Insider Trading Legislation and Acquisition Announcements: Do Laws Matter?[*]

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This draft: February 1, 2008

Abstract

In this paper we investigate how the enactment and enforcement of insider trading restrictions affect the way in which information about acquisitions is released before the actual acquisition announcement. We analyze a sample with almost 19,000 acquisition announcements from 48 countries. We find that insider trading legislation strongly affects the information revealed to the market in the runup phase before the announcement whereas the impact of subsequent enforcement actions by regulators is much weaker and insignificant in some tests. We conclude that market participants rationally anticipate law enforcement.

JEL classifications: G14, G34, K22, K42

Keywords: Insider Trading, Mergers and Acquisitions, Securities Law, Law and Finance

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[*] We are grateful to Laura Beny, Jochen Bigus, Arturo Bris, Ingolf Dittmann, Lars-Hendrik Röller, Karin Thorburn, David Yermack, two anonymous referees, and to seminar participants at Erasmus University, the University of Exeter, the Hong Kong University of Science and Technology, Humboldt University, Paris-Dauphine, the German Finance Association meetings in Augsburg, the SFB-TR conference in Mannheim, and the European Finance Association meetings in Zürich for comments and discussions. We also wish to express our gratitude to the Rudolf von Bennigsen-Foerder Foundation, the Collaborative Research Center 649 "Economic Risk" in Berlin, the SFB 504 "Rationality Concepts, Decision Making and Economic Modeling" in Mannheim, and the SFB/TR 15 “Governance and the Efficiency of Economic Systems” for financial support.

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Abstract

In this paper we investigate how the enactment and enforcement of insider trading restrictions affect the way in which information about acquisitions is released before the actual acquisition announcement. We analyze a sample with almost 19,000 acquisition announcements from 48 countries. We find that insider trading legislation strongly affects the information revealed to the market in the runup phase before the announcement whereas the impact of subsequent enforcement actions by regulators is much weaker and insignificant in some tests. We conclude that market participants rationally anticipate law enforcement.

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1. Introduction

This paper investigates how insider trading laws and the enforcement of insider trading laws affect the incorporation of information into stock prices before acquisition announcements. A recent literature emphasizes the difference between law enactment and law enforcement, especially with respect to insider trading laws. Bhattacharya and Daouk (2002), (2004) document that the enforcement of insider trading laws has a strong impact on the cost of capital, whereas passing insider trading laws without enforcing them does not have this effect. Bushman, Piotroski and Smith (2005) make the same observation with respect to the impact of analyst followings in a study of 100 countries. Bris (2005) documents that the enforcement of insider trading legislation increases the profitability of trades by corporate insiders and Fernandes and Ferreira (2007) show that enforcing insider trading laws improves price informativeness. Unlike these studies we find that enacting insider trading laws has a significant impact on the variance of announcement returns and on the way pre-announcement runup returns predict announcement returns for acquisitions. We conclude that enacting insider trading laws matters and suggest that market participants rationally anticipate future enforcement.

Acquisition announcements provide a natural setting in which to investigate the effectiveness of insider trading laws and their enforcement. The fact that information about future acquisitions is revealed to the market before the actual announcement has been documented before, and researchers who contribute to this literature routinely refer to insider trading as a likely channel whereby information is transmitted to market participants.[1]

Insider trading laws and their enforcement change the informational environment of acquisitions in two ways. Firstly, they have an impact on the amount of legal versus illegal insider trading. Secondly, they have an impact on public disclosure before the event, as market participants comply with “disclose or abstain” rules and other mandatory disclosure requirements. The study of the impact of insider trading laws poses a methodological problem because we can neither observe all the information available to market participants, nor distinguish between different types of information received by market participants before an acquisition announcement. In most cases we also have no way of distinguishing legal from illegal insider trading. We therefore assess the impact of insider trading laws and their enforcement by

[1] The earliest analysis to the best of our knowledge is Keown and Pinkerton (1981). Prior research also includes Givoly and Palmon (1985) and Jarrell and Poulsen (1989). The paper closest to ours in terms of methodology is Schwert (1996).

analyzing the means, medians, and variances of announcement returns and pre-announcement runup returns, and, in particular, the correlation between these two variables. (In the following we refer to pre-announcement runup returns simply as runups.) In line with previous studies on public targets, we show that this correlation is higher if insider trading is prohibited than if insider trading is not prohibited (see also Schwert, 1996).

For our empirical investigation we collect data on 18,752 acquisitions from 48 countries. Our data ranges from 1990 to 2003 and most countries – especially those for which we have data – either had insider trading laws already before 1990 or passed such laws during the sample period. Our study focuses on the acquisition of subsidiaries and analyzes the returns of parents of these subsidiaries, which has several advantages in the context of our analysis. Firstly, mandatory disclosure requirements for public targets generate significant information releases during the runup phase. Accordingly, our focus on subsidiary acquisitions allows us to analyze trading by corporate insiders without having to distinguish public disclosures from information revealed through insider trading. Second, our main analysis concerns the predictability of announcement returns from runups and cannot be performed for public targets, as we do not have enough observations for acquisitions that were not subject to any insider trading laws. Finally, the profitability of insider trading in public targets has already been analyzed in the literature (Bris, 2005).

Our main result is that enacting insider trading laws has a very strong influence. While the impact of insider trading laws is highly significant, the additional impact of subsequent enforcement actions is small on average and insignificant in several tests. From our simple regressions we conclude that market participants anticipate future enforcement actions by regulatory authorities. Our findings corroborate the conclusion of Beny (2001, 2004) that the enactment of insider trading laws matters. We therefore question the focus of the existing literature on the enforcement of insider trading laws.

Other researchers have investigated insider trading in acquisition announcements before. To the best of our knowledge the earliest study is Keown and Pinkerton (1981), who document the information leakage of merger announcements using event study methodology. Pound and Zeckhauser (1990) analyze takeover rumors published in the "Heard on the Street" column in the Wall Street Journal and find that these rumors predict subsequent takeovers well and that they are preceded by price and volume changes. Arshadi and Eyssel (1991) find that the Insider Trading Sanctions Act in the United States had a significant impact on deterring insider trading before tender offers. Our methodology is closest to that of Schwert (1996), who investigates U.S. data and documents how the predictability of announcement returns by runups reflects (legal) insider trading as well as public information releases.

Bris (2005) is the only study so far that relates insider trading laws to acquisition announcements in a large international cross-section. He focuses on the profitability of insider trading and documents that insider trading profits increase after enforcing insider trading laws. His methodology is therefore different from ours and we comment on this difference below. Other international studies of insider trading include Durnev and Nain (2004), who analyze the relationship between insider trading, ownership, and earnings opacity. Beny (2006) shows on a firm-level dataset that companies in common law countries have higher stock market valuations if insider trading regulation is more stringent. Finally, Fernandes and Ferreira (2007) measure the impact of insider trading enforcement on price informativeness, but they do not address the impact of law enactment.

The paper is organized as follows. The following Section 2 develops our hypotheses and the methodology employed in this paper. Section 3 describes the data. Section 4 presents the empirical analysis of the paper and Section 5 concludes.

2. Hypotheses and Methodology

We consider acquisitions where a company buys a subsidiary from another company, so that two parties are naturally insiders to this transaction, namely the buyer and the seller of the company. We distinguish between two different phases of an acquisition:

- The runup phase before the official announcement of the acquisition. During this phase the acquirer and the seller negotiate the terms of the acquisition. For our empirical implementation we have to fix a specific date $t = \tau_0$ before the announcement, which marks the beginning of the runup phase. The runup phase ends on date $t = \tau_1$.
- The disclosure of the acquisition, where some or all of the information regarding the acquisition is publicly disclosed. For empirical purposes we have to fix a window, which begins at date $t = \tau_1$ and ends on some later date $t = \tau_2$.

We then analyze the means, medians, variances, and covariances of these two return variables to make inferences about insider trading. We shall refer to the sum of the runups and the announcement return as the total return. For our empirical implementation we use cumulative abnormal returns instead of raw returns and denote the cumulative abnormal return for the runup phase by $\mathrm{CAR}(\tau_0, \tau_1)$ and that for the announcement phase by $\mathrm{CAR}(\tau_1, \tau_2)$.

Suppose that the value of the acquisition to the target (i.e., to the seller of the subsidiary) results in a total share price increase of *D* per share, where *D* is a random variable. These gains have been widely documented in the literature, and the source of these gains is immaterial for our analysis.[2]

If there is insider trading, then we expect that more information about *D* is revealed during or even before the runup phase, and that less information is revealed at the public announcement of the acquisition itself.[3] Note that this does not preclude that *some* public information is released before the announcement. Based on this notion we develop three groups of tests.

(a) **Tests on means and medians.** We expect that with insider trading the price jump in response to the announcement, which we measure by $CAR(\tau_1, \tau_2)$, reflects a smaller part of the total share price increase *D* than without insider trading. Similarly, we expect that with insider trading a larger proportion of *D* is revealed during or even before the runup phase, so that $\mathrm{CAR}(\tau_0, \tau_2)$, the total return over the runup phase and the announcement phase combined, is larger with insider trading prohibitions than without such prohibitions.

(b) **Tests on variances.** If less information is revealed at the announcement than before, then we expect the variance of announcement returns to be lower. Hence, we expect a higher variance of announcement returns $\mathrm{CAR}(\tau_1, \tau_2)$ with effective insider trading prohibitions. If insider trading also reveals more information before the runup phase, then we also expect that the variance of total returns $\mathrm{CAR}(\tau_0, \tau_2)$ is higher with effective insider trading prohibitions than without such prohibitions.[4]

(c) **Tests on covariances.** Schwert (1996) shows that differences in the incidence of insider trading have a significant impact on the slope coefficient in the following regression:

$$CAR_i(\tau_1, \tau_2) = \alpha + \beta CAR_i(\tau_0, \tau_1) + u_i, \tag{1}$$

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[2] Recent contributions to the literature on divestiture gains include Dittmar and Shivdasani (2003) and Vijh (2002). Bruner (2004), chapter 6 summarizes the literature and provides an extensive overview of the results.

[3] From a conceptual point of view it would be preferable to define the runup phase so that there is no insider trading before the beginning of the runup phase. However, this cannot be established with any certainty and would most likely result in very long runup windows and is therefore not practical. We therefore have most likely also some insider trading before the beginning of our runup window.

[4] This test is somewhat related to the argument by Fernandes and Ferreira (2007), who measure price informativeness outside an acquisition context.

where i denotes a specific transaction. He applies his results to public targets, but the economic argument is very similar. The main difference is that the mean and the variance of D are both much larger for publicly traded targets than for acquisitions of subsidiaries because subsidiaries represent only a fraction of the value of the firm. This aspect should have no implications for the covariance of announcement returns and runups and therefore for the slope coefficient of regression (1).

Schwert shows that groups of transactions where illegal insider trading was detected have a negative β-coefficient in (1), so that a higher runup is compensated by a lower announcement return.[5] To see why this may be the case consider a simple setup where insider trading reveals a pending acquisition, so that the market discovers that there may be an acquisition with some probability q, but the market does not learn anything about the value increase D for the target, which remains uncertain. Then the target's stock price increases by $q\Delta$, where q is a random variable. As a consequence, the increase in price at the announcement is only $(1-q)D$.[6] Then the covariance $Cov(q\Delta,(1-q)D)$ and therefore the slope coefficient in (1) are both negative, and Schwert refers to this argument as the substitution hypothesis. His empirical results show that the runup coefficient β in (1) is positive absent insider trading or when news releases publicly reveal details of the bid before the first official announcement. This may be the case for a number of reasons, for example, because the pre-bid announcement increases the visibility of the target and invites competing bids. Ultimately, this increases the price and therefore the value to the seller, so that a higher runup in response to the news before the announcement is also followed by a higher price or "markup" the bidder has to pay for the target.

With this background we can therefore interpret the β-coefficient of regression (1) as an indirect measure of non-public insider trading. Note that this measure does not distinguish legal insider trading (when no laws exist), from illegal trading (when laws are broken). Our methodology is then to compare the β-coefficients across subsamples that are subject to different regulations of insider trading. This methodology does not allow us to make inferences about exactly how insider trading laws work. Studies on U.S. data have typically correlated the incidence of legal insider trading with stock market activity directly, whereas our approach does

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5 Schwert uses the total return CAR(τ_0,τ_2) as a dependent variable, so his coefficient b relates to our coefficient β as $b=1+\beta$. He measures b=0.667 with insider trading (so β=-0.333) and b=1.087 without insider trading (so β=0.087).

6 We assume that D is known at the announcement date $t \in (\tau_1, \tau_2)$, but that it does not become known through insider trading before the announcement.

not rely on such data. Instead we infer changes of the informational environment from changes in the runup coefficient. Note also that changes in means and variances (our first types of tests in a) and b) above) measure the total amount of information that becomes available to the market, whereas the regression test from c) refers to the origin of this information, and is not related to the total quantity of information that becomes available. Insider trading prohibitions and their enforcement may have a different impact on these two dimensions of information that is incorporated into prices.

A number of papers have argued that insider trading legislation itself does not matter unless it is enforced (Bhattacharya and Daouk, 2002, 2004; Bushman, Piotroski, and Smith, 2005). On this basis we hypothesize that insider trading legislation has no impact on the information revealed through price runups before the announcement so that insider trading legislation that has not yet been followed by enforcement actions has no impact on the amount of information revealed during the runup phase. We refer to such a regime as a "Law Only" regime and compare it to a "No Law" regime, where insider trading is completely unregulated. Based on this literature and our testing methodology as described in (a) to (c) above we therefore formulate the following hypothesis:

Hypothesis 1 (Ineffectiveness of enactment): *The enactment of insider trading laws without subsequent enforcement actions has no impact on the information revealed before and during the runup phase. Accordingly, for all acquisitions subject to a Law Only regime we expect to observe:*

(a) The mean announcement return and the mean total return are identical for subsamples of acquisitions subject to the No Law regime and those subject to the Law Only regime.

(b) The variance of announcement returns and the variance of total returns are identical for subsamples of acquisitions subject to the No Law regime and those subject to the Law Only regime.

(c) The regression coefficient in (1) *is the same for subsamples of acquisitions subject to the No Law regime and those subject to the Law Only regime.*

With effective insider trading prohibitions, we expect that, contrary to (a), mean announcement returns and mean total returns increase. However, we cannot make good predictions about runups, because we do not have a well-founded prior about how insiders spread their trades (legal or illegal) over time, so we do not know how much of this would be captured by the runup window in our analysis. With effective insider trading prohibitions we expect the total amount of information that is incorporated at the announcement to increase, and the amount of public information releases to increase as well, which should lead to an increase in the runup coefficient.

Based on the literature cited above we would expect that enforcement actions by regulators against insider trading do have an impact and that they reduce the amount of information revealed before and during the runup phase. We refer to a regulatory insider trading regime where such enforcement actions have occurred in the past as the "Enforcement" regime. We therefore expect that we can reject the following hypothesis:

Hypothesis 2 (Effectiveness of enforcement): *The enforcement of insider trading laws has no impact on the information revealed before and during the runup phase. For all acquisitions subject to the Enforcement regime we expect to observe:*

(a) The mean announcement return and the mean total return are identical for subsamples of acquisitions subject to the Enforcement regime compared to those subject to the Law Only regime.

(b) The variance of announcement returns and the variance of total returns are identical for subsamples of acquisitions subject to the Enforcement regime and those subject to the Law Only regime.

(c) The regression coefficient in (1) is identical for subsamples of acquisitions subject to the Law Only regime and those subject to the Enforcement regime.

Our testing methodology relies on two working hyoptheses. Firstly, all acquisitions in our sample are drawn from the same distribution, so that D is distributed with mean Δ and variance σ^2. In our empirical analysis we control for observable characteristics of acquisitions as well as for unobserved heterogeneity using fixed effects, so we really assume that the distributions of returns after subtracting these effecs are the same.[7] Secondly, we have to rely on stock prices even for regimes with no effective insider trading prohibitions, where markets are less liquid and prices therefore less reliable. We address this issue by eliminating observations that are likely to be particularly unreliable.

3. Data

3.1 Sources and sample selection

Insider Trading variables. Data on the existence and enforcement of insider trading laws is obtained from Bhattacharya and Daouk (2002), who first collected this data for 103 countries.

[7] For non-parametric tests on medians, we can allow for differences in the variances of these distributions.

They distinguish three distinct legal situations with respect to insider trading that correspond to the three regimes described above. Out of the 48 countries considered in this study, 47 countries have insider trading laws or pass such laws during the 1990s, the exception being Zimbabwe. Bhattacharya and Daouk (2002) identify the year in which the first prosecutions based on these laws took place. They use these years where the authorities prosecuted somebody for insider trading offences for the first time as an indicator of enforcement. Here enforcement does not necessarily imply that somebody was found guilty and punished. The quality of this variable as a proxy variable is potentially limited as enforcement is often a staged process rather than a one-off event. However, despite this limitation Bhattacharya and Daouk (2002), (2004), Bushman, Piotroski, and Smith (2005), and Fernandes and Ferreira (2007) find that this variable has substantial explanatory power. Insider trading laws are enforced at least for some years in 32 countries in our sample. The US is the first country that established insider trading laws in 1934 and also the first country to enforce insider trading laws in 1961. Venezuela most recently established insider trading laws in 1998. Similarly, India and Spain most recently enforced insider trading laws in 1998.

Beny (2004) constructs an index of the stringency of insider trading laws based on written law, which she establishes from published sources (Gaillard, 1992, and Stamp and Welsh, 1996). Her index of insider trading quality adds 1 for each of the following provisions: (1) tippees are not allowed to trade on inside information, (2) insiders are not allowed to tip outsiders, (3) penalties for illegal insider trading are proportional to insider trading profits, (4) investors have a private right of action, and (5) violating insider trading prohibitions is a criminal offence. This index is available for 35 countries. Only three countries (Canada, South Korea, and the United States) achieve the top score of 5 on this index, and only two countries (Mexico and Norway) have an index value of one. Countries without insider trading laws are not scored.

Country-specific variables. Data on judicial efficiency and the rule of law comes from La Porta et. al. (1998) with higher scores indicating better laws. Judicial efficiency indicates how well the laws in place are carried out, while rule of law assesses the quality of the local judicial system. Data on GDP per capita, the number of listed companies per country, and the market capitalization of listed companies is obtained from the World Development Indicators (WDI) database, a comprehensive database offered by the World Bank covering 593 indicators and 208 countries.

M&A Transactions. The data on M&A transactions ranges from 1990 to 2003 and comes from Thomson Financial's SDC Platinum 'Worldwide Mergers & Acquisitions' database. For M&A transactions, we require that transactions have been completed and that acquirers and targets must be unrelated and both come from one of the given 48 countries for which the insider trading and country-specific data is available. We exclude leveraged buyouts (LBOs), spin-offs, recapitalizations, repurchases, minority stake purchases, and acquisitions of remaining interest. We need to match transactions to stock market data and include only transactions in which the target is identified by a SEDOL number, a code used by the London Stock Exchange to identify stocks. We delete transactions where target and acquirer are identified by the same SEDOL. If there are multiple announcements associated with the same transaction, then we keep only the first announcement. For reasons mentioned in the introduction we focus on divestitures of subsidiaries only.[8] In addition, we identify for each transaction whether it was rumored before the official announcement by using a rumor-dummy provided within Thomson Financial's SDC Platinum 'Worldwide Mergers & Acquisitions' database. Imposing these criteria leads to a sample of 29,522 transactions.

We delete 104 transactions where the insider trading regime of the target country changes during the 40 trading days before the announcement, which reduces the sample to 29,418. This is necessary in order to be able to assign transactions unambiguously to insider trading regimes. Target firms' stock prices are then obtained from Thomson Financial's Datastream. We lose transactions that cannot be matched using SEDOL numbers, or where Datastream does not report valid stock price data. We also disregard stocks where the daily return equals zero in more than 25% of all days counting from 240 days before to 40 days after the M&A announcement (thin trading). This should also eliminate observations where stock prices are particularly unreliable. We only consider transactions where we can calculate all abnormal returns from 40 days before until 3 days after the M&A announcement. Return data comes from parents of subsidiaries. Our final dataset contains 18,752 observations for 48 countries. All variable definitions and data sources are summarized in Table 1.

[Insert Table 1 about here]

8 We perform the analysis for public targets and find similar results to Bris (2005).

3.2 Descriptive statistics

Events. The dataset is highly skewed towards developed countries, and English-speaking countries in particular. The US alone accounts for 45% of the observations and the UK and Canada account for another 11% and 4%, respectively. In these countries insider trading restrictions were enforced already before the beginning of our sample period. The effective dataset on which we can test changes in insider trading regimes is therefore much smaller. We have 3,014 observations (16% of the sample) from 6 countries (Australia, Denmark, Germany, Hong Kong, Italy, and Spain) which have observations from all three insider trading regimes and 1,605 observations (8.5% of the sample) from another 15 countries with observations from two different insider trading regimes. All this should not create an endogeneity bias for our regressions as the insider trading regime is clearly pre-determined for any given transaction in the sample. However, this does imply that we have little to say about the impact of insider trading laws outside the OECD.

Table 2 breaks down the number and changing proportions of observations across insider trading regimes over time.

[Insert Table 2 about here.]

Table 2 reflects the fact that insider trading was increasingly outlawed and legislation was also increasingly enforced during the 1990s. The last transactions in a regime without insider trading laws occurred in 1997. The proportion of transactions under regimes with enforcement increases from 82% in 1990 to 97% in 1996 and stabilizes almost at that level. This is the main reason why we cannot conduct our analysis by country as the number of transactions in countries and years where no insider trading law was in force is too small. We also looked at the time interval between the year of enactment and the year of enforcement, and this interval has decreased significantly over time, and all countries where this interval is longer than ten years enacted insider trading laws in 1981 (Turkey) or earlier.

Table 2 also shows the number of transactions across time. The patterns reflect the M&A-boom and the stock market boom at the end of the 1990s. There is a steep increase during the 1990s with a peak in the year 1999 with 2,320 transactions. The numbers suggest a strong need to control for this temporal pattern of transactions in our analysis.

[Insert Table 3 about here]

Control variables. Table 3 tabulates descriptive statistics for our main control variables. We cover the whole range of developed as well as developing countries, so GDP per capita ranges from $254 (Nigeria) to $47,064 (Switzerland). Also the number of listed companies (per population in million) and the market capitalization of listed companies (as a percentage of GDP) show a considerable range from 0.8 (Indonesia) to 142.6 (Hong Kong) for the number of listed companies, and from 0.8 (Uruguay) to 385.1 (Hong Kong) for the market capitalization. Judicial efficiency is optimal in 14 countries (with a score of 10), while Indonesia is considered to have the least efficient judicial system (with a score of 2.5). Twelve countries have top scores for the rule of law, while Sri Lanka has the worst rule of law (with a score of 1.9). In our regression analysis we normalize all variables to lie in the unit interval in order to facilitate the interpretation of interactive terms in the regressions. To address concerns whether part of our transactions may be rumored we use a rumor-dummy. As Table 3 shows rumors are rare in our sample and occur in only 118 transactions (less than 1%).

4. Empirical Analysis

4.1 Univariate tests

We estimate a market model by using the national stock market index for each country obtained from Thompson Financial’s Datastream. We calculate cumulative abnormal returns for each transaction from 60 days before to 40 days after the announcement. Figure 1 shows the cumulative abnormal returns for subsidiary transactions separately for each insider trading regime.

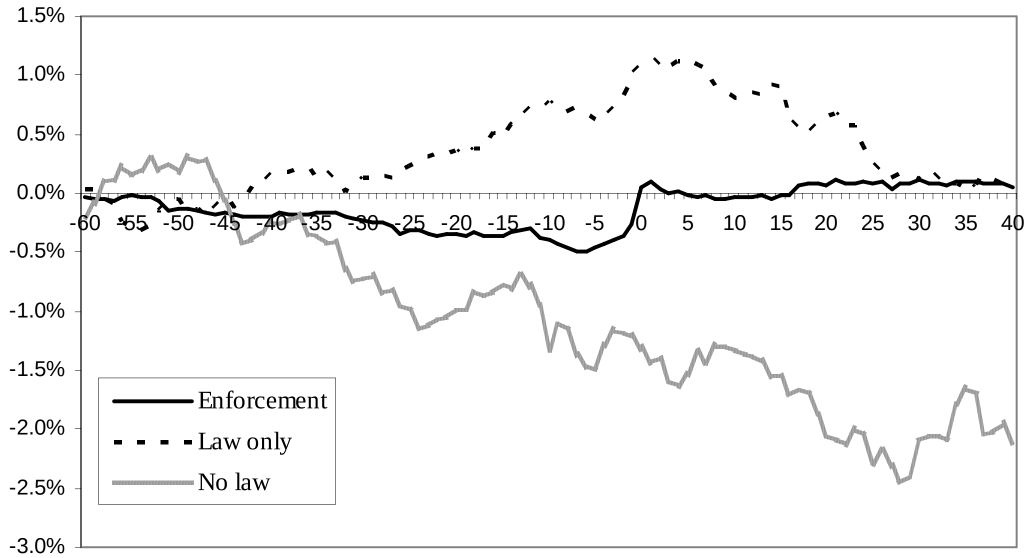

Figure 1: The figure charts the cumulative abnormal returns for subsidiary transactions. Abnormal returns are calculated for the target’s parents using the market model with a broad national stock market index. Returns are log returns.

Table 4 tabulates statistics for the runup (defined over the (-40,-4)-window), the announcement return (defined over the (-3,+3)-window), and the total return (accordingly defined over the (-40,+3)-window). Using a runup period of 40 trading days before the announcement seems reasonable from inspecting Figure 1 and also conforms with Schwert (1996), who uses 42 days. The event window needs to be long enough to allow for potential data errors but sufficiently short so that subsequent events (for example, bidding contests) do not create additional noise in our data. We also experimented with shorter and longer runup periods (20 to 60 days before the announcement) and shorter and longer announcement windows, without affecting our results. Due to some extreme outliers in our return data, we winsorize the top and bottom 2% of our runup, announcement, and total returns to ensure that our results are not driven by a few extreme observations. The modification influences the size and statistical power of our estimates, in particular for the No Law sample. However, our qualitative results are mostly unaffected by this approach.

[Insert Table 4 about here]

Panel A of Table 4 shows return differences across insider trading regimes for subsidiaries. Because of the skewness in our return data we focus on median returns, means show a similar pattern. The median total acquisition return ranges from 0.04% (no insider trading laws) to 0.62% (enacted insider trading laws). The median return for the Enforcement regime lies somewhat below at a value of 0.31%. The announcement return varies between 0.16% (enforced insider trading laws) and (insignificant) 0.35% (no insider trading laws), while for the Law Only regime the median announcement return equals 0.24%. Our non-parametric Wilcoxon signed-rank test indicates that in both cases median returns are positive and significantly different from zero only in regimes where insider trading laws are enforced or enacted and zero if no insider trading laws exist. Median runups are never significant regardless of the status of insider trading legislation.

Panel B provides univariate tests for the difference in median acquisition returns across the three insider trading regimes. P-values are obtained from two tests. First, we perform a standard Wilcoxon ranksum test that is robust to the skewness in our return data. Second, we want to control for year- and country-fixed effects. We therefore regress our three return variables on year- and country-dummies to extract the return variation from those fixed effects. In a second step we then take the residuals from this regression and test whether these residual returns differ significantly between two insider trading regimes (again using the non-parametric Wilcoxon ranksum test). Our results show only insignificant differences of runups across insider trading regimes. The median announcement returns change insignificantly by -0.11% after the initial enactment of insider trading laws. Once we control for country- and year-fixed effects we observe an slight decrease of median announcement returns by -0.08% (significant at the 5% level) due to subsequent enforcement of insider trading legislation. For median total returns we find an insignificant increase of 0.58% after law enactment and a drop of -0.31% after the enforcement of insider trading laws, which is significant at the 5% level (specification with country- and year-fixed effects). We also include the corresponding tests on runups, but we remind the reader that our predictions on runups are ambiguous, and the tests cannot reject the null hypothesis that runups do not change, either because of enactment or because of enforcement.

Overall, we cannot reject Hypothesis 1a) that mean announcement and total returns are affected by the enactment of insider trading legislation. Under the Enforcement regime, runups are never affected significantly, whereas the comparisons of total returns and announcement returns are insignificant only if we do not control for fixed effects. With fixed effects the differences across regimes become significant for announcement returns and total returns, but the sign is the opposite of what we would expect under the alternative hypothesis. We can therefore not reject Hypothesis 2a) either.

Panel B of Table 4 also shows a test on differences in return variance across insider trading regimes and tests part b) of Hypotheses 1 and 2. The results indicate a higher variance of runup, announcement and total returns after enactment and enforcement of insider trading prohibitions, which are always significant at the 1% level or better. We therefore reject Hypothesis 1a) that the variance of announcement and total returns is unaffected by the mere existence of insider trading laws. Similarly, we reject Hypothesis 2b) and conclude that announcement returns and total returns become more dispersed when insider trading prohibitions are enforced. Our findings suggest that less information is released to markets during and before the runup phase once insider trading laws are enacted and also again after they are enforced for the first time.

4.2 Regression analysis

We now investigate part c) of Hypotheses 1 and 2 and analyze regression (1) above. We identify the runup with the cumulative abnormal return over the (-40,-4)-window and the announcement return with the cumulative abnormal return over the (-3,+3)-window. We estimate regression (1) separately for each insider trading regime, where the subscript i indexes transactions. We then test for the equality of the slope coefficient β across insider trading regimes using a standard t-test.[9] Our main regression specification controls for country- and year-fixed effects and we drop the intercept in these regressions accordingly.

[Insert Table 5 about here]

Table 5 provides the results from regression (1) as model 1. We regard the test on the equality of runup coefficients for the No Law regime and the Law Only regime as a test on the relevance of insider trading laws (see the line “Relevance of Law” in Panel B of Table 5) and the test on the equality across the Law Only and the Enforcement regimes as a test on the relevance of insider trading law enforcement (see the line “Relevance of Enforcement”). We find that only the test on the relevance of law rejects at all conventional significance levels. The coefficient on the runup variable increases from an insignificant -0.034 to a significant 0.025 once insider trading laws are enacted. Subsequent enforcement does not change this coefficient significantly, and the test on the relevance of enforcement cannot reject the null hypothesis.

The values for the runup coefficient here are somewhat smaller than the values found by Schwert (1996) for the United States for a different sample period. The values are not significantly different for other countries that passed and enforced insider trading laws prior to the beginning of our sample period. We therefore conclude from Table 5 that the runup coefficient increases significantly in magnitude as a result of enacting insider trading laws but not as a result of subsequent enforcement. From our discussion in Section 2 we therefore conclude that insider trading prohibitions reduce insider trading.

We find that β in (1) is small, negative, and insignificantly different from zero for regimes where trading by corporate insiders is unrestricted. After the enactment of insider trading laws the runup coefficient becomes significantly positive (0.025). We therefore reject Hypothesis 1c) that β is equal for subsamples of acquisitions subject to the No Law regime and those subject to the Law Only regime. With respect to Hypothesis 2c) we conclude that subsequent enforcement actions have no influence on the covariance between announcement returns and runups as we observe no change in the runup coefficient after insider trading laws are enforced. Our findings therefore suggest that it is mainly the enactment of insider trading legislation that affects the way runups can predict announcement returns.

Our results are all the more surprising given that we have many more observations on the Enforcement regime than we have on the No Law regime, so the power to reject the null

[9] Note that the estimation of a full model, where we control for the insider trading regimes via dummy variables, is no valid procedure as the residual variances differ significantly across insider trading regimes.

hypothesis that enforcement is not relevant is larger than the power to reject the null hypothesis that mere law enactment is irrelevant. Yet, we can reject the latter but not the former hypothesis. The results are very similar if we introduce dummy-variables for years and for countries (model 2 in Table 5). Both sets of dummy variables are statistically significant at least for the No Law and for the Enforcement regime, but without affecting the estimates on the runup coefficients very much. Our regression analysis therefore appears robust to heterogeneity across time periods and countries.

To further address issues of heterogeneity regarding country-specific characteristics (in addition to year- and country-fixed effects) we introduce several other control variables (model 3 in Table 5). These include the logarithm of per capita GDP to control for differences in the economic efficiency between countries, the countries' rule of law and judicial efficiency indices to control for differences in the legal system, and the number of listed firms per population (in millions) and the market capitalization of listed firms (as a percentage of GDP) to control for differences in the size and the efficiency of financial markets.[10] In addition, we introduce a rumor-dummy, which equals '1' if SDC reports rumors prior to the official announcement and '0' otherwise. We provide a detailed description of all control variables in Table 1. Again, we find a significant effect after the enactment of insider trading legislation, while the enforcement effect is not significantly different from zero. The overall runup coefficient estimates are almost unaffected by the inclusion of control variables, indicating that our results are robust to several other country-specific characteristics that could have influenced our estimates. We will use the model with fixed effects and control variables (model 3) as a benchmark for our subsequent analysis.

Our methodology rests on the notion that we measure the runup coefficient β for the Enforcement regime and then investigate to what extent and how the other regimes deviate from it. It is therefore important that we determine the runup coefficient for the Enforcement regime correctly. A particular concern is that there are many countries included in the Enforcement regime that may enforce insider trading laws of a different quality and to differing degrees that are not captured by the coarse classification of Bhattacharya and Daouk (2002) we are using here. We therefore repeat the analysis (model 2 of Table 5) for the enforcement case in Panel C of Table 5 first for the United States only, which is one of the three countries with a perfect score of

10 See Rajan and Zingales (2003) for a detailed description of these variables.

5 on Beny's (2004) quality of insider trading scale (column 1).[11] Then we repeat the analysis (model 2 and 3 of Table 5) for all those countries that score below 5 (columns 2 to 3). The results are virtually identical across subsamples, confirming our results above. Hence, there is no evidence for measurement errors from pooling data across countries for the Enforcement regime.

Altogether, we conclude that the impact of enacting insider trading laws is large and significant, whereas the additional impact of subsequent enforcement actions is small and often not detectable with our regression method.

4.3 The quality of insider trading laws

Our analysis so far relies exclusively on the classification of Bhattacharya and Daouk (2002), which does not distinguish between different degrees of stringency of insider trading laws. However, we would not be surprised to learn that enacting a weak prohibition of insider trading has little effect on market participants. We therefore also analyze the stringency of insider trading prohibitions and use Beny's (2004) index of the quality of insider trading laws for this purpose. Her index offers a more detailed approach than the classification we have used so far.

[Insert Table 6 about here]

For the lowest and the highest index value we have only observations for one insider trading regime and differentiating between six values of the index-level creates very small subsamples. We therefore stratify the sample into three subsamples, where insider trading quality is low (index values of 0 and 1), intermediate (values of 2 and 3), and high (index values of 4 and 5). The results are shown in Table 6. We do not find any impact if insider trading quality is low, neither for the enactment nor for the enforcement of laws, which are weak in this case. The runup coefficient never differs significantly from zero, regardless of the status of insider trading legislation. On the contrary, the enactment of high quality insider trading laws (high index values) increases the runup coefficient significantly from -0.026 to 0.033 (p-value of 0.06, not tabulated) and is then virtually unaffected by subsequent enforcement. Here the values are statistically indistinguishable from those reported in Table 5, Panel B for the entire Enforcement regime and from those in Table 5, Panel C for the United States.[12]

11 We do not include Canada and South Korea, the other two countries with a perfect score, as the United States accounts for 90% of the observations among these three countries.

12 Only Canada, South Korea, and the United States have a perfect index value of 5, and the United States accounts for 90.6% of the observations of these three countries, so the results for model 1 in Table 5, Panel C are very close to what we would obtain for Index=5 in the Enforcement regime.

For intermediate quality laws (index values of 2 and 3) we observe an increase in the runup coefficient with law enactment, which is marginally significant with a p-value of 10.4%. Enforcement of low quality insider trading laws results in a statistically insignificant drop of β. The results here are therefore similar to those for high quality insider trading laws. The results for intermediate index values of 2 and 3 are more difficult to interpret compared to those for more extreme values because identical intermediate index values hide a significant degree of heterogeneity between countries. For example, the UK, South Africa and Hong Kong all score 3 on the index for the quality of insider trading laws. However, tipping is allowed in South Africa, but not in the UK or Hong Kong; insider trading is not a criminal offence in Hong Kong, whereas it is in the UK and in South Africa; and investors have a private right of action in South Africa, but not in the UK and Hong Kong. As a consequence, this index can probably only distinguish between high quality and low quality laws, but cannot accurately reflect the gradation in the quality of laws between these extremes.

Altogether, we take our findings as evidence that in countries with high quality insider trading legislation individuals correctly anticipate subsequent enforcement actions by regulatory authorities. By contrast, neither the enactment nor the enforcement of insider trading legislation seem to affect the information flow in subsidiary acquisitions when laws are weak.

4.4 The quality of the judicial system

From our analysis so far we doubt that passing insider trading laws is always irrelevant, and we conjecture that the relevance of insider trading legislation is related to the quality of a country's legal system. Bhattacharya et. al. (2000) argue successfully that insider trading legislation in Mexico is ineffective. However, Mexico is characterized by low values of the rule of law (5.35 on a scale of 10) and judicial efficiency (6 on a scale of 10). This puts Mexico significantly below the median on both indicators of the quality of its legal system (cf. Table 3). However, we expect insider trading legislation in jurisdictions with a good legal system to have a stronger impact even before the law is enforced for the first time, as market participants rationally anticipate that regulators will enforce newly passed laws. We therefore hypothesize that insider trading legislation is more effective in jurisdictions with higher quality legal systems. If this hypothesis is correct, then we should see that the coefficient β in our regressions depends on the quality of the judicial system. More specifically, we should expect that the β-coefficient increases with the quality of the legal system.

[Insert Table 7 about here]

We test this hypothesis by interacting the runup variable with indices for the quality of the legal system. We use two indicators for the quality of the judicial system that have been used frequently in the law and finance literature, the rule of law index and the index for judicial efficiency. Table 7 reports regression results where we repeat our baseline regression from Table 5 and include the interactive terms with the two indicators of the quality of the judicial system for the whole sample (models 1 and 2). We also enter both indices jointly (model 3). For the No Law regime, the runup coefficient is now always negative and much larger in absolute value than before, whereas the interactive coefficient of runup and the rule of law-index (normalized to lie in the unit interval) is of similar magnitude, but with the opposite sign. This pattern holds across model specifications, but almost vanishes for the Enforcement regime.

Our main focus is again on the significance of changes in the runup coefficient, but now we compare also the coefficients on the interactive effects. We conduct the same tests as we did above in Table 5 and extend them to the interactive coefficients. The test on the relevance of law is significant for the runup coefficient for all specifications. For the interactive rule of law coefficient the test is also significant except when the rule of law and judicial efficiency enter jointly, indicating potential multicollinearity problems as the standard errors become large for this specification; then only the coefficient on the interaction of runup and judicial efficiency is significant. The interactive coefficients are either significant and positive, or insignificant. The pattern is still somewhat puzzling, since we would have expected the coefficient to be positive for the Law Only-regime rather than for the other two regimes.

The test on the relevance of enforcement can never reject the null hypothesis that insider enforcement does not significantly affect the runup coefficient for any of our specifications, in model 1 the test for the relevance of enforcement on the runup coefficient is marginally significant at the 10%-level, but numerically smaller than the equivalent test for the relevance of enactment. We obtain the same result for the interactive coefficients for models 1 and 2. We therefore find strong support for the hypothesis that law enactment matters, and only weak evidence that enforcement matters.

4.5 Robustness checks

We performed several robustness checks (results not tabulated). We changed the event window and measured the runup over the (-20,-2)-window and the announcement return over the (-1,+1)-window. This changes the magnitude of the coefficients in some cases, but never by a significant amount. The significance levels of our hypothesis tests remain unchanged.

The classifications of Bhattacharya and Daouk (2002) are appropriate for our purposes for all countries where they report an incidence of insider trading enforcement. However, there may be countries where insider trading was enforced subsequent to the writing of their paper in 2001. This concerns 15 countries in our sample where a total of 155 transactions are potentially misclassified.[13] We note that these observations constitute only 0.8% of our sample, but we still reran all our regressions for the reduced sample where we can exclude the possibility of misclassifying transactions. As expected, the changes were only minor and do not affect our results.

We may have omitted variables that predict the impact of insider trading legislation. Bushman, Piotroski, and Smith (2005) control for the date of financial liberalization and hypothesize that financial liberalization constitutes a major regime shift, which affects the openness and transparency of financial markets. However, we cannot test for the impact of this variable as practically all our transactions are concentrated in countries that were already liberalized throughout our sample period and we have no power to test hypotheses regarding market liberalization.[14]

5. Conclusion

In this paper we investigate the impact of insider trading laws and the subsequent enforcement of these laws by analyzing pre-announcement stock price runups and announcement returns in acquisition announcements for subsidiaries. We find that the impact of passing insider trading laws is strong, whereas that of subsequent enforcement actions is sometimes detectable and sometimes not. These results are plausible as market participants rationally anticipate future law enforcement.

Our results seem to contradict previous studies that found that insider trading laws have no impact before they are enforced. Note, however, that our sample and the samples used in previous studies differ substantially. Bhattacharya and Daouk (2002) use 103 countries and Bushman, Piotroski and Smith (2005) use 100 of these countries for their studies, and their methodologies give a significant weight to developing countries with less effective judicial systems. By contrast, our dataset is highly skewed towards developed countries and only 912 observations, which account for 4.9% of our sample are from countries with per capita GDP

13 The 15 countries are Austria, Colombia, Ecuador, Egypt, Ireland, Mexico, New Zealand, Nigeria, Pakistan, Philippines, Portugal, South Africa, Uruguay, Venezuela and Zimbabwe.

14 We somehow tried to address this point by including measures for size and efficiency of financial markets.

below $10,000 in the year of the transaction. A significant proportion of our observations on unregulated insider trading is for Australia and Germany, where law enforcement is generally strong.[15] Hence, the effects of poor law enforcement and low judicial efficiency do not dominate our analysis nearly as much as they have dominated the results of previous studies.

We need to be cautious with respect to the conclusions regarding the impact of enforcement. The measurement of enforcement in our analysis as well as in previous papers is imprecise. Enforcement is usually a gradual process that starts with establishing regulators and courts, and continues with indictments and the provision of additional resources. The variable we use (and other authors have used before) captures only one aspect of this. Hence, any positive findings of enforcement effects must be treated as effects that can be measured notwithstanding the noise in this variable, whereas negative findings can also be attributed to this noise.

We would also not go as far as to conclude that insider trading laws are generally effective. Rather, our results show that insider trading laws are associated with a significant shift in the informational environment surrounding acquisition announcements. This indicates that the major part of the impact of insider trading prohibitions is realized even before the law has been enforced for the first time. This shift may also include the enactment of other laws that govern securities markets, or the setup of new regulatory authorities. We cannot separate the impact of such broader reforms from more narrowly defined insider trading laws given our data.

An important question that cannot be addressed with our methodology is whether insider trading increases the total costs of an acquisition, i.e., whether runups from insider trading have to be regarded as an additional cost the buyer has to pay. We leave this question for future research.

15 The case of Germany is particularly striking. There a union leader (Steinkühler) who was sitting on the board of Daimler-Benz was caught with insider trading. His act was entirely legal as it happened after the new law against insider trading was passed but before it came into effect. Even though he had technically not broken the law he still had to resign his position as a result of the media campaign against him. The ethical standards of the new law had become effective already before the law itself.

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Tables

Table 1: Variables and data sources

This table provides the definition of the variables used in this study and the corresponding data sources.

Variable	Definition and data source
IT laws established	This variable lists the year in which insider trading laws were established in each country and is based on Bhattacharya and Daouk (2002).
IT laws enforced	This variable lists the year in which insider trading laws were enforced in each country and is based on Bhattacharya and Daouk (2002).
Judicial efficiency	This variable assesses the “efficiency and integrity of the legal environment as it affects business, particularly foreign firms.” This measure is produced by the country risk rating agency Business International Corp. and represents investors’ assessments of conditions in the country in question. The value stated is the average value between 1980 and 1983. The variable is scaled from zero to ten, with lower scores indicating lower efficiency levels and is based on La Porta et al. (1998).
Rule of law	This variable assesses the law and order tradition in the country produced by the country risk rating agency International Country Risk (ICR). The value is calculated as the average of the months of April and October of the monthly index between 1982 and 1995. The scale ranges from zero to ten, with lower scores for less respect for law and order (the scale was changed from its original range going from zero to six). The variable has been taken from La Porta et al. (1998).
Insider trading quality	This variable measures the strictness of the insider trading laws in place and ranges from one to five. Higher scores indicate stricter and hence better insider trading laws. The index is constructed by adding 1 if: (1) insiders are prohibited from trading on material non-public information; (2) insiders are prohibited from tipping outsiders about material non-public information; (3) monetary penalties are proportional to insiders’ trading profits; (4) investors have a private right of action; or (5) violation of the insider trading law is a criminal offense. This variable has been obtained from Beny (2004).
GDP per capita	This variable measures GDP per capita in constant 1995 USD. This data is available on a yearly basis from the WDI (World Development Indicators) database.
Listed companies per capita	This variable measures the number of listed companies in a country divided by the country’s population in millions. This data is available on a yearly basis from the WDI (World Development Indicators) database.
Market capitalization of listed companies (as percent of GDP)	This variable measures the market capitalization of the companies listed in a country as percent of the country’s GDP. The data is available on a yearly basis from WDI (World Development Indicators) database.
Rumor-dummy	This variable is a dummy variable that takes the value ‘1’ if the acquisition is rumored and ‘0’ otherwise. Information on acquisition rumors is provided by Thomson Financial’s SDC Platinum ‘Worldwide Mergers & Acquisitions’ database.

Table 2: Number of transactions per year

This table shows the number and the changing proportions of transactions across insider trading regimes over time.

Year	Number of transactions	Enforcement	Law Only	No Law
1990	759	81.7%	6.6%	11.7%
1991	799	74.5%	17.9%	7.6%
1992	919	75.6%	15.6%	8.8%
1993	678	72.4%	19.9%	7.7%
1994	775	77.3%	22.3%	0.4%
1995	1,252	87.3%	12.3%	0.4%
1996	1,442	96.9%	3.0%	0.1%
1997	1,731	95.1%	4.6%	0.2%
1998	2,071	95.9%	4.1%	
1999	2,320	96.4%	3.6%	
2000	2,231	96.4%	3.6%	
2001	2,039	95.5%	4.5%	
2002	938	96.2%	3.8%	
2003	798	96.6%	3.4%	
Sum/Mean	18,752	91.4%	7.1%	1.6%

Table 3: Descriptive statistics of explanatory variables

This table provides descriptive statistics of the control variables used. All variable definitions are in Table 1.

Variable	Mean (weighted by country)	Mean (weighted by observation)	Median (weighted by country)	Median (weighted by observation)	Minimum	Maximum	Stand. Dev.
Judicial efficiency	7.7	9.5	7.3	10.0	2.5	10.0	1.2
Rule of law	6.9	9.4	6.9	10.0	1.9	10.0	1.2
Insider trading quality	3.1	4.1	3.0	5.0	1.0	5.0	1
GDP per capita	14,787	26,501	12,503	27,809	254	47,064	7,861
Listed companies per capita	22.5	29.5	12.9	27.5	0.8	142.6	20.8
Market capitalization	63.8	104.6	40.3	101.5	0.8	385.1	53.3
Rumor-Dummy	0.00	0.01	0	0	0	1	0.08

Table 4: Descriptive statistics of returns

This table provides cumulative abnormal return (CAR) summary statistics. Panel A shows the runup (defined over the (-40,-4)-window), the announcement return (defined over the (-3,+3)-window) and the total return (defined over the (-40,+3)-window) for each insider trading regime. The data is winsorized for the top and bottom 2% of runup, announcement, and total returns. We use the Wilcoxon signed-rank test to assess if the median returns equal zero. Panel B documents the differences in median returns and p-values for the hypothesis that median returns differ between two insider trading regimes. Tests are conducted for the original return data and for a specification where we include year- and country-fixed effects: We regress runup, announcement, and total returns on year- and country-dummies and test whether the residual returns differ between insider trading regimes. We use the non-parametric Wilcoxon ranksum test to assess statistical significance. Panel B also contains p-values for a test on differences in return variance between two insider trading regimes.

Panel A: Return characteristics

Insider trading regime: No Law (NL)

Statistic	**Runup**	**Announcement**	**Total**
Observations	297	297	297
Mean	-0.76%	0.30%	-0.46%
p-value (Wilcoxon signed-rank test)	0.454	0.162	0.644
Median	-0.38%	0.35%	0.04%
Min	-40.01%	-16.52%	-56.54%
Max	45.65%	35.79%	46.62%
Standard deviation	11.45%	4.81%	12.05%
Skewness	-28.65%	97.62%	-19.15%

Insider trading regime: Law Only (LO)

Statistic	**Runup**	**Announcement**	**Total**
Observations	1,324	1,324	1,324
Mean	0.44%	0.53%	0.96%
p-value (Wilcoxon signed-rank test)	0.298	0.022	0.017
Median	0.08%	0.24%	0.62%
Min	-40.01%	-16.52%	-56.54%
Max	45.65%	35.79%	77.35%
Standard deviation	13.10%	6.14%	15.04%
Skewness	12.35%	80.31%	8.04%

Insider trading regime: Enforcement (EN)

Statistic	**Runup**	**Announcement**	**Total**
Observations	17,131	17,131	17,131
Mean	0.18%	0.62%	0.80%
p-value (Wilcoxon signed-rank test)	0.730	0.000	0.000
Median	-0.03%	0.16%	0.31%
Min	-40.01%	-16.52%	-56.54%
Max	45.65%	35.79%	81.44%
Standard deviation	15.27%	7.05%	17.24%
Skewness	15.19%	112.10%	27.83%

Panel B: Tests on differences in median returns and variances

Statistic	**Runup**	**Announcement**	**Total**
Law enactment – Return Difference	0.47%	-0.11%	0.58%
p-value (Wilcoxon ranksum test)	0.266	0.964	0.129
p-value incl. year- & country-fixed effects	0.704	0.469	0.535
p-value (Test on difference in variances)	0.004	< 0.001	< 0.001
Law enforcement – Return Difference	-0.12%	-0.08%	-0.31%
p-value (Wilcoxon ranksum test)	0.381	0.713	0.360
p-value incl. year- & country-fixed effects	0.138	0.025	0.032
p-value (Test on difference in variances)	< 0.001	< 0.001	< 0.001

Table 5: Basic tests on the impact of insider trading regimes

The table shows estimation results for regression of announcement returns (cumulative abnormal returns defined over the (-3,+3)-window) on runups (cumulative abnormal returns defined over the (-40,-4)-window). The data is winsorized for the top and bottom 2% of runups and announcement returns. Panel A provides parameter estimates for the three different insider trading regimes. Panel B reports the changes in the coefficients between regimes and documents tests for the hypothesis that slope coefficients are identical across insider trading regimes. In model 2 we regress announcement returns on runups and include year- and country-fixed effects. Model 1 only considers runups without any fixed effects. Model 3 additionally introduces several variables to control for further country-specific characteristics.

In Panel C the observations in countries with enforced insider trading legislation are split according to the quality of their insider trading legislation. The first model in Panel C repeats the estimation from model 2 and 3 for the U.S. only, while the second model considers all countries with an insider trading index less than 5.

Panel A: Parameter estimates

No Law (NL)	**Model 1**	**Model 2**	**Model 3**
Intercept	0.003	-	-
Runup	-0.034	-0.024	-0.026
Log (per capita GDP)	-	-	-3.597
Rule of law index	-	-	-1.194
Judicial efficiency index	-	-	4.366
No. of listed companies (per population in mil.)	-	-	0.007
Market capitalization (as percentage of GDP)	-	-	0.000
Rumor-dummy	-	-	-0.009
Country- and year-dummies	no	yes	yes
Observations	297	297	297
R-squared	0.66%	11.74%	12.21%
Law Only (LO)			
Intercept	0.005***	-	-
Runup	0.025*	0.028**	0.029**
Log (per capita GDP)	-	-	0.040
Rule of law index	-	-	-0.023
Judicial efficiency index	-	-	-0.022
No. of listed companies (per population in mil.)	-	-	-0.001
Market capitalization (as percentage of GDP)	-	-	0.000
Rumor-dummy	-	-	0.009
Country- and year-dummies	no	yes	yes
Observations	1,324	1,324	1,324
R-squared	0.29%	5.62%	5.93%
Enforcement (EN)			
Intercept	0.006***	-	-
Runup	0.030***	0.029***	0.029***
Log (per capita GDP)	-	-	-0.050
Rule of law index	-	-	0.014
Judicial efficiency index	-	-	-0.011
No. of listed companies (per population in mil.)	-	-	0.000
Market capitalization (as percentage of GDP)	-	-	0.000
Rumor-dummy	-	-	0.003
Country- and year-dummies	no	yes	yes
Observations	17,131	17,131	17,095
R-squared	0.43%	1.90%	1.94%

Panel B: – Test on differences in coefficient estimates

Relevance of Law (LO - NL)			
Runup	0.059**	0.052*	0.055*
Relevance of Enforcement (EN - LO)			
Runup	0.005	0.001	0.001

***, **, * indicate significance at the 1%, 5%, and 10% level.

Panel C: Separating the enforcement sample

	(1)	(2)	(3)
	Only U.S. (insider trading quality = 5)	**Countries with insider trading quality < 5**	
Runup	0.024***	0.034***	0.035***
Log (per capita GDP)	-	-	-0.023
Rule of law index	-	-	0.016
Judicial efficiency index	-	-	0.000
No. of listed companies (per population in mil.)	-	-	0.000
Market capitalization (as percentage of GDP)	-	-	0.000
Rumor-dummy	-	-	0.010
Country- and year-dummies	yes	yes	yes
Observations	8,499	7,750	7,714
R-squared	0.91%	1.68%	1.73%
Relevance of Enforcement (EN- LO)			
Runup	-0.004	0.006	0.006

***, **, * indicate significance at the 1%, 5%, and 10% level.

Table 6: Splitting the sample according to insider trading quality

The table shows estimation results for regression model 3 from table 5. The data is winsorized for the top and bottom 2% of runups and announcement returns. The sample is split according to the index of insider trading quality for index levels from 0 to 1 (low), 2 to 3 (intermediate), and 4 to 5 (high). Panel A provides parameter estimates for the three different insider trading regimes. Panel B reports the changes in the coefficients between regimes and tests the hypothesis that the slope coefficients are identical across insider trading regimes. We regress announcement returns (cumulative abnormal returns defined over the (-3,+3)-window) on runups (cumulative abnormal returns defined over the (-40,-4)-window), and include year- and country-fixed effects as well as the control variables from model 3 of Table 5 (log of per capita GDP, rule of law index, judicial efficiency index, number of listed companies relative to population, market capitalization relative to GDP, rumor-dummy) to control for country-specific characteristics.

Panel A: Paramater Estimates		**Insider Trading Quality**	
No Law (NL)	**Index = Low**	**Index = Intermediate**	**Index = High**
Runup		-0.026	
Control variables		yes	
Year- and country-dummies		yes	
Observations		297	
R-squared		12.21%	
Law Only (LO)			
Runup	0.019	0.026	0.033*
Control variables	yes	yes	yes
Year- and country-dummies	yes	yes	yes
Observations	156	709	459
R-squared	17.52%	7.01%	5.15%
Enforcement (EN)			
Runup	-0.004	0.037***	0.025***
Control variables	yes	yes	yes
Year- and country-dummies	yes	yes	yes
Observations	160	5670	11265
R-squared	10.96%	2.37%	1.82%

Panel B: Tests on difference in coefficients			
Relevance of Law (LO - NL)			
Runup	0.045	0.053	0.059*
Relevance of Enforcement (EN - LO)			
Runup	-0.023	0.011	-0.008

***, **, * indicate significance at the 1%, 5%, and 10% level.

Table 7: Rule of law and judicial efficiency - Interactive term regression

This table displays estimation results where the baseline regression (model 3 Table 5) is augmented by interactions of the runup variable with indices for the rule of law and judicial efficiency. The data is winsorized for the top and bottom 2% of runups and announcement returns. Panel A provides parameter estimates for the three different insider trading regimes. Panel B reports the changes in the coefficients between regimes and tests the hypothesis that slope coefficients are identical across insider trading regimes. We regress announcement returns (cumulative abnormal returns defined over the (-3,+3)-window) on runups (cumulative abnormal returns defined over the (-40,-4)-window), on normalized rule of law, normalized judicial efficiency, both interacted with runup, and include year- and country-fixed effects and the control variables from model 3 of Table 5 (log of per capita GDP, rule of law index, judicial efficiency index, number of listed companies relative to population, market capitalization relative to GDP, rumor-dummy).

Panel A: Parameter Estimates

No Law (NL)	**Model 1**	**Model 2**	**Model 3**
Runup	-0.489**	-0.449***	-0.159
Runup * Rule of Law	0.525**	-	-0.848
Runup * Judicial Efficiency	-	0.511***	1.062**
Control variables	yes	yes	yes
Country- and year-dummies	yes	yes	yes
Observations	297	297	297
R-squared	13.60%	14.83%	15.41%
Law Only (LO)			
Runup	0.026	0.030	0.031
Runup * Rule of Law	0.003	-	0.015
Runup * Judicial Efficiency	-	-0.002	-0.018
Control variables	yes	yes	yes
Country- and year-dummies	yes	yes	yes
Observations	1,324	1,324	1,324
R-squared	5.93%	5.93%	5.93%
Enforcement (EN)			
Runup	0.122***	0.097***	0.121***
Runup * Rule of Law	-0.099***	-	-0.102**
Runup * Judicial Efficiency	-	-0.071**	0.004
Control variables	yes	yes	yes
Country- and year-dummies	yes	yes	yes
Observations	17,095	17,095	17,095
R-squared	1.99%	1.96%	1.99%

Panel B: Tests on differences in coefficients

Relevance of Law (LO - NL)			
Runup	0.515**	0.479***	0.190
Runup * Rule of Law	-0.522**	-	0.863
Runup * Judicial Efficiency	-	-0.513***	-1.080**
Relevance of Enforcement (EN - LO)			
Runup	0.096*	0.067	0.090
Runup * Rule of Law	-0.102	-	-0.117
Runup * Judicial Efficiency	-	-0.070	0.022

***, **, * indicate significance at the 1%, 5%, and 10% level.