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Insider Trading and the Market Abuse Directive: Are Voluntary and Mandatory Takeover Bids Different?

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Abstract

This study analyzes the effectiveness of the Market Abuse Directive (MAD) in reducing possible profits from insider trading during takeover bids. Exploiting the quasi-experimental setting provided by the introduction of the MAD, our event-study analysis on the Italian market suggests that the new regulation did produce effects, for mandatory offers, on the magnitude of abnormal returns and volumes noted before their announcement. Instead, we find no effect for voluntary offers, which prove to be intrinsically different from the latter ones. Multivariate econometric analyses based on regression and matching methods confirm this result. We interpret our results in light of the choice problem of the optimal amount of insider trading, based on the comparison of marginal costs and benefits of the illegal activity, after considering the differences between voluntary and mandatory offers.

Keywords. Market Abuse Directive; Takeover bids; Event study

JEL Classification. K2 · K4 · G34 · G14

1. Introduction

This work contributes to the debate on the effectiveness of the insider-trading regulation. We specifically consider the effect of the Market Abuse Directive (MAD) in reducing insider trading during takeover bids in Italy, by paying specific attention to the difference between voluntary and mandatory offers.

In many countries, primary and secondary regulation intended to prevent and punish the abuse of privileged information (so-called 'insider trading') has been in place for quite a long time (Linciano 2003). As Rose and Søpstad (2015: 248) note,

> '(t)he motivation for regulating insider trading is based upon an assumption that markets will function better and be more efficient if all participants are on an 'equal footing'.'

In a related vein, European regulation (Directive 2014/57/EU) states that

> '(a)n integrated and efficient financial market and stronger investor confidence requires market integrity. The smooth functioning of securities markets and public confidence in markets are prerequisites for economic growth and wealth. Market abuse harms the integrity of financial markets and public confidence in securities, derivatives and benchmarks.'

If insider trading (Madura et al. 2014) increases the conflict between minority shareholders (outsiders) and managers and/or controlling shareholders (insiders), an effective regulation should reduce agency costs and therefore increase firm value, even though the literature is not unanimous on the negative role of insider trading in the agency relationship (Beny 2008).[1] Beny (2008) finds that, in common-law countries, stricter insider-trading regulation and enforcement increase firm value. However, the same result is not found in civil-law countries, where, as Beny (2008: 292) notes, 'investor protections are relatively weaker'. Therefore, the effectiveness of insider-trading regulation

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[1] As Madura et al. (2014: 515) note, '(t)he theoretical foundation for insider-trading arguments is the assumption that insiders will only reveal their private information if they can profitably trade on it. If there is no legal way to profit from insider information, and the enforcement of anti-insider-trading laws is efficient, there would be no motivation for insiders to hold onto their private information in order to eventually profit from it.'

may depend on the institutional context. Furthermore, the introduction of insider-trading regulation does not always produce the intended effects.[2] Bhattacharya and Daouk (2002) show that an effective insider-trading regulation requires proper enforcement. Bris (2005), examining a sample of 52 countries, adds that the introduction of insider-trading regulation makes, at times, insider trading even more profitable without appreciably decreasing its frequency. As Bris (2005: 309) notes, this puzzling result is at the heart of the 'fundamental dilemma' faced by regulators: 'by prohibiting insider trading, they make it more profitable'.

In the European Union, insider-trading regulation was initially contained in the Market Abuse Directive (Directive 2003/6/CE – MAD) (Engelen 2006; Forbes 2013). In 2011, the European Commission proposed a reform of the MAD to address its heterogeneous application in individual member states. Such reform came to fruition in 2014 with the transformation of the MAD into Regulation 596/2014 (MAR) and the introduction of Directive 2014/57/EU (MAD2), intended to establish a framework for criminal prosecution. Both MAR and MAD2 became effective on 3 July 2016.

The literature on the effects of the MAD shows mixed results about its effectiveness. Christensen et al. (2011, 2016) find improved liquidity after the introduction of the MAD. In addition, the MAD has proved effective in reducing pre-event informed trading: among others, Shahzad and Mertens (2017) focus on earnings announcements in Germany, while Prevoo and Ter Weel (2010) analyze corporate news announcements in the Netherlands. However, analyzing the profitability of legal trading made by insiders, Bartosz Gębka et al. (2017) find that the MAD had no appreciable effects on returns generated by insider portfolios.

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[2] Indeed, a recent strand of literature questions the inspiring principles and legitimacy of insider-trading laws (e.g., Padilla 2011). While we deem mentioning this strand of literature important, discussing it in depth would take us too far afield with respect to the main topic of our research.

Our study contributes to this debate by comparing abnormal price movements before the announcement of takeover bids in Italy, pre-MAD (from May 1998 to December 2005) and post-MAD (from January 2006 to July 2015). Monteiro et al. (2007) provide a similar comparison for the UK after the introduction of the Financial Services and Markets Act, but, to the best of our knowledge, no similar study exists that investigates the effects of the MAD and distinguishes between mandatory and voluntary takeovers – a distinction that can be crucial in determining and interpreting the results. More specifically, we investigate two research hypotheses: (i) if the MAD were effective against illegal insider trading, after its application we should observe lower abnormal returns before the bid announcement and higher abnormal returns in the announcement day (Bris 2005); (ii) in addition, we expect to observe a stronger MAD effect for mandatory bids than for voluntary bids. For the reasons we detail later, mandatory bids have a higher potential risk of insider trading as they are typically preceded by lengthy negotiations between the target (listed) firm and the bidder (often an unlisted firm).

Before describing the steps of our analysis, we think it useful to explain why we focused on takeovers bids in Italy. We choose to analyze takeover bids because, as noted by Keown and Pinkerton (1981), Meulbroek (1992), Bris (2005) and Pattitoni et al. (2015), they are operations in which the risk of insider trading is particularly high. For example, during the period covered by our analysis, most of all insider-trading investigations started by the Italian National Commission for Companies and the Stock Exchange (Consob) concerned takeover bids. In these operations, the abuse of privileged information can be very profitable. Moreover, focusing on this type of corporate event allows us to better identify the day when the operation is publicly announced, since the official announcement of takeover bids is rarely preceded by other press releases that can influence markets. On the other hand, we focus on Italy given the evidence of reduced effectiveness of insider-trading regulation in civil-law countries. Additionally, outsider protection in Italy is considered limited (Melis 2000; Volpin 2002; Di Carlo 2014). As Volpin (2002: 62) notes, Italy is a country that 'features weak legal

protection of creditors and shareholders, inefficient law enforcement, high ownership concentration'. This low protection may indeed simplify the econometric identification, as it worsens the problems caused by agency conflicts between outsiders and insiders. Moreover, focusing our attention to Italy allows us to bypass institutional heterogeneity issues. Finally, and most importantly, the number of Italian voluntary and mandatory offers is quite balanced. These favorable conditions simplify the econometric comparison of the effect of the MAD between cases of voluntary and mandatory offers. Thus, given the extent and heterogeneity of the operations we analyze, Italy is an ideal setting for our analysis.

We test our research hypotheses in different steps. In the first step of our analysis, we exploit the quasi-experimental setting (Muravyev 2013, Quinn 2014) provided by the introduction of the MAD and test, by means of an event study, if the new regulation effectively discouraged insider-trading tied to takeover bids. This analysis specifically aims at evaluating whether the introduction of the MAD produced appreciable effects on the magnitude of abnormal returns and volumes registered before the announcement of takeover bids, when the information on the operation is still confidential. In a second step, based on regression and matching methods, we check the robustness of our results in a multivariate setting. These latter analyses allow us to control for relevant idiosyncratic features of our data set and to facilitate the generalization of our conclusions to other contexts. In this second step, we also analyze more in depth the different impact of the MAD on voluntary and mandatory bids, by using a 'Difference-in-Difference'-like approach.

Both steps reveal that implementation of the MAD had a limited effect (Bartosz Gębka et al., 2017). We find a reduction in post-MAD abnormal returns for the sample of mandatory bids only. These results seem unjustified in the light of the regulation changes introduced by the MAD, but can be rationalized considering the objective limits in proving insider trading to the competent bodies (especially when insiders try to hide their operations) and the structural differences between voluntary and mandatory bids. These findings contribute to the debate on the revision to the MAD until more

data on the application of MAR and MAD2 become available and, more generally, provides further evidence on the conflicting results of insider-trading regulation (Bhattacharya and Daouk, 2002; Bris, 2005).

The paper is organized as follows. In Section 2, we provide some background for our analysis. More specifically, we illustrate the changes in insider-trading regulation arising from the adoption of the MAD and the approval of the savings-protection reform law (L. 28/12/05, n. 262) in Italy. Furthermore, we explain the theoretical background we use to interpret our empirical results. In Section 3, we present our empirical analysis. First, we describe our sample and methodology. Then, we present our univariate and multivariate analyses, and interpret the main findings. In Section 4, we provide some policy suggestions and draw together our conclusions.

2. Background and Theoretical framework

2.1 Regulatory Context in Italy

Our empirical analysis proposes a comparison between the pre- and post-MAD adoption periods in Italy. For the reasons explained below, the dividing line is set to 31 December 2005. In this section, we illustrate the Italian institutional context and how adopting the provisions of the European Directive modified Italian regulation.

Insider trading is a form of market abuse. The dense body of preventive and punitive provisions on insider trading intends to ensure the integrity of financial markets and to increase investor confidence. Until recent revisions (Regulation 596/2014 (MAR) and Directive 2014/57/EU (MAD2), effective starting from 3 July 2016), European legislation on insider trading was based on the MAD (Directive 2003/6/EC of January 28, 2003), which sets forth the combined rules to contrast insider trading and market manipulation (together defined as market abuses). The adoption of the MAD followed the so-called Lamfalussy Procedure. This procedure regulates the cascading application of a first-level

directive (the MAD) and a series of second-level directives (2003/124/EC, 2003/125/EC, 2004/72/EC) and European regulation (2003/2272/EC). The procedure also relies on the guidelines of the Committee of European Securities Regulators (CESR/04-505b, CESR/06-562b, CESR/09-219), now the European Securities and Markets Authority.

In Italy, the MAD was adopted by Law no. 62 of 18 April 2005, coming into effect on 12 May 2005. This law included changes to the Finance Act (*Testo Unico della Finanza,* TUF), the primary Italian regulatory source for financial markets. Because of these changes, Consob reformed the Issuers' Regulation (*Regolamento Emittenti*) and the Market Regulation (*Regolamento Mercati*). Emended secondary rules concerning market abuse were effective on 1 January 2006, except for some new prevention measures (i.e. the Insider List) effective on 1 April 2006.

Analyzing potential insider profits from legal insider dealing, Gębka et al. (2017) set the MAD implementation date to May 2005. However, we choose to postpone our reference date to December 2005, since the reform process in Italy came into force on 1 January 2006.[3] To check the robustness of our results, we also test the May 2005 date.

According to Annunziata (2006), insider-trading regulation in Italy has undergone profound changes following the introduction of the MAD. Here, we briefly summarize the main changes arising from the adoption of the MAD that focus on aspects related to insider trading.

- **Broadening of the definition of privileged information.** The MAD broadened the definition of 'privileged information'. The new definition extends to events or circumstances that have not yet occurred, but which could reasonably occur. This extension requires that listed firms pay greater attention to the process of formation, circulation, and communication of privileged information to third parties, so as to avoid market abuse. Not all scholars agree on this point,

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[3] Despite the full enforcement of the new rules in 2006, the first application of penalties was in the Spring of 2007 (Ifil, Exor, CMI, Unipol).

though. Iemma and Carbonaro (2005) argue that the new definition of 'privileged information' does not broaden the old one: although the pre- and post-MAD definitions *formally* differ, they coincide *in practice* for Consob when it comes to application.

- **Legitimization of the delay in communication of privileged information.** Public communication of privileged information may be postponed when the disclosure of information could hurt the legitimate interests of the listed firms, provided the delay does not mislead the public. However, the firm must guarantee the confidentiality of the privileged information for the entire period of the delay.

- **Limitation of selective disclosure.** The MAD limits the risk of privileged information being disclosed only to certain parties (selective disclosure), therefore allowing disclosure to third parties only through 'the normal course of the exercise of his employment, profession, or duties.' If the third party is not bound by obligations of confidentiality, the privileged information must be immediately provided to the public in order to restore the conditions of parity of information. The disclosure to the public must be simultaneous in the event of intentional selective disclosure and 'without delay' in the case of unintentional selective disclosure.

- **New prevention measures.** The MAD introduced new prevention measures intended to promote traceability of offenses and transparency of operations conducted by primary insiders and refined the existing measures. Listed firms must create a record of persons with access to privileged information (Insider List). This record must be regularly updated. In addition, intermediaries are obliged to report operations that could have originated in insider trading or market manipulation. Reporting is made only to the competent authorities, without informing the subjects on behalf of whom the operations were performed.

- **Introduction of administrative penalties in addition to the criminal penalties already in place.** One of the most innovative elements of the MAD is the introduction of further administrative penalties. Because of Law No. 262/2005, following the adoption of the MAD, the amounts of administrative penalties increased fivefold: from a minimum of €60,000 to a maximum of €15,000,000. Administrative penalties were introduced also for secondary insiders and, more generally, for anyone capable of recognizing the 'privileged' nature of the acquired information.

- **Reinforcing of the powers of Consob.** The MAD provided increased investigation and verification powers to Consob, which also has the option of directly applying administrative penalties.

2.2 Theoretical Framework

Our analysis tests the effectiveness of the MAD in reducing possible profits from insider trading during takeover bids. We pay particular attention to the difference in the effect of the MAD on voluntary and mandatory offers, since these operations are very different. This section provides a simple theoretical framework to interpret our results.

Let us imagine that the potential insider weighs the benefits and the expected costs when selecting the magnitude of the insider-trading operation. Suppose that the benefits are an increasing and concave function $B(q)$ of the amount of insider trading q, with $B'(q) > 0$, and $B''(q) < 0$. Let us assume also that the cost of being caught by the authorities is proportional (through the constant θ) to the amount of insider trading: $C(q) = \theta q$. Then, if we indicate with π the probability of being caught, the expected costs of insider trading are equal to $\pi C(q) = \pi \theta q$, and the economic problem of the potential insider is the following:

$$\max_{q} B(q) - \pi\theta q, \tag{1}$$

the maximum of which is implicitly determined by the condition $B'(q*) = \pi\theta$.[4] A simple comparative statics exercise reveals that the optimal amount of insider trading decreases with the increase of the *ex-ante* penalty and with an increase in the *ex-post* probability of being caught:

$$\frac{\partial q*}{\partial \theta} = \frac{\pi}{B''(q*)}, \text{ and } \frac{\partial q*}{\partial \pi} = \frac{\theta}{B''(q*)}. \tag{2}$$

Therefore, we may expect that increasing the size of the penalty (θ) reduces the amount of insider trading and, in this sense, the MAD represents a step forward from previous regulation. In fact, most of the new elements introduced by the MAD, as discussed in Section 2.1, are geared towards increasing θ. On the other hand, the success of insider-trading regulation requires the presence of effective *ex-post* monitoring activities by competent bodies (enforcement). If due to the objective difficulties in investigating cases of insider trading the probability of being caught is relatively low (or close to zero), the *ex-ante* punitive measures may be only partially effective (or even non-effective, since in Equation (2) $\pi \to 0$ implies $\partial q*/\partial\theta \to 0$). In such case, the only concern of the insiders would be 'masking' their operations so to further decrease π, a result consistent with the 'stealth trading hypothesis' (Barclay and Warner 1993; Ryu, 2012), which predicts that potential insider traders perform small transactions in order not to reveal their confidential information.

From a general standpoint, our simple model relies on the interpretative framework that distinguishes between the introduction of insider trading regulation and its actual enforcement.[5] This framework is

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[4] This formulation is based on a more general model by Becker (1968) on the economics of crime and is presented in these terms in Balducci et al. (2001). A similar model is applied to the topic of tax evasion in Allingham and Sandmo (1972).

[5] Regarding the case of Italy, only limited information is available on investigation activities and enforcement by Consob (see Consob's annual reports from 1998 to 2015). Alleged cases of abuse of privileged information went down, in the post-MAD period, from 12 to 4.2 per year. The number of involved subjects also went down from 98.5 to 13.4 per year.

consistent with the perspective put forth by Bhattacharya and Daouk (2002). After analyzing insider

trading laws and enforcement for a global sample of 103 countries, the authors note that:

> '… the easy part—the establishment of insider trading laws—is not associated with a reduction in the
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> cost of equity. It is the difficult part—the enforcement of insider trading laws—that is associated with
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> a reduction in the cost of equity in a country.'

From a more specific point of view, our theoretical framework can be a key to reading studies that

question the effectiveness of the MAD. Specifically, our model encompasses all the explanations of

Gębka et al. (2017) for the substantial ineffectiveness of the MAD (even if the authors refer to legal

trading made by insiders), excluding the one based on a gradual and delayed effect of the MAD,

which we can discard, as we analyze a period of ten years after the regulation change. Gębka et al.

(2017) propose three more explanations. First, the authors suggest that the penalties for insider trading

provided by the MAD are not strict enough. Second, the authors suggest that pre-MAD regulations

were already good enough so that the marginal effect of the introduction of the MAD was therefore

limited. In our model, these two explanations are accounted for by the parameter θ. However, given

the significant changes introduced by the MAD (see Section 2.1), we believe that these two

explanations do not fit well in our context. The third explanation is that the MAD was ineffective due

to enforcement problems, which translates into low values of the parameter π, which make the MAD

only partially effective even if the *ex-ante* punitive measures increase. This explanation is consistent

with the viewpoint by Bhattacharya and Daouk (2002), and would be the most likely one to apply to

our setting, were we to empirically find that the MAD is ineffective: takeovers are complex

operations, rendering cases of insider trading difficult to detect and investigate.

So, our theoretical framework suggests that the effect of MAD is not obvious, and depends heavily

on the level of enforcement. This theoretical framework could also suggest a difference in the effect

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Variation in figures does not allow to make inference on the level of enforcement, as they can be seen either as being due
to the effect of the new regulation or as an indication of constraints to enforcement.

of the MAD on voluntary and mandatory offers. As mentioned in Section 1, the information process behind mandatory bids generally lasts longer than the one behind voluntary ones, since mandatory bids are preceded by a negotiation phase between the target and the bidder. According to the MAD, all listed firms must fill an insider list, indicating all people with access to privileged information.[6] In the case of mandatory offers, this list makes it easy for the competent bodies to identify all people directly or indirectly involved in the operation, facilitating possible investigations. In the case of voluntary offers, target firms are often unaware of the takeover attempt, and therefore their insider lists are less informative. In this situation, it is more difficult for the competent bodies to detect potential insider trading cases. All this may possibly result in greater values of π in the case of mandatory offers than for voluntary ones. In the situation of mandatory offers, the measures provided by the MAD may have found a more fertile ground for application.

In the next section, we will specifically examine the difference in the effect of the MAD on voluntary and mandatory offers, in addition to an analysis of its overall effectiveness.

3. Empirical Analysis

3.1 Sample and Variables

Our initial data set includes the population of Italian takeover bids occurred between May 1998 and July 2015 according to the archives of the Italian stock exchange (Borsa Italiana S.p.A.), for a total of 288 takeover bids and 335 target shares (some bids targeted multi-class stocks). From this data set, we exclude the operations preceded by events that might have prepared the financial community for a potential takeover bid[7] (and which would therefore make it impossible to determine whether

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[6] At an earlier stage, a similar list, the 'relevant information list', must be created when information has the potential to become 'privileged'.

[7] An example is the voluntary takeover bid of convertible savings shares of Marzotto in September 2007, which was preceded by a previous one in July 2007 (included in our sample). For the same reason, we excluded all (mandatory) residual offers.

abnormal market movements occurred), and those for which it was not possible to obtain the necessary data for the analysis. We point out that missing values are mostly attributable to nonsystematic misreporting by our data sources. For this reason, while we cannot completely rule out the risk of selection bias, we deem this risk as unlikely to play a major role in our case. The final sample, which has a size greater than similar works in the literature (e.g. Linciano, 2003; Pattitoni et al. 2015), includes 218 takeover bids concerning 242 target shares, which can be classified as follows: 211 ordinary shares, 26 savings shares, and 5 preferred shares. Pre-MAD observations (target shares) are 146 and post-MAD observations are 96.

In our sample, voluntary takeover bids have 146 observations, while mandatory offers have 96 observations.[8] In a voluntary takeover, the offeror is free to set the bid price and the number of shares willing to acquire. In a mandatory offer, the bid price and the number of shares must be set according to regulation: a) the bid price has to be the highest payed to the target's stakeholders (through a direct negotiation or market operations) to buy shares representing the 30% or more of voting rights (the threshold that makes the takeover bid mandatory); b) the bid is addressed to all the stakeholders for all their holdings of ordinary shares.

Half of voluntary takeovers have insider promoters, while the other half have outsider promoters. Consistent with similar studies in the literature, we classify as insider-promoted bids the voluntary takeovers initiated by a shareholder directly or indirectly holding at least 20 per cent of voting rights and a share at least twice the one of the second-largest shareholder. The 20 per cent threshold is typically used in the literature on corporate control (La Porta et al. 1999; Faccio and Lang 2002). The condition of the largest shareholder's holding being at least twice the one of the second largest is related to the two-thirds quorum required by the Italian law for the control of the extraordinary shareholders' meeting (Bajo et al. 2011).

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[8] By pure chance, the number of voluntary and mandatory bids coincide with the number of pre- and post-MAD observations.

For each of the 242 observations of our data set, we collect the following information from the offer prospectuses and Datastream on the firm undergoing the takeover bid:

- the degree of **ownership concentration**, measured by the share of the largest shareholder;

- two dummy variables indicating **industrial** and **financial firms**;

- a dummy variable indicating **ordinary shares**;

- **target firm size**, measured by the natural logarithm of market capitalization in millions;

- the degree of **liquidity** of the share, measured by the turnover ratio in the 250 days prior to the observation period.

Table 1 shows the composition of our sample in terms of bid type (mandatory vs voluntary) and promoter origin (inside vs outside), as well as some descriptive statistics.

Table 1. Sample composition and descriptive statistics

Variable	Mean	SD	Min	Max
Voluntary bids	0.6033	0.4902	0	1
Insider-promoted bids	0.3017	0.4599	0	1
Ownership concentration	0.4910	0.2184	0	1
Industrial firm	0.4132	0.4934	0	1
Financial firm	0.3058	0.4617	0	1
Ordinary share	0.8719	0.3349	0	1
Firm size	5.3124	1.8608	0	10.6439
Liquidity (x 10^2)	0.3494	1.6531	0.0002	25.61

3.2 Methodology

Our analysis intends to determine the effect of the introduction of the MAD on insider-trading activities. The occurrence of insider-trading activities is typically proven through careful investigation by the competent authorities (e.g., Consob). However, abnormal changes in the prices and volumes of shares undergoing a takeover bid during the days preceding the announcement may be considered as an indication of transactions based on confidential information (Lin and Howe 1990; Bris 2005; Betzer and Theissen 2009; Madura and Marciniak 2014). For example, the Committee of

European Securities Regulators (CESR; now the European Securities and Markets Authority) describes the following possible indicators of insider trading (CESR/04-505b):

> '(u)nusual trading in the shares of a company before the announcement of price sensitive information relating to the company; transactions resulting in sudden and unusual changes in the volume of orders and shares prices before public announcements regarding the security in question.'

An event-study approach (Brown and Warner 1985; Ajinkya and Jain 1989; MacKinley 1997; Muravyev 2013; Kleinow et al. 2014) can be used to determine the effects of a particular event on the trade prices and volumes of shares, and therefore represents an indirect method to identify the presence of insider trading.[9] As Bris (2005: 309) notes, '(t)his approach circumvents the difficulties of drawing any empirical conclusion from the sole use of detected insider trading'.

To employ an event-study approach, we need to identify the moment in which the privileged information becomes public (the so-called event date, indicated by convention as $t = 0$). Based on the TUF, this happens through the dissemination of a press release. Therefore, in our study the event date is the one of the press release announcing the takeover bid according to Article 102 (or 114) of the TUF, or the following day, if the press release is issued when the market is closed.

Once the date of the event is identified, we can calculate the abnormal returns (ARs) and volumes (AVs) that occur concurrently with it. The abnormal return for the security i at time t is defined as:

$$AR_{it} = r_{it} - E(r_{it}) \tag{3}$$

where AR_{it}, r_{it} and $E(r_{it})$ are the abnormal return, the observed return and the expected return (or normal return), respectively. To calculate the expected return, we use the market model and the

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[9] In the case of the Italian market, Baccolini et al. (1991) use the event-study methodology on a sample of 18 firms in the period before the approval of the national anti-insider-trading law (Law 157/1991). They find evidence of abnormal returns in the days preceding the publication of privileged information concerning acquisitions, earnings surprises or changes in ownership.

industry-adjusted model (Campbell et al. 1997; Cervellati et al. 2014). The two models produce similar results. Therefore, for the sake of simplicity, we report the results of the industry-adjusted model (results obtained using the market model are available upon request). In the industry-adjusted model, we estimate $E(r_{it})$ as the return of the sector stock index corresponding to the examined securities.[10] The overall reaction of the market in the period prior to the communication of the takeover bid is calculated as the cumulative abnormal return (CAR) of the 25 days preceding the notification of the takeover bid (Jensen and Ruback 1983; Mikkelson and Ruback 1985; Sanders and Zdanowicz 1992; Bris 2005).[11,12] Having computed the ARs and the CARs for each of the target stocks in our sample, we calculate the average AR and CAR and test the statistical significance of the results.[13]

ARs and CARs are imperfect proxies to measure the MAD effect, since their pre- and post-MAD dynamics also reflect possible changes in bid-premiums due to variations in economic conditions and/or in takeover regulation.[14] To overcome this confounding effect, we compute an additional variable: the ratio CAR(–25,–1)/CAR(–25,0). Given that CAR(–25,–1) and CAR(–25,0) typically have a concordant sign, and that the majority of market reaction occurs in $t = 0$ (so that in general |CAR(–25,0)| > |CAR(–25,–1)|), this variable (henceforth, CAR-ratio) may be interpreted as an indicator of the part of total market reaction that is attributable to trading activities made before the

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[10] We believe that there are theoretical reasons for preferring the industry-adjusted model in the context of our study. In the market model, the expected return is estimated using a regression model. Since the degree of liquidity of some of the securities in our sample is relatively limited, the application of this model could result in an underestimation of the beta of the security, with a consequent overestimate of abnormal returns. This model could also hide certain structural characteristics of the security, which, however, a sectoral index (being composed of comparable firms) considers.

[11] In the literature, the length of the pre-event window is generally set between 10 and 60 days (Sanders and Zdanowic, 1992). Our choice of 25 days follows the one of Keon and Pinkerton (1981) and is halfway between the 30 days of Prevoo and ter Weel (2010) and the 20 days of Linciano (2003).

[12] Since we are interested in the potential profit for an insider trader, we revert the sign of the CAR (and AR_{i0}) when the bid price is below the stock price at time $t = –26$; in this situation the insider can profit assuming a short position on the target share and a self-financing long position on the industry index.

[13] We consider both parametric (Boehmer et al. 1991), non-parametric (Wilcoxon test) and bootstrap tests. As the results obtained using the three methods are qualitatively similar, we only report the traditional parametric tests.

[14] In fact, European and national rules on takeover bids have changed in the period we consider. While these changes possibly affected the number of offers and bid-premiums, it is unlikely that they affected insider-trading activity. Moreover, according to Dissanaike et al. (2019), the European Takeover Directive has not led to significant changes in the Italian regulation.

announcements. An advantage of this variable over the simple CAR(–25,–1) is that it corrects, by putting together two CARs referring to the same event, for the potential effect of the bid premium on the size of the CARs, and for potential unexplained heterogeneity (Linciano (2003) uses a similar ratio). However, the empirical distribution of this variable presents some issues: some outliers inflate the standard deviation of the distribution, and some negative values make this indicator more difficult to interpret. To solve at least the second issue, we provide additional robustness checks using only positive CAR-ratios (CAR-ratio+).

We perform a similar event study on the volumes (measured using the turnover ratio).[15] The abnormal volume for the security i at time t is:

$$AV_{it} = V_{it} - E(V_{it}) \tag{4}$$

where AV_{it}, V_{it} and $E(V_{it})$ are the abnormal volume, the observed volume, and the expected volume (or normal volume), respectively. To estimate the expected volume, we use the average turnover ratio of the security calculated in an estimation window of 250 days which range from $t = -275$ to $t = -26$ from the day of the event. The overall market reaction in the period prior to the notification of the takeover bid is calculated by summing the abnormal volumes (CAV). Based on the AVs and the CAVs of the individual stocks, we calculate the average AV and CAV and test the statistical significance of the results.

To conclude this section, it is worth mentioning that, in the case of insider trading, it is plausible to observe abnormal returns and volumes in the days preceding the announcement. However, the presence of abnormal returns and volumes could also be attributable to the capacity of the market to anticipate the event and/or to rumors circulating in the media. To consider abnormal returns and

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[15] Several measures for volumes and abnormal volumes exist. However, different measures often produce comparable results (Cervellati et al. 2014). We follow the approach of Pacini and Marlett (2001) and Clarkson et al. (2006).

volumes as a *true* indicator of insider trading, we therefore need to control for these alternative explanations in our empirical analysis.

3.3 Event Study

3.3.1 Voluntary and Mandatory Bids

Since voluntary and mandatory bids are intrinsically different, in this section we compare the market reaction to these two types of bids with respect to potential insider trading activity. Table 2 shows the event study results for voluntary and mandatory bids over the entire period (i.e. we do not yet distinguish between the pre- and post-MAD period).

Table 2. Event study and voluntary and mandatory bids difference tests. Full sample (242 obs.)

Variable	Voluntary (146 obs.)			Mandatory (96 obs.)			Difference		
	Coef	SE	Sign	Coef	SE	Sign	Coef	SE	Sign
CAR(–25,0)	0.2007	0.0134	***	0.1283	0.0158	***	0.0724	0.0207	***
CAR(–25,–1)	0.0863	0.0101	***	0.0695	0.0125	***	0.0168	0.0161	
AR(0)	0.1144	0.0085	***	0.0588	0.0113	***	0.0555	0.0142	***
CAR-ratio	0.3377	0.1759	*	0.7831	0.1810	***	–0.4455	0.2524	*
CAR-ratio+ (206 obs.)	0.6953	0.0800	***	1.2589	0.1785	***	–0.5637	0.1956	***
CAV(–25,–1)	–0.0154	0.0345		0.0262	0.0092	***	–0.0416	0.0357	
AV(0)	0.0445	0.0079	***	0.0199	0.0035	***	0.0246	0.0086	***

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Voluntary bids show higher abnormal returns than mandatory bids in the announcement day and no difference in prior days. However, considering the CAR-ratio, the fraction of abnormal returns occurred before the announcement (when the knowledge of the bid was a privileged information) is larger for mandatory takeovers. As for abnormal volumes, voluntary bids exhibit greater abnormal volumes on the announcement day and smaller abnormal volumes in the pre-event period, though the latter difference is not statistically significant. All in all, we conclude that the voluntary and mandatory samples have specific characteristics, which may be worth additional separate analyses.

Among voluntary bids, we make a further distinction between insider- and outsider-promoted to consider possible differences in the behavior of different actors. However, the results presented in Table 3 do not reveal any statistical difference.

Table 3. Event study and insider- and outsider-promoted bids difference tests. Voluntary bids (146 obs.)

Variable	Insider (73 obs.)			Outsider (73 obs.)			Difference		
	Coef	SE	Sign	Coef	SE	Sign	Coef	SE	Sign
CAR(–25,0)	0.2055	0.0161	***	0.1958	0.0215	***	0.0097	0.0268	
CAR(–25,–1)	0.0786	0.0129	***	0.0940	0.0157	***	–0.0153	0.0203	
AR(0)	0.1269	0.0108	***	0.1019	0.0131	***	0.0250	0.0170	
CAR-ratio	0.1475	0.3046		0.5278	0.1750	***	–0.3803	0.3513	
CAR-ratio+ (129 obs.)	0.5819	0.0663	***	0.8104	0.1456	***	–0.2285	0.1600	
CAV(–25,–1)	0.0129	0.0099		–0.0437	0.0682		0.0566	0.0689	
AV(0)	0.0491	0.0144	***	0.0398	0.0064	***	0.0093	0.0158	

*p < 0.10, **p < 0.05, ***p < 0.01

3.3.2 The Effect of the MAD

In this section, we assess whether the introduction of the MAD reduced the size of abnormal returns and volumes before the announcement of the takeover bid.

In Table 4, we compare the CARs and the CAVs from the pre-MAD period with those from the post-MAD period. The results show significant CARs in both the pre- and post-MAD periods. However, the difference between the pre- and post-MAD CARs is not statistically significant. These results seem to suggest that the introduction of the MAD has not reduced abnormal returns before the announcement. Similar results apply to abnormal volumes.

Table 4. Event study and pre- and post-MAD difference tests. Full sample (242 obs.)

Variable	Pre-MAD (146 obs.)			Post-MAD (96 obs.)			Difference		
	Coef	SE	Sign	Coef	SE	Sign	Coef	SE	Sign
CAR(–25,0)	0.1758	0.0144	***	0.1662	0.0147	***	–0.0096	0.0206	
CAR(–25,–1)	0.0865	0.0108	***	0.0692	0.0112	***	–0.0174	0.0155	
AR(0)	0.0893	0.0089	***	0.0970	0.0115	***	0.0078	0.0145	
CAR-ratio	0.4539	0.1864	**	0.6064	0.1583	***	0.1526	0.2445	
CAR-ratio+ (206 obs.)	0.8707	0.0950	***	0.9650	0.1645	***	0.0944	0.1899	
CAV(–25,–1)	–0.0134	0.0345		0.0232	0.0091	**	0.0366	0.0357	
AV(0)	0.0362	0.0077	***	0.0326	0.0048	***	–0.0036	0.0091	

*p < 0.10, **p < 0.05, ***p < 0.01

Before drawing any conclusions on these first results, we present some robustness checks. As mentioned in the previous section, pre-announcement abnormal returns do not necessarily imply insider trading, for example because of the market's capacity to anticipate the event and/or of rumors. As a robustness check, in Table 5 we recalculate all figures excluding cases that could be influenced by rumors. To this aim, we exclude observations of events partially anticipated by articles appearing in the main national financial newspapers (Il Sole 24 Ore and MF-Milano Finanza) in the five days preceding the official announcement. After excluding 58 cases from the sample, we do not find changes to the results of Table 4, except for CAV(–25,–1), which is now not significant, even post-MAD. In the light of these results, it is therefore plausible that the presence of CARs in the days prior to the communication is attributable (at least in part) to insider trading. Furthermore, significant CARs and non-significant CAVs seems to suggest that the transactions generating abnormal returns are limited in magnitude. This evidence is consistent with the aforementioned 'stealth trading hypothesis' (Barclay and Warner 1993; Ryu, 2012), which suggests that insiders often execute small transactions in order not to disclose their private information.

Table 5. Event study and pre- and post-MAD difference tests. No rumors (184 obs.)

Variable	Pre-MAD (110 obs.)			Post-MAD (74 obs.)			Difference		
	Coef	SE	Sign	Coef	SE	Sign	Coef	SE	Sign
CAR(–25,0)	0.1910	0.0176	***	0.1776	0.0171	***	–0.0134	0.0245	
CAR(–25,–1)	0.0886	0.0133	***	0.0748	0.0132	***	–0.0138	0.0187	
AR(0)	0.1024	0.0108	***	0.1028	0.0131	***	0.0005	0.0169	
CAR-ratio	0.3628	0.2259		0.4885	0.0996	***	0.1257	0.2469	
CAR-ratio+ (158 obs.)	0.8165	0.0833	***	0.7373	0.0821	***	–0.0793	0.1169	
CAV(–25,–1)	–0.0318	0.0454		0.0110	0.0074		0.0429	0.0460	
AV(0)	0.0366	0.0096	***	0.0278	0.0048	***	–0.0088	0.0108	

*p < 0.10, **p < 0.05, ***p < 0.01

A further critical aspect, when assessing the effects of a new regulation, is the choice of the implementation date. As an additional robustness check, we report in Table 6 updated results based on the same implementation date (May 2005) adopted by Gębka et al. (2017). This new exercise confirms our previous findings.

Table 6. Event study and pre- and post-MAD difference tests. Full sample (242 obs.), implementation date is May 2005

Variable	Pre-MAD (129 obs.)			Post-MAD (113 obs.)			Difference		
	Coef	SE	Sign	Coef	SE	Sign	Coef	SE	Sign
CAR(–25,0)	0.1866	0.0160	***	0.1553	0.0129	***	–0.0312	0.0205	
CAR(–25,–1)	0.0862	0.0120	***	0.0722	0.0098	***	–0.0140	0.0155	
AR(0)	0.1004	0.0094	***	0.0832	0.0105	***	–0.0172	0.0141	
CAR-ratio	0.3982	0.2067	*	0.6471	0.1421	***	0.2489	0.2509	
CAR-ratio+ (206 obs.)	0.8284	0.1025	***	1.0021	0.1429	***	0.1737	0.1758	
CAV(–25,–1)	–0.0206	0.0389		0.0259	0.0081	***	0.0466	0.0398	
AV(0)	0.0391	0.0087	***	0.0298	0.0042	***	–0.0093	0.0096	

*p < 0.10, **p < 0.05, ***p < 0.01

Considering these results, we can draw some first conclusions. As shown by Bris (2005), effective insider-trading regulation implies an increase of the reaction in the day of the announcement and a reduction in the prior days, a result that our analysis seems to reject. Therefore, although the adoption of the MAD formally reinforced insider-trading regulation, the price dynamics observed in the days preceding the announcement of the takeover bid seem to contain, even post-MAD, indications of insider trading that are very similar to those in the pre-MAD period. Considering the theoretical framework provided in Section 2.2, these findings seem to suggest that the problem may be due to the ability of insiders to hide their transactions to the competent bodies (stealth-trading hypothesis), or to the objective limits in proving insider trading for the competent bodies. These two problems may result in a decrease of the ex-post probability of being caught, despite the positive effect of the ex-ante increase in punitive measures.

Given the differences between voluntary and mandatory takeovers presented above in Table 2, we now test whether the effectiveness of the MAD depends on bid characteristics. Tables 7 and 8 report separate pre- and post-MAD abnormal returns and volumes for voluntary and mandatory takeovers.

Table 7. Event study and pre- and post-MAD difference tests. Voluntary bids (146 obs.)

Variable	Pre-MAD (95 obs.)			Post-MAD (51 obs.)			Difference		
	Coef	SE	Sign	Coef	SE	Sign	Coef	SE	Sign
CAR(–25,0)	0.1960	0.0184	***	0.2094	0.0174	***	0.0135	0.0253	
CAR(–25,–1)	0.0803	0.0131	***	0.0974	0.0158	***	0.0171	0.0205	
AR(0)	0.1156	0.0109	***	0.1120	0.0136	***	–0.0036	0.0175	
CAR-ratio	0.1856	0.2525		0.6210	0.1769	***	0.4353	0.3083	
CAR-ratio+ (129 obs.)	0.6609	0.0688	***	0.7595	0.1904	***	0.0986	0.2024	
CAV(–25,–1)	–0.0376	0.0526		0.0259	0.0114	**	0.0635	0.0538	
AV(0)	0.0473	0.0114	***	0.0393	0.0074	***	–0.0080	0.0136	

*p < 0.10, **p < 0.05, ***p < 0.01

Table 8. Event study and pre- and post-MAD difference tests. Mandatory bids (96 obs.)

Variable	Pre-MAD (51 obs.)			Post-MAD (45 obs.)			Difference		
	Coef	SE	Sign	Coef	SE	Sign	Coef	SE	Sign
CAR(–25,0)	0.1382	0.0224	***	0.1171	0.0224	***	–0.0211	0.0317	
CAR(–25,–1)	0.0981	0.0190	***	0.0371	0.0146	**	–0.0609	0.0240	**
AR(0)	0.0401	0.0127	***	0.0800	0.0189	***	0.0399	0.0228	*
CAR-ratio	0.9535	0.2406	***	0.5900	0.2739	**	–0.3635	0.3646	
CAR-ratio+ (77 obs.)	1.2623	0.2309	***	1.2541	0.2867	***	–0.0082	0.3681	
CAV(–25,–1)	0.0316	0.0117	***	0.0201	0.0145		–0.0115	0.0186	
AV(0)	0.0154	0.0044	***	0.0251	0.0056	***	0.0096	0.0071	

*p < 0.10, **p < 0.05, ***p < 0.01

Results show that only mandatory bids exhibit significant differences between the two periods. We observe a decline of the pre-announcement CAR and an increase of the abnormal reaction on the announcement. This result seems to suggest that the MAD is effective in the case of mandatory bids. This important finding was discussed in Section 3.2, and is possibly due to the structural differences between voluntary and mandatory bids that may result in greater values of the probability of being caught for the latter.

The non-significant findings for voluntary bids deserve additional investigation. Tables 9 and 10 present separate results for insider- and outsider-promoted voluntary bids. However, the results do not reveal any statistical difference between pre- and post-MAD abnormal returns and volumes.

Table 9. Event study and pre- and post-MAD difference tests. Insider-promoted voluntary bids (73 obs.)

Variable	Pre-MAD (50 obs.)			Post-MAD (23 obs.)			Difference		
	Coef	SE	Sign	Coef	SE	Sign	Coef	SE	Sign
CAR(–25,0)	0.1943	0.0215	***	0.2298	0.0206	***	0.0355	0.0297	
CAR(–25,–1)	0.0712	0.0159	***	0.0948	0.0220	***	0.0236	0.0271	
AR(0)	0.1232	0.0136	***	0.1350	0.0179	***	0.0119	0.0225	
CAR-ratio	0.0412	0.4455		0.3787	0.0807	***	0.3375	0.4527	
CAR-ratio+ (65 obs.)	0.6024	0.0932	***	0.5324	0.0331	***	–0.0700	0.0989	
CAV(–25,–1)	0.0162	0.0130		0.0057	0.0138		–0.0105	0.0190	
AV(0)	0.0572	0.0207	***	0.0316	0.0092	***	–0.0256	0.0226	

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table 10. Event study and pre- and post-MAD difference tests. Outsider-promoted voluntary bids (73 obs.)

Variable	Pre-MAD (45 obs.)			Post-MAD (28 obs.)			Difference		
	Coef	SE	Sign	Coef	SE	Sign	Coef	SE	Sign
CAR(–25,0)	0.1978	0.0309	***	0.1927	0.0267	***	–0.0051	0.0409	
CAR(–25,–1)	0.0905	0.0214	***	0.0996	0.0226	***	0.0090	0.0311	
AR(0)	0.1073	0.0176	***	0.0931	0.0195	***	–0.0142	0.0263	
CAR-ratio	0.3461	0.2047	*	0.8200	0.3125	**	0.4739	0.3736	
CAR-ratio+ (64 obs.)	0.7316	0.1027	***	0.9254	0.3273	***	0.1938	0.3431	
CAV(–25,–1)	–0.0974	0.1102		0.0425	0.0170	**	0.1399	0.1115	
AV(0)	0.0363	0.0077	***	0.0455	0.0112	***	0.0092	0.0136	

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

3.4 Multivariate Analysis

3.4.1 The Overall Impact of the MAD

The results of our event study show significant pre-event CARs in both the pre- and post-MAD periods. However, excluding the case of mandatory bids, their difference is not statistically significant, suggesting a limited role of the MAD in reducing abnormal returns. A potential problem with the univariate approach of our event study is that, when comparing results pertaining to different subsamples, any result, either significant or non-significant, may be due to unobserved heterogeneity (in our case, differences in the characteristics of the firms in the pre- and post-MAD periods) (Angrist and Pischke, 2008). Thus, in this section we exploit the quasi-experimental setting provided by the introduction of the MAD and check whether idiosyncratic features of our sample drive the negligible effect of this regulation change. We specifically focus on the three variables that in Sections 3.3 appear to be the most informative for our analysis: CAR(–25,0), CAR(–25,–1) and AR(0).

We start by carrying out difference tests for firm characteristics, distinguishing between the pre- and post-MAD periods. These tests, presented in Table 11, reveal a certain degree of heterogeneity. The most significant difference concerns the proportion of ordinary shares: this is probably due to the structural reduction in the number of savings and preferred shares compared to ordinary shares observed in Italy during the period considered (Mancinelli and Ozkan 2006). Other statistically weaker differences concern the fraction of voluntary and insider-promoted bids, which are both lower in the post-MAD period, the ownership concentration, and the incidence of industrial firms among targets.

Table 11. Pre- and post-MAD difference tests. Full sample (242 obs.)

Variable	Pre-MAD (146 obs.)	Post-MAD (96 obs.)	Diff	Sign
Voluntary bids	0.6507	0.5313	–0.1194	*
Insider-promoted bids	0.3425	0.2400	–0.1029	*
Ownership concentration	0.5105	0.4614	–0.0490	*
Industrial firm	0.3699	0.4792	0.1093	*
Financial firm	0.3288	0.2708	–0.0579	
Ordinary share	0.8082	0.9688	0.1605	***
Firm size	5.2696	5.3775	0.1080	
Liquidity	0.0041	0.0026	–0.0014	

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$. Two-sample t-tests for continuous variables and two-sample tests of proportions for dummy variables

Since the two subsamples are unbalanced, we perform, as a further robustness check, a multivariate analysis to estimate the effect of the MAD after controlling for firm characteristics. We consider two approaches. The first approach is based on regression adjustment techniques, where pre- and post-MAD CARs are compared after adjusting for the effect of control variables (Verbeek, 2012). More specifically, we run the following regression

$$y_i = \boldsymbol{\beta}'\mathbf{x}_i + \boldsymbol{\gamma}'\mathbf{x}_i p_i + \varepsilon_i, \tag{5}$$

where y_i is the dependent variable (either CAR(–25,0), CAR(–25,–1) or AR(0)), $\mathbf{x}_i$ is a vector of control variables (see Table 11) including a constant, p_i is a variable indicating if the observations is relative to the post-MAD period, ε_i is an error term, and $\boldsymbol{\beta}$ and $\boldsymbol{\gamma}$ indicate the vectors of parameters to be estimated. The effect of the MAD is calculated by considering the contrast of predictive margins,

that is, the average of the difference between the prediction of y_i when $p = 1$ (post-MAD) and $p = 0$ (pre-MAD) (Cameron and Trivedi, 2005).

The second approach is based on nearest-neighbor matching (Angrist and Pischke, 2008; Greene, 2011), where we compare pre- and post-MAD cases which are as similar as possible in terms of control variables. More specifically, we match cases based on their Mahalanobis distance computed with respect to the control variables. Then, we calculate the average difference between the CARs of the matched cases (i.e. the average treatment effect).

In Tables 12 and 13, we show the effects of the MAD using the two methods. In both tables, the column and row headings indicate which dependent variable and sample are used for calculating the effect respectively. As it is usual in the literature on the impact of regulatory changes, we report in the intersection of each column and row only the effects of interest (even because the nearest-neighbor matching estimator is nonparametric). The results suggest that, even after accounting for firm characteristics, the effects of the MAD are generally not significant, casting further doubts on its overall effectiveness. As in the univariate analysis of Section 3.3.2, the only exception is the subsample of mandatory bids, for which we observe a reduction of CAR(–25,–1) and increase of AR(0). This result again suggests that the MAD is effective for mandatory bids only.

Table 12. Effect of MAD. Regression adjustment

Sample	CAR(–25,0)	CAR(–25,–1)	AR(0)
Full sample	0.0081	–0.0145	0.0226
	[0.0231]	[0.0159]	[0.0159]
Voluntary	0.0113	0.0067	0.0046
	[0.0281]	[0.0227]	[0.0216]
Mandatory	–0.0136	–0.0689***	0.0552**
	[0.0343]	[0.0243]	[0.0223]
Outsider	0.0316	–0.0201	0.0517
	[0.0664]	[0.0442]	[0.0790]
Insider	0.0276	0.0254	0.0022
	[0.0292]	[0.0257]	[0.0200]

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table 13. Effect of MAD. Nearest-neighbor matching

Sample	CAR(–25,0)	CAR(–25,–1)	AR(0)
Full sample	0.011	–0.0181	0.0291
	[0.0269]	[0.0208]	[0.0183]
Voluntary	0.0042	0.0024	0.0018
	[0.0281]	[0.0285]	[0.0229]
Mandatory	0.0051	–0.0723**	0.0774***
	[0.0399]	[0.0335]	[0.0253]
Outsider	0.0596	–0.0018	0.0614
	[0.0816]	[0.0405]	[0.0910]
Insider	0.0361	0.0395	–0.0034
	[0.0302]	[0.0355]	[0.0315]

*p < 0.10, **p < 0.05, ***p < 0.01

3.4.2 A Comparison between Voluntary and Mandatory Bids

In this section, we analyze more in depth the different impact of the MAD on voluntary and mandatory bids. To do so, we use a 'Difference-in-Difference'-like approach, where we measure potential differential MAD effects for the subgroups of voluntary and mandatory offers while controlling for firm characteristics.[16] Here the idea is that, since the effect on voluntary offers is negligible, we can consider voluntary offers as a 'quasi-control' or 'almost-non-treated' group and better appreciate the effect on mandatory offers by comparison. Following Greene (2011), we estimate the following model:

$$y_i = \gamma p_i + \varphi m_i + \theta p_i m_i + \boldsymbol{\beta}' \mathbf{x}_i + \varepsilon_i, \qquad (6)$$

where y_i is the dependent variable (either CAR(–25,0), CAR(–25,–1) or AR(0)), p_i is a post-MAD dummy variable, m_i is a dummy variable indicating mandatory bids, $\mathbf{x}_i$ is a vector of control variables, and ε_i is an error term. In Equation (6), γ, φ and θ are parameters and $\boldsymbol{\beta}$ is a vector of parameters. After controlling for the variables in the vector $\mathbf{x}_i$, γ may be interpreted as a 'time trend', φ considers all permanent differences between voluntary and mandatory offers, and θ measures the difference in

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[16] We thank an anonymous reviewer for suggesting this analysis.

the effect of the MAD on voluntary and mandatory offers. Drawing from the literature on takeover premia (Alexandridis et al. 2013; Simonyan 2014; Li and Tong 2018), we insert in vector **x** some target characteristics: ownership concentration, size, stock liquidity, belonging to industrial or financial sectors; other control variables are the presence of rumors before the announcement, the nature of insider-promoted bid, and the type of share.

Table 14 presents the results. We find that the differential MAD effect for mandatory bids is negative for CAR(–25,–1) and positive for AR(0), implying a non-significant effect for CAR(–25,0). These results suggest a decline of the pre-announcement CAR of mandatory bids with respect to voluntary ones and a contextual increase of the abnormal reaction on the announcement day, consistent with the evidence shown in previous sections that the MAD is effective in the case of mandatory bids.

Table 14. Effect of MAD. Voluntary and mandatory bids.

Variable	CAR(–25,0)	CAR(–25,–1)	AR(0)
Post-MAD	0.0075	0.0121	–0.0046
	[0.0270]	[0.0217]	[0.0172]
Mandatory bids	–0.0602*	0.0128	–0.0731***
	[0.0354]	[0.0277]	[0.0198]
Mandatory bids × Post-MAD	–0.0153	–0.0737**	0.0584**
	[0.0418]	[0.0327]	[0.0278]
Insider-promoted bids	–0.0162	–0.0193	0.0031
	[0.0251]	[0.0203]	[0.0170]
Ownership concentration	0.1242**	0.0005	0.1237***
	[0.0515]	[0.0402]	[0.0310]
Industrial firm	0.0723***	0.0293	0.0430**
	[0.0276]	[0.0223]	[0.0168]
Financial firm	0.0662**	0.0161	0.0500***
	[0.0270]	[0.0221]	[0.0166]
Ordinary share	–0.0202	–0.0073	–0.0129
	[0.0368]	[0.0279]	[0.0226]
Firm size	–0.0107*	–0.0054	–0.0053
	[0.0065]	[0.0045]	[0.0047]
Liquidity	–0.4888*	–0.2628	–0.2260
	[0.2518]	[0.1663]	[0.1505]
Rumors	–0.0224	–0.0019	–0.0205
	[0.0232]	[0.0181]	[0.0160]
constant	0.1717***	0.1088**	0.0629*
	[0.0656]	[0.0520]	[0.0355]
N	241	241	241
Adj. R^2	0.11	0.01	0.17

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

From the control variables, we can observe that the market reaction at the announcement of the takeover is greater when the target has concentrated ownership and belongs to the industrial or financial sectors. The size of the target and the liquidity of the shares instead have a negative effect. The latter effect (for liquidity) is the opposite of what expected (Li and Tong 2018).

Figure 1 shows the margin plots of CAR(–25,–1) and AR(0), based on the predictions of the models in Table 14. The plots clearly show that the effect of the MAD on voluntary offers is almost null both when considering CAR(–25,–1) and AR(0). On the other hand, the effect of the MAD on mandatory offers is quite significant: we observe a reduction of about 5 percent points in the pre-announcement reaction (when the information is still confidential) and an increase of the same extent at the announcement date.

Figure 1. Effect of MAD. Voluntary and mandatory bids.
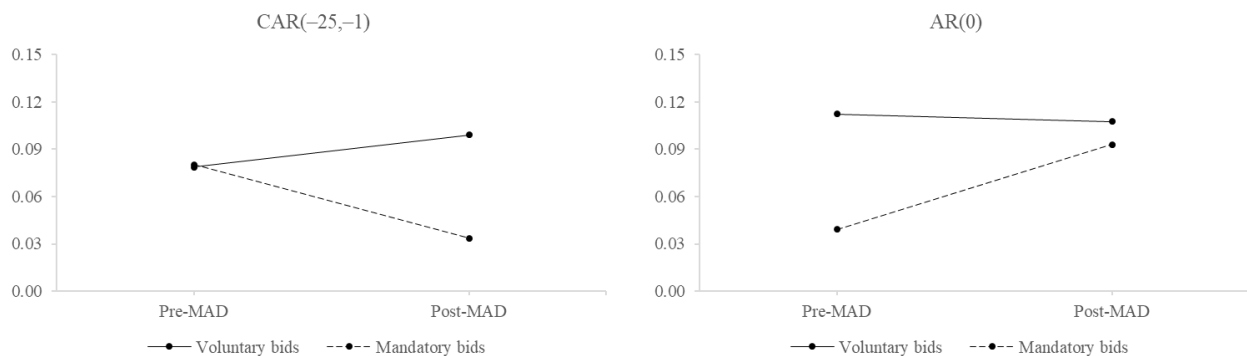
4. Concluding Remarks

Our empirical analysis examined the population of Italian takeover bids occurred between May 1998 and July 2015. We aimed to determine whether the introduction of the MAD resulted in appreciable differences in the potential profits achieved before the announcement of the operation and which can be attributed, at least in part, to insider-trading activities.

Our results, corroborated by several robustness checks, show that the abnormal returns and volumes recorded in the pre- and post-MAD periods do not significantly differ when looking at both insider-

and outsider-promoted voluntary offers, suggesting at least a partial ineffectiveness of the MAD. On the other hand, we found a statistically significant effect in the case of mandatory bids. The results of regression and matching estimators, which exploit the quasi-experimental setting provided by the introduction of the MAD and allow us to increase the generalization potential of our analysis, confirm this main finding after controlling for idiosyncratic features of our sample.

Our findings might seem unjustified in the light of the considerable regulation changes introduced by the MAD. However, some theoretical considerations seem to suggest that the problem is to be found in the attention of insiders in hiding their transactions to the competent bodies. Due to the objective limits in proving insider trading for the competent bodies, the effect of the *ex-ante* increase in punitive measures is plausibly vanished *ex post* by low probabilities of being caught. This theoretical framework can help explain the relative effectiveness of the MAD for mandatory bids. These bids generally last longer than the voluntary ones, being preceded by a negotiation phase with the main shareholders of the target; furthermore, in contrast with voluntary offers, the insider lists are directly or indirectly informative on all people involved in the operation. In this context, where the insider's probability of being caught is possibly higher, the MAD seems to have been effective in deterring the circulation of confidential information by means of instruments like insider lists and limitation of selective disclosure discussed in Section 2.1. Our evidence seems to suggest a clear policy implication: insider lists should be required not only from listed firms, but also from unlisted firms that plan to launch a voluntary takeover bid.

While our results can be easily extended to most civil-law countries in the European Union, as they share the same market rules, we believe that further research could expand our results by considering other markets. Although the Italian market may be considered as an ideal context for our empirical analysis due to its structural characteristics, research on other European countries could investigate further implications of the model, in the context of quasi-experiments (Dharmapala and Khanna 2013). These implications could be useful to stimulate further debate regarding the changes brought

about by the MAD and, more in general, to develop an effective insider-trading regulation. Conditional to data availability, a desirable extension of this work in the spirit of Bhattacharya and Daouk (2002) would be to empirically analyze the difference between the introduction of insider-trading regulation and its effective enforcement. Finally, once the recent changes made to the MAD (MAR and MAD2) are fully enforced and data become available, future studies may be able to test how (and if) these changes have been effective in improving the effectiveness of insider trading regulation.

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