Insider Trading Legislation and Acquisition Announcements:

Do Laws Matter?\(^1\)

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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 mostly insignificant. The impact of insider trading legislation is stronger in countries with more effective judicial systems. We conclude that market participants rationally anticipate law enforcement.

**JEL classifications:** G14, G34, K22, K42

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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 has emphasized 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. Unlike these studies we find that insider trading laws do have a significant impact on the way pre-announcement returns predict announcement returns for acquisitions and conclude that market participants rationally anticipate future enforcement, particularly in countries with efficient judicial systems.

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 contributed to this literature routinely referred to insider trading as a likely channel whereby information is transmitted to market participants.\(^1\)

Insider trading laws and enforcement change the informational environment of acquisitions in two ways. Firstly, it has an impact on the amount of legal versus illegal insider trading. Secondly, it has an impact on public disclosures before the event, as market participants comply with “disclose or abstain” rules and other mandatory disclosure requirements. In order to clarify the impact of pre-announcement information that reaches the market through public disclosures or through the impact of (legal or illegal) insider trades, we develop a simple model where announcement returns and pre-announcement returns (to which we refer as “runups”) are affected by two components, namely, information about the premium to be paid in an acquisition, and information about the likelihood of a future acquisition. We show that pre-announcement revelation of information has

\(^1\)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).
implications for the relative size and variance of returns as well as for the correlation between announcement returns and runups. Our tests therefore rely on these statistics.

The theoretical analysis shows that the correlation between runups and announcement returns can in principle be negative or positive. If pre-announcement information is mainly about the likelihood of the acquisition without revealing much information about the premium that would be paid, then the correlation will be negative: a higher runup already incorporates a large part of the premium and the announcement return is going to be correspondingly smaller. Conversely, if information revealed before the announcement is mainly about the magnitude of the bid premium, then a higher runup indicates a correspondingly higher acquisition premium. The relative importance of both effects is an empirical question.

We use this framework in order to analyze the informational environment of acquisition announcements and how it is affected by insider trading regulation. The model suggests that such an analysis can be based on the average size of announcement returns and runups, the variances of these variables, and their correlations, which can be established in a simple regression framework.

The study of the impact of insider trading laws poses a methodological problem because we cannot observe all the 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 propose a methodology to study the impact of insider trading laws that does not rely on any direct knowledge of disclosures or data on insider trading. The objective of our study is to identify the impact of the enactment of insider trading laws relative to the impact of the enforcement of these laws. We therefore compare how the statistics suggested by the model change as a result of the enactment and the enforcement of insider trading laws, respectively.

For our empirical investigation we collect data on 18,914 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. Over the same period, acquisition premia increased substantially. Therefore, the transactions governed by legal regimes where insider trading was either unregulated or where restrictions were not enforced were concentrated in the early 1990s where acquisition premia in most countries were lower. We
show that once these effects are controlled for, little of the impact of legislation and subsequent enforcement remains significant.

Based on our theoretical argument we then test how insider trading laws change the correlation of announcement returns and runups. Our main result is that passing insider trading laws has a very strong influence. Passing insider trading laws reduces this correlation to almost zero. While the impact of insider trading laws is highly significant, the additional impact of subsequent enforcement actions is small on average.

From our simple regressions we conclude that market participants anticipate future enforcement actions by regulatory authorities. We hypothesize that this effect is concentrated in countries with high quality legal systems. There investors change their behavior after insider trading laws have been enacted and before they have been enforced. In countries with less effective legal systems laws may have no impact as investors anticipate that they will not be enforced. We find evidence for this conjecture and show that insider trading laws change the runup coefficient in countries with a high quality legal system, whereas subsequent enforcement changes this coefficient in countries with lower quality legal systems. We also hypothesize that insider trading laws are more effective in jurisdictions where the public transmission of financial information is easier. We measure this by looking at accounting standards and newspaper circulation and find support for this hypothesis as well.

Overall, and in contrast to the earlier literature, we find that cases of law enforcement are at most of marginal importance for the way announcement returns can be predicted by pre-announcement runups of acquisitions. Our findings corroborate the conclusion of Beny (2001, 2004) that insider trading laws matter.

Other researchers have investigated insider trading in acquisition announcements before.\(^2\) 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

\(^2\)There is a much larger theoretical and empirical literature that analyzes a much broader set of questions on insider Trading. See Bainbridge (1998) for a survey.
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 pre-announcement 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. To the best of our knowledge the only other international studies are Durnev and Nain (2004), who analyze the relationship between insider trading, ownership, and earnings opacity, and Beny (2006), who shows on a firm-level dataset that companies in common law countries have higher stock market valuations if insider trading regulation is more stringent.

The paper is organized as follows. The following Section 2 presents the theoretical framework, explains the methodology, and derives hypotheses. Section 3 describes the data. Section 4 presents the empirical analysis of the paper and Section 5 concludes. Proofs and technical details are deferred to the appendix.

2 Theoretical Framework and Methodology

Consider an economy with $N$ identical firms. Each firm has an intrinsic value $v$ for its assets in their current use. In addition, any firm may be acquired at a premium $p$ above its intrinsic pre-acquisition value. The premium $p$ is a random variable with mean $\pi > 0$ and variance $\sigma^2$ (we explain the random components below). The price the bidder pays is therefore $v + p$ and may be a compensation for synergies or a payment for private benefits of control. The source of the acquisition premium is irrelevant for our argument. We also include acquisitions of subsidiaries in our analysis. Then $p$ reflects the benefits from divestitures that accrue to the parent company.

We consider three points in time. At $t = 0$, there is a common probability $q_0 > 0$ to be acquired which is identical for each firm. At $t = 2$, there may be an acquisition announcement. If there is
an acquisition announcement, then the premium is $p$, so the value of the firm is $v_2 + p$. Therefore, the price of the firm at $t = 2$ equals

$$P_2 = \begin{cases} v_2 + p & \text{if acquired} \\ v_2 & \text{otherwise} \end{cases}.$$  \hspace{1cm} (1)

We can also interpret $t = 2$ as the beginning of a bidding contest where $p$ would then represent the expected value the premium to be paid as of $t = 2$. The details of the acquisition process after the announcement are immaterial to our argument and we refer to $t = 2$ as the (potential) acquisition date.\footnote{In our empirical analysis we focus on completed acquisitions. In the model we abstract from the fact that, at the time of the acquisition announcement, the likelihood of completing the acquisition is close to one but not exactly equal to one.} The interest rate is zero. Hence, as of $t = 0$, each firm trades at a price

$$P_0 = v_0 + q_0 \pi.$$  \hspace{1cm} (2)

In order to discuss insider trading and pre-announcement disclosures we now introduce an intermediate point in time $t = 1$ where the market receives additional information. We therefore write the premium as $p = \pi + \varepsilon_1 + \varepsilon_2$. Here, $\varepsilon_1$ represents information released to the market between $t = 0$ and $t = 1$, and $\varepsilon_2$ represents information released between $t = 1$ and $t = 2$. The market may receive information either through public announcements or in the form of trading on private information. The source of this information is immaterial here, although it matters for the interpretation of our analysis. Additional information can take two forms.

- The market may observe a signal about the \textit{likelihood} of an acquisition. The new probability to be acquired is then $q_1$, where $q_1$ may be higher or lower than $q_0$. Across all firms in the population $E(q_1) = q_0$, where $E$ denotes expectations with respect to information available at $t = 0$. Insider trading is likely to affect information about $q_1$ available to market participants.

- The market may also receive information about the \textit{premium} to be paid in a potential acquisition. This takes the form of a signal $\varepsilon_1$ such that the expected premium as of time $t = 1$ equals $\pi + \varepsilon_1$. 

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\end{itemize}
In addition, the market adjusts its view of the intrinsic value of the firm \( v \) itself and we write

\[ v_2 = v_0 + \eta_1 + \eta_2, \]

where \( \eta_1 \) and \( \eta_2 \) are the innovations that become known at \( t = 1 \) and \( t = 2 \), respectively. Assume that \( q_1, \varepsilon_1, \varepsilon_2, \eta_1, \) and \( \eta_2 \) are independently distributed. Note that \( \varepsilon_1 \) and \( \varepsilon_2 \) (respectively, \( \eta_1 \) and \( \eta_2 \)) are independent by construction as \( \varepsilon_1 \) and \( \eta_1 \) represent conditional expectations as of \( t = 1 \). Also, the variance of \( \varepsilon_1 + \varepsilon_2 \) equals \( \sigma_{\varepsilon}^2 \) and the variance of \( \varepsilon_1 \) alone is \( \lambda \sigma_{\varepsilon}^2 \), where \( \lambda \) measures the degree of anticipation of the acquisition premium as of \( t = 1 \), so \( \lambda \in [0, 1] \).

If no news about the premium \( p \) arrives at \( t = 1 \), then \( \lambda = 0 \). Conversely, if the premium becomes fully known at \( t = 1 \), then \( \lambda = 1 \). Clearly, the variance of \( \varepsilon_2 \) is \( (1 - \lambda) \sigma_{\varepsilon}^2 \). Generally, \( \lambda \) will be some intermediate value between 0 and 1. Note that we do not model the potential correlation between \( \varepsilon_1 \) and \( q_1 \). It is plausible to assume that stronger pre-announcement rumors about acquisitions (higher \( q_1 \)) are positively correlated with higher acquisition premia. However, such an extension would only increase the complexity of the analysis without contributing further insights. As we do not attempt structural estimation we do not pursue this. The variance of \( \eta_1 \) is denoted by \( \sigma_{\eta}^2 \) (we never refer to the variance of \( \eta_2 \)).

We can write the market price at \( t = 1 \) as

\[ P_1 = v_1 + q_1 (\pi + \varepsilon_1). \]  \[ (3) \]

We use this theoretical framework in order to generate two types of tests for changes in the informational environment.

### 2.1 Tests on means

The first set of tests hypothesizes that insider trading in anticipation of takeover announcements leads to more information revelation during the runup phase and less information revealed in the announcement to the market. We therefore investigate average price changes in the runup and the announcement phase. We will always refer to \( P_1 - P_0 \) as the runup and to \( P_2 - P_1 \) as the announcement return.\(^4\) At \( t = 2 \), only those firms that are acquired become part of our sample.

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\(^4\) Technically, these are price changes and not rates of return. We use the term "return" here in view of our later empirical analysis.
This deliberate selection bias needs to be reflected in our statistical analysis. We will now use the notation \( \bar{q} = E(q_1 | S) \) and \( \bar{V} = Var(q_1 | S) \) to denote expectations and variances with respect to information available at \( t = 0 \) and conditional on being acquired at \( t = 2 \) (‘S’ stands for the distribution of the acquisition sample). Clearly, \( \bar{q} \) is different from the population moment where \( E(q_1) = q_0 \).

**Proposition 1 Runup and announcement returns:** The average runup for all firms in the acquisition sample is:

\[
E(P_1 | S) - P_0 = \pi (\bar{q} - q_0) > 0
\]

whenever \( Var(q_1) > 0 \). The average announcement return is

\[
E(P_2 | S) - E(P_1 | S) = \pi (1 - \bar{q}) .
\]

The runup is increasing and the announcement return is decreasing in \( Var(q_1) \).

Therefore, on average we observe a positive runup in the acquisition sample even though for all \( N \) firms in the population, martingale pricing holds, so that \( E(P_1) = P_0 \). However, the average \( t = 1 \) value for the firms in our sample equals \( E(P_1 | S) = v + \bar{q}\pi > P_0 \), because the likelihood of being in the sample is correlated with \( q_1 \). Clearly, announcement returns are decreasing and runups are increasing in \( \bar{q} \), which in turn is increasing in \( Var(q_1) \) for any given \( q_0 \). This is intuitive: the more acquisitions are anticipated, the more information is revealed through the runup and correspondingly less information is revealed in the announcement. This is a testable implication of the model. Note also that average returns are independent of \( \lambda \) and therefore not related to information about the size of a potential acquisition premium. We refer to \( E(P_2 | S) - P_0 = \pi (1 - q_0) \) as the average total acquisition premium, which is smaller than \( \pi \) because the possibility of an acquisition is already factored into the pre-acquisition price.

2.2 Regression analysis

Our methodology focuses on the following regression:
\[ P_2 - P_1 = \alpha + \beta (P_1 - P_0) + u. \quad (6) \]

This is Schwert’s (1996) regression (5) of the announcement return (which he termed ‘markup’) on the runup.

**Proposition 2 (Regression):** (i) The slope-coefficient in the regression of the announcement return on the runup is related to the structural parameters of regression (6) as:

\[
\beta = \frac{-\bar{V} \left[ 1 + \lambda \left( \sigma_{\varepsilon}/\pi \right)^2 \right] + \lambda \left( \sigma_{\varepsilon}/\pi \right)^2 \bar{q} (1 - \bar{q})}{\bar{V} \left[ 1 + \lambda \left( \sigma_{\varepsilon}/\pi \right)^2 \right] + \lambda \left( \sigma_{\varepsilon}/\pi \right)^2 \bar{q}^2 + \left( \sigma_{\eta}/\pi \right)^2}. \quad (7)
\]

The slope coefficient is decreasing in \( \bar{V} \) and increasing in \( \lambda \). It is increasing in \( \bar{q} \) if \( \bar{q} \) is sufficiently small and decreasing in \( \bar{q} \) otherwise. \( \beta \) is bounded so that \(-1 \leq \beta < 1/\bar{q} - 1 \). For the intercept we have \( \alpha = \pi (1 + q_0 - \bar{q} (1 + \beta)) \).

(ii) Assume that \( \sigma_{\varepsilon}^2 > 0 \), \( \sigma_{\eta}^2 > 0 \), and \( \pi > 0 \). We have \( \beta = 0 \) if and only if one of the following conditions hold: (1) \( \lambda = \bar{V} = 0 \), (2) \( q_0 = 1 \), or (3)

\[
\bar{V} = \frac{\lambda (\pi/\sigma_{\varepsilon})^2 \bar{q} (1 - \bar{q})}{1 + \lambda (\pi/\sigma_{\varepsilon})^2}. \quad (8)
\]

As an immediate corollary from Proposition 2 we obtain that \( \beta \) is positive if \( \bar{V} \) falls below a critical value given by the right hand side of (8). Conversely, \( \beta \) is negative for larger variances \( \bar{V} \) and regression (6) can therefore result in a positive or in a negative slope coefficient. There are two effects. Firstly, stronger anticipation of a future acquisition itself results in a higher value of \( \bar{V} \), which reduces \( \beta \). This is intuitive, since a larger fraction of the acquisition premium gets incorporated into market prices before the announcement, so the announcement return becomes smaller if the pre-bid runup becomes larger. Secondly, pre-announcement information about the premium that would result from a potential acquisition results in a higher \( \lambda \) and therefore a higher \( \beta \). This is also intuitive, as now a higher runup indicates that the bidder will pay a higher premium in case of a future acquisition. If the probability of an acquisition increases in a subsequent announcement,
then the announcement return must also be correspondingly larger.\textsuperscript{5} The regression coefficient $\beta$ can therefore be positive or negative depending on whether there is more pre-announcement information about the premium or about the likelihood of an acquisition. For our empirical analysis this means that we should be interested in changes of $\beta$ across insider trading regimes rather than in the sign and magnitude of $\beta$ for any particular regime itself. Moreover, we need to be somewhat careful with interpreting the case where $\beta = 0$ as this can be the outcome of three different situations:

1. There may be no insider trading nor any other source of pre-announcement information, so that $\lambda = \bar{V} = 0$. Then no information is disclosed so that $\bar{q} = q_0$. Then Proposition 1 implies that runups are zero, but announcement returns are positive (recall that $\pi > 0$).

2. The acquisition may already be fully anticipated at time $t = 0$, so that the market only learns about the premium itself ($q_0 = 1$). Then we should observe that the average announcement return as well as the average total acquisition premium are equal to zero from simply adding equations (4) and (5). We can therefore test for this scenario as well.

3. The two countervailing effects described above may just offset each other, so that the degree of anticipation of the premium and the degree of anticipation of the acquisition itself are related as in (8). This cannot generally be the case as condition (8) is not generic and the situation described in equation (8) will prevail only with probability zero. However, we alert the reader to the fact that we cannot test for this.

It follows that $\beta = 0$ is generally not a valid test for the effectiveness of insider trading legislation. Observe also that the regression coefficient $\beta$ in (7) depends on the distribution of $p$ only through the coefficient of variation $\sigma_\varepsilon/\pi$. This is relevant for our empirical analysis where we will pool observations from different countries and years that have potentially different means (that largely depend on $\pi$). It is therefore important to note that $\beta$ does not depend on $\pi$ directly but only through its coefficient of variation. If we can assume that $\sigma_\varepsilon$ and $\pi$ change proportionately, then $\beta$ remains unaffected by such a heterogeneity in our dataset. In contrast to this, $\alpha$ depends on $\pi$

\textsuperscript{5}In our model the probability increases to one as we equate the likelihood of an acquisition with that of an acquisition bid, although we really refer to the latter.
(Proposition 2). From this it is sufficient to introduce dummy variables that control for differences in acquisition premia.

Our empirical methodology rests on the tests suggested by Propositions 1 and 2. Some of the literature (e.g. Jarrel and Poulsen, 1989; Meulbroek, 1992) constructs a “runup index,” where the cumulated abnormal return over the runup period is divided by the cumulated return over the entire event window. Schwert (1996) has exposed the weakness of this approach, and we therefore do not use it. Bris (2005) studies imputed abnormal profits from insider trading and therefore needs to construct a measure of abnormal trading volume and relate it to insider trading. By contrast, our methodology here relies only on price data.

2.3 Methodology

We estimate regression (6) by estimating cumulated abnormal returns (CARs) in order to control for exogenous changes in the value of the standalone firm $v$. For our empirical analysis we work with returns even though the theoretical framework was cast in terms of price changes. We need to fix three points in time $\tau_0$, $\tau_1$, and $\tau_2$ that correspond to the respective dates in the theoretical setup ($t=0,1,2$) and estimate the runup as $\text{CAR}(\tau_0, \tau_1 - 1)$ and the announcement return as $\text{CAR}(\tau_1, \tau_2)$. Clearly, $\tau_1$ has to lie before (or on) the announcement date and $\tau_2$ has to lie after (or on) the announcement date. Then the expectations in Section 2.1 can be estimated by their sample equivalents. Also, our basic regression equation (6) can be estimated as:

$$\text{CAR}_i(\tau_1, \tau_2) = \alpha + \beta \text{CAR}_i(\tau_0, \tau_1 - 1) + u_i,$$

where $i$ is the transaction index.

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 is not followed by enforcement actions has no impact on average abnormal announcement returns and runups, and
also has no impact on the magnitude of the regression coefficient $\beta$ in (9). Alternatively, if our conjecture is incorrect then, on the basis of our theoretical analysis, we should expect that insider trading legislation increases average announcement returns and reduces runups (Proposition 1).

Based on the same literature we would expect that enforcement actions by regulators against insider trading do have an impact on the information revealed through price runups before the announcement so that subsequent to enforcement actions by regulators we should observe higher average announcement returns, lower runups, and a change of the regression coefficient $\beta$ in (9).

We proceed as follows. Similar to the existing literature we distinguish three regimes of insider trading regulation. In the “Enforcement” regime insider trading laws exist and they are enforced. In the “Law Only” regime, insider trading laws exist, but they are not enforced. Finally, in the “No Law” regime insider trading laws do not exist. Then we estimate $\beta$ for each regime and test for the impact of enforcement by comparing the $\beta-$values for the “Enforcement” regime with those for the “Law Only” regime. Similarly, we test for the impact of the enactment of insider trading laws by comparing the $\beta-$values for the “Law Only” regime with those for the “No Law” regime. If we find that $\beta$ changes substantially after laws are enacted, but before they are enforced, then we attribute this effect to the impact of legislation and infer that market participants anticipate effective enforcement in the future. Similarly, if $\beta$ does not change after laws are enacted, but does change after they are enforced, then we conclude that mere legislation is ineffective and that market participants only believe that insider trading laws have any bite after they have observed cases of enforcement.

Note that our 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 not rely on such data. Instead we infer changes of the informational environment from changes in the runup coefficient $\beta$ in regression (9).
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. They distinguish three distinct legal situations with respect to insider trading that correspond to the three regimes described above. Out of the 49 countries considered in this study, 47 countries have insider trading laws or pass such laws during the 1990s, the exceptions being Jordan and 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, Bhattacharya and Daouk as well as Bushman, Piotroski, and Smith (2005) 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 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, rule of law, and accounting standards stems from La Porta et al. (1998) with higher scores indicating better laws and standards. Judicial efficiency indicates how well the laws in place are carried out, rule of law assesses the quality of the local judicial system and accounting standards rate the quality of accounting standards. Unfortunately, the accounting standards for eight countries have not been rated. Data on GDP per capita and on daily newspaper circulation 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 49 countries for which data on law enforcement and accounting standards 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. Imposing these criteria leads to a sample of 28,808 transactions.

We delete transaction where the insider trading regime of the target country changes during the 40 trading days before the announcement. This is necessary in order to be able to assign transactions unambiguously to insider trading regimes. We discard 82 transactions because they cannot be allocated to one particular insider trading regime, reducing the sample to 28,726. 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).
only consider transactions for which we can calculate all abnormal returns from 40 days before until 3 days after the M&A announcement. Our final dataset contains 18,914 observations for 48 countries. Of these, 2,348 are acquisitions of public targets and 16,566 are subsidiaries.

All variable definitions and data sources are summarized in Table 1.

3.2 Descriptive statistics

Events. Table 2 contains a breakdown of all 18,914 observations by country, the percentage of publicly listed targets per country, the years where insider trading laws were first enacted or enforced, and a number of control variables.

Clearly, the dataset is highly skewed towards Anglo-American countries, with the US alone accounting for 51% of the observations and Canada and the UK accounting for another 5% each. 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 2,739 observations (14% of the sample) from 6 countries (Australia, Denmark, Germany, Hong Kong, Italy, and Spain) which have observations in all three insider trading regimes and 1,519 observations (8% of the sample) from another 14 countries with observations from two different insider trading regimes.

Figure 1 breaks down the percentage of observations for each insider trading regime by year. Figure 1 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 (for subsidiaries) and 1993 (for public targets). 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 enforcement, and this interval

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Figure 1: The figure shows the changing proportions of transactions across insider trading regimes over time. The year is the year of the announcement. The number of transactions per year is normalized is always 100%. The bottom part of the column reflects the proportion of transactions in the "No Law"-regime, the top part the proportion under the "Enforcement"-regime and the middle part the "Law only"-regime.

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.

Figure 2 charts the number of transactions across time, separately for subsidiaries and publicly listed firms. 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 a total of 2,228 transactions, of which 1,857 involve subsidiaries. The pattern is more pronounced for publicly listed companies than for subsidiaries. Figures 1 and 2 suggest a strong need to control for this temporal pattern of transactions in our analysis.

Control variables. Table 3 tabulates descriptive statistics about our main variables.

[Insert Table 3 about here]

All variables are normalized to lie in the unit interval in order to facilitate the interpretation of interactive terms in the regressions. Newspapers per 1,000 inhabitants has the largest relative range (in terms of normalized variables) from a low of practically zero (Peru) to almost 3 newspapers per 4 inhabitants (Hong Kong). We cover the whole range of developed as well as developing countries,
Figure 2: The figure gives the number of transactions by year of the announcement for subsidiaries (broken line, left hand scale) and publicly listed targets (solid line, right hand scale).

so GDP per capita ranges from $258 (Nigeria) to $45,952 (Switzerland). Only accounting standards are significantly less volatile, ranging from 24 (Egypt) to 83 (Sweden). 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 in the rule of law, while Sri Lanka has the worst rule of law (with a score of 1.9).

**Correlations** Table 4 reports correlations across countries.

We can infer from the correlations in Table 5 that transactions are concentrated in those jurisdictions and years where insider trading laws were enacted and enforced early and where they are enforced effectively. The correlations between the number of observations and the years of enactment and enforcement of insider trading laws are large and negative and remain significant even if the United States - a clear outlier - is removed from the list of countries. The correlations with the measures of the quality of the judicial system are both positive. This is also corroborated by Table 3, which shows that the average country-weighted judicial efficiency score is 7.67, whereas it is 9.48 if we weigh countries by the number of observations. A similar statement applies to the
rule of law.

As is well known from previous research, all institutional indicators (except the quality of accounting standards) are highly correlated with per capita GDP, with the rule of law having the highest correlation at 84%. Interestingly, the variables describing insider trading laws are only weakly correlated with per capita GDP. All institutional variables are negatively correlated with the percentage of targets that are public as opposed to subsidiaries, and the correlations are statistically significant for the rule of law and judicial efficiency. This most likely reflects the fact that a functioning market for corporate control and bids for public companies are found more often in countries where ownership is dispersed, which in turn is positively associated with stock market development. We control for country-specific effects by introducing dummy variables for all countries.

Overall, our sample is heavily skewed towards developed countries, and Anglo-Saxon countries in particular. 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.

4 Empirical Analysis

4.1 Univariate tests

We estimate a market model by using the national stock market index for each country. We calculate cumulated abnormal returns for each transaction from 60 days before to 40 days after the announcement.

Figure 3 shows the cumulated abnormal returns for subsidiaries and Figure 4 shows the cumulated abnormal returns for publicly listed companies, in each case separately for each insider trading regime. Table 5 tabulates statistics for the runup (defined over the (-40,-4)-window), the announcement return (defined over the (-3,+3)-window), and the total return (the acquisition premium, defined over the (-40,+3) window). Using a runup-period of 40 trading days before the announcement seems reasonable from inspecting Figures 3 and 4 and also conforms with Schwert
Figure 3: The figure charts the cumulated abnormal returns for subsidiaries. Abnormal returns are calculated for the target’s parent using the market model with a broad national stock market index. Returns are calculated as \( \ln \left( \frac{P_t + D_t}{P_{t-1}} \right) \) where \( P_t \) is the stock price and \( D_t \) is the dividend payment.

(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 (e.g. bidding contests) do not create additional noise in our data. We also experimented with shorter and longer runup periods (20 to 60 days) and shorter and longer announcement windows, without affecting our results.

Table 5 shows pronounced differences across insider trading regimes for public targets, with the average acquisition premium ranging from 3.5% (no insider trading laws) to 21.6% (enforced insider trading laws), with non-enforced laws ranging somewhat below the midpoint between these two values at 8.9%. Note also that the relative magnitudes of runups and announcement returns compared to total returns are approximately the same for the regime with enforcement and without enforcement. Naturally, the returns for parents of subsidiaries are smaller in magnitude. Announcement returns are significant in the “Law Only” regime and the enforcement regime, whereas for the “No Law” regime all returns have an insignificant negative sign.

The standard deviations of runups exhibit a remarkable pattern. For parents of subsidiaries as well as for publicly listed targets we observe higher standard deviations of runups for the enforce-
Figure 4: The figure charts cumulated abnormal returns for publicly listed targets. Abnormal returns are calculated using the market model and a broad national stock market index. Returns are calculated as $\ln \left( \frac{P_t + D_t}{P_{t-1}} \right)$ where $P_t$ is the stock price and $D_t$ is the dividend payment.

In the enforcement regime than for the “Law Only” regime, which again has higher standard deviations than the “No Law” regime. For public targets we observe the same pattern for announcement returns and total returns. These results are in marked contrast to what we would expect from Proposition 1 and rejects our hypothesis that insider trading laws and enforcement reduce the information contained in runups. This reflects the fact that the volatility of total acquisition premia differs across regimes, which for both categories of transactions is highest for the enforcement regime and lowest for the “No Law” regime, with the “Law Only” regime in between.

Table 6 shows that the differences in variances across regimes are all statistically highly significant, which we interpret as a rejection of the assumption implicit in our model that at time 0 there is no acquisition relevant information available yet. These results suggest that either the transactions undertaken differ remarkably across insider trading regimes, or, more likely, that much of the information is released to markets more than 40 days before the official announcement. It seems that more information is concentrated in the runup phase to the announcement if insider trading laws exist and even more if they are also enforced. It seems that if everybody trades on inside information, the incidence of insider trading becomes dispersed over longer period and then
there is no identifiable announcement date anymore where the stock market receives most of the information. Based on our model we infer that \( q_0 \) is close to one (even if \( t = 0 \) is identified with a date 40 to 60 trading days before the event date). Then the average premium \( (1 - q_0) \pi \) becomes indistinguishable from zero.

Table 6 provides univariate tests for the difference in means across insider trading regimes. The visual impression that announcement returns for public targets are significantly different across insider trading regimes is only partially supported by our tests. While the test that the means are not affected by enforcement is rejected at any conventional significance level, the economically large difference of 2.5% of announcement returns between the “No Law” regime and the “Law Only” regime is not statistically significant (see previous Table 5 for actual levels of returns). This is mainly due to the fact that the standard deviation of announcement returns is large and there are only 14 announcements of completed acquisitions of publicly listed firms conducted under the “No Law” regime.

The runup is larger with enforcement than without by 5.2%. This difference is significant at the 1%-level. Clearly, enforcement of insider trading does not generate smaller runups before the announcement. This corroborates our observations on variances and is further evidence against the notion that information released before acquisition announcements is inside information. Rather, it seems that better insider trading laws and their enforcement results in more information being released throughout the (-40,+3)-event window rather than before. We tried to capture the release of information before our event window by extending the window to cover dates up to 80 trading days before the event date, without any impact on this observation. We conjecture that insider trading in these cases happens over a longer time span and that the signal to noise-ratios of statistics based on cumulative abnormal returns are then so small that we cannot detect these effects with event study methodology.

All tests for the significance of insider trading law enforcement become insignificant once we include year and country fixed-effects.\(^6\) It therefore appears that the apparent impact of insider   

\(^6\)These tests are implemented using dummy regressions. In order to ensure that the resulting parameters are uniquely identified, we eliminated one country (Italy) and one year dummy (1990) from the regressions. These choices do not significantly affect our results.
trading laws and insider trading law enforcement can be attributed to the distribution of acquisitions across countries and time rather than to changes in the legal environment. This is plausible as the M&A-boom in the 1990s was accompanied by increasing acquisition premia, so that higher premia in the late 1990s (where we have more transactions in the “Enforcement” regime) compare to lower premia in the early 1990s (where we have all the observations in the “No Law” regime and most of those for the “Law Only” regime). In fact, the mean acquisition premium measured over the (-40,+3)-interval for public targets in our sample increases from 2.9% in 1990 to 28.7% in 2001 and then falls back to 24.9% in 2003 (results not tabulated). For both types of targets, entering country-dummies and year-dummies jointly renders all tests on the difference between insider trading regimes insignificant.\footnote{Note that our analysis is therefore different from that in Bris (2005), who presents facts similar to our Table 5 and Figure 4.}

Altogether, our univariate tests produce some patterns that seem to reject our hypotheses, but they do not yield definite answers on the question whether insider trading law enactment or enforcement has an impact on announcement effects, as these tests only compare the means of returns across regimes. We therefore investigate the correlations between runups and announcement returns.

### 4.2 Regression analysis

In order to implement regression (9) we continue to identify the runup with the CAR over the (-40,-4)-window. We denote the announcement return by $AR_i$ and equate it to the CAR over the (-3,3)-event window. Then we estimate the regression

$$AR_i = \alpha + \beta \times \text{runup}_i + u_i ,$$

(10)

separately for each insider trading regime, where the subscript $i$ indexes transactions. The differences in the $\alpha's$ and $\beta's$ across insider trading regimes measures the impact of insider trading legislation and enforcement on the way in which the runup helps to predict announcement returns. We test for the equality of the slope coefficient $\beta$ across insider trading regimes using a standard
t-test. We do not conduct tests on the intercept $\alpha$ as the intercept depends on the premium $\pi$, which may vary across countries and years, whereas $\beta$ depends only on the coefficient of variation $\sigma_e/\pi$, which can more easily be assumed to be constant across years and countries (see Proposition 2).

Table 7 provides the results from this regression as model 1 for subsidiaries (left part) and for public targets (right part). We regard the test on the equality of runup-coefficients for the “No Law” and the “Law Only” regimes as a test on the relevance of insider trading laws (section “Relevance of law” in Table 7A) and the test on the equality across the “Law Only” and the “Enforcement” regime as a test on the relevance of insider trading law enforcement (section “Relevance of enforcement” in Table 7A). We analyze the results for subsidiaries first. We find that only the test on the relevance of laws rejects at all conventional significance levels. The coefficient on the runup drops from a highly significant 0.547 to an insignificant 0.035 once insider trading laws are enacted. Subsequent enforcement does not change this coefficient, and the test on the relevance of enforcement cannot reject the null hypothesis.

The values for the runup-coefficient here correspond closely to the values found by Schwert (1996) for the United States for a different sample period and are not significantly different for other countries that passed and enforced insider trading laws prior to the beginning of our sample period.\(^8\) We therefore conclude from Table 7 and on the basis of Proposition 2 that the runup coefficient falls significantly in magnitude as a result of enacting insider trading laws but not as a result of subsequent enforcement.

We find that $\beta$ in (9), respectively (6) is significantly positive for all insider trading regimes and largest without insider trading laws. In the context of our theoretical analysis this means that without insider trading restrictions we have a large $\lambda$ (information about the premium becomes public during the runup period) whereas $\bar{V}$ is comparatively small, so less additional information

\(^8\)Schwert puts the total return $P_2 - P_0$ on the right hand side, which is equivalent to adding $P_1 - P_0$ to both sides of regression (6). Hence, his slope coefficients correspond to $\beta + 1$ in our regression specification and we need to subtract 1 from his estimates to compare them with ours.
transpires about the likelihood of an acquisition (see Proposition 2). The drop in the regression coefficient associated with the enactment of insider trading legislation could be attributed either to an increase in $\bar{V}$ (more information is revealed about the likelihood of an acquisition) or to a drop in $\lambda$ (less information is revealed about the premium $p$). We cannot identify the source of this change, but the results on the increase in the size and the variance of the runup above suggests that more information is revealed in the runup phase once insider trading laws have been enacted and enforced, so an increase in $\bar{V}$ appears more likely than a reduction in $\lambda$. This suggests that insider trading laws have the effect that more information about a pending acquisition is communicated to markets in a short window before the actual announcement. Without laws the distribution of information releases seems to stretch over a much longer period of time and can therefore not be traced using event study methodology.

Interestingly, the reduction of $\beta$ happens completely as a result of legislation and seems unaffected by subsequent enforcement actions. This is 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 hypothesis that enforcement is irrelevant is larger than the power to reject the null hypothesis that laws are irrelevant. Yet, we can reject the latter but not the former hypothesis. If we introduce dummy-variables for years and for countries (model 2 in Table 7), then the results stay largely the same. Both sets of dummies are statistically significant, but without affecting the estimates on the runup-coefficients very much. Our regression analysis therefore appears robust to heterogeneity across time periods and countries, whereas the analysis of means in the previous subsection was strongly affected by this heterogeneity. We will use the model with fixed effects (model 2) as a benchmark for our subsequent analysis.

Our methodology rests on the notion that we measure the runup coefficient $\beta$ 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 for the enforcement case in Table 7B first for the United States only, which is one of the three countries that achieve a perfect score of 5 on Beny’s (2004) quality of insider trading scale (regression 1).\(^9\) Then we repeat the analysis for all those countries that scored below 5 (regression 2 in Table 7B). The results are virtually identical across these subsamples, confirming our results above. Hence, there is no measurement error arising from pooling data across countries for the enforcement regime.

For publicly listed companies we find qualitatively similar results for the coefficient estimates. As with subsidiaries, the coefficient is reduced by legislation (here from 0.160 to 0.061) but does not change much with enforcement (change here from 0.061 to 0.041). However, none of these effects is statistically significant because of paucity of data. For our subsequent analysis we will therefore neglect the analysis of public targets.

Altogether we conclude that the impact of insider trading legislation is large and significant, whereas the additional impact of subsequent enforcement is small and insignificant.

4.3 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) have successfully argued 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 $\beta$ in our regressions depends on the quality of the judicial system. In particular, we should observe a significant difference in the $\beta$–values between the “Law Only” regime and the

\(^9\)We do not include Canada and South Korea, the other two countries with a perfect score, as the United States accounts for 92% of the observations among these three countries.
enforcement regime if the quality of the judicial system is low, because market participants change their behavior only after laws have been enforced. For the same reason we should not observe any change between the “Law Only” regime and the enforcement regime when the quality of the judicial system is high and market participants expect laws to be enforced. Similarly, we expect that there is no difference between the “No Law” regime and the “Law Only” regime if the quality of the judicial system is low, but we expect a significant difference if the quality of the judicial system is high, as market participants realize that regulators are - somewhat unexpectedly - willing to enforce the law.

[Insert Table 8 about here]

We test this hypothesis in two different ways, first non-parametrically by partitioning the sample and then 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 8 reports our regression results when we repeat the analysis from Table 7 (model 2) and partition the sample. Country-specific variables assume only a limited number of different values in our sample, so we cannot partition the sample in any way we wish. We attempt to test the top quartile against the bottom quartile and always choose the partition of the dataset that is closest to a separation of the sample into quartiles. For the rule of law index 68.8% of the transactions of our subsidiary-sample take place in countries with a perfect score of 1.0 of our normalized index and therefore form the top part of our partition. The cut-off for the bottom quartile is 0.90 and contains 25.6% of the data. The partition is similar for judicial efficiency, as for both cases the top value applies also to the United States, which accounts for half of the observations. We therefore repeat the analysis for the top partition also for those subsamples that exclude all U.S. observations.

The evidence corroborates our hypotheses. The difference between the runup-coefficients is not significant between the “No Law” regime and the “Law Only” regime for the subsamples where the rule of law is low (regression 1) or where judicial efficiency is low (regression 4) with p—values exceeding 50% in both cases. Hence, passing insider trading laws has no impact on the behavior of
market participants in countries with weaker legal institutions. By comparison, the enforcement of insider trading laws seems to have some impact, even though the results are statistically significant only at the 10%-level. The pattern of our test results reverses for the subsample with perfect scores for the rule of law (regression 2) and for judicial efficiency (regression 5). Here we find that the test for the relevance of insider trading laws always rejects the null hypothesis of no relevance at any conventional significance level, which clearly indicates that market participants in these countries have adjusted their behavior as soon as a law was passed. We detect no further change in behavior once these laws have been enforced, so it seems that enforcement was fully anticipated. Excluding observations from the United States has no impact on any of these results (compare regressions 2 and 3, respectively 5 and 6).

[Insert Table 9 about here]

Table 9 reports regression results where we test for the same hypothesis by interacting the runup variable with the two indicators of the quality of the judicial system for the whole sample. 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 vanishes for the “Law Only” regime and the enforcement regime.

Our main focus is again on the significance of changes in the runup-coefficient. We first conduct the same tests as we did above in Table 7, but we now test for changes in the runup-coefficient as well as for the interactive coefficient. 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.

In contrast to Table 7, the test on the relevance of enforcement now rejects the null hypothesis at the 1%-level for the runup-coefficient for all specifications. Again, it rejects at the 5%-level for the interactive coefficient for models 1 and 2, but not for model 3 because of multicollinearity. To
interpret this result, observe that the estimate of $\beta$ as a function of the rule of law for model 1 is:

\[
\hat{\beta}_{IT\text{-}regime} (I_{Rule\ of\ Law}) = \hat{\beta}_{Runup} + \hat{\beta}_{Runup \times Rule\ of\ Law} \times I_{Rule\ of\ Law},
\]

(11)

where $\hat{\beta}_{Runup}$ and $\hat{\beta}_{Runup \times Rule\ of\ Law}$ are the coefficient estimates from Table 9 and $I_{Rule\ of\ Law}$ is the normalized index for the rule of law of the country. The computations for models 2 and 3 are analogous and shown in panel B of Table 9. We define the enforcement effect as

\[
\hat{\beta}_{Enforcement} (I_{Rule\ of\ Law}) - \hat{\beta}_{Law\ only} (I_{Rule\ of\ Law})
\]

and compute it for countries with a high quality legal system (rule of law = 1.0, judicial efficiency = 1.0) and compare this to the values obtained for countries with a low quality legal system (rule of law = 0.5, judicial efficiency = 0.6).

For countries with a weak legal system the enforcement effect has the opposite sign and is about four to six times larger in absolute value compared to countries with a high quality legal system. The analysis from Table 9 therefore corroborates the inference made from Table 8 and we conclude that the relevance of enforcement is concentrated in those countries with lower quality legal systems.

### 4.4 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 10 about here]

We first run separate regressions for each value of the index, which assumes values between zero and five for the original index and between zero and one for our normalized index. For the lowest and the highest index value we have only observations for one insider trading regime, so we can only run our tests for index values between 1 and 4. The results are shown in Table 10. Interestingly, we
find no impact for the enforcement of weak laws (index equals 1), but we find a strong impact for the enforcement of moderately weak laws (index value of 2), a statistically weaker impact for strong laws (index value of 4), but no impact for the enforcement of intermediate quality laws (index value of 3, the median). We attribute this to the fact that the coefficients for the “Law Only” regime are estimated only very imprecisely when the index value equals 3. Enacting stringent insider trading laws (index = 4) has a strong impact. We check that this is not just an indirect effect of higher quality legal systems as the quality of insider trading laws and indicators of the general quality of the legal system are correlated, although the correlations are not very high (see Table 4). Of the seven countries that have an index value of 4 for insider trading laws, only one (New Zealand) scores the top value of 10 for judicial efficiency (average across 7 countries is 7.89) and the rule of law (average across 7 countries is 8.62). The impact of the quality of insider trading laws is therefore mixed based on our sample partition. We also repeated our approach by interacting the index with our runup variable in the same way as we have done in Table 9 above, but we did not obtain a significantly different answer (results not reported).

4.5 Transparency and information transmission

In addition to the quality of the judicial system, we also expect insider trading legislation to be affected by the informational environment. Consider a country where the dissipation of information is prohibitively costly. Then insider trading laws that prescribe that information needs to be public before insiders can trade (“disclose or abstain”) would be ineffective as the mere production of information is irrelevant if it cannot also be transmitted to market participants. On the other hand, countries where financial information flows freely and dissipates fast should benefit more from insider trading restrictions. We therefore hypothesize that insider trading legislation and enforcement is more effective in jurisdictions characterized by an informational environment that allows for a fast and efficient transmission of financial information.

There is no standard indicator for the ease of information transmission. We therefore use two proxy variables that have been used by other researchers before in related contexts. Firstly, we use
newspaper circulation per 1,000 inhabitants.\textsuperscript{10} Secondly, we use an index that measures the quality of financial disclosures through accounting statements. Both indicators proxy for the quality of information production (accounting) and transmission (newspapers) and should be correlated with the ease of making financial information available to investors.

[Insert Table 11 about here]

We start again by partitioning the sample into subsamples with high and low newspaper circulation and with high and low accounting standards. Table 11 presents the results. For the subsample with high newspaper circulation, the enactment of insider trading laws matters, whereas there is no discernible effect of enforcement, which corroborates the analysis of Table 7 and parallels the results of Table 8. For the subsample with low newspaper circulation there is neither a measurable impact from enacting nor from enforcing insider trading laws, which is consistent with our hypothesis that insider trading legislation will affect stock markets only when information can also be dissipated. The same observation holds for countries with low accounting standards. Interestingly, for countries with high accounting standards there is a measurable influence from enacting as well as from enforcing insider trading laws and the test for the relevance of enforcement rejects at all significance levels. We would not have expected this result based on our previous analysis as this subsample is dominated by observations from countries with a good legal system where the impact of enforcement is low (the correlations of accounting standards with the rule of law and judicial efficiency are 0.53 and 0.55, respectively, see Table 4).

[Insert Table 12 about here]

We analyze the influence of transparency and information transmission also with regressions where newspaper circulation and accounting standards enter interactively with the runup-variable. Table 12 presents the regression results, first for both variables separately (models 1 and 2) and then for both variables entering jointly (model 3). The pattern of coefficient values for the quality of accounting standards alone are very similar to those for the rule of law and judicial efficiency (see

\textsuperscript{10}See Dyck and Zingales (2002, 2004), who used this variable in law and finance research before.
Table 9 above). The tests for the relevance of law always reject the null hypothesis of no relevance at any conventional significance level either for the runup coefficient itself, or for its interaction with the respective explanatory variable, or for both. We cannot detect any impact of the relevance of enforcement here. We conclude that there is an unequivocal and strong impact of enacting insider trading laws in environments where information transmission and transparency are high, whereas there is no such impact when transparency and the ease of information transmission are lacking. The results for a separate impact of enforcement are more mixed, as the tests in Table 12 do not reject and those in Table 11 reject only for one variable.

4.6 Robustness checks

Table 5 reveals that announcement returns and runups are very volatile. We therefore suspect that some results may be driven by outliers. Table 13 repeats some of our regression analysis above for a winsorized sample where we set the top and the bottom 1% of the announcement returns equal to the values at the respective percentiles. Model 1 corresponds to model 2 in Table 7, model 2 corresponds to model 3 of Table 9, and model 3 corresponds to model 3 of Table 12. The coefficient estimates for the “Law Only” regime and the enforcement regime change only slightly, but they are measured with greater accuracy than before. The coefficient estimates for the “No Law” regime differ substantially, reflecting the lower precision of our estimates for this subsample, a consequence of the smaller subsample for this regime. However, the tests reported in Table 13 convey the same message as those reported in Tables 7, 9, and 12 above. The impact of enforcement is significant only in regressions 2 and 3, showing that our analysis above and the interpretations are robust to the exclusion of outliers.

We changed the event window and measured the runup over the (-20,-2)-day window and the announcement return over the (-1,+1)-day 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 432 transactions for subsidiaries are potentially misclassified.\textsuperscript{11} We note that these observations constitute only 2.3\% 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 that 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.

5 Conclusion

In this paper we investigate the impact of insider trading laws and the subsequent enforcement of these laws by looking at the relationship between pre-announcement stock price runups and announcement returns in acquisition announcements. We find that the impact of passing insider trading laws is strong, whereas that of subsequent enforcement actions is minor. These results are plausible as market participants rationally anticipate future law enforcement in jurisdictions where the authorities have acquired a reputation for enforcement. We investigate this by looking at the influence of the quality of the judicial system and find that the change in runup coefficients occurs in countries with high quality legal system as a result of the enactment of insider trading laws, and in countries with weaker legal systems as a result of subsequent 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

\textsuperscript{11} The 15 countries are Austria, Colombia, Ecuador, Egypt, Ireland, Kenya, Mexico, New Zealand, Nigeria, Pakistan, Phillipines, Portugal, South Africa, Uruguay, and Venezuela.
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 861 observations that account for 4.6% of our sample are from countries with per capita GDP below $10,000 in the year of the transaction. A significant proportion of our observations on unregulated insider trading is for Australia or Germany, where law enforcement is generally strong.\textsuperscript{12} 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 as well as previous papers is imprecise. Enforcement is usually a gradual process that starts with establishing regulators and courts, and continues with indictments, provision of additional resources, and so forth. 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 are cautious not 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, and that this impact is often fully 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.

\textsuperscript{12}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.
6 Appendix

Proof of Proposition 1:

Denote by $F$ the cumulative distribution function for $q_1$, so that

$$q_0 = E(q_1) = \int_0^1 q_1 dF(q_1).$$  \hfill (12)

In order to analyze the moments of the sample, we need to replace the cdf $F(q_1)$ with another cdf $G(q_1)$ for the sample. The ex ante probability for a firm to be in the acquisition sample is $q_0$. The ex ante likelihood of a firm of type $q_1$ to be in our sample is $q_1 dF(q_1)$, and the likelihood to observe a value $q_1$ conditional on being in our sample is $dG(q_1) = q_1 dF(q_1) / q_0$. Therefore, $G$ is the relevant probability law to describe the distribution of prices conditional on being in our sample. The expected value for $q_1$ across all sample firms is then

$$\bar{q} = E(q_1 | S) = \int_0^1 q_1 dG(q_1) = \int_0^1 q_1^2 dF(q_1).$$ \hfill (13)

Note that from (13), if and only if

$$\bar{q} - q_0 = \frac{1}{q_0} \left[ \int_0^1 q_1^2 dF(q_1) - q_0^2 \right] = \frac{1}{q_0} \left[ E(q_1^2) - (E(q_1))^2 \right]$$

$$= \frac{Var(q_1)}{q_0} > 0,$$

where all moments are evaluated with respect to $F(q_1)$. Then the comparative static results follow.

Proof of Proposition 2:

(i) From standard regression analysis we have

$$\beta = \frac{Cov(P_1 - P_0, P_2 - P_1 | S)}{Var(P_1 - P_0 | S)}. \hfill (14)$$

From (3) and (2):

$$P_1 - P_0 = (q_1 - q_0) \pi + q_1 \varepsilon_1 + \eta_1. \hfill (15)$$
Also, $E (P_1 - P_0 | S) = (\bar{q} - q_0) \pi$. Then observe:

$$Var (P_1 - P_0 | S) = E \left[ (q_1 \varepsilon_1 + (q_1 - \bar{q}) \pi)^2 | S \right] + \sigma_{\eta}^2$$

$$= E \left( q_1^2 | S \right) \lambda \sigma_{\varepsilon}^2 + \bar{V} \pi^2 + \sigma_{\eta}^2$$

$$= (\bar{V} + \bar{q}^2) \lambda \sigma_{\varepsilon}^2 + \bar{V} \pi^2 + \sigma_{\eta}^2. \quad (16)$$

Rearrange (16) to obtain

$$Var (P_1 - P_0 | S) = \bar{V} \left[ \pi^2 + \lambda \sigma_{\varepsilon}^2 \right] + \lambda \sigma_{\varepsilon}^2 \bar{q}^2 + \sigma_{\eta}^2. \quad (17)$$

From (1) and (3):

$$P_2 - P_1 = (1 - q_1) (\pi + \varepsilon_1) + \varepsilon_2 + \eta_2 \quad (18)$$

and $E (P_2 - P_1 | S) = (1 - \bar{q}) \pi$. Then, from (15) and (18):

$$Cov (P_1 - P_0, P_2 - P_1 | S)$$

$$= E \left( ((1 - q_1) \varepsilon_1 - (q_1 - \bar{q}) \pi + \varepsilon_2 + \eta_2) (q_1 \varepsilon_1 + (q_1 - \bar{q}) \pi + \eta_1) | S \right)$$

$$= E \left( ((1 - q_1) q_1 \varepsilon_1^2 | S) - E (q_1 - \bar{q} | S)^2 \pi^2. \right.$$

Then applying $\bar{q} = E (q_1 | S)$ and $E (q_1^2 | S) = \bar{V} + \bar{q}^2$ gives:

$$Cov (P_1 - P_0, P_2 - P_1 | S) = -\bar{V} \left[ \pi^2 + \lambda \sigma_{\varepsilon}^2 \right] + \lambda \sigma_{\varepsilon}^2 \bar{q} (1 - \bar{q}). \quad (19)$$

Substituting (17) and (19) into (14) and dividing through by $\pi^2$ gives (7). The lower bound on $\beta$ is obtained from setting $\lambda = 0$, the upper bound is obtained from $\lambda = 1$ and $\bar{V} = 0$. For $\alpha$ we use

$$\alpha = E (P_2 - P_1 | S) - \beta E (P_1 - P_0 | S)$$

$$= (1 - \bar{q}) \pi - \beta (\bar{q} - q_0) \pi,$$

which yields the result.
By direct computation we obtain:

\[
\begin{align*}
\frac{\partial \beta}{\partial \tilde{q}} &= \frac{\lambda (\sigma_\varepsilon / \pi)^2 \tilde{V} \left[ 1 + \lambda (\sigma_\varepsilon / \pi)^2 \tilde{q}^2 \right] - \lambda (\sigma_\varepsilon / \pi)^2 \tilde{q}^2}{\left[ \tilde{V} \left[ 1 + \lambda (\sigma_\varepsilon / \pi)^2 \right] + \lambda (\sigma_\varepsilon / \pi)^2 \tilde{q}^2 \right]^2}, \\
\frac{\partial \beta}{\partial \tilde{V}} &= \frac{- \left[ 1 + \lambda (\sigma_\varepsilon / \pi)^2 \right] \lambda (\sigma_\varepsilon / \pi)^2 \tilde{q}}{\left[ \tilde{V} \left[ 1 + \lambda (\sigma_\varepsilon / \pi)^2 \right] + \lambda (\sigma_\varepsilon / \pi)^2 \tilde{q}^2 \right]^2} < 0, \\
\frac{\partial \beta}{\partial \lambda} &= \frac{\tilde{q} \text{Var} \left( q_1 | S \right) (\sigma_\varepsilon / \pi)^2}{\left[ \tilde{V} \left[ 1 + \lambda (\sigma_\varepsilon / \pi)^2 \right] + \lambda (\sigma_\varepsilon / \pi)^2 \tilde{q}^2 \right]^2} > 0.
\end{align*}
\]

Solving \( \frac{\partial \beta}{\partial \tilde{q}} = 0 \) gives:

\[
\tilde{q} = \sqrt{\frac{\tilde{V} \left[ 1 + \lambda (\sigma_\varepsilon / \pi)^2 \right]}{\lambda (\sigma_\varepsilon / \pi)^2}}.
\]

Since the numerator of \( \frac{\partial \beta}{\partial \tilde{q}} \) is decreasing in \( \tilde{q} \), it must be the case that \( \frac{\partial \beta}{\partial \tilde{q}} \) is positive for small \( \tilde{q} \) and negative for large \( \tilde{q} \).

(ii) As we assume that \( \sigma_\varepsilon^2 > 0, \sigma_\eta^2 > 0 \) and \( \pi > 0 \), the numerator of (7) can be zero only if either the first and the second expression are identically zero, or if both expressions are equal in magnitude, but with opposite signs. The first expression can only be zero if \( \tilde{V} = 0 \) as the expression in brackets is always positive. The second expression can be zero only if either \( \tilde{q} = 0 \), \( \tilde{q} = 1 \), or \( \lambda = 0 \). If \( \tilde{q} = 0 \) or \( \tilde{q} = 1 \), then \( \tilde{V} = 0 \) as well which is equivalent to \( \tilde{q} = q_0 \). We exclude \( \tilde{q} = q_0 = 0 \) by assumption. Then all remaining possibilities are described by conditions (1) - (3), where (8) follows from setting the numerator of (7) to zero and solving for \( \tilde{V} \). ■
Table 1: Variables and data sources

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

<table>
<thead>
<tr>
<th>Variable</th>
<th>Definition and data source</th>
</tr>
</thead>
<tbody>
<tr>
<td>IT laws established</td>
<td>This variable lists the year in which insider trading laws were established in each country and is based on Bhattacharya and Daouk (2002).</td>
</tr>
<tr>
<td>IT laws enforced</td>
<td>This variable lists the year in which insider trading laws were enforced in each country and is based on Bhattacharya and Daouk (2002).</td>
</tr>
<tr>
<td>Judicial efficiency</td>
<td>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).</td>
</tr>
<tr>
<td>Rule of law</td>
<td>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).</td>
</tr>
<tr>
<td>Accounting standards</td>
<td>This variable measures the quality of accounting standards. This measure is created by examining and rating companies’ 1990 annual reports on their inclusion or omission of 90 items. These items fall into seven categories (general information, income statements, balance sheets, flow of funds statement, accounting standards, stock data, and special items). A minimum of three companies in each country were studied. The companies represent a cross section of various industry groups. Industrial companies represent 70 percent, and financial companies represent the remaining 30 percent of all companies studied. The variable has been taken from La Porta et al. (1998).</td>
</tr>
<tr>
<td>Newspaper circulation</td>
<td>This variable indicates the newspaper circulation per 1,000 inhabitants. This data is available on a yearly basis from the WDI (World Development Indicators) database compiled by the World Bank.</td>
</tr>
<tr>
<td>GDP per capita</td>
<td>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.</td>
</tr>
<tr>
<td>Developing</td>
<td>This variable is a dummy variable and takes on the value ‘1’ if the GDP per capita variable from WDI (World Development Indicators) is below 10,000 USD and ‘0’ otherwise.</td>
</tr>
<tr>
<td>Public status of target</td>
<td>This variable is a dummy variable and takes on the value ‘1’ if the target is publicly listed and ‘0’ otherwise. Information on public status of the target is taken from Thomson Financial’s SDC Platinum ‘Worldwide Mergers &amp; Acquisitions’ database. Acquisition transactions that listed the SEDOL code of the target were treated as public targets. Transactions that only listed the SEDOL code of the targets’ parents were treated as subsidiaries. The SEDOL code is a seven-digit identification code assigned to all securities trading on the London Stock Exchange and on other smaller exchanges in the U.K..</td>
</tr>
<tr>
<td>Percentage of public targets per country</td>
<td>This variable shows the percentage of public targets per country and was calculated by dividing the number of public targets in a country by the number of observations per country.</td>
</tr>
<tr>
<td>Insider trading quality</td>
<td>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).</td>
</tr>
</tbody>
</table>
Table 2: Country level data

This table contains a breakdown of all 18,914 observations by country and contains the 49 countries for which data on judicial efficiency and rule of law is available from La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998). The table lists the number of target observations per country. All variable definitions are explained in Table 1.

<table>
<thead>
<tr>
<th>Country</th>
<th>Observations</th>
<th>IT laws established</th>
<th>IT laws enforced</th>
<th>Judicial efficiency</th>
<th>Rule of law</th>
<th>Accounting standards</th>
<th>Newspaper circulation per 1,000 inhabitants (1990 level)</th>
<th>GDP per capita in USD (1990 level)</th>
<th>Developing (in 1990)</th>
<th>Percentage of public targets per country</th>
<th>Insider trading quality</th>
</tr>
</thead>
<tbody>
<tr>
<td>Argentina</td>
<td>81</td>
<td>1991</td>
<td>1995</td>
<td>6.00</td>
<td>5.35</td>
<td>45</td>
<td>123</td>
<td>5,776</td>
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<td>4.94%</td>
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<td>1996</td>
<td>10.00</td>
<td>10.00</td>
<td>75</td>
<td>302</td>
<td>18,632</td>
<td>0</td>
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<td>-</td>
<td>9.50</td>
<td>10.00</td>
<td>54</td>
<td>350</td>
<td>27,502</td>
<td>0</td>
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</tr>
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<td>1994</td>
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<td>10.00</td>
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<td>201</td>
<td>25,659</td>
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<td>1978</td>
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<td>54</td>
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<td>4,079</td>
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<td>-</td>
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<td>-</td>
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<td>1996</td>
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<td>1996</td>
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<td>1996</td>
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<td>3.98</td>
<td>-</td>
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<td>777</td>
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<td>8.98</td>
<td>65</td>
<td>587</td>
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<td>71</td>
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<td>-</td>
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<td>-</td>
</tr>
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<td>Country</td>
<td>Observations</td>
<td>IT laws established</td>
<td>IT laws enforced</td>
<td>Judicial</td>
<td>Rule of</td>
<td>Accounting standards</td>
<td>GDP per capita in USD (1990 level)</td>
<td>Developing in 1990</td>
<td>Percentage of public targets per country</td>
<td>Insider trading quality</td>
<td></td>
</tr>
<tr>
<td>--------------</td>
<td>--------------</td>
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<td>----------</td>
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<tr>
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<td>301</td>
<td>24,978</td>
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<td>-</td>
<td>10.00</td>
<td>10.00</td>
<td>70</td>
<td>291</td>
<td>15,200</td>
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<td>-</td>
<td>7.25</td>
<td>2.73</td>
<td>59</td>
<td>18</td>
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<td>1</td>
<td>0.00%</td>
<td></td>
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<tr>
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<td>1990</td>
<td>10.00</td>
<td>10.00</td>
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<td>610</td>
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</tr>
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<td>-</td>
<td>5.00</td>
<td>3.03</td>
<td>-</td>
<td>17</td>
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<td>1</td>
<td>0.00%</td>
<td></td>
</tr>
<tr>
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<td>1991</td>
<td>1994</td>
<td>6.75</td>
<td>2.50</td>
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<td>1,905</td>
<td>1</td>
<td>16.67%</td>
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</tr>
<tr>
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<td>-</td>
<td>4.75</td>
<td>2.73</td>
<td>65</td>
<td>56</td>
<td>1,091</td>
<td>1</td>
<td>9.52%</td>
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</tr>
<tr>
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<td>-</td>
<td>5.50</td>
<td>8.68</td>
<td>36</td>
<td>45</td>
<td>9,959</td>
<td>1</td>
<td>26.83%</td>
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</tr>
<tr>
<td>Singapore</td>
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<td>1978</td>
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<td>8.57</td>
<td>78</td>
<td>250</td>
<td>17,620</td>
<td>0</td>
<td>23.08%</td>
<td></td>
</tr>
<tr>
<td>South Africa</td>
<td>107</td>
<td>1989</td>
<td>-</td>
<td>6.00</td>
<td>4.42</td>
<td>70</td>
<td>38</td>
<td>4,113</td>
<td>1</td>
<td>16.82%</td>
<td></td>
</tr>
<tr>
<td>South Korea</td>
<td>86</td>
<td>1976</td>
<td>1988</td>
<td>6.00</td>
<td>5.35</td>
<td>62</td>
<td>280</td>
<td>7,967</td>
<td>1</td>
<td>29.07%</td>
<td></td>
</tr>
<tr>
<td>Spain</td>
<td>315</td>
<td>1994</td>
<td>1998</td>
<td>6.25</td>
<td>7.80</td>
<td>64</td>
<td>89</td>
<td>13,959</td>
<td>0</td>
<td>4.76%</td>
<td></td>
</tr>
<tr>
<td>Sri Lanka</td>
<td>2</td>
<td>1987</td>
<td>1996</td>
<td>7.00</td>
<td>1.90</td>
<td>-</td>
<td>32</td>
<td>616</td>
<td>1</td>
<td>0.00%</td>
<td></td>
</tr>
<tr>
<td>Sweden</td>
<td>303</td>
<td>1971</td>
<td>1990</td>
<td>10.00</td>
<td>10.00</td>
<td>83</td>
<td>526</td>
<td>27,252</td>
<td>0</td>
<td>4.95%</td>
<td></td>
</tr>
<tr>
<td>Switzerland</td>
<td>213</td>
<td>1988</td>
<td>1995</td>
<td>10.00</td>
<td>10.00</td>
<td>68</td>
<td>456</td>
<td>45,952</td>
<td>0</td>
<td>7.51%</td>
<td></td>
</tr>
<tr>
<td>Taiwan</td>
<td>50</td>
<td>1988</td>
<td>1989</td>
<td>6.75</td>
<td>8.52</td>
<td>65</td>
<td>-</td>
<td>-</td>
<td>1</td>
<td>24.00%</td>
<td></td>
</tr>
<tr>
<td>Thailand</td>
<td>47</td>
<td>1984</td>
<td>1993</td>
<td>3.25</td>
<td>6.25</td>
<td>64</td>
<td>81</td>
<td>1,997</td>
<td>1</td>
<td>12.77%</td>
<td></td>
</tr>
<tr>
<td>Turkey</td>
<td>24</td>
<td>1981</td>
<td>1996</td>
<td>4.00</td>
<td>5.18</td>
<td>51</td>
<td>71</td>
<td>2,576</td>
<td>1</td>
<td>50.00%</td>
<td></td>
</tr>
<tr>
<td>United Kingdom</td>
<td>1,030</td>
<td>1980</td>
<td>1981</td>
<td>10.00</td>
<td>8.57</td>
<td>78</td>
<td>388</td>
<td>18,072</td>
<td>0</td>
<td>0.29%</td>
<td></td>
</tr>
<tr>
<td>United States</td>
<td>9,738</td>
<td>1934</td>
<td>1961</td>
<td>10.00</td>
<td>10.00</td>
<td>71</td>
<td>250</td>
<td>26,141</td>
<td>0</td>
<td>15.35%</td>
<td></td>
</tr>
<tr>
<td>Uruguay</td>
<td>4</td>
<td>1996</td>
<td>-</td>
<td>6.50</td>
<td>5.00</td>
<td>31</td>
<td>232</td>
<td>4,870</td>
<td>1</td>
<td>0.00%</td>
<td></td>
</tr>
<tr>
<td>Venezuela</td>
<td>18</td>
<td>1998</td>
<td>-</td>
<td>6.50</td>
<td>6.37</td>
<td>40</td>
<td>144</td>
<td>3,350</td>
<td>1</td>
<td>5.56%</td>
<td></td>
</tr>
<tr>
<td>Zimbabwe</td>
<td>1</td>
<td>-</td>
<td>-</td>
<td>7.50</td>
<td>3.68</td>
<td>-</td>
<td>20</td>
<td>654</td>
<td>1</td>
<td>0.00%</td>
<td></td>
</tr>
</tbody>
</table>
Table 3: Descriptive statistics of explanatory variables

This table provides descriptive statistics of the control variables used. Panel A lists summary statistics of the original values weighted by countries in the sample, while panel B provides summary statistics of the original values weighted by observations. Panel C provides the summary statistics of the normalized values weighted by observations ranging from 0 to 1. All variable definitions are explained in Table 1.

Panel A: Original values (weighted by countries)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Median</th>
<th>Minimum</th>
<th>Maximum</th>
<th>Stan. Dev.</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Newspaper circulation</td>
<td>202</td>
<td>135</td>
<td>0</td>
<td>745</td>
<td>183</td>
<td>47</td>
</tr>
<tr>
<td>GDP per capita</td>
<td>12,618</td>
<td>8,963</td>
<td>258</td>
<td>45,952</td>
<td>12,122</td>
<td>48</td>
</tr>
<tr>
<td>Accounting standards</td>
<td>60.93</td>
<td>64</td>
<td>24</td>
<td>83</td>
<td>13.40</td>
<td>41</td>
</tr>
<tr>
<td>Rule of law</td>
<td>6.85</td>
<td>6.78</td>
<td>1.90</td>
<td>10.00</td>
<td>2.63</td>
<td>49</td>
</tr>
<tr>
<td>Judicial efficiency</td>
<td>7.67</td>
<td>7.25</td>
<td>2.50</td>
<td>10.00</td>
<td>2.05</td>
<td>49</td>
</tr>
<tr>
<td>Insider trading quality</td>
<td>3.11</td>
<td>3</td>
<td>1</td>
<td>5</td>
<td>0.99</td>
<td>35</td>
</tr>
</tbody>
</table>

Panel B: Original values (weighted by observations)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Median</th>
<th>Minimum</th>
<th>Maximum</th>
<th>Stan. Dev.</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Newspaper circulation</td>
<td>250</td>
<td>213</td>
<td>0</td>
<td>819</td>
<td>113</td>
<td>18,864</td>
</tr>
<tr>
<td>GDP per capita</td>
<td>27,585</td>
<td>29,237</td>
<td>254</td>
<td>47,064</td>
<td>7,871</td>
<td>18,914</td>
</tr>
<tr>
<td>Accounting standards</td>
<td>69.81</td>
<td>71.00</td>
<td>24</td>
<td>83</td>
<td>5.68</td>
<td>18,836</td>
</tr>
<tr>
<td>Rule of law</td>
<td>9.44</td>
<td>10.00</td>
<td>1.90</td>
<td>10.00</td>
<td>1.16</td>
<td>18,914</td>
</tr>
<tr>
<td>Judicial efficiency</td>
<td>9.48</td>
<td>10.00</td>
<td>2.50</td>
<td>10.00</td>
<td>1.16</td>
<td>18,914</td>
</tr>
<tr>
<td>Insider trading quality</td>
<td>4.19</td>
<td>5</td>
<td>1</td>
<td>5</td>
<td>1.05</td>
<td>18,777</td>
</tr>
</tbody>
</table>

Panel C: Normalized values (weighted by observations)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Median</th>
<th>Minimum</th>
<th>Maximum</th>
<th>Stan. Dev.</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Newspaper circulation</td>
<td>0.3058</td>
<td>0.2596</td>
<td>0.0001</td>
<td>1.0000</td>
<td>0.1386</td>
<td>18,864</td>
</tr>
<tr>
<td>GDP per capita</td>
<td>0.5861</td>
<td>0.6212</td>
<td>0.0054</td>
<td>1.0000</td>
<td>0.1672</td>
<td>18,914</td>
</tr>
<tr>
<td>Accounting standards</td>
<td>0.8411</td>
<td>0.8554</td>
<td>0.2892</td>
<td>1.0000</td>
<td>0.0685</td>
<td>18,836</td>
</tr>
<tr>
<td>Rule of law</td>
<td>0.9444</td>
<td>1.0000</td>
<td>0.1900</td>
<td>1.0000</td>
<td>0.1160</td>
<td>18,914</td>
</tr>
<tr>
<td>Judicial efficiency</td>
<td>0.9475</td>
<td>1.0000</td>
<td>0.2500</td>
<td>1.0000</td>
<td>0.1163</td>
<td>18,914</td>
</tr>
<tr>
<td>Insider trading quality</td>
<td>0.8383</td>
<td>1.0000</td>
<td>0.2000</td>
<td>1.0000</td>
<td>0.2093</td>
<td>18,777</td>
</tr>
</tbody>
</table>
Table 4: Correlations of explanatory variables

This table shows the correlations between the control variables summarized in Table 3. Each country receives an equal weight. All variable definitions are explained in Table 1. Tests are for the null hypothesis that the correlation is zero (two-sided tests).

<table>
<thead>
<tr>
<th></th>
<th>No. of observations</th>
<th>IT laws established</th>
<th>IT laws enforced</th>
<th>Judicial efficiency</th>
<th>Rule of law</th>
<th>Accounting</th>
<th>Newspaper circulation</th>
<th>GDP per capita</th>
<th>Developing</th>
<th>Percentage of public targets</th>
<th>Insider trading quality</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. of observations</td>
<td>1.000</td>
<td>-0.724***</td>
<td>-0.679***</td>
<td>0.253*</td>
<td>0.277*</td>
<td>0.206</td>
<td>0.115</td>
<td>0.270*</td>
<td>-0.264*</td>
<td>-0.015</td>
<td>0.367**</td>
</tr>
<tr>
<td>IT laws established</td>
<td>1.000</td>
<td>0.864***</td>
<td>-0.203</td>
<td>-0.204</td>
<td>-0.409***</td>
<td>-0.051</td>
<td>-0.148</td>
<td>0.156</td>
<td>-0.095</td>
<td>-0.402**</td>
<td></td>
</tr>
<tr>
<td>IT laws enforced</td>
<td>1.000</td>
<td>-0.240</td>
<td>-0.297*</td>
<td>-0.338*</td>
<td>-0.061</td>
<td>-0.204</td>
<td>0.235</td>
<td>-0.026</td>
<td>-0.541***</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Judicial efficiency</td>
<td>1.000</td>
<td>0.643***</td>
<td>0.552***</td>
<td>0.719***</td>
<td>0.728***</td>
<td>-0.759***</td>
<td>-0.377**</td>
<td>0.130</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Rule of law</td>
<td>1.000</td>
<td>0.528***</td>
<td>0.685***</td>
<td>0.838***</td>
<td>-0.786***</td>
<td>-0.312*</td>
<td>0.306*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Accounting</td>
<td>1.000</td>
<td>0.515***</td>
<td>0.497***</td>
<td>-0.586***</td>
<td>-0.256</td>
<td>0.149</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Newspaper circulation</td>
<td>1.000</td>
<td></td>
<td>0.791***</td>
<td>-0.703***</td>
<td>-0.239</td>
<td>-0.142</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>GDP per capita</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td>-0.865***</td>
<td>-0.259</td>
<td>0.071</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Developing</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.236</td>
<td></td>
<td>-0.146</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Percentage of public targets</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.142</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Insider trading quality</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

***, ** and * denote significance at the 1%, 5% and 10% levels respectively.
Table 5: Descriptive statistics of returns

This table provides cumulative abnormal return (CAR) summary statistics of 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 and separately for public targets and parents of subsidiaries. The t-test is for the hypothesis that the mean equals zero.

<table>
<thead>
<tr>
<th>Insider trading regime: Enforcement</th>
<th>Subsidiaries</th>
<th>Public targets</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Runup</td>
<td>Announcement</td>
</tr>
<tr>
<td>Observations</td>
<td>15,328</td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>0.04%</td>
<td>0.53%</td>
</tr>
<tr>
<td>t-test for mean</td>
<td>0.20</td>
<td>6.30</td>
</tr>
<tr>
<td>Median</td>
<td>0.06%</td>
<td>0.18%</td>
</tr>
<tr>
<td>Minimum</td>
<td>-529.56%</td>
<td>-249.54%</td>
</tr>
<tr>
<td>Maximum</td>
<td>329.38%</td>
<td>133.11%</td>
</tr>
<tr>
<td>Standard deviation</td>
<td>22.90%</td>
<td>9.55%</td>
</tr>
<tr>
<td>Skew</td>
<td>-279.89%</td>
<td>-169.90%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Insider trading regime: Law only</th>
<th>Subsidiaries</th>
<th>Public targets</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Runup</td>
<td>Announcement</td>
</tr>
<tr>
<td>Observations</td>
<td>1,043</td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>0.22%</td>
<td>0.47%</td>
</tr>
<tr>
<td>t-test for mean</td>
<td>0.49</td>
<td>2.26</td>
</tr>
<tr>
<td>Median</td>
<td>-0.19%</td>
<td>0.19%</td>
</tr>
<tr>
<td>Minimum</td>
<td>-93.49%</td>
<td>-45.94%</td>
</tr>
<tr>
<td>Maximum</td>
<td>136.06%</td>
<td>73.54%</td>
</tr>
<tr>
<td>Standard deviation</td>
<td>15.36%</td>
<td>7.26%</td>
</tr>
<tr>
<td>Skew</td>
<td>111.34%</td>
<td>125.00%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Insider trading regime: No law</th>
<th>Subsidiaries</th>
<th>Public targets</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Runup</td>
<td>Announcement</td>
</tr>
<tr>
<td>Observations</td>
<td>195</td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>-0.95%</td>
<td>-0.69%</td>
</tr>
<tr>
<td>t-test for mean</td>
<td>1.25</td>
<td>1.21</td>
</tr>
<tr>
<td>Median</td>
<td>-0.38%</td>
<td>0.03%</td>
</tr>
<tr>
<td>Minimum</td>
<td>-84.36%</td>
<td>-148.98%</td>
</tr>
<tr>
<td>Maximum</td>
<td>24.27%</td>
<td>16.71%</td>
</tr>
<tr>
<td>Standard deviation</td>
<td>10.91%</td>
<td>11.54%</td>
</tr>
<tr>
<td>Skew</td>
<td>-251.49%</td>
<td>-1103.22%</td>
</tr>
</tbody>
</table>

41
Table 6: Tests on differences in mean returns

This table shows p-values obtained from two tests and the difference in mean cumulative abnormal returns (CARs) between two insider trading regimes. The p-values come from a regression where one specific cumulative abnormal return (CAR) type is regressed on an insider trading regime dummy. In each case, either the ‘no law’, ‘law only’, or ‘enforcement’ subsample is included in the regression. In the first test, the ‘no law’ dummy coefficient is compared to the ‘law only’ dummy coefficient. In the second test, we compare the ‘law only’ dummy coefficient to the ‘enforcement’ dummy coefficient. The regression is specified with and without year-fixed effects and country-fixed effects. Runup returns are defined over the (-40,-4)-window, announcement returns are defined over the (-3,+3)-window, and total returns are defined over the (-40,+3)-window. The difference in mean returns for two insider trading regime types are listed in the fifth row of each test. The last test is for the hypothesis that the variances of cumulated returns are identical (Chi-square test).

Panel A: Subsidiaries

<table>
<thead>
<tr>
<th>Specification</th>
<th>Variable</th>
<th>year-fixed effects</th>
<th>country-fixed effects</th>
<th>runup</th>
<th>Announcement</th>
<th>total</th>
</tr>
</thead>
<tbody>
<tr>
<td>p-values of test on significance of law:</td>
<td></td>
<td>no</td>
<td>no</td>
<td>0.2022</td>
<td>0.1742</td>
<td>0.1206</td>
</tr>
<tr>
<td></td>
<td>yes</td>
<td>no</td>
<td></td>
<td>0.9177</td>
<td>0.2089</td>
<td>0.4942</td>
</tr>
<tr>
<td>no law (NL) = law only (LO)</td>
<td></td>
<td>no</td>
<td>yes</td>
<td>0.1364</td>
<td>0.4544</td>
<td>0.6600</td>
</tr>
<tr>
<td></td>
<td>yes</td>
<td>yes</td>
<td></td>
<td>0.3111</td>
<td>0.6263</td>
<td>0.6981</td>
</tr>
<tr>
<td>Difference (LO-NL)</td>
<td></td>
<td></td>
<td></td>
<td>1.17%</td>
<td>1.16%</td>
<td>2.33%</td>
</tr>
<tr>
<td>Test on Equality of Variances</td>
<td></td>
<td></td>
<td></td>
<td>34.00***</td>
<td>87.41***</td>
<td>4.41**</td>
</tr>
<tr>
<td>p-values of test on significance of enforcement:</td>
<td></td>
<td>no</td>
<td>no</td>
<td>0.7291</td>
<td>0.4221</td>
<td>0.8319</td>
</tr>
<tr>
<td></td>
<td>yes</td>
<td>no</td>
<td></td>
<td>0.9845</td>
<td>0.4931</td>
<td>0.7656</td>
</tr>
<tr>
<td>law only (LO) = enforcement (EN)</td>
<td></td>
<td>no</td>
<td>yes</td>
<td>0.7169</td>
<td>0.4873</td>
<td>0.9615</td>
</tr>
<tr>
<td></td>
<td>yes</td>
<td>yes</td>
<td></td>
<td>0.5270</td>
<td>0.4819</td>
<td>0.4000</td>
</tr>
<tr>
<td>Difference (EN-LO)</td>
<td></td>
<td></td>
<td></td>
<td>-0.18%</td>
<td>0.05%</td>
<td>-0.12%</td>
</tr>
<tr>
<td>Test on Equality of Variances</td>
<td></td>
<td></td>
<td></td>
<td>260.47***</td>
<td>137.29***</td>
<td>239.76***</td>
</tr>
</tbody>
</table>

Panel B: Public Targets

<table>
<thead>
<tr>
<th>Specification</th>
<th>Variable</th>
<th>year-fixed effects</th>
<th>country-fixed effects</th>
<th>runup</th>
<th>Announcement</th>
<th>total</th>
</tr>
</thead>
<tbody>
<tr>
<td>p-values of test on significance of law (no law = law only)</td>
<td></td>
<td>No</td>
<td>no</td>
<td>0.5161</td>
<td>0.8204</td>
<td>0.3652</td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>no</td>
<td></td>
<td>0.4206</td>
<td>0.7296</td>
<td>0.4244</td>
</tr>
<tr>
<td>no law (NL) = law only (LO)</td>
<td></td>
<td>No</td>
<td>yes</td>
<td>0.5390</td>
<td>0.1383</td>
<td>0.7793</td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>yes</td>
<td></td>
<td>0.4346</td>
<td>0.9044</td>
<td>0.6375</td>
</tr>
<tr>
<td>Difference (LO-NL)</td>
<td></td>
<td></td>
<td></td>
<td>2.90%</td>
<td>2.46%</td>
<td>5.36%</td>
</tr>
<tr>
<td>Test on Equality of Variances</td>
<td></td>
<td></td>
<td></td>
<td>0.55</td>
<td>6.13**</td>
<td>1.29</td>
</tr>
<tr>
<td>p-values of test on significance of enforcement</td>
<td></td>
<td>No</td>
<td>no</td>
<td>0.0091</td>
<td>0.0000</td>
<td>0.0000</td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>no</td>
<td></td>
<td>0.6647</td>
<td>0.2264</td>
<td>0.2566</td>
</tr>
<tr>
<td>law only (LO) = enforcement (EN)</td>
<td></td>
<td>No</td>
<td>yes</td>
<td>0.2353</td>
<td>0.4323</td>
<td>0.6914</td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>yes</td>
<td></td>
<td>0.7076</td>
<td>0.8728</td>
<td>0.7088</td>
</tr>
<tr>
<td>Difference (EN-LO)</td>
<td></td>
<td></td>
<td></td>
<td>5.20%</td>
<td>9.52%</td>
<td>14.72%</td>
</tr>
<tr>
<td>Test on Equality of Variances</td>
<td></td>
<td></td>
<td></td>
<td>32.02***</td>
<td>14.36***</td>
<td>22.71***</td>
</tr>
</tbody>
</table>
Table 7: Basic tests on significance of insider trading regime

Panel A: Parameter estimates and tests

The table shows estimation results for regression (9). Panel A provides parameters for the three different insider trading regimes. Tests are for the hypothesis that slope coefficients are identical across insider trading regimes. In model 2 we regress announcement returns (cumulated abnormal returns defined over the (-3,+3)-window) on runups (cumulated abnormal returns defined over the (-40,-4)-window) and including year- and country-fixed effects for both subsidiaries and public targets. Model 1 only considers runups without any fixed effects.

In Panel B subsidiaries in countries with enforced insider trading legislation are split according to the quality of their insider trading legislation. The first model in Panel B only considers the U.S. and the second model considers all countries with an insider trading quality score less than 5.

<table>
<thead>
<tr>
<th>Subsidiaries</th>
<th>Public targets</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Model 1</strong></td>
<td><strong>Model 2</strong></td>
</tr>
<tr>
<td>Intercept</td>
<td>-0.0017</td>
</tr>
<tr>
<td>Runup</td>
<td>0.5473***</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>no</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>no</td>
</tr>
<tr>
<td>Observations</td>
<td>195</td>
</tr>
<tr>
<td>R-square</td>
<td>26.77%</td>
</tr>
<tr>
<td><strong>Model 1</strong></td>
<td><strong>Model 2</strong></td>
</tr>
<tr>
<td>Intercept</td>
<td>0.0046**</td>
</tr>
<tr>
<td>Runup</td>
<td>0.0351**</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>no</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>no</td>
</tr>
<tr>
<td>Observations</td>
<td>1,043</td>
</tr>
<tr>
<td>R-square</td>
<td>0.55%</td>
</tr>
<tr>
<td><strong>Model 1</strong></td>
<td><strong>Model 2</strong></td>
</tr>
<tr>
<td>Intercept</td>
<td>0.0053***</td>
</tr>
<tr>
<td>Runup</td>
<td>0.0290***</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>no</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>no</td>
</tr>
<tr>
<td>Observations</td>
<td>15,328</td>
</tr>
<tr>
<td>R-square</td>
<td>0.48%</td>
</tr>
</tbody>
</table>

**Tests**

**Relevance of law (LO-NL)**

| Runup | -0.5122*** | -0.5685*** | -0.0995 | -0.2317 |

**Relevance of enforcement (EN-LO)**

| Runup | -0.0062 | -0.0096 | -0.0195 | -0.0235 |

***, ** and * denote significance at the 1%, 5% and 10% levels respectively.
Panel B: Separating subsidiaries in the enforcement sample according to insider trading quality

<table>
<thead>
<tr>
<th>Parameter estimates</th>
<th>Only U.S. (insider trading quality = 5)</th>
<th>Countries with insider trading quality &lt; 5</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Enforcement (EN)</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0389***</td>
<td>0.0144***</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes</td>
<td>yes**</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes***</td>
<td>yes*</td>
</tr>
<tr>
<td>Observations</td>
<td>8,243</td>
<td>6,237</td>
</tr>
<tr>
<td>R-square</td>
<td>1.28%</td>
<td>1.19%</td>
</tr>
<tr>
<td><strong>Tests</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Relevance of enforcement (EN-LO)</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup difference</td>
<td>0.0007</td>
<td>-0.0238</td>
</tr>
</tbody>
</table>
Table 8: Rule of law and judicial efficiency – partitioned sample

The table shows estimation results for regression (9). Estimations use data for parents of subsidiaries only, provides parameters for the three different insider trading regimes. Tests are for the hypothesis that slope coefficients are identical across insider trading regimes. We regress announcement returns (cumulated abnormal returns defined over the (-3,+3)-window) on runups (cumulated abnormal returns defined over the (-40,-4)-window) including year- and country-fixed effects. The sample is partitioned into countries with a low rule of law score (normalized score below 0.9), countries with a perfect rule of law score (normalized index = 1), countries with low judicial efficiency (normalized score below 0.93), and countries with high judicial efficiency (perfect score on normalized index of 1.0). Cut-offs are chosen to divide the sample into quartiles, which is not possible for top quartiles where the U. S. accounts for more than 50% of the sample.

<table>
<thead>
<tr>
<th>Parameter estimates</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Rule of law low</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0441</td>
<td>1.3952***</td>
<td>1.3952***</td>
<td>0.0756**</td>
<td>1.4843***</td>
<td>1.4843***</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes***</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes***</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>57</td>
<td>30</td>
<td>30</td>
<td>164</td>
<td>27</td>
<td>27</td>
</tr>
<tr>
<td>R-square</td>
<td>39.90%</td>
<td>73.68%</td>
<td>73.68%</td>
<td>14.46%</td>
<td>79.48%</td>
<td>79.48%</td>
</tr>
<tr>
<td><strong>Rule of law high</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0551**</td>
<td>0.0195</td>
<td>0.0195</td>
<td>0.0519**</td>
<td>-0.0013</td>
<td>-0.0013</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes**</td>
<td>yes</td>
<td>yes</td>
<td>yes**</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes*</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>526</td>
<td>480</td>
<td>480</td>
<td>547</td>
<td>402</td>
<td>402</td>
</tr>
<tr>
<td>R-square</td>
<td>10.90%</td>
<td>3.14%</td>
<td>3.14%</td>
<td>10.99%</td>
<td>3.69%</td>
<td>3.69%</td>
</tr>
<tr>
<td><strong>Rule of law high (except U.S.)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0142**</td>
<td>0.0332***</td>
<td>0.0124</td>
<td>0.0109</td>
<td>0.0316***</td>
<td>0.0161***</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes**</td>
<td>yes</td>
<td>yes</td>
<td>yes*</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes</td>
<td>yes***</td>
<td>yes</td>
<td>yes</td>
<td>yes***</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>3,656</td>
<td>10,890</td>
<td>2,647</td>
<td>3,315</td>
<td>11,912</td>
<td>3,669</td>
</tr>
<tr>
<td>R-square</td>
<td>1.28%</td>
<td>1.06%</td>
<td>0.82%</td>
<td>1.25%</td>
<td>1.11%</td>
<td>1.06%</td>
</tr>
</tbody>
</table>

**Tests**

| Relevance of law (LO-NL) |     |     |     |     |     |     |
| Runup                    | 0.0110 | -1.3758*** | -1.3758*** | -0.0237 | -1.4856*** | -1.4856*** |

| Relevance of enforcement (EN-LO) |     |     |     |     |     |     |
| Runup                       | -0.0409* | 0.0137 | -0.0071 | -0.0410* | 0.0329 | 0.0174 |

**Description of sample**

| Cutoff for subsample |     |     |     |     |     |     |
| Total number of observations | 4,239 | 11,400 | 3,157 | 4,026 | 12,341 | 4,098 |

| Observations as percent of sample | 25.59% | 68.82% | 19.06% | 24.30% | 74.50% | 24.74% |

***, ** and * denote significance at the 1%, 5% and 10% levels respectively.
Table 9: Rule of law and judicial efficiency

This table displays estimation results where the baseline regression is augmented by interactions of the runup variable with indices for the rule of law and judicial efficiency. Panel A of this table provides parameter estimates for the three different insider trading regimes. Estimations use data for parents of subsidiaries only. Tests are for the hypothesis that slope coefficients are identical across insider trading regimes. We regress announcement returns (cumulated abnormal returns defined over the (-3,+3)-window) on runups (cumulated 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. Panel B shows point estimates for the enforcement effect for a low rule of law country (normalized index = 0.5) and a high rule of law country (normalized index = 1.0), a country with a low judicial efficiency score (normalized index = 0.6) and a high judicial efficiency score (normalized index = 1.0). Estimates are calculated according to equation (11).

Panel A: Parameter estimates and tests

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Parameter estimates</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>No Law (NL)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>-5.0078***</td>
<td>-2.6396***</td>
<td>-5.1950***</td>
</tr>
<tr>
<td>Runup * Rule of law</td>
<td>6.0410***</td>
<td>-</td>
<td>6.5714**</td>
</tr>
<tr>
<td>Runup * Judicial efficiency</td>
<td>-</td>
<td>3.6313***</td>
<td>-0.3421</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>195</td>
<td>195</td>
<td>195</td>
</tr>
<tr>
<td>R-square</td>
<td>53.75%</td>
<td>52.49%</td>
<td>53.76%</td>
</tr>
<tr>
<td><strong>Law Only (LO)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0887*</td>
<td>0.1335**</td>
<td>0.1469**</td>
</tr>
<tr>
<td>Runup * Rule of law</td>
<td>-0.0666</td>
<td>-</td>
<td>0.0864</td>
</tr>
<tr>
<td>Runup * Judicial efficiency</td>
<td>-</td>
<td>-0.1190</td>
<td>-0.2174</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes**</td>
<td>yes**</td>
<td>yes**</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes**</td>
<td>yes*</td>
<td>yes**</td>
</tr>
<tr>
<td>Observations</td>
<td>1,043</td>
<td>1,043</td>
<td>1,043</td>
</tr>
<tr>
<td>R-square</td>
<td>7.64%</td>
<td>7.75%</td>
<td>7.79%</td>
</tr>
<tr>
<td><strong>Enforcement (EN)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>-0.0588*</td>
<td>-0.0608</td>
<td>-0.0749*</td>
</tr>
<tr>
<td>Runup * Rule of law</td>
<td>0.0916***</td>
<td>-</td>
<td>0.0701</td>
</tr>
<tr>
<td>Runup * Judicial efficiency</td>
<td>-</td>
<td>0.0920**</td>
<td>0.0377</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes**</td>
<td>yes*</td>
<td>yes**</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes**</td>
<td>yes***</td>
<td>yes***</td>
</tr>
<tr>
<td>Observations</td>
<td>15,328</td>
<td>15,328</td>
<td>15,328</td>
</tr>
<tr>
<td>R-square</td>
<td>1.09%</td>
<td>1.08%</td>
<td>1.09%</td>
</tr>
<tr>
<td><strong>Tests</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Relevance of law (LO-NL)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>5.0965***</td>
<td>2.7731***</td>
<td>5.3418***</td>
</tr>
<tr>
<td>Runup * Rule of law</td>
<td>-6.1076***</td>
<td>-</td>
<td>-6.4850**</td>
</tr>
<tr>
<td>Runup * Judicial efficiency</td>
<td>-</td>
<td>-3.7502***</td>
<td>0.1247</td>
</tr>
<tr>
<td><strong>Relevance of enforcement (EN-LO)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>-0.1475***</td>
<td>-0.1944***</td>
<td>-0.2218***</td>
</tr>
<tr>
<td>Runup * Rule of law</td>
<td>0.1582**</td>
<td>-</td>
<td>-0.0163</td>
</tr>
<tr>
<td>Runup * Judicial efficiency</td>
<td>-</td>
<td>0.2110**</td>
<td>0.2550</td>
</tr>
</tbody>
</table>

***, ** and * denote significance at the 1%, 5% and 10% levels respectively.
Panell B: Estimates of total runup coefficients from equation (11)

<table>
<thead>
<tr>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Beta Law only</td>
<td>0.0220</td>
<td>0.0145</td>
</tr>
<tr>
<td>Beta Enforcement</td>
<td>0.0328</td>
<td>0.0311</td>
</tr>
<tr>
<td>Enforcement effect</td>
<td>0.0108</td>
<td>0.0166</td>
</tr>
</tbody>
</table>

High quality legal system
Rule of Law = 1.0, Judicial Efficiency = 1.0

| Beta Law only  | 0.0553      | 0.0621      | 0.0597      |
| Beta Enforcement| -0.0130     | -0.0057     | -0.0172     |
| Enforcement effect | -0.0683     | -0.0678     | -0.0769     |

Low quality legal system
Rule of Law = 0.5, Judicial Efficiency = 0.6
Table 10: Splitting the sample according to insider trading quality

The table shows estimation results for regression (9). Estimations use data for parents of subsidiaries only. Tests are for the hypothesis that slope coefficients are identical across insider trading regimes. The sample is split according to the index of insider trading quality for index levels from 1 to 4. We provide parameters for the three different insider trading regimes and test whether these parameters differ from each other. We regress announcement returns (cumulated abnormal returns defined over the (-3,-3)-window) on runups (cumulated abnormal returns defined over the (-40,-4)-window), and include year- and country-fixed effects.

<table>
<thead>
<tr>
<th>Parameter estimates</th>
<th>Insider trading quality</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Index =1</td>
</tr>
<tr>
<td>No Law (NL)</td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>-</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>-</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>-</td>
</tr>
<tr>
<td>Observations</td>
<td>-</td>
</tr>
<tr>
<td>R-square</td>
<td>-</td>
</tr>
<tr>
<td>Law Only (LO)</td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0755</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>90</td>
</tr>
<tr>
<td>R-square</td>
<td>17.79%</td>
</tr>
<tr>
<td>Enforcement (EN)</td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0053</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>93</td>
</tr>
<tr>
<td>R-square</td>
<td>0.0524</td>
</tr>
</tbody>
</table>

Tests

Relevance of law (LO-NL)

| Runup | n.a.  | n.a.  | -0.0682  | -1.1501*** |

Relevance of enforcement (EN-LO)

| Runup         | -0.0702     | -0.1560*** | 0.0275    | -0.0355*   |

***, ** and * denote significance at the 1%, 5% and 10% levels respectively.
Table 11: Newspaper circulation and accounting standards - partitioned sample

The table shows estimation results for regression (9). Estimations use data for parents of subsidiaries only. Tests are for the hypothesis that slope coefficients are identical across insider trading regimes. We regress announcement returns (cumulated abnormal returns defined over the (-3,+3)-window) on runups (cumulated abnormal returns defined over the (-40,-4)-window), and include year- and country-fixed effects. The sample is partitioned into countries with low newspaper circulation, countries with high newspaper circulation, countries with low accounting standards, and countries with high accounting standards.

<table>
<thead>
<tr>
<th>Parameter estimates</th>
<th>(1) Newspaper circulation low</th>
<th>(2) Newspaper circulation high</th>
<th>(3) Accounting standards low</th>
<th>(4) Accounting standards high</th>
</tr>
</thead>
<tbody>
<tr>
<td>No Law (NL)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0441</td>
<td>0.7214***</td>
<td>0.0632*</td>
<td>1.5653***</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes*</td>
<td>yes</td>
<td>yes***</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>55</td>
<td>139</td>
<td>174</td>
<td>20</td>
</tr>
<tr>
<td>R-square</td>
<td>22.63%</td>
<td>41.78%</td>
<td>14.71%</td>
<td>83.01%</td>
</tr>
<tr>
<td>Law Only (LO)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0374*</td>
<td>0.0093</td>
<td>0.0593**</td>
<td>0.0565</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes*</td>
<td>yes</td>
<td>yes**</td>
<td>yes***</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes</td>
<td>yes</td>
<td>yes***</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>538</td>
<td>288</td>
<td>612</td>
<td>160</td>
</tr>
<tr>
<td>R-square</td>
<td>10.08%</td>
<td>4.25%</td>
<td>11.16%</td>
<td>9.69%</td>
</tr>
<tr>
<td>Enforcement (EN)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0192**</td>
<td>0.0166***</td>
<td>0.0205***</td>
<td>0.0082</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes</td>
<td>yes***</td>
<td>yes**</td>
<td>yes</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>2,459</td>
<td>3,712</td>
<td>3,050</td>
<td>3,168</td>
</tr>
<tr>
<td>R-square</td>
<td>1.64%</td>
<td>1.34%</td>
<td>1.73%</td>
<td>0.63%</td>
</tr>
<tr>
<td>Tests</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Relevance of law (LO-NL)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>-0.0067</td>
<td>-0.7121***</td>
<td>-0.0039</td>
<td>-1.5088***</td>
</tr>
<tr>
<td>Relevance of enforcement (EN-LO)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>-0.0182</td>
<td>0.0073</td>
<td>-0.0388</td>
<td>-0.0483***</td>
</tr>
<tr>
<td>Description of sample</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cutoff for subsample</td>
<td>&lt; 0.259</td>
<td>&gt; 0.365</td>
<td>&lt;0.82</td>
<td>&gt; 0.89</td>
</tr>
<tr>
<td>Total number of observations</td>
<td>3,052</td>
<td>4,139</td>
<td>3,836</td>
<td>3,348</td>
</tr>
<tr>
<td>Observations as percent of sample</td>
<td>18.42%</td>
<td>24.98%</td>
<td>23.16%</td>
<td>20.21%</td>
</tr>
</tbody>
</table>

***, ** and * denote significance at the 1%, 5% and 10% levels respectively.
Table 12: Newspaper circulation and accounting standards

This table displays estimation results where the baseline regression is augmented by interactions of the runup variable with newspaper circulation and a score for accounting standards. Estimations use data for parents of subsidiaries only. Tests are for the hypothesis that slope coefficients are identical across insider trading regimes. We regress announcement returns (cumulated abnormal returns defined over the (-3,+3)-window) on runups (cumulated abnormal returns defined over the (-40,-4)-window), on normalized newspaper circulation, normalized accounting standards, both interacted with runup and include year- and country-fixed effects. Model 1 only considers newspaper circulation, while Model 2 only considers accounting standards.

<table>
<thead>
<tr>
<th>Parameter estimates</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>No Law (NL)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0164</td>
<td>-5.7665***</td>
<td>-5.9567***</td>
</tr>
<tr>
<td>Runup * News</td>
<td>1.7335**</td>
<td>0.8592*</td>
<td></td>
</tr>
<tr>
<td>Runup * Accounting</td>
<td>-</td>
<td>7.9502***</td>
<td>7.8226***</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes*</td>
<td>yes***</td>
<td>yes***</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes</td>
<td>yes***</td>
<td>yes***</td>
</tr>
<tr>
<td>Observations</td>
<td>195</td>
<td>195</td>
<td>195</td>
</tr>
<tr>
<td>R-square</td>
<td>38.70%</td>
<td>70.17%</td>
<td>70.68%</td>
</tr>
<tr>
<td><strong>Law Only (LO)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0473*</td>
<td>0.1205</td>
<td>0.1207</td>
</tr>
<tr>
<td>Runup * News</td>
<td>-0.0413</td>
<td>-0.0342</td>
<td></td>
</tr>
<tr>
<td>Runup * Accounting</td>
<td>-</td>
<td>-0.1067</td>
<td>-0.0970</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes**</td>
<td>yes***</td>
<td>yes***</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes**</td>
<td>yes*</td>
<td>yes*</td>
</tr>
<tr>
<td>Observations</td>
<td>1,043</td>
<td>992</td>
<td>992</td>
</tr>
<tr>
<td>R-square</td>
<td>7.54%</td>
<td>7.21%</td>
<td>7.22%</td>
</tr>
<tr>
<td><strong>Enforcement (EN)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0305***</td>
<td>0.0569</td>
<td>0.0597</td>
</tr>
<tr>
<td>Runup * News</td>
<td>-0.0056</td>
<td>-0.0035</td>
<td></td>
</tr>
<tr>
<td>Runup * Accounting</td>
<td>-</td>
<td>-0.0336</td>
<td>-0.0355</td>
</tr>
<tr>
<td>Country-dummies</td>
<td>yes**</td>
<td>yes*</td>
<td>yes**</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes***</td>
<td>yes***</td>
<td>yes***</td>
</tr>
<tr>
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<td>15,290</td>
<td>15,303</td>
<td>15,265</td>
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<td>1.02%</td>
<td>1.03%</td>
</tr>
<tr>
<td><strong>Tests</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Relevance of law (LO-NL)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0309</td>
<td>5.8869***</td>
<td>6.0774***</td>
</tr>
<tr>
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<td>-1.7748**</td>
<td>-</td>
<td>-0.8934*</td>
</tr>
<tr>
<td>Runup * Accounting</td>
<td>-</td>
<td>-8.0569***</td>
<td>-7.9196***</td>
</tr>
<tr>
<td>Relevance of enforcement (EN-LO)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>-0.0169</td>
<td>-0.0636</td>
<td>-0.0609</td>
</tr>
<tr>
<td>Runup * News</td>
<td>0.0357</td>
<td>-</td>
<td>0.0306</td>
</tr>
<tr>
<td>Runup * Accounting</td>
<td>-</td>
<td>0.0731</td>
<td>0.0615</td>
</tr>
</tbody>
</table>

***, ** and * denote significance at the 1%, 5% and 10% levels respectively.
Table 13: Winsorized sample

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<tr>
<th>Parameters</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>No Law (NL)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.1119***</td>
<td>-0.4143**</td>
<td>-0.3572***</td>
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<td>-</td>
<td>0.6825</td>
<td>-</td>
</tr>
<tr>
<td>Runup * Judicial efficiency</td>
<td>-</td>
<td>-0.1628</td>
<td>-</td>
</tr>
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<td>Runup * News</td>
<td>-</td>
<td>-</td>
<td>0.1569</td>
</tr>
<tr>
<td>Runup * Accounting</td>
<td>-</td>
<td>-</td>
<td>0.4807***</td>
</tr>
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<td>Country-dummies</td>
<td>yes***</td>
<td>yes***</td>
<td>yes***</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes***</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>195</td>
<td>195</td>
<td>195</td>
</tr>
<tr>
<td>R-square</td>
<td>23.78%</td>
<td>29.15%</td>
<td>24.11%</td>
</tr>
<tr>
<td><strong>Law Only (LO)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0280**</td>
<td>0.1260***</td>
<td>0.1820***</td>
</tr>
<tr>
<td>Runup * Rule of law</td>
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<td>0.1532</td>
<td>-</td>
</tr>
<tr>
<td>Runup * Judicial efficiency</td>
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<td>-0.2609**</td>
<td>-</td>
</tr>
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<td>Runup * Accounting</td>
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<td>-</td>
<td>-0.1907***</td>
</tr>
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<td>Country-dummies</td>
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<td>yes**</td>
<td>yes***</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes**</td>
<td>yes**</td>
<td>yes</td>
</tr>
<tr>
<td>Observations</td>
<td>1,043</td>
<td>1,043</td>
<td>992</td>
</tr>
<tr>
<td>R-square</td>
<td>7.13%</td>
<td>7.74%</td>
<td>7.34%</td>
</tr>
<tr>
<td><strong>Enforcement (EN)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Runup</td>
<td>0.0265***</td>
<td>0.0237***</td>
<td>0.0286***</td>
</tr>
<tr>
<td>Runup * Rule of law</td>
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<td>-0.0102</td>
<td>-</td>
</tr>
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<td>Runup * Judicial efficiency</td>
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<td>-0.0095</td>
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<td>yes***</td>
<td>yes***</td>
</tr>
<tr>
<td>Year-dummies</td>
<td>yes***</td>
<td>yes***</td>
<td>yes***</td>
</tr>
<tr>
<td>Observations</td>
<td>15,328</td>
<td>15,328</td>
<td>15,265</td>
</tr>
<tr>
<td>R-square</td>
<td>1.11%</td>
<td>1.11%</td>
<td>1.10%</td>
</tr>
</tbody>
</table>

**Tests**

**Relevance of law (LO-NL)**

<table>
<thead>
<tr>
<th>Parameters</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
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<td>-0.0839**</td>
<td>0.5403***</td>
<td>0.5392***</td>
</tr>
<tr>
<td>Runup * Rule of law</td>
<td>-</td>
<td>-0.5294</td>
<td>-</td>
</tr>
<tr>
<td>Runup * Judicial efficiency</td>
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</tr>
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<td>-</td>
</tr>
<tr>
<td>Runup * Accounting</td>
<td>-</td>
<td>-0.6714***</td>
<td>-</td>
</tr>
</tbody>
</table>

**Relevance of enforcement (EN-LO)**

<table>
<thead>
<tr>
<th>Parameters</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
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<td>-0.1534**</td>
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<tr>
<td>Runup * Rule of law</td>
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<td>-</td>
</tr>
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</tr>
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<td>-</td>
<td>0.0285</td>
</tr>
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<td>-</td>
<td>0.1812**</td>
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</tbody>
</table>

***, ** and * denote significance at the 1%, 5% and 10% levels respectively.
The table shows estimation results for regression (9). This table provides results where both runups and markups have been winsorized at the one percent level. Estimations use data for parents of subsidiaries only. Tests are for the hypothesis that slope coefficients are identical across insider trading regimes. We regress announcement returns (cumulated abnormal returns defined over the (-3,+3)-window) on runups (cumulated abnormal returns defined over the (-40,-4)-window), on a normalized index for the rule of law, a normalized index for judicial efficiency, normalized newspaper circulation, and a normalized index of accounting standards all interacted with runup, and include year- and country-fixed effects.
References


