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 U.S., 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 U.S., non-U.S. studies also document abnormal returns over similar windows (e.g., Gregory et al. 1997 for U.K. 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.

and Li 2004; Ofek and Yermack 2000; Piotroski and Roulstone 2005; Seyhun 1986; Skaife et al. 2013; Ryan, Tucker and Zhou 2016). With respect to firm characteristics, we control for firm size (the natural logarithm of market capitalization, *Ln(MV$_{t-1}$)*), growth (book-to-market ratio, *BTM$_{t-1}$*) and return momentum (annual buy-and-hold abnormal returns, *BHAR*$_{t-1}$). Those variables not only 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)—but also capture risk factors that explain variation in post-transaction market-adjusted returns and profits (e.g., Fidrmuc et al. 2013).

To capture the firm's information environment, we control for analyst coverage (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 profitability (Frankel and Li 2004). Total institutional ownership (*INST$_{t-1}$*), an indicator for R&D expense (*R&D$_{t-1}$*) and return volatility (*RetVol$_{t-1}$*) are included to further control for differences in information asymmetry across firms. Insiders trade less profitably when institutional ownership is higher (Hillegeist and Weng 2018), more often and 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, following Chen, Goldstein and Jiang (2006) we use price non-synchronicity (*Non-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.[10] 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 control for the total beneficial ownership of insiders (i.e., CEO, CFO, and COO) top-3 executives (*InsiderOwn$_{t-1}$*) to capture the potentially higher propensity to trade to reduce their exposure to firm risk by top executives with higher stock ownership.

Stock (flow) variables are measured at the end (during) of fiscal year t-1. Market capitalization is converted from local currency to US dollar based on the historical exchange rate.

[10] Price non-synchronicity is defined as one minus the R square from the following regression over the past 36 months (with at least 15 months available data): $RET_{i,k,t} = \alpha + \beta_1 MKTRET_{k,t} + \beta_2\, MKTRET_{US,t} + \varepsilon_{i,k,t}$
where $RET_{i,k,t}$ is the return in month *t* of firm *i* belonging to country *k*, $MKTRET_{k,t}$ and $MKTRET_{US,t}$ is the value weighted market return in country *k* and the value-weighted market return in the US, respectively.

Detailed definitions and data sources for the above variables are in Appendix A. To mitigate the potential effect of outliers, all continuous variables are winsorized at the 1% and 99% levels.

3. Empirical analyses: changes in insider trading profitability around Say on Pay

3.1 Summary statistics

Table 2, Panel A, reports the summary statistics of the variables used in our tests of Eq. (1). The mean insider trading profitability at the firm-year level over a 6-month (12-month) period is 0.008% (0.014%) of market value. The average firm-year observation in the full sample has a market value of $2.75 billion, a book-to-market ratio of 0.73 and past year's buy-and-hold abnormal returns of 9.0%. About 33% of the firm-year observations have non-zero reported R&D, the average number of analysts covering a firm is 15.2 (34.0% of the observations have no analyst coverage - untabulated), and institutional and insider ownership are, respectively, 27.5% and 3.0% on average.

Panel B of Table 2 displays the correlations between insider profitability and our control variables for the full sample (below the diagonal) and the conditional sample (above the diagonal). In line with prior studies, insider trading profitability is 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).

3.2 Baseline results: SoP laws and insider trading profitability

Table 3 presents our baseline results about the effect of SoP adoption on insider trading profitability, based on the difference in-differences 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 avoid the issue of "bad controls" (firm characteristics potentially also impacted by the treatment) affecting our ability to draw causal inferences (Gormley and Matsa, 2014). In line with our hypothesis, the coefficient of $SoP_{i,t}$ is positive and significant at the 1% level for both insider trading profitability 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 $PROFIT_{12m}\%_{it}$) implies that the dollar value of insider trading profitability increases by 0.011% of the firm's market capitalization. Given the average market capitalization of about $2.75 billion (Table 2 Panel A), this translates to approximately a $303,000 increase in annual implicit compensation, i.e., an average of over $100,000 per executive. This increase represents about 10.1% of the mean annual total pay for executives in our sample ($990,000; untabulated), and thus is economically substantial. Ideally, one would want to compare this amount to the potential loss in explicit compensation under a SoP regime. While a direct comparison is difficult, in their sample Correa and Lel (2016) estimate that annual total CEO pay decreases by 9.12% (corresponding to a decline of $99,576) for firms in the bottom quartile of industry-adjusted stock performance, and by 3.34% (corresponding to a decline of $38,215) for firms in the top quartile.[11]

As for the control variables insider trading profitability is positively associated with return volatility (Frankel and Li, 2004) and asynchronicity (Piotroski and Roulstone, 2004), consistent with insiders trading more profitably in stocks with more idiosyncratic price movements. The positive coefficient for book-to-market is consistent with insiders' ability to spot mispricing along the value/glamour dimension (Lakonishok and Lee, 2001; Gregory et al., 2013). The positive coefficient on firm size (statistically significant only for six-month returns) likely reflects greater insider trading value when market value is higher. The positive coefficient for R&D (statistically significant for 12-month returns) is consistent with Aboody and Lev (2000). While these results are generally in line with prior studies, it should be noted that our regressions (unlike some prior studies) include firm fixed effects, and some covariates do not exhibit much within-firm variation, which explains their limited explanatory power (e.g., whether a company reports R&D or the investor base).

To sum up, our baseline tests suggest an economically significant increase in insider trading profitability around the adoption of SoP, consistent with executives relying on insider trading to compensate for the additional risk and potential loss in explicit compensation expected under a SoP regime.

[11] The estimate in Correa and Lel (2016) is likely to be a lower bound. The effect of many changes in compensation contracts (e.g., changes in severance terms, performance targets, vesting criteria) may not materialize in realized pay until later periods. More generally, the reduction in explicit pay under a SoP regime does not fully reflect the enhanced compensation risk an executive is subject to.

3.3 Assumptions behind Difference-in-Differences Estimations

Our research design departs from a simple difference-in-differences (DiD) model with a single "treatment" event and exploits instead 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 effects DiD can produce severely biased estimates (e.g., Abraham and Sun 2018; Chaisemartin and D'Hautfeuille 2020). 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, the treatment effect for early adopters may take place gradually over time, which may compromise their ability to act as "good" control sample for late adopters. Also, the treatment effect itself may differ between late and early adopters.

Studies using a DiD estimations typically perform a dynamic decomposition to examine whether pre-treatment trends are similar between treatment and control firms, i.e., whether they are consistent with the parallel trend assumption which underlies the difference-in-differences estimation. However, recent methodological reviews in accounting and finance (e.g., Baker et al. 2021; Barrios 2021) highlight the importance of taking additional steps to correct for the potential biases due to heterogeneity and time variation in treatment effects discussed above. Thus, we perform two additional tests to ensure the validity of our findings.

First, we adopt the methodology developed by Goodman-Bacon (2019), which classifies the observations into the following four categories: Early Adopters (i.e., firms in countries that adopt an SOP law towards the beginning of the sample period), Late Adopters (i.e., firms in countries that adopt an SOP law towards the end of the sample period), Treated (i.e., firms in countries that adopt an SOP law at any point during the sample period, regardless of whether 'early' or 'late'), Never Treated (i.e., firms in countries that never adopt an SOP law during our sample period), and Already Treated (i.e., firms in countries that adopt an SOP law before the beginning of our sample period). As noted earlier, the baseline DiD estimate is a weighted average of various comparisons. Using the above groups, the Goodman-Bacon decomposition identifies the weight of each of the potential comparisons as well the treatment effect estimate attributed to each comparison.

It is important to stress that the Goodman-Bacon (2019) 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 24 out of the 25 countries used in our baseline results in Table 3. The cost of this choice is that four countries have received the treatment by 2006 and will thus not exhibit any variation during this time period (they will belong to the group of "Already Treated").

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.[12] Next, it shows the weight and treatment effect estimate for each of the underlying comparisons. It appears that 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 (31.7%). In contrast, and importantly, the weights associated with the "timing" comparisons - Early Adopters vs. Late Adopters (with Late Adopters used controls) and Late Adopters vs. Early Adopters (with Early Adopters used as controls) – are fairly low, respectively at 7.5% and 4.6%.[13] As noted earlier, these "timing" comparisons can introduce significant bias if the treatment effects are time-varying and heterogeneous. Thus, it is comforting that they contribute only marginally to our DiD estimate.

As for the treatment effect estimates (the Beta column in Panel A), it is also comforting to see that they are generally similar when comparing Treated vs. Never Treated and Treated versus Already Treated (e.g., 0.010 and 0.007 for $PROFIT_{6m}\%_{it}$, respectively). The comparison Early Adopters vs. Late Adopters also yields a positive coefficient, but its magnitude is much lower (close to zero). Furthermore, the comparison Late Adopters vs. Early Adopters generates a *negative* coefficient.[14] Combined, weights and coefficients suggest that any bias due to heterogeneous treatment effects should be modest in our sample.

[12] The Goodman-Bacon decomposition is estimated without covariates and thus the "benchmark" for the estimates of the *SoP* coefficient in the top row are columns 1 and 2 in Table 3.

[13] 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).

[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 profitability.

Notwithstanding the above, following the recommendations in Barrios (2021) we also report the "stacked" methodology proposed by Cengiz, Dube, Lindner, and Zipperer (2019). Specifically, we construct a separate dataset for each treatment event (i.e., the adoption of SoP by a given country), in such a way that a given firm can only appear either in the treatment or the control group with respect to such 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, using the same balanced sample as in Panel A of Table 4.[15] 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 firm could be a "bad" control in the presence of heterogeneous, time-varying treatment effects.

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). Using this approach, our coefficient of interest remains positive and statistically significant at conventional levels across all four specifications (two profitability measures, with and without additional controls). In terms of magnitude, our effects remain comparable to the baseline effects from Table 3, though slightly larger possibly because this stacked procedure partially corrects for the downward bias induced by the 'timing' groups (as highlighted via the Goodman-Bacon decomposition).

Finally, we perform the 'traditional' dynamic decomposition which is used to assess the existence of a pre-trend and thus the validity of the parallel trend assumption that underlies DiD estimates. However, 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 time period 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 $PROFIT_{12m}\%_{it}$ measure (Figure 1b). The figures reveals that our coefficients of interest do not exhibit a pre-trend in the pre-regulation period, while being positive and statistically significant for most post-SoP

[15] This means that the control sample includes all firms that do not adopt SoP during the window, whether because they adopted it prior to the window, subsequent to the window or, because they never adopted it during our sample period.

years across both specifications. Overall, this test does not suggest that the parallel trend assumption is violated.

Overall, the above tests suggested by recent methodological advances (Baker et al. 2021; Barrios 2021) support our inferences based on the staggered difference-in-differences estimates and do not identify a violation of the parallel trend assumption.

3.4 Cross-sectional tests

Our interpretation of the evidence in Tables 3 and 4 is that executives responded to the additional compensation risk and potential losses expected under the SoP regime by engaging in more profitable insider trading activity. To corroborate this interpretation, in this section we develop a number of firm-level cross-sectional tests. In particular, we examine whether the effect of SoP adoption on insider trading profitability is more pronounced among firms most 'affected' by the adoption of SoP, i.e., firms where SoP had the greatest impact on executive pay. Correa and Lel (2016) examine the adoption of SoP laws and find that SoP had greater impact on executive pay (e.g., higher pay-for-performance sensitivity, lower growth rate in pay levels) at firms with higher excess pay and weaker governance prior to the passage of SoP laws. Thus, we predict that executives at these firms will trade more aggressively.

To examine the role of excess pay, we modify Equation (1) by adding an interaction term between *SoP* and an indicator, *Dum*, denoting firms in SoP adopting countries (hereinafter "SoP firms") with higher excess pay prior to SoP. To construct this indicator, we run a pooled regression of average (top-3) executive pay on its economic determinants, including year, industry and country fixed effects for the entire sample.[16] Then, for each SoP firm we take the yearly firm-year residual (our proxy for excess pay) and compute its average across all pre-SoP years.[17] The indicator *Dum* is set equal to one for SoP firms with pre-SoP average excess pay above the country median.[18]

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[16] Following prior research (e.g., Core, Guay and Larcker 2008) we include as economic determinants the natural logarithm of sales, industry-adjusted stock returns, the market-to-book ratio, annualized stock return volatility, and leverage. To account for cross-country differences in economic growth, we also control for the gross domestic product (GDP) growth. Our source for executive compensation data is Capital IQ.

[17] We obtain similar results if (i) we only use 3 years prior to SoP adoption in estimating excess pay; (ii) if we compute excess pay each year as the simple difference between total pay and industry average pay, over either all pre-SOP years or the 3 years prior to SoP adoption; (iii) if we estimate the excess pay residual by running separate regressions for each country-industry-year combination instead of a pooled regression with industry, year and country fixed effects.

[18] We only interact *Dum* with *SoP* without including it as main effect for two reasons. Empirically, we cannot measure pre-SOP excess pay for non-SoP countries, lacking an adoption date. Conceptually, we do not predict a relation

The results are reported in Table 5, columns 1 and 2 (the sample size drops from 91,692 to 63,937 observations because of limited executive pay data availability in Capital IQ). For both dependent variables ($PROFIT_{6m}\%_{it}$ and $PROFIT_{12m}\%_{it}$), the coefficient of *SoP* is positive and significant at the 5% level, while the interaction term (*SoP*Dum*) is positive and statistically significant at the 1% level, indicating that the effect of SoP on insider trading profitability is stronger in firms with greater pre-SoP excess pay, consistent with our prediction.[19]

As noted earlier, Correa and Lel (2016) also find that the effect of SoP adoption on executive pay is more pronounced in firms with weaker governance, as proxied by lower board independence, greater presence of busy directors, and lower institutional ownership. Besides, we expect boards of such firms to be more permissive of insider trading as an implicit compensation mechanism post-SoP adoption. That is, we predict that *both* the incentives and the opportunity to engage in more insider trading are higher at weak governance firms, leading us to predict a greater effect of SoP on insider trading profitability at such firms. To examine this prediction, we re-define the *Dum* indicator, alternatively, as equal to one for (ii) SoP firms with a percentage of busy directors (i.e., directors with two or more other seats) above the country median (column 3 and 4); (ii) SoP firms with board independence (percentage of outside directors) below the country median (column 5 and 6); and (iii) SoP firms with institutional ownership below the country median. All three variables are measured at the end of the year prior to SoP adoption (the sample size drops slightly due to limited data availability for the governance variables).

For the board-level variables the interaction term *SoP*Dum* is positive and significant (at the 1%-10% level) for both $PROFIT_{6m}\%_{it}$ and $PROFIT_{12m}\%_{it}$, while the main effect of *SoP* remains positive but is not significant, suggesting that the impact of SoP on insider trading profitability is concentrated in firms with weaker boards. As for the institutional ownership variable, the interaction term (*SoP*Dum*) is positive, though not significant at conventional levels. For all three

between level of excess pay and changes in insider trading profitability, other than for SoP countries via the effect of pre-SOP excess pay on future compensation. The same holds for the other partitions used for the cross-sectional tests.

[19] In unreported analyses we repeat the cross-sectional test re-defining *Dum* as equal to one for firms experiencing a *decrease* in average excess pay from the pre- to the post-SoP adoption years (and zero otherwise). The interaction *SOP*Dum* is positive (though significant only for $PROFIT_{6m}\%_{it}$), suggesting that the increase in insider trading profitability is more pronounced in firms which ex post experience a decrease in excess pay around the SoP adoption, in line with our hypothesis. While this test suffers from endogeneity concerns (i.e. the problem of measuring 'most treated' firms ex post, *after* the treatment), it corroborates the finding based on the ex ante proxy of the impact of SoP (i.e. the pre-SOP level of excess pay).

variables, the coefficient of *SoP* in the partition of interest is positive and significant (see F-test for sum of coefficients).

Overall, these findings suggest that the post-SoP increase in insider trading profitability is concentrated or most pronounced in firms where (according to prior studies) executive pay was most affected by SoP, consistent with the notion that executives at these firms intensified their insider trading activity to "offset" the compensation impact of SoP.

In additional analyses, we also consider two country-level cross-sectional tests. The first test exploits the notion that, ceteris paribus, executives' opportunity to increase their insider trading activity in response to SoP regulations should be stronger in countries with weaker monitoring of insider trading. We obtain data on country-level insider trading closed periods from SmartInsider (formerly known as Director Deals). If SmartInsider classifies a country as having "no mandated non-trading period", we consider the country as having low insider trading restrictions. Otherwise, if the country has a mandate in place (either by banning trading in a specified period—typically a certain number of days before earnings announcements—or by requiring firms to come up with such period on their own), we consider it as having high insider trading restrictions. We then create an indicator (*Dum*) equal to one for SoP adopting countries which do not mandate a non-trading period for insiders,[20] and interact it with *SoP* in the regression. The results (untabulated) suggest that the association between mandatory SOP laws and insider trading profitability is driven by countries with low insider trading restrictions. While this finding is consistent with economic intuition, we acknowledge that a more powerful test would ideally exploit the variation in firm-level insider trading restrictions, but the required data are not immediately available.

A second potential country-level cross-sectional test exploits the variation in types of SoP regimes, in terms of advisory versus binding votes. It may be tempting to assume that a binding vote would be more impactful, since an advisory vote can be ignored by the board. However, the differences between the two regimes are more complex. Under an advisory SoP regime, shareholders cast an advisory vote on the *past* year's compensation *report* (detailing the pay packages awarded to the top executives). Under a binding SoP regime, shareholders cast a *forward-looking*, binding vote on the proposed remuneration *policy* and its principles. Thus, the

[20] In particular, we code Belgium, Canada, Germany, Italy, Norway, Portugal, Spain, Switzerland, U.S. as SoP adopting countries with low insider trading restrictions. We classify the U.S. as a low insider trading restriction country because, even though SmartInsider indicates that it has non-trading closed periods, those are only recommended, not regulatorily mandated.

two regimes also differ in what is being voted upon. Accordingly, theoretical work shows that the relative effectiveness of these two SoP regimes depends on a number of factors, and ex ante it is not clear which regime would be more likely to impact executive pay (see Ch.3, Ferri and Göx, 2018). Indeed, in our (untabulated) tests we find no difference between the two regimes.

3.5 Robustness tests

Table IA.1 in the Internet Appendix presents a number of robustness tests. As a benchmark, in Panel A the first row reproduces the coefficients of $SoP_{i,t}$ from Table 3, followed by seven sets of robustness tests. The first test excludes the SoP adoption year (i.e., the first full year under the new regime). In our main tests we include the adoption year because SoP laws are usually proposed during the prior year. For example, say on pay was introduced in the U.S. as part of the Dodd-Frank Act, which was enacted in July 2010, but the regulations covering say on pay became effective in January 2011. Thus, it is reasonable to assume that insider trading behavior is affected already during the first SoP year. To the extent this was not the case, including the adoption year would cause us to understate the effect of SoP. Nonetheless, by excluding the SoP adoption year we rule out the possibility that somehow its inclusion drives our results.

Second, we re-define insider trading profitability using only CEOs' trades, to ensure that our results are not driven by non-CEO insider trades. Given the visibility and level of their pay, CEOs are supposed to be affected by SoP as much as (if not more than) other top executives. Thus, if results were only driven by non-CEO insider trades, our interpretation of the findings could be questionable. At the same time, CEOs' trades may be subject to greater scrutiny, possibly making the cost-benefit tradeoff of greater use of insider trading less favorable for CEOs than for other top executives. Thus, examining the effect of SoP on CEO trades only is interesting in itself.

Third, the results are robust to excluding firms from the United States, suggesting that the effects are not driven by the country with the largest number of observations (about 1/3 of the sample). The results are also robust to excluding one country at the time (see Panel B).

The fourth and fifth tests re-define the control sample. In the main tests of Table 3 our control group includes pre-SoP observations from countries eventually mandating SoP as well as observations from countries never mandating SoP regime during the sample period. To alleviate concerns that the results may reflect differences between adopting and non-adopting countries, in the fourth robustness test we only include countries eventually mandating SoP during our sample period. That is, the effect is identified by comparing post-SoP observations to pre-SoP observations from countries eventually mandating SoP, arguably a more homogeneous control sample. The fifth test also relates to the definition of the control group but deals with a different issue. Our control sample in the main tests includes two countries (Canada and Germany) that did not adopt a mandatory SoP regime during our sample period but introduced a "voluntary" SoP regime (typically via a governance code allowing and recommending a say on pay vote). It also includes all firm-year observations prior to the adoption of mandatory SoP from two countries (Spain and Switzerland) that had first introduced a voluntary SoP regime (respectively, in 2008 and 2009). Correa and Lel (2016) show that voluntary SoP countries did not experience an increase in PPS or a reduction in pay growth as a result of SoP, providing justification for our choice to include these observations in the control sample. However, the results are robust to excluding all firm-year observations under a voluntary SoP regime.

In the sixth test we repeat the analysis including only firm-year observations with non-zero insider trading activity (i.e., with at least one trade by a firm top executive). The coefficient of $SoP_{i,t}$ is positive and significant across all four columns, ranging from 0.017% to 0.021%. Not surprisingly (since this analysis is based on observations with non-zero insider trading activity) the coefficient of $SoP_{i,t}$ (at, respectively, 0.023% and 0.037%) and the explanatory power (0.230 and 0.248 - unreported) are larger than in the full sample of Table 3.

Finally, while in our main tests we cluster the standard errors by firm, in the last robustness test we report the results when clustering, alternatively, by firm and year, by country, and by country and year. The positive coefficient of *SoP* continues to be significant at the 5% or 10% level. In the Internet Appendix we also present a placebo test, where we examine the changes in trades' profitability around mandatory adoption of SoP for independent (i.e., non-executive) directors. This group arguably represent a valid placebo sample because independent directors are not directly affected by SoP votes, which only cover the pay of executives - though their validity as placebo groups is weakened if independent directors mimic the trades of executives at their firms.

Table IA.2, columns 1 and 2, shows that when insider trading profits are measured for non-executive directors instead of the top 3 executives, using the subset of firms with at least one independent director's trade during the sample period. The coefficient of *SoP* is not significant when using $PROFIT_{12m}\%_{it}$, and is significant only at the 10% level (and substantially smaller than in Table 3) when using $\%PROFIT_{6m}\%_{it}$. Next, to make a better comparison between executives and independent directors, in columns 3-6 we show the results when using a subset of firms with insider trading by *both* executives and independent directors within our sample period (though not necessarily in the same year – such sample is very small). For the top 3 executives, the coefficient of interest is significant at 1% or 5% level, while for non-independent directors is significant - but substantially smaller in magnitude - only at the 10% level and only when using $PROFIT_{6m}\%_{it}$. With caution, this placebo test is consistent with the adoption of SoP laws mostly affecting the trading behaviour of those directly covered by such laws (i.e., top executives).

4. Decomposing the change in insider trading profitability around Say on Pay

Our analyses in Section 3 suggest an increase in firm-level insider trading profitability due to the adoption of SoP. A natural question is whether this finding is driven by insider purchases, sales, or both. Also, as noted in Section 2.3, our firm-year level insider trading profitability measure combines trade informativeness (i.e., its ability to predict future stock returns) and trade intensity (which, in turn, is a function of trade size and trade frequency; Shaike et al. 2013), raising the question of which of these components change around SoP laws, and how. In this section we address these questions by decomposing the change in insider trading profitability.

4.1 Insider trades: buy vs. sale transactions

To explore whether the increase in insider trading profitability around the adoption of SoP laws is driven by buy or sale transactions (or both), in Table 6 Panel A we repeat the analysis after re-estimating our two measures of insider trading profitability using only buy (columns 1-2) or only sale (columns 3-4) transactions. Ex ante, it is not clear whether one would expect the increase in insider trading profitability to be driven by buy transactions, sale transactions, or both. On one hand, because SoP has been shown to increase the sensitivity of pay to performance especially when performance is poor (Ferri and Maber 2013; Correa and Lel 2016), one may conjecture that executives will be more inclined to engage in insider sales when they expect a drop in performance (and, thus, in their explicit pay) to compensate for the expected loss. More generally, they may use insider sales to reduce their increased exposure to firm-specific risk under SoP (which has been shown to increase PPS and equity pay). On the other hand, because it was driven by concerns with "rewards for failure", the adoption of SoP laws is likely to increase monitoring of insider sales, which may be viewed as reducing the alignment between executives and shareholders upon poor performance. Insider purchases timed before price run-ups and suspected to be based on private information are also likely to trigger negative criticism. Plus, insiders may be reluctant to purchase equity and further increase their exposure to firm-specific risk under SoP, unless it helps prolong their tenure (Armstrong, Blackburne, and Quinn 2021). Thus, overall it is not clear whether an executive subject to higher compensation risk and scrutiny under SoP will focus more on purchases or sales as a source of trading profits.

Panel A shows that the coefficient of $SoP_{i,t}$ is positive and significant for both buy and sale transactions, with the magnitude larger for insider sales. As for the control variables, compared to Table 3, insider trading profitability is positively associated with return volatility only for insider sales and with book-to-market, asynchronicity, and R&D only for purchases, consistent with insiders profiting from their purchases based on public and private information (Veenman 2013). Furthermore, the distinction between buys and sales reveals a clear pattern: the negative (positive) sign on firm size and past returns for insider buys (sales) is consistent with insiders trading as contrarian, i.e., buying low (selling high) (Piotroski and Roulstone 2005).

4.2 Insider trades' informativeness, size and frequency

To examine the change in the three components of insider trading profitability (informativeness, size and frequency) around the adoption of SoP laws, we replace insider trading profitability with each of the three components as dependent variable. When we examine all insider trades (including buys and sales), all three components are positive and significant (untabulated). Thus, next we perform the analysis separately for buy (Panel B) and sale (Panel C) transactions.

We start by examining whether the extent to which top executives trade on private information—as proxied by the excess return following individual trades—changes around the adoption of SoP laws. To do so, we modify Equation (1) by regressing future excess returns on the $SoP_{i,t}$ indicator using individual *transaction-level* data (over 220,000 transactions by 20,831 unique executives). We measure future excess returns, alternatively, as the buy-and-hold abnormal return either over 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 profitability, namely, the size of each trade ($VALUE\%_{i,t}$), computed as the dollar value of the transaction 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 the natural log of the number of insider trades (namely, number of buys in Panel B and number of sales in Panel C) at the firm-year level (i.e., across all top-3 executives) as dependent variable ($Ln(NUM)_{i,t}$).

Panel B suggests that for insider purchases the three components do no change significantly around the adoption of SoP. In other words, the increase in the aggregate profitability of insider buys (Panel A) is not driven by any specific component, but, rather, it appears to be the result of their joint effect.[21] In contrast, Panel C shows that after the adoption of SoP insider sales become more frequent, larger, and, so tome extent, more predictive of future performance (the latter result only holds when using $PROFIT_{6m}\%_{it}$).

Combined, Tables 3 and 6 suggest that the increase in insider trading profitability around the adoption of SoP laws is mostly driven by larger and more frequent insider sales. This finding is consistent with executives trying to reduce their greater exposure to firm-specific risk (stemming from the increase in equity holdings and PPS induced by SoP) and suggests that some of the policy objectives of SoP votes (e.g., increase pay-for-poor-performance sensitivity, increase equity holdings) may be partly neutralized by insider's trading behaviour. As for the predictive ability of insider trades, there seems to be an increase only for insider sales, and only when using $PROFIT_{6m}\%_{it}$. A potential explanation is that insiders generally refrain from using more private information arguably due to the additional monitoring of their behavior under a SoP regime.

4.3 Insider trades' timing

Finally, we consider whether insiders are more likely to time their trades during informative-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 information-sensitive window – increases around the SoP adoption.[22]

The results are reported in Table 7 (the sample size drops to 37,347 firm-year observations because for some firm-years the annual earnings announcement dates are missing). The positive and significant coefficients of $SoP_{i,t}$ in columns 1 and 2 indicate an increase of 1.9% (2.4%) 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 re-defining $FracVALUE_{i,t}(FracNUM_{i,t})$ as the fraction of

[21] Alternatively, we acknowledge that the lack of significance might come from a loss of statistical power of our tests due to a reduced sample size.
[22] Recent papers have used a similar approach to explain within-firm/individual changes in insider trading profitability (e.g., Bourveau et al., 2021).

the total value (number) of all insider trades taking place during any of the one-month periods prior to quarterly earnings announcement dates during the year. The sample size further drops to 24,613 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, in particular those in countries with semi-annual reporting frequency. 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 find that the result in Panel A is mostly driven by sales transactions, suggesting that that the greater predictive ability of insider sales after the adoption of SoP documented in Table 6 (at least with respect to 6–month trading profits) may partly be due to a greater fraction of insider sales taking place during information-sensitive windows.

5. 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. Say on pay imposes substantial compensation risk and potential losses to executives, with prior studies showing that it is associated with higher pay-for-performance sensitivity and a decline in the growth of pay levels. We posit that insiders may respond to this increased compensation risk by engaging in greater insider trading activity. Using the staggered adoption of say in pay across countries, we indeed find an increase in insider trading profitability. Cross-sectional tests indicate that the effect is more pronounced at firms whose executives are most negatively affected by the say on pay regime, i.e., firms with higher excess pay and weaker governance. While insiders profit significantly more from both their purchases and sales after SoP adoption, further analyses show that the increase in insider trading profits is driven by significantly larger and more frequent sales, presumably because insiders re-balance their exposure to firm-specific risk in view of the increase in pay-for-performance sensitivity induced by the adoption of say on pay. Insider sale transactions are also more predictive of six-month excess returns and more likely to occur during information-sensitive windows after SoP. Our findings highlight an important unintended consequence of say on pay and have implications for policy makers and investors.

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Appendix A: Variable Definition

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.
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 the fiscal year *t*, scaled by market value of equity at the end of the 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 computed as the buy-and-hold abnormal return over 6 months after the trading day *j*, minus the value-weighted market return for the firm's country; $VALUE\_BUY_{itj}$ ($VALUE\_SALE_{itj}$) 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*. In Table 7, Panel A, *$PROFIT_{6m}\%_{i,t}$* is re-computed, alternatively, using only buy transactions and only sale transactions.
$PROFIT_{12m}\%_{i,t}$	Similar to *$PROFIT_{6m}\%$*, except that $BHAR_{itj}$ is computed over 12 months after the trading day *j*. In Table 7, Panel A, *$PROFIT_{12m}\%_{i,t}$* is re-computed, alternatively, using only buy transactions and only sale transactions.
$BHAR_{6m\ (12m)\ i,t,j}$	Buy-and-hold abnormal return over 6 (12) months after the trading day *j*, minus the value-weighted market return for the firm's country.
$VALUE\%_{i,t,j}$	Value of individual trade *j* by top-3 executives of firm *i* during the fiscal year *t*, scaled by the market value of equity at the end of the fiscal year *t-1*.
$Ln(NUM)_{i,t}$	In Table 7, Panel A, the natural logarithm of the total number of insider trades by top-3 executives of firm *i* during the fiscal year *t*. In Table 7, Panel B (Panel C), the natural logarithm of the total number of insider purchases (sales) by top-3 executives of firm *i* during the fiscal year *t*.
$FracVALUE_{i,t}$	In Table 8, column 1, 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 8, column 3, fraction of 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 8, column 2, fraction of 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 8, column 4, fraction of 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 dollar based on the historical exchange rate. Data sources: Center for Research in Security Prices (CRSP) for US firms, Compustat/North America for Canada firms, 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 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*, calculated as the raw buy-and-hold return of each firm minus the value-weighted total market buy-and-hold return for the country where the firm's headquarters are located. Data sources: Center for Research in Security Prices (CRSP) for US firms, Compustat/North America for Canada 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-Synch $_{i,t-1}$	Price non-synchronicity, defined as one minus the R-square from the following regression over the past 36 months (I require at least 15 months 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*, $MKTRET_{k,t}$ and $MKTRET_{US,t}$ is the value weighted return in country *k* and the value-weighted return in US, respectively. Data sources: Center for Research in Security Prices (CRSP) for US firms, Compustat/North America for Canada firms, Compustat/Global for firms in other countries.
R&D $_{i,t-1}$	Indicator variable that equals one if 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 US and Canada, 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 daily stock returns of firm *i* during the 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, Compustat/Global for firms in other countries.

Figure 1. Stacked Difference-in-Differences Parallel Trend

Figure 1a (1b) plots time-varying treatment effects on insider trading profitability of 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, respectively. 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).

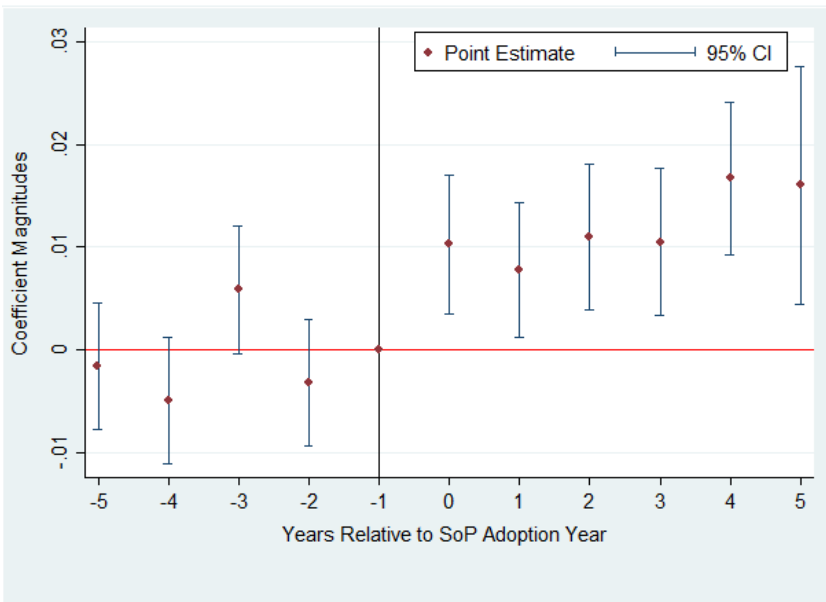

Figure 1a

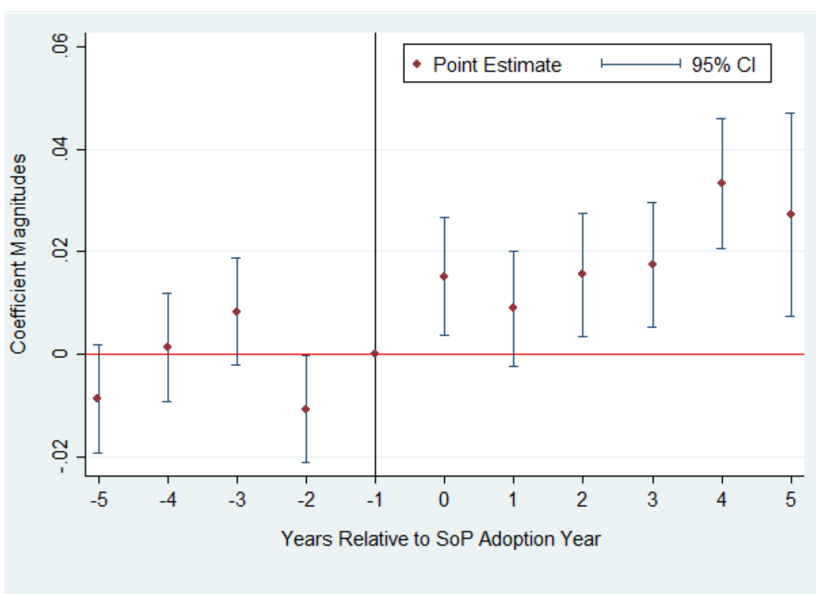

Figure 1b

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 which 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 which during the same period did not adopt such regime. 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 (see text for details). The information on SoP adoption is based on Thomas and Van der Elst (2015), Correa and Lel (2016) and web searches.

Country	SoP adoption year	Advisory vs Binding	# of firms	# of firm-years
Australia	2005	A	824	7316
Austria	-	-	33	270
Belgium	2012	A	51	540
Canada			649	6226
China	-	-	1154	7556
Denmark	2007	B	33	300
Finland	-	-	87	967
France	2014	A	386	3501
Germany			311	3424
Hong Kong	-	-	512	5115
India	-	-	394	3138
Italy	2012	A	172	1708
Malaysia	-	-	39	391
Netherlands	2004	B	97	1073
New	-	-	33	329
Norway	2008	B	130	1083
Portugal	2010	A	21	153
Singapore	-	-	62	649
South	2011	B	181	1467
Spain	2011	A	59	680
Sweden	2006	B	277	2678
Switzerland	2013	B	141	1191
Thailand	-	-	156	1600
United	2003	A(B)	1017	8890
USA	2011	A	3266	31447

Table 2. Descriptive Statistics

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

Panel A: Summary Statistics

Variable	MEAN	P5	P25	P50	P75	P95	STD	N
$PROFIT_{6m}\%$	0.008	-0.667	0.000	0.000	0.000	0.094	0.171	91,692
$PROFIT_{12m}\%$	0.014	-0.971	0.000	0.000	0.000	0.147	0.274	91,692
MV_{t-1}	2748.2	3.8	77.9	334.0	1399.2	13414.4	8290.4	91,692
BTM_{t-1}	0.733	0.056	0.320	0.542	0.892	2.010	0.691	91,692
$BHAR_{t-1}$	0.090	-0.876	-0.237	-0.008	0.268	1.136	0.602	91,692
$Coverage_{t-1}$	15.174	0.000	0.000	6.000	22.000	62.000	20.635	91,692
$Non\text{-}Synch_{\ t-1}$	0.809	0.294	0.684	0.880	0.988	1.000	0.203	91,692
$R\&D_{t-1}$	0.328	0.000	0.000	0.000	1.000	1.000	0.469	91,692
$INST_{t-1}$	0.275	0.000	0.007	0.133	0.465	0.940	0.318	91,692
$InsiderOwn_{t-1}$	0.030	0.000	0.000	0.002	0.010	0.188	0.091	91,692
$RETVOL_{t-1}$	0.032	0.009	0.018	0.026	0.037	0.072	0.025	91,692

Panel B: Correlation Table

	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	*MV*	*BTM*	*BHAR*	*Coverage*	*Non-Synch*	*R&D*	*INST*	*InsiderOwn*	*RETVOL*
$PROFIT_{6m}\%$	1.000										
$PROFIT_{12m}\%$	0.696	1.000									
MV	-0.015	-0.017	1.000								
BTM	0.030	0.035	-0.129	1.000							
BHAR	0.004	-0.003	-0.004	-0.239	1.000						
Coverage	-0.026	-0.031	0.562	-0.174	-0.040	1.000					
Non-Synch	0.027	0.032	-0.252	0.109	0.029	-0.411	1.000				
R&D	0.004	0.003	0.091	-0.100	-0.007	0.130	0.015	1.000			
INST	-0.024	-0.031	0.188	-0.163	0.005	0.429	-0.431	0.152	1.000		
InsiderOwn	0.048	0.067	-0.090	0.130	0.014	-0.152	0.133	-0.045	-0.193	1.000	
RETVOL	0.034	0.041	-0.099	0.171	0.026	-0.167	0.160	-0.001	-0.187	0.091	1.000

Table 3. The effect of SoP laws adoption on insider trading profitability

Table 3 reports the results of the effect of SoP laws adoption on (firm-level) insider trading profitability of Top 3 executives (CEO, CFO and COO) using the difference-in-differences specification in Equation (1). Insider trading profitability is computed using all open market buy and sale transactions. Firm and year fixed effects are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

	(1)	(2)	(3)	(4)
	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$
$SoP_{i,t}$	0.006***	0.010***	0.007***	0.011***
	(2.93)	(2.73)	(3.16)	(2.95)
$ln(MV_{i,t-1})$			0.003**	0.004
			(2.08)	(1.52)
$BTM_{i,t-1}$			0.008***	0.011***
			(3.20)	(2.78)
$BHAR_{i,t-1}$			0.004***	0.003
			(3.10)	(1.29)
$LnCoverage_{i,t-1}$			0.001	0.001
			(1.36)	(0.73)
$Non\text{-}Synch_{i,t-1}$			0.009**	0.020***
			(2.22)	(3.04)
$R\&D_{i,t-1}$			0.003	0.008*
			(1.11)	(1.67)
$INST_{i,t-1}$			0.007	0.011
			(0.94)	(1.02)
$InsiderOwn_{i,t-1}$			-0.001	0.113**
			(-0.02)	(2.15)
$RETVOL_{i,t-1}$			0.090**	0.145**
			(2.11)	(2.14)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.137	0.148	0.138	0.148
N. of Obs.	91,692	91,692	91,692	91,692

Table 4. Stacked DiD and DiD Decomposition

Table 4 Panel A reports the results of Goodman-Bacon decomposition of our treatment effects for $PROFIT_{6m}\%$ and $PROFIT_{6m}\%$ respectively in a balanced sample (the decomposition is estimated without covariates). Panel B reports the results of the effect of mandatory SoP adoption on insider trading profitability 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 (see discussion in Section 3.3). Firm and year fixed effects are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

Panel A: Goodman-Bacon DiD decomposition

Overall Estimation on Treatment Effect		$PROFIT_{6m}\%$	$PROFIT_{12m}\%$
$SoP_{i,t}$		0.007	0.012
Treatment Effect Decomposition			
	Total weights	Beta	Beta
Early Adopters vs. Late Adopters (Control)	0.075	0.001	0.000
Late Adopters vs. Early Adopters (Control)	0.046	-0.003	-0.010
Treated vs. Never Treated (Control)	0.562	0.010	0.017
Treated vs. Already Treated (Control)	0.317	0.007	0.008

Panel B: Stacked Events Approach

	(1)	(2)	(3)	(4)
	$PROFIT_{6m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{12m}\%$
$SoP_{i,t}$	0.009***	0.015***	0.010***	0.017***
	(3.14)	(3.10)	(3.47)	(3.33)
$ln(MV_{i,t-1})$			0.003	0.001
			(0.81)	(0.19)
$BTM_{i,t-1}$			0.012*	0.021*
			(1.82)	(1.75)
$BHAR_{i,t-1}$			-0.001	-0.001
			(-0.30)	(-0.31)
$LnCoverage_{i,t-1}$			0.001	0.001
			(0.57)	(0.19)
$Non\text{-}Synch_{i,t-1}$			0.016**	0.036***
			(2.31)	(2.90)
$R\&D_{i,t-1}$			0.004	0.012
			(0.75)	(1.24)
$INST_{i,t-1}$			0.003	0.015
			(0.17)	(0.58)
$InsiderOwn_{i,t-1}$			0.001	0.095
			(0.01)	(0.90)
$RETVOL_{i,t-1}$			-0.005	-0.033
			(-0.07)	(-0.27)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.150	0.161	0.151	0.162
N. of Obs.	171,548	171,548	171,548	171,548

Table 5: Cross-sectional heterogeneity

This table reports the results of cross-sectional tests of the baseline result with full set of controls from Table 3. In each model, firm-year observations under a SoP regime are partitioned using the indicator variable *Dum*, which is then interacted with the *SoP* indicator. In Panel A columns 1 and 2 (High Excess Pay), *Dum* is equal to one for firms with above country median excess pay in the pre-SoP period. In column 3 and 4 (Busy Boards), *Dum* is equal to one for firms with a percentage of busy directors (i.e., directors with two or more other seats) above the country median at the end of the year prior to SoP adoption. In columns 5 and 6 (Low Board Independence), *Dum* is equal to one for firms with a percentage of outside directors below the country median at the end of the year prior to SoP adoption. In columns 7 and 8 (Low Institutional Ownership), *Dum* is equal to one for firms below the country median at the end of the year prior to SoP adoption. See Section 3.4 for more details on the construction of the above variables. Firm and year fixed effects are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

	High Excess Pay		Busy Boards		Low Board Independence		Low Institutional Ownership	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$
$SoP_{i,t} \times Dum$	0.004***	0.004***	0.005**	0.010**	0.004*	0.010***	0.003	0.008
	(4.95)	(2.76)	(2.23)	(2.28)	(1.74)	(2.58)	(1.11)	(1.60)
$SoP_{i,t}$	0.006**	0.008**	0.004	0.006	0.005	0.005	0.005**	0.006
	(2.17)	(2.02)	(1.37)	(1.11)	(1.21)	(0.85)	(2.23)	(1.48)
$SoP_{i,t} + SoP_{i,t} \times Dum$	0.010***	0.012***	0.009***	0.016***	0.009***	0.015***	0.008***	0.014***
F Statistics	16.74	12.33	8.06	9.17	9.07	12.67	7.55	7.27
Controls	Included	Included	Included	Included	Included	Included	Included	Included
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.131	0.141	0.135	0.147	0.135	0.147	0.138	0.148
N. of Obs.	63,937	63,937	83,502	83,502	83,502	83,502	85,562	85,562

Table 6. Decomposing the change in insider trading profitability around Say on Pay laws

This table reports the results of the effect of SoP laws adoption on (firm-level) insider trading profitability using the difference-in-differences specification in Equation (1). In contrast to Table 3, insider trading profitability is computed using, alternatively, only buy transactions or only sale transactions. Firm and year fixed effects are included. Panel B and Panel C report the results of the effect of SoP laws adoption on insider trades' informativeness, size and frequency, respectively, for 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 ($BHAR_{12m}$). In column 3 the dependent variable is the dollar value of each insider trade (*VALUE%*), expressed as a percentage of the firm's market value of equity at the end of the fiscal year t-1. In columns 1 to 3 the unit of analysis is at the individual transaction (trade) level. Firm, year-month and insider fixed effects are included. In column 4 the dependent variable is *Ln(NUM)*, where *NUM* is the total number of insider buys (Panel B) or insider sales (Panel C) during the year for a given firm and the unit of analysis is at the firm-year level. Firm and year fixed effects are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively

Panel A: Decomposing the change in insider trading profitability: buy vs. sale transactions

	Buy Transactions		Sale Transactions	
	(1)	(2)	(3)	(4)
	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$	$PROFIT_{6m}\%$	$PROFIT_{12m}\%$
$SoP_{i,t}$	0.003***	0.003**	0.005***	0.007***
	(3.39)	(2.22)	(3.46)	(2.87)
$ln(MV_{i,t-1})$	-0.006***	-0.011***	0.009***	0.017***
	(-8.31)	(-10.32)	(9.42)	(10.98)
$BTM_{i,t-1}$	0.007***	0.010***	0.001	-0.001
	(4.52)	(4.59)	(0.47)	(-0.45)
$BHAR_{i,t-1}$	-0.001	-0.002***	0.006***	0.006***
	(-1.28)	(-2.71)	(5.79)	(4.00)
$LnCoverage_{i,t-1}$	-0.000	-0.001	0.001*	0.002*
	(-0.15)	(-1.02)	(1.71)	(1.73)
$Non\text{-}Synch_{i,t-1}$	0.007***	0.010***	-0.001	0.002
	(3.58)	(3.50)	(-0.19)	(0.43)
$RND_{i,t-1}$	0.002**	0.005***	0.002	0.003
	(2.27)	(2.62)	(0.93)	(0.88)
$INST_{i,t-1}$	-0.001	-0.003	0.007	0.011
	(-0.45)	(-0.75)	(1.40)	(1.42)
$InsiderOwn_{i,t-1}$	-0.021	-0.010	0.014	0.102***
	(-1.43)	(-0.46)	(0.61)	(2.89)
$RETVOL_{i,t-1}$	-0.022	-0.014	0.084***	0.106***
	(-1.38)	(-0.54)	(3.15)	(2.65)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.136	0.147	0.151	0.164
N. of Obs.	91,692	91,692	91,692	91,692

Panel B: Components of insider trading profitability: buy transactions only

	(1)	(2)	(3)	(4)
	$BHAR_{6m}$	$BHAR_{12m}$	*VALUE%*	*Ln(NUM)*
$SoP_{i,t}$	0.029	0.039	0.042	-0.004
	(1.25)	(0.95)	(1.33)	(-0.16)
$ln(MV_{i,t-1})$	-0.122***	-0.261***	-0.213***	0.002
	(-8.56)	(-8.24)	(-7.16)	(0.14)
$BTM_{i,t-1}$	0.034**	0.020	-0.040*	0.020*
	(2.34)	(0.71)	(-1.72)	(1.68)
$BHAR_{i,t-1}$	-0.020*	-0.073***	-0.021	0.001
	(-1.88)	(-2.71)	(-1.09)	(0.14)
$LnCoverage_{i,t-1}$	0.027	0.114	-0.019	-0.003
	(0.61)	(1.13)	(-0.34)	(-0.28)
$Non\text{-}Synch_{i,t-1}$	-0.015	-0.057***	0.018	0.124***
	(-1.55)	(-2.70)	(1.25)	(2.90)
$R\&D_{i,t-1}$	0.032	0.078	-0.041	0.030
	(1.31)	(1.28)	(-1.03)	(1.24)
$INST_{i,t-1}$	-0.107	-0.194	0.281***	0.071
	(-1.47)	(-1.63)	(2.64)	(1.18)
$InsiderOwn_{i,t-1}$	0.089	0.267	-1.942***	0.088
	(0.66)	(0.96)	(-5.48)	(0.84)
$RETVOL_{i,t-1}$	0.084	0.535	0.068	0.005
	(0.24)	(1.01)	(0.13)	(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	0.471	0.547	0.326	0.503
N. of Obs.	63,227	63,227	63,227	18,190

Panel C: Components of insider trading profitability: sale transactions only

	(1)	(2)	(3)	(4)
	$BHAR_{6m}$	*$BHAR_{12m}$*	*VALUE%*	*Ln(NUM)*
$SoP_{i,t}$	0.019**	0.011	0.035***	0.273***
	(2.35)	(0.76)	(3.68)	(14.33)
$ln(MV_{i,t-1})$	0.146***	0.275***	-0.058***	0.039***
	(20.57)	(24.59)	(-6.18)	(3.35)
$BTM_{i,t-1}$	-0.025*	-0.034*	0.024	-0.029**
	(-1.80)	(-1.66)	(1.25)	(-2.01)
$BHAR_{i,t-1}$	0.003	0.016**	-0.005	0.068***
	(0.73)	(2.13)	(-0.97)	(8.46)
$LnCoverage_{i,t-1}$	0.004	0.013	-0.002	-0.011
	(0.32)	(0.61)	(-0.17)	(-1.42)
$Non\text{-}Synch_{i,t-1}$	0.018***	0.019***	0.002	-0.061**
	(3.66)	(2.91)	(0.41)	(-2.16)
$R\&D_{i,t-1}$	-0.026**	-0.020	-0.018	-0.002
	(-2.09)	(-1.01)	(-0.66)	(-0.08)
$INST_{i,t-1}$	0.031	-0.004	-0.051**	0.084
	(1.06)	(-0.09)	(-2.21)	(1.45)
$InsiderOwn_{i,t-1}$	-0.174	-0.032	2.826***	0.001
	(-1.52)	(-0.26)	(6.97)	(0.01)
$RETVOL_{i,t-1}$	0.517**	1.088***	0.927	-0.893**
	(2.11)	(2.69)	(1.41)	(-2.05)
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	0.355	0.450	0.639	0.515
N. of Obs.	160,405	160,405	160,405	31,672

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 insider buy and sell transactions (Panel A), for insider buy transactions (Panel B) and insider sells transactions (Panel C), respectively. 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 all insider trades taking place during the one-month period prior to the annual earnings announcement date – a proxy for information-sensitive window. In column 3 (4), for each firm-year observation, $FracVALUE_{i,t}(FracNUM_{i,t})$ is re-defined 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 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 firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

Panel A: Buy & Sell transactions

	Annual EA		Quarterly EA	
	(1)	(2)	(3)	(4)
	FracVALUE	*FracNUM*	*FracVALUE*	*FracNUM*
$SoP_{i,t}$	0.019***	0.024***	0.043***	0.049***
	(3.55)	(4.84)	(3.75)	(4.48)
$ln(MV_{i,t-1})$	0.006*	0.005	0.003	0.004
	(1.79)	(1.59)	(0.47)	(0.68)
$BTM_{i,t-1}$	0.003	0.006	-0.011	-0.007
	(0.74)	(1.36)	(-1.21)	(-0.79)
$BHAR_{i,t-1}$	0.001	0.001	-0.003	-0.002
	(0.56)	(0.40)	(-0.65)	(-0.39)
$LnCoverage_{i,t-1}$	0.001	0.001	-0.010*	-0.009*
	(0.38)	(0.40)	(-1.89)	(-1.78)
$Non\text{-}Synch_{i,t-1}$	0.004	0.004	0.018	0.014
	(0.48)	(0.48)	(1.25)	(1.01)
$R\&D_{i,t-1}$	0.023***	0.019***	0.033**	0.042***
	(3.14)	(2.88)	(2.52)	(3.49)
$INST_{i,t-1}$	0.002	0.005	0.019	0.025
	(0.14)	(0.32)	(0.78)	(1.09)
$InsiderOwn_{i,t-1}$	0.045	0.043	0.001	0.025
	(1.38)	(1.44)	(0.01)	(0.27)
$RETVOL_{i,t-1}$	-0.077	-0.101	0.153	0.244
	(-0.67)	(-0.92)	(0.59)	(1.02)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.114	0.146	0.143	0.182
N. of Obs.	37,347	37,347	24,613	24,613

Panel B: Only buy transactions

	Annual EA		Quarterly EA	
	(1)	(2)	(3)	(4)
	FracVALUE	*FracNUM*	*FracVALUE*	*FracNUM*
$SoP_{i,t}$	0.004	0.005	0.009	0.012**
	(1.14)	(1.58)	(1.54)	(2.12)
$ln(MV_{i,t-1})$	-0.004*	-0.003	0.000	-0.001
	(-1.70)	(-1.48)	(0.01)	(-0.35)
$BTM_{i,t-1}$	0.006*	0.007**	0.006	0.007*
	(1.84)	(2.30)	(1.59)	(1.86)
$BHAR_{i,t-1}$	-0.001	-0.000	-0.000	0.001
	(-0.41)	(-0.23)	(-0.18)	(0.35)
$LnCoverage_{i,t-1}$	0.003*	0.003*	-0.001	-0.000
	(1.78)	(1.68)	(-0.44)	(-0.11)
$Non\text{-}Synch_{i,t-1}$	0.001	0.001	-0.003	-0.003
	(0.14)	(0.30)	(-0.70)	(-0.75)
$R\&D_{i,t-1}$	0.017***	0.016***	0.021***	0.021***
	(3.51)	(3.51)	(3.79)	(3.92)
$INST_{i,t-1}$	-0.018**	-0.018**	-0.011	-0.011
	(-2.13)	(-2.18)	(-1.62)	(-1.57)
$InsiderOwn_{i,t-1}$	0.015	0.014	-0.011	-0.015
	(0.59)	(0.62)	(-0.32)	(-0.47)
$RETVOL_{i,t-1}$	-0.081	-0.067	-0.043	-0.081
	(-0.97)	(-0.81)	(-0.45)	(-0.91)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.144	0.146	0.179	0.184
N. of Obs.	37,347	37,347	24,613	24,613

Panel C: Only sell transactions

	Annual EA		Quarterly EA	
	(1)	(2)	(3)	(4)
	FracVALUE	*FracNUM*	*FracVALUE*	*FracNUM*
$SoP_{i,t}$	0.015***	0.019***	0.022***	0.025***
	(3.55)	(4.87)	(3.61)	(4.19)
$ln(MV_{i,t-1})$	0.010***	0.009***	0.010**	0.011**
	(3.55)	(3.22)	(2.35)	(2.55)
$BTM_{i,t-1}$	-0.003	-0.002	-0.008	-0.003
	(-0.96)	(-0.70)	(-1.38)	(-0.50)
$BHAR_{i,t-1}$	0.002	0.001	0.000	-0.000
	(1.02)	(0.70)	(0.06)	(-0.01)
$LnCoverage_{i,t-1}$	-0.002	-0.002	-0.001	-0.001
	(-0.93)	(-0.83)	(-0.45)	(-0.48)
$Non\text{-}Synch_{i,t-1}$	0.004	0.003	0.006	0.005
	(0.47)	(0.38)	(0.59)	(0.50)
$R\&D_{i,t-1}$	0.006	0.003	0.007	0.002
	(1.09)	(0.58)	(0.92)	(0.33)
$INST_{i,t-1}$	0.020	0.023*	0.018	0.021
	(1.45)	(1.69)	(1.12)	(1.30)
$InsiderOwn_{i,t-1}$	0.030	0.029	0.026	0.046
	(1.37)	(1.38)	(0.39)	(0.76)
$RETVOL_{i,t-1}$	0.005	-0.035	0.172	0.154
	(0.06)	(-0.47)	(1.10)	(1.03)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj.R-squared	0.124	0.176	0.143	0.182
N. of Obs.	37,347	37,347	24,613	24,613

Internet Appendix

Table IA.1. Robustness Tests

This table presents a number of robustness tests based on the baseline results in Table 3 (reported in the first row for comparison). In particular, in Panel A, columns 1-2 replicate, respectively, columns 3-4 from Table 3 (i.e., the models with the full set of controls). The robustness test in each row is described in the left column and detailed in Section 3.2.1. For ease of exposition, only the coefficient on *SoP* and the sample size are reported (control variables, year and firm fixed effects are included but not reported; adjusted R-square and T-statistics based on robust standard errors clustered at firm-level are not reported).
Panel B reports the sensitivity of the estimate of $SoP_{i,}$ from Table 3 to the exclusion of a given country. For ease of exposition, only the coefficient of *SoP* is reported (control variables, year and firm fixed effects are included but not reported; adjusted R-square and T-statistics based on robust standard errors clustered at firm-level are not reported). In both panels *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

Panel A

			(1)	(2)
			$PROFIT_{6m}\%$	$PROFIT_{12m}\%$
	Baseline Model (Table 3)	*SoP*	0.007***	0.011***
		N. of Obs.	91,692	91,692
(1)	*Exclude SoP adoption year*	*SoP*	0.007***	0.011***
		N. of Obs.	87,596	87,596
(2)	*Include only CEO trades in measuring PROFIT*	*SoP*	0.007***	0.012***
		N. of Obs.	86,272	86,272
(3)	*Exclude US firms*	*SoP*	0.007***	0.007*
		N. of Obs.	60,245	60,245
(4)	*Only include countries eventually mandating SoP*	*SoP*	0.006***	0.006***
		N. of Obs.	62,027	62,027
(5)	*Exclude "voluntary" SoP firm-year observations from control sample*	*SoP*	0.006**	0.010**
		N. of Obs.	81,213	81,213
(6)	*Include only firm-year obs. with insider trading*	*SoP*	0.023***	0.037***
		N. of Obs.	44,686	44,686
(7a)	*Cluster by firm and year*	*SoP*	0.007**	0.011*
(7b))	*Cluster by country*	*SoP*	0.007**	0.011**
(7c)	*Cluster by country and year*	*SoP*	0.007**	0.011*
		N. of Obs.	91,692	91,692

Panel B

		(1)	(2)
Alternative Sample		*PROFIT6m%*	*PROFIT12m%*
Exclude Australia	*SoP*	0.006**	0.008**
Exclude Austria	*SoP*	0.007***	0.011***
Exclude Belgium	*SoP*	0.007***	0.011***
Exclude Canada	*SoP*	0.007***	0.010***
Exclude China	*SoP*	0.008***	0.012***
Exclude Denmark	*SoP*	0.007***	0.011***
Exclude Finland	*SoP*	0.007***	0.011***
Exclude France	*SoP*	0.008***	0.013***
Exclude Germany	*SoP*	0.006***	0.010***
Exclude Hong Kong	*SoP*	0.006***	0.008**
Exclude India	*SoP*	0.007***	0.011***
Exclude Italy	*SoP*	0.006***	0.010***
Exclude Malaysia	*SoP*	0.007***	0.011***
Exclude Netherlands	*SoP*	0.007***	0.011***
Exclude New Zealand	*SoP*	0.007***	0.011***
Exclude Norway	*SoP*	0.007***	0.011***
Exclude Portugal	*SoP*	0.007***	0.010***
Exclude Singapore	*SoP*	0.007***	0.011***
Exclude South Africa	*SoP*	0.006***	0.010***
Exclude Spain	*SoP*	0.007***	0.010***
Exclude Sweden	*SoP*	0.008***	0.012***
Exclude Switzerland	*SoP*	0.007***	0.010***
Exclude Thailand	*SoP*	0.007**	0.011*
Exclude United Kingdom	*SoP*	0.006**	0.010***
Exclude USA	*SoP*	0.007***	0.007*

Table IA.2. Placebo Test

This table presents the results of a placebo-type test using independent (i.e., non-executive) directors as placebo treatment group. We regress the aggregated insider trading profitability on $SoP_{i,t}$ using our main model specification (Equation (1)). Insider trading profitability is measured using independent directors in columns 1-4 and top 3 executives (similar to Table 3) in columns 5-6. In columns 1-2 the sample includes all firms with at least one trade from an independent director during our sample period. In columns 3-6 the sample includes all firms at least one trade by both executives and non-independent directors during our sample period. Both firm and year- fixed effect are included. T-statistics, reported in parentheses, are based on robust standard errors clustered at both the country and firm- level. *, **, and *** indicate significance at the 10%, 5%, and 1% two-tailed levels, respectively.

	Non-executive Directors		Non-executive Directors		Top 3	
	(1)	(2)	(3)	(4)	(5)	(6)
	PROFIT6m	*PROFIT12m*	*PROFIT6m*	*PROFIT12m*	*PROFIT6m*	*PROFIT12m*
$SoP_{i,t}$	0.002*	0.003	0.002*	0.003	0.012***	0.014**
	(1.72)	(1.26)	(1.67)	(1.28)	(2.61)	(1.97)
$ln(MV_{i,t-1})$	-0.002**	-0.003*	-0.001	-0.002	0.001	-0.004
	(-1.99)	(-1.68)	(-1.38)	(-0.95)	(0.31)	(-0.65)
$BTM_{i,t-1}$	0.001	0.003	0.002*	0.005**	0.011**	0.022***
	(1.25)	(1.53)	(1.82)	(2.27)	(2.18)	(2.60)
$BHAR_{i,t-1}$	0.000	0.000	0.001	0.001	0.006**	0.004
	(0.56)	(0.19)	(0.64)	(0.43)	(2.00)	(0.79)
LnCoverage	0.001	0.003***	0.001**	0.002**	0.002	0.002
	(1.56)	(3.17)	(2.07)	(2.54)	(1.14)	(0.55)
$Non\text{-}Synch_{i,t-1}$	0.003	0.005	0.001	0.001	0.017*	0.028*
	(1.33)	(1.17)	(0.43)	(0.13)	(1.69)	(1.80)
$R\&D_{i,t-1}$	0.001	0.002	0.001	0.002	0.008	0.016*
	(0.66)	(0.63)	(0.65)	(0.76)	(1.45)	(1.86)
$INST_{i,t-1}$	0.009**	0.013	0.011**	0.013	0.023	0.054**
	(1.96)	(1.61)	(2.15)	(1.55)	(1.36)	(2.19)
$InsiderOwn_{i,t-}$	0.006	0.010	0.005	0.009	0.016	0.179*
	(0.42)	(0.46)	(0.36)	(0.44)	(0.23)	(1.90)
$RETVOL_{i,t-1}$	0.018	0.025	0.012	0.010	0.062	0.038
	(1.27)	(1.14)	(0.82)	(0.42)	(1.29)	(0.49)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj.R	0.135	0.143	0.129	0.136	0.126	0.138
N. of Obs.	47845	47845	41598	41598	41598	41598