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Why has the FCAs market cleanliness statistic for takeover announcements decreased since 2009?

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After remaining close to 30% for four years, the FCA's market cleanliness statistic for takeovers decreased to about 15% between 2010 and 2013. This paper investigates the possible explanations for the fall in the statistic. We find evidence to exclude methodological bias, information leaking earlier relative to the takeover announcement or changes in sample characteristics as explanations for the decrease. This supports the hypothesis that the recent decrease in the statistic indicates that markets are cleaner. We note that the change in the statistic coincides with a material increase in regulatory enforcement activity and sanctions. Finally, we show that international comparisons of market cleanliness statistics can be misleading, and so should not be made, because countries have different disclosure regimes that affect the overall level of the statistic.

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1. Introduction

Since 2008, the FSA/FCA Annual Report has included our 'market cleanliness statistic' as an indicator of the level of insider trading in UK equity markets. To calculate it, we analyse the scale of share price movements in the two days prior to takeover announcements to identify abnormal movements in share prices. The market cleanliness statistic is then computed as the percentage of those announcements that show abnormal pre-announcement price movements (APPMs). The statistic was first developed in Dubow and Monteiro (2006) and in Monteiro, Zaman and Leittersdorf (2007).[1]

Between 2007 and 2009, the market cleanliness statistic for takeover announcements remained close to 30%. However, from 2010 onwards, we observe a significant decline in the measure to a level of about 15% in 2013. This paper explores different hypotheses that could explain its decline over the last four years.

Figure 1: APPMs as % of announcements over time and averages for the periods 2002 to 2009 and 2010 to 2013. Dashed lines depict the 95% confidence interval.

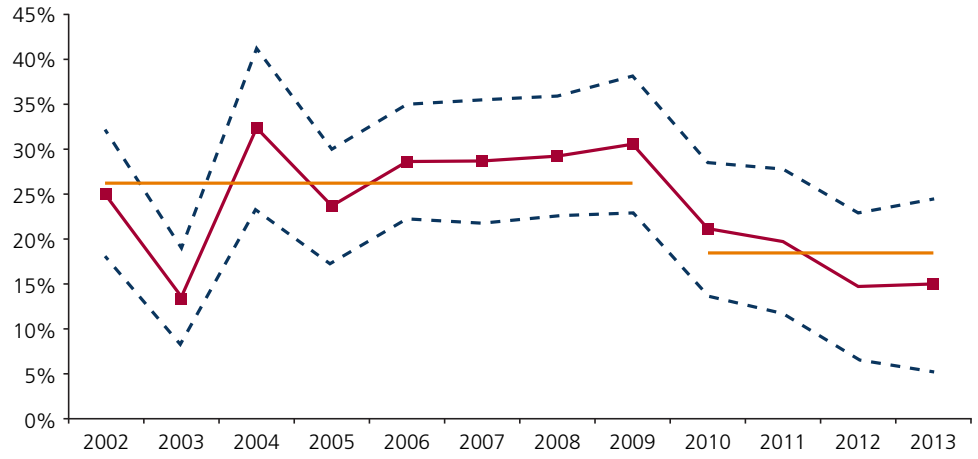


Figure 1 shows the percentage of APPMs measured at the end of each year, along with a 95% confidence interval.[2]

1 From 2008 to 2010 we analysed regulatory announcements of two types, either formal announcements of take-over bids registered by the Takeover Panel or announcements for FTSE 350 stocks headed 'trading statement', 'trading update', 'contract award' or 'drilling report' (as price-sensitive information often appears under these headings). Dubow and Monteiro (2006) provide further details on the selection of announcements. Following concerns over the quality of the input data for FTSE 350 stocks, in our Annual Report 2010/11 we stated that we would only report a market cleanliness measure for takeover announcements.

2 We use the normal approximation method for the binomial proportion confidence intervals.

We find that the market cleanliness statistic for 2010 to 2013 is significantly lower than for the period 2007 to 2009, and we investigate different hypotheses that could explain the decline. We find no evidence of the recent reduction being caused by our methodological choices. Recalculating the measure with a number of changes – different critical values, methods of calculating abnormal returns or event window positions or lengths – yields qualitatively similar results.

Secondly, we explore whether the characteristics of firms that were takeover targets in 2010 to 2013 differed relative to those in 2007 to 2009 and whether this could explain the decline. Although we have seen a steep decline in merger and acquisition activity, we find that firm and deal characteristics are broadly similar from 2010 to 2013 to previous years. Using a regression model, we find that the decline in our measure for 2010 to 2013 remains statistically significant after controlling for changes in firm and deal characteristics and market volatility.

We conclude that the recent fall in the market cleanliness statistic does not seem to be explained by these hypotheses. Instead, it is likely that there is less incorporation of information into stock prices before takeover announcements between 2010 and 2013 than before this period. As such, the fall probably represents reduced informed trading related to takeover announcements.

It is possible that the fall in APPMs is a consequence of market conditions. For example, traders may be more risk averse and less willing than in the past to undertake risky activities, such as insider trading, or career insider traders may have exited the market during the crisis (given that insider trading only became illegal in 1985). It is also possible that insider trading only decreased around takeover events while the cleanliness in other market segments did not change.

While we cannot rule out alternative explanations for the decline, the reduction seen in insider trading coincides with increased regulatory activity. The prominence of the FSA/FCA's insider dealing enforcement since 2009 may have changed traders' perceptions of the likelihood and consequences of being caught. The first quarter of 2009 saw the first successful criminal sentence for insider trading pursued by the FSA and this has been followed by a series of similar high-profile prosecutions against insider trading. When we analyse the market cleanliness statistic quarter by quarter we find that the decline began in the final quarter of 2009.

We also look at possible future avenues of research. Comparing market cleanliness levels internationally for example is conceptually very appealing. However, we find that, in particular, differences in disclosure regimes and differences in other market and firm characteristics make such a comparison difficult to carry out.

2. Measuring market cleanliness for 2010 to 2013

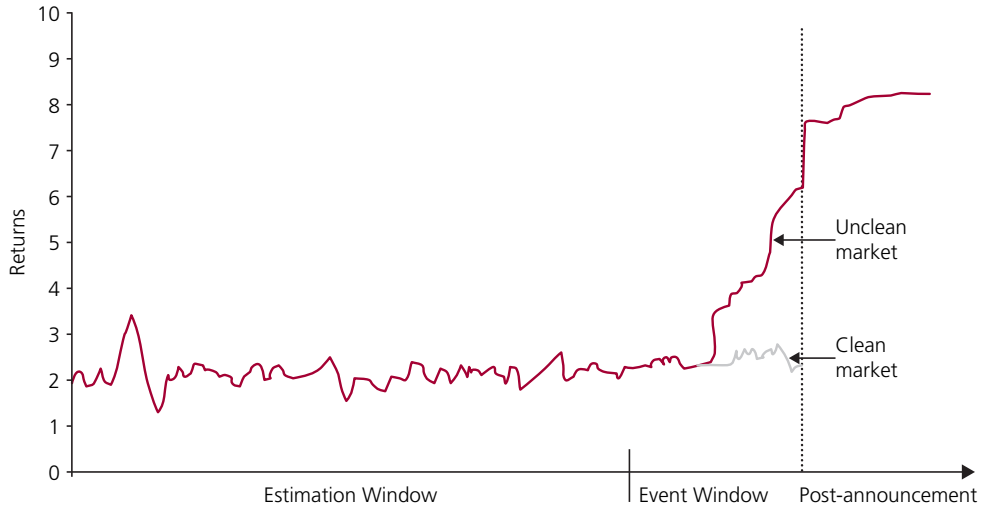
This section explains the calculations of the market cleanliness statistic for recent years and checks that the results are not caused by parameter choices, such as the length of the event window that we examine before the announcement. First, we explain the methodology and the data used. Then, we calculate the statistic with the different parameter choices.

2.1 Methodology

The FCA's methodology for the market cleanliness statistic for takeover announcements is based on Monteiro, Zaman and Leittersdorf (2007). We reiterate the main elements of the method below.

Conceptually, the measure of market cleanliness is very simple. On the publication of a price-sensitive regulatory announcement conveying good news, we expect the company's stock price to increase. This is typically true for takeover targets.[3] Therefore, price increases that are (i) immediately prior to announcements and (ii) significantly different from normal stock movements, can signal insider trading. This is illustrated in Figure 2, below.

Figure 2: Key concept



3 See, for example, Eckbo (2009) for an overview.

To calculate the market cleanliness statistic, as Monteiro, Zaman and Leittersdorf (2007) did, we start by calculating the daily return Ri,t of stock i on day t using:

$$R_{i,t} = \frac{P_{i,t} - P_{i,t-1}}{P_{i,t-1}}$$

where Pi,t is the last price of stock i on day t.

Abnormal returns (AR) are defined as the difference between the actual return (R) and the expected return (ER):

$$AR_{i,t} = R_{i,t} - ER_{i,t}$$

For the analysis of takeover announcements, we use a simple mean model to calculate expected returns. For a stock, the expected return is the mean of the daily returns over the estimation window. Monteiro, Zaman and Leittersdorf (2007) argue that an extended model, such as the capital asset pricing model (CAPM), does not add much value in modelling expected returns[4], in line with Brown & Warner (1980) who show that the difference between mean-adjusted returns and market adjusted is typically small. We further examine this position in this paper.

The event window is the period used to test whether a significant price movement occurred before the announcement. As in Monteiro, Zaman and Leittersdorf (2007), we use a two-day event window. For each stock, we calculate cumulative abnormal returns (CARs) covering the two days immediately prior to the announcement, but not on the day of the announcement. We will use the notation (-p, -q) to characterise a window running from the opening price p days before announcement to the closing price q days before the announcement. In the present case, the two-day event window is (-2,-1).[5] Company i's CAR is the sum of the abnormal returns over these two days:

$$CAR_{i,2} = \sum_{j=1}^{2} AR_{i,t-j}$$

The estimation window is the period used to calculate expected returns. Our annual market cleanliness study uses the period starting 250 trading days prior to announcement and stopping 11 days prior to announcement to predict returns (240 trading days). Actual returns are then deducted from these expected returns to obtain abnormal returns. This (-250, -11) 'clean period' estimation window assumes that leakage of information is unlikely to occur in the UK market more than 10 days prior to announcement. As explained below, this assumption is more likely to hold in markets like the UK, where a strict disclosure regime is effectively enforced. This is explained further in section 2 of Appendix 1.

To determine whether the CAR during this event window constitutes a significant price movement, it is compared against the ninetieth percentile of the empirical abnormal return distribution in the clean period estimation window.[6] Those events with a higher CAR than this threshold value are defined as abnormal pre-announcement price movements (APPMs).

4 The authors used an extended model for the analysis of FTSE350 regulatory announcements. Further on in this paper, we apply this extended model to the analysis of the takeover announcements. We find that the extended model does not materially change the results of the market cleanliness statistic.

5 This two-day window considers the stock return from the end of the third day before the announcement to the end of the day before the announcement.

6 This approach has been advocated in particular for single-firm single-event studies, e.g., in Gelbach and Helland (2013), and Hein and Westfall (2004).

The market cleanliness statistic is the percentage of all takeover announcements that are observed to be associated with APPMs.

Using the ninetieth percentile corresponds to a 10% significance level which means that, on average, for an absolutely clean market we would expect the market cleanliness statistic to be 10%. Therefore, the absolute level of the statistic does not, by itself, convey precise information about the incidence of insider trading before takeover announcements. The measure is sensitive to the choice of the significance level. In particular, it will go down as the threshold increases. However, as we examine in Chapter 2.3, the reduction of recent years is robust to methodological changes.[7]

2.2 Data

The market cleanliness analysis is created from the list of takeover announcements that the Takeover Panel shares with the FCA.[8] There may be several announcements during a given takeover process. The analysis only considers the first public announcement made concerning a given targeted company. This prevents the estimation window from being contaminated by previous takeover announcements.

For this set of announcements, we collect end-of-day prices from Datastream for each target stock. We clean the sample by excluding securities that have no data available, by excluding all stocks for which prices in the event window (-2,-1) are not available and also stocks where we do not have at least 100 days of return data during the estimation window.

2.3 Results for 2002 to 2013

Table 1 shows the market cleanliness statistic for takeover announcements over the period 2002 to 2013.

Table 1: The measure of market cleanliness for takeovers

Year	Announcements	Abnormal pre-announcement price movements (APPMs)	APPMs announcements
2002	147	37	25.1%
2003	160	22	13.8%
2004	102	33	32.4%
2005	177	42	23.7%
2006	199	57	28.6%

7 A higher (lower) threshold reduces (increases) the probability of identifying an announcement as suspicious when it is not but it increases (reduces) the probability of classifying suspicious events as unsuspicious. As discussed in section three, the observed change in the statistic is not due to threshold choices.

8 In the UK, the Panel on Takeovers and Mergers (the Takeover Panel) is an independent body, established in 1968, whose main functions are to issue and administer the City Code on Takeovers and Mergers (the Code) and to supervise and regulate takeovers and other matters to which the Code applies. Its central objective is to ensure fair treatment for all shareholders in takeover bids, http://www.thetakeoverpanel.org.uk/ Our sample only includes takeover announcements submitted to us by the takeover panel. It does not include announcements stating that no takeover is imminent.

2007	167	48	28.7%
2008	181	53	29.3%
2009	144	44	30.6%
2010	118	25	21.2%
2011	96	19	19.8%
2012	74	11	14.9%
2013	53	8	15.1%

The volatility in the statistic over 2002 to 2005 contrasts with the stability around the 28%-30% level of 2006 to 2009. However, the last four years exhibit a material reduction in the statistic. The statistic in the period from 2010 to 2013 is about 19%, compared to 26% in the period from 2002 to 2009, as can be seen by the horizontal lines in Figure 1.

The percentage of APPMs in any given year can vary just by chance. To be able to say that there has been a significant difference in the market cleanliness measure from one year to another, we need to apply a statistical test. We need to distinguish whether the decrease in the market cleanliness statistic is sufficiently large that it most likely represents a shift in the underlying likelihood of APPMs.

Table 2 and Table 3 present tests of the difference in market cleanliness statistics. Table 2 shows the difference of the market cleanliness statistics in the period from 2010 to 2013 compared to 2002 to 2006 and 2007 to 2009. Whether the difference is statistically significant is tested using a Z-test, a statistical test of the difference in proportions.[9] [10] Under the assumptions of the test, the statistic for 2010 to 2013 is significantly lower than the ones for the two earlier periods. Table 3 shows the year-on-year differences for all years starting from 2007. The change from one year to the next can be seen on the lower diagonal in the table. The market cleanliness statistic in each of the years from 2010 to 2013 is significantly lower than the statistic in 2009. The most significant drop in the statistic occurred from 2009 to 2010 while subsequent drops do not differ statistically from the preceding year. The tests therefore suggest that our indicator for market cleanliness decreased in 2010 and remained low thereafter. We assess possible reasons for the reduction in the next section.

9 The tests assumes that each takeover within a given group has the same chance of being an APPM and the chance for each takeover is independent of that of other takeovers. If n1 is the number of observations in group 1, p1 is the probability of a takeover being an APPM, q1 = 1 –p1, n1p1 ≥ 5 and n1q1 ≥ 5 then the Z-statistic = $\frac{\hat{p}_1 - \hat{p}_2}{\sqrt{\hat{p}\hat{q}\left(n_1^{-1} + n_2^{-1}\right)}}$ where p and q = 1-p are the average proportion over both groups and the 'hats' above letters (e.g.) indicate that the proportion is the actual proportion observed, as opposed to the underlying probability. The Z-statistic is approximately standard normal distributed.

10 We also ran tests developed in Monteiro, Zaman and Leittersdorf (2007) (Table A14, page 55) that use the actual distribution of returns in the clean period to estimate the distribution of the market cleanliness statistic and test for changes between years. These tests assume that the distribution in the 'clean period' estimation window, which is assumed to have no insider trading, is the same as in the two days before. As the distribution before the takeover announcement may differ, our preferred tests are the Z-statistic or the logistic regression used later in the paper. The results do not change.

Table 2: Difference In the market cleanliness statistic between the periods 2007-2009 and 2010-2013

Market cleanliness (MC) statistic before and after 2009

Period	Sample	APMs	MC statistic	Difference from 2010 to 2013
2002-2006	785	191	24.33%	5.86%**
2007-2009	492	145	29.47%	11.00%***

* Statistically significant at the 10% level, ** statistically significant at the 5% level, *** statistically significant at the 1% level

Table 3: Year-on-year differences in market cleanliness

Difference in MC statistic from base year

Base year	2007	2008	2009	2010	2011	2012	2013
2007		0.54%	1.81%	-7.56%	-8.95%	-13.88%**	-13.65%**
2008			1.27%	-8.10%	-9.49%*	-14.42%**	-14.19%**
2009				-9.37%*	-10.76%*	-15.69%**	-15.46%**
2010					-1.39%	-6.32%	-6.09%
2011						-4.93%	-4.70%
2012							0.23%

* Statistically significant at the 10% level, ** statistically significant at the 5% level, *** statistically significant at the 1% level

3. What explains the decrease of the market cleanliness statistic since 2009?

3.1 Methodological choices

Our market cleanliness measure is determined by a number of parameters and other methodological choices. To test whether the 2010 to 2013 results are robust to changes in our methodologies, we recalculate the measure using modified versions of our original methodology. We describe the main results below and present the full details of these checks in Appendix 1. The following analysis is limited to the period from 2007 to 2010 because of limited data availability for the earlier years.

3.1.1 Critical values

To calculate whether a particular takeover announcement has an APPM, we estimate a critical value that determines whether a price movement before an announcement is abnormally positive using data from the clean period estimation window. To form this threshold, we choose a significance level of 10% for the basic statistic, as discussed in section 2.1. This corresponds to a confidence interval of 90%, i.e., an abnormal return is statistically significant at the 10% level if it is larger than the 90% confidence level.

We recalculate the measure using alternative confidence levels of 95% and 99%, setting a higher threshold for defining an APPM. We find that 2010 to 2013 still has a lower percentage of APPMs than the years 2007 to 2009 and we find that these differences remain statistically significant. So the decline in the measure is not dependent on the choice of a 90% confidence interval.

3.1.2 Modelling expected returns

As noted in section 2.1, a simple mean model is used to calculate expected returns. Especially for short event windows, the expected return model is typically not as important (Brown and Warner, 1980). However, particularly in times of high stock market volatility, there may be cases where market movements could be driving our statistic. We recalculated the expected returns using the CAPM model, an asset pricing model that takes into account market risk, and found that it causes only small differences in our market cleanliness measures. On average, the CAPM method shifts the MC measure slightly up, but the declining trend remains stable.

3.1.3 Event window position and length

We also test whether using a two-day event window before the announcement could artificially drive the decline in the market cleanliness statistic. We use a two-day window for consistency with our earlier publications and based on the experience of insider trading activity by the FCA's supervision and enforcement staff. However, it is possible that from 2010 to 2013 information leaked earlier than from 2007 to 2009 and that our two-day window does not capture this potential earlier leakage of information.

To test this possibility, we look at event windows up to ten days in length before the announcement rather than just two days. We find, for all event window lengths, that 2010

and 2013 always have the lowest percentage of APPMs. Using a Z-test, the difference between the average market cleanliness measures from 2007 to 2009 and from 2010 to 2013 is always statistically significant. However, we do see an increase in the statistic for longer event windows in the last two years of the sample. Because of the small sample size for these years, 74 and 53 takeover events respectively, we cannot conclude at this stage that this increase represents a reversal of the trend. We will monitor the future development of the statistic carefully, also for longer event windows.

We also test for earlier information leakage by keeping a two-day event window but moving it earlier relative to the announcement, e.g. three to four days before the announcement rather than immediately before the announcement. We find less measurable information leakage the further away the event window is from the announcement day. The two-day window immediately before the announcement captures the largest number of suspicious events. In addition, 2010 to 2013 still tend to have lower market cleanliness measures than 2007 to 2009. We conclude that a two-day window before the announcement is the least likely to under-report insider trading and that the reduction of our measure from 2010 to 2013 is not driven by our event window choice.

3.2 Takeover sample composition

If a certain type of firm is more likely to be associated with an APPM, changes in the characteristics of takeover stocks could affect the market cleanliness statistic from one year to the next. Since takeover targets are not selected at random[11], it is possible that sample characteristics vary with macroeconomic conditions or market sentiment. Similarly, it is likely that different target firms have different propensities to have an APPM. For example, insider trading in small, thinly traded and opaque firms could be more likely because of information asymmetries and easier to detect because of the low volumes traded. Therefore, differences in sample characteristics could lead us to conclude that a change in cleanliness has occurred while it is merely a change in some exogenous factor like the macroeconmic environment that is driving the results.

In this section, we further check whether the mix of firm and deal characteristics could affect the difference between the 2007 to 2009 and the 2010 to 2013 results.

3.2.1 Descriptive statistics

Tables 4 to 6 show that the sample characteristics have not dramatically changed over the sample period. As shown in Table 4, median market capitalisation in the period from 2010 to 2013 is slightly higher compared to 2007 to 2009. The opposite result holds when looking at the average market capitalisation, pointing to a few large transactions in the earlier period. Overall, the changes in firm size seem relatively small. In Table 4 we also see that volatility and bid-ask spreads were at their highest levels in 2009, presumably a result of market-wide effects caused by the financial crisis. Similarly, volumes traded were highest for takeover targets in the years 2007 and 2008.

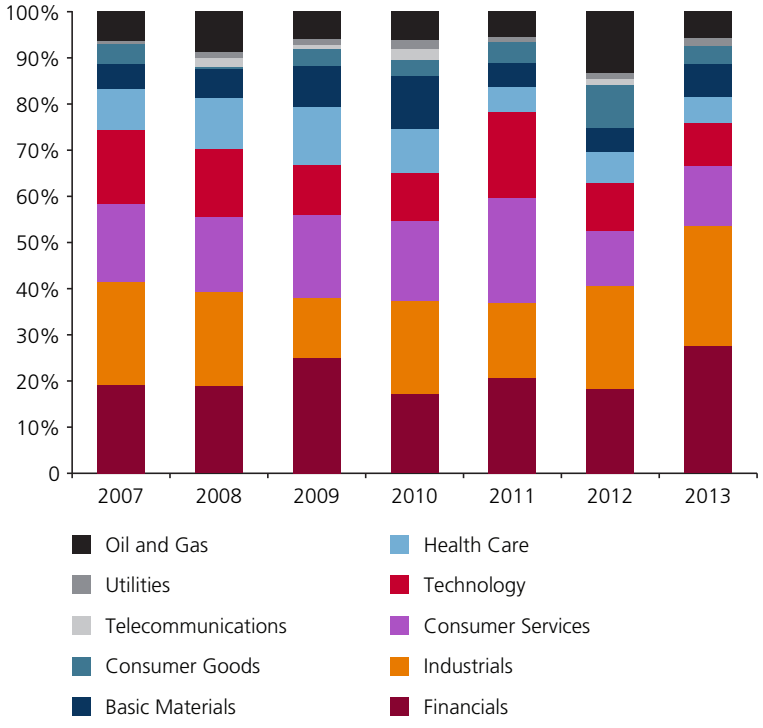
11 For example, Hasbrouck (1985) finds that target firms with a low ratio of market value to book value are more likely to become takeover targets.

Table 4: Sample characteristics. The table shows mean and median values for the annualised standard deviation, the bid-ask spread, the value of trades per day and the market capitalization of all sample firms.

	Annualised standard deviation		Bid-ask spread		Value traded per day (in thousand £)		Market capitalisation (in million £)	
Year	Mean	Median	Mean	Median	Mean	Median	Mean	Median
2007	56.79%	39.15%	5.69%	3.47%	7,736	128	899.06	59.04
2008	71.47%	54.59%	5.28%	3.96%	4,175	67	356.40	24.445
2009	103.38%	77.11%	10.88%	8.50%	1,632	30	301.11	13.77
2010	93.71%	52.11%	7.91%	4.86%	931	50	265.90	32.91
2011	71.91%	49.11%	6.40%	4.48%	289	43	241.47	29.75
2012	60.81%	43.29%	5.51%	3.55%	2,974	33	971.15	27.775
2013	67.95%	50.07%	8.39%	5.23%	610	33	324.53	22.565

The characteristics of takeover target firms did not change materially over the sample period (Table 5). We see little change in the percentage of stocks being available for trading, firms with more concentrated ownership may differ in their corporate governance arrangements and may have poorer controls to prevent information leakage.[12] However, we do find that a smaller percentage of the target firms were represented in a major stock market index.

Figure 3: Industry composition of takeover sample.



12 See, for example, Beny (2006) for a discussion of the literature on the relationship between controlling shareholders and insider.

It is possible that takeover targets differ in terms of their innovativeness or their financial situation, which may affect the value of information released through a takeover announcement and, therefore, market participants' incentives to trade on inside information.[13] As shown in Figure 3, the mix of industries varies from year to year but did not materially change from 2010 to 2013 relative to 2007 to 2009. We see a slightly smaller proportion of technology firms in 2010, 2012 and 2013 than at the beginning of the sample period, however the composition of firms, in particular in 2009 and 2010, seems very similar. Market-to-book ratios are lower in the second half of the sample, which could be an indicator for less growth-oriented firms in the sample. Another distinctive target characteristic is that many of the takeover targets were in financial distress, median stock market performance in the year before the takeover is negative for all but two years, and a large proportion of firms had earnings that were lower than interest expenses. However, we do not see any material changes over time.

Table 5: Takeover sample firm characteristics.

Year	% of firms in major stock market Index	% Freefloat (median)	Debt-equity ratio (mean)	% with EBITDA < interest expenses	Stock market performance (median)	Age (median)	Market-to-book (median)
2007	9.19%	65	136.54	21.84%	3.70%	10	1.89
2008	14.13%	64.5	98.33	35.05%	-44.86%	8	1.09
2009	9.40%	66	837.26	53.13%	-60.19%	8	0.47
2010	13.64%	68	147.75	42.19%	3.34%	10	0.95
2011	16.67%	64	58.09	39.02%	-14.95%	11	0.96
2012	12.82%	64	94.38	44.83%	-25.46%	10	0.76
2013	17.54%	63	103.36	33.33%	-3.94%	10	0.85

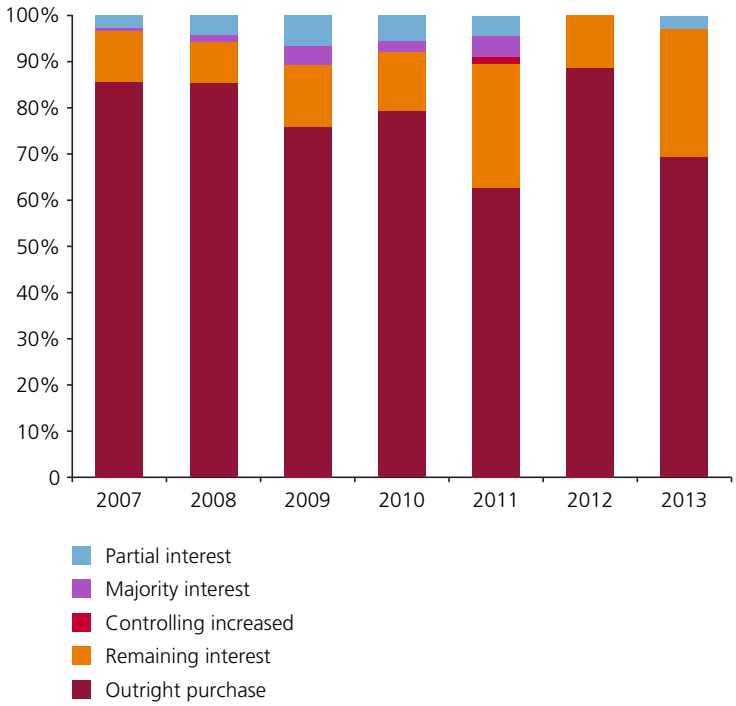
We also obtain deal level data from Dealogic on a subset of the takeover events. Table 6 shows that, in most transactions, the acquirer purchases 100% of the stocks outstanding and does not have a substantial initial stake in the target company. Figure 4 provides more detail on the characteristics of the deals, most of which are outright purchases but the proportion of deals in which only the remaining interest has been purchased seems to have increased in the last three years. The percentage of hostile takeovers seems to be varying over time but does not show a clear trend. We may expect that incumbent management has incentives to leak information in a hostile takeover event. Total deal value and premiums paid by the acquirer have been varying from year to year so we do not expect these differences to drive our results.

13 For example, Prevoo and Weel (2010) find that small firms and technology firms on the Amsterdam stock exchange are more responsive to changes in market abuse regulation than other firms. Similarly, Ahmed and Schneibel Jr (2007) argue that SEC disclosure rules affect small firms and high-tech firms more than others because of differences in analyst coverage and in the value and complexity of information.

Table 6: Deal characteristics.

Year	Deal value (million US$, median)	Acquired stake (%, median)	Final stake (%, median))	Premium (%, 1 month, median)	Hostile takeover
2007	47.4	100	100	24.6	9%
2008	36.9	100	100	35.795	13%
2009	12.1	100	100	36.51	13%
2010	34.3	100	100	38.95	16%
2011	37.5	81.6	100	26.99	13%
2012	45.2	100	100	32.835	6%
2013	12.4	84.48	100	38.165	19%

Figure 4: Deal characteristics



3.2.2 Regression analysis

To check the observations on sample characteristics more formally, we run a regression to see whether the probability of an APPM does decrease from 2010 to 2013, relative to 2007 to 2009, when controlling for characteristics of the takeover sample. We use a logit model as shown in the equation below.

$$Logit(P_{APPM}) = ln(P_{APPM} / (1- P_{APPM}))$$

$$= \alpha_0 + ß \text{ firm \& deal characteristics} + \lambda \text{ 2010-2013 dummy} + \gamma \text{ industry dummies}$$

The dependent variable in the equation, ln(PAPPM / (1- PAPPM)), is the log of the odds ratio of the probability of occurrence to the probability of non-occurrence of an APPM. An odds ratio of one indicates that a takeover announcement is as likely to occur with an APPM as without an APPM; if the odds ratio is greater than one, then an APPM is more likely to occur and vice versa. On the right hand side, we include control variables for firm, deal and market characteristics and a dummy variable for the period from 2010 to 2013. This variable takes value one in the period from 2010 to 2013 and zero otherwise.[14]

The three regression specifications can be seen in Table 7. In (1) we control for firm characteristics such as the size of the firm measured as the logarithm of its market value, the market-to-book-ratio and industry effects.[15] In (2) we add controls for the target firm being part of the AIM index, a dummy that takes value one if the takeover has been classified as hostile and a control for deal size. Specification (3) controls for stock market characteristics, the stock market return in the year prior to the announcements and the standard deviation of the FTSE All Share index at the time of announcement.

The results are shown in Table 7. The table shows the odds ratios for each coefficient as these can be more easily interpreted than the logit coefficients: if you subtract 1 from the odds ratio and multiply the result by 100 you get the percentage change in the odds for a unit increase in the variable of interest. For example, the odds ratio for the post-2009 dummy is 0.533 in column one; this means that being in the years 2010 to 2013 versus the years 2007 to 2009 decreases the chance of an APPM occurring, controlling for other factors, by 46.7%.

Table 7: Logit regression of APPM dummy on firm, deal and stock market characteristics

	Odds ratio		
	(1)	**(2)**	**(3)**
Post-2009 dummy	0.533***	0.571***	0.63***
ln(market value)	0.991		0.995
Market-to-book	0.908***	0.921***	
AIM		0.775	
Hostile takeover dummy		0.692	
ln(deal value)		0.934	
Return in previous year			0.62**
FTSE All Share stand. Dev.			2.16
Industry dummies	None significant	None significant	None significant
N	754	554	784
Pseudo R2	0.0343	0.0512	0.033

* Statistically significant at the 10% level, ** statistically significant at the 5% level, *** statistically significant at the 1% level

14 Alternatively, we include year dummies with qualitatively similar results.

15 Industry effects are measured using eight industry dummies defined by the Datastream industry classification of the target firms.

Column one shows that neither the industry of the target firm nor its market capitalisation significantly predict the probability of observing an APPM. APPMs were significantly less likely to occur from 2010 to 2013 than from 2007 to 2009. This result holds, independent of the regression specification. We expected firms listed on the alternative investment market (AIM) to have less good corporate governance arrangements than those listed on the main market and that, therefore, information might leak more easily. We find no evidence for that hypothesis, as can be seen from the insignificant coefficient in column two of Table 7. We also find no evidence that a deal being friendly or hostile would influence the likelihood of observing an APPM. Market capitalisation and deal value are measures of the profitability of an event for a potential inside trader. Neither seems to matter for the likelihood of observing an APPM.

In column three we control for past stock market performance of the firm in the year of the event.[16] We have seen in Table 5 that many of the takeover firms seem to be in financial distress, raising the question whether that may drive some of our results. The relevant odds ratio is smaller than one and significant, as shown in column three of Table 7, i.e., we are more likely to observe an APPM for badly performing firms.

It is possible that high market volatility causes the traditional event-study methodology to be inaccurate. In our setting, this would be the case if the volatility of prices in the event window (around the announcement) was very different from the volatility in the estimation window. High volatility during the event period relative to low volatility in the clean period estimation window could cause events to be incorrectly identified as APPMs. We use the volatility of the FTSE All Share index at the event day as a control for such effects and do not find that volatility matters for the likelihood of observing an APPM.[17]

We do not find that the sample characteristics that we control for explain the decrease in the market cleanliness statistic from 2010 to 2013. However, we are only controlling for a few variables and the variables that we are not controlling for, 'unobservables', could possibly explain the decrease. While pseudo R2s have to be interpreted with caution, still the low values indicate that the characteristics used are not particularly good at predicting APPMs.

3.2.3 Changes to the disclosure regime and intra-day movements

We next consider the mix of types of takeover announcements to see if this changed from 2010 to 2013 compared to the earlier period. From 2008, takeover announcement data was divided into three categories according to the reason for the announcement.

- **Category 1:** the announcement is required under Rule 2 of the Takeover Code because of a price movement. The announcement may be forced by the Takeover Panel or voluntarily issued by the company because it recognises that the Panel would force it otherwise. There may or may not have been press speculation to accompany the price movement.

- **Category 2:** the announcement is required under Rule 2 of the Takeover Code but without a price movement. This will typically happen when the news breaks after the market has closed, most often at a weekend. An announcement is made either voluntarily or at the request of the Takeover Panel prior to the next market opening.

- **Category 3:** all other announcements (i.e., no speculation and no price movement).

16 Return in previous year is the stock market performance over the 250 trading days ending 11 days before the announcement.

17 As a robustness test we used the volatility of the stock itself and the difference between pre-event volatility and event-volatility as controls with similar results. Another alternative would be to use a GARCH model to estimate changes in volatility, but these models are not very reliable with small sample sizes.

Our market cleanliness measure identifies whether there is an abnormal price movement immediately prior to the announcement. Category 1 announcements cover events where the Takeover Panel requires an announcement because a significant price movement in the target firm took place. Although the Takeover Panel does not use exactly the same statistical methodology as ours to determine if a price movement is significant, Category 1 announcements are likely to be flagged as suspicious under our analysis, at least when the announcement occurs on the day following the price movement. A decrease in insider trading before takeover announcements is, therefore, likely to show as both a decrease in Category 1 announcements and a decrease in the market cleanliness statistic.

As shown in Figure 5, the percentage of takeover announcements that are Category 1 has decreased from 42% in 2008 to around 20% from 2010 to 2013. As discussed, a smaller proportion of Category 1 announcements could be a sign of cleaner markets. However, it could also reflect a change in the disclosure regime, a higher number of announcements happening on the day of the price movement or inside traders trading with less price impact.

Figure 5: Announcements by takeover category.

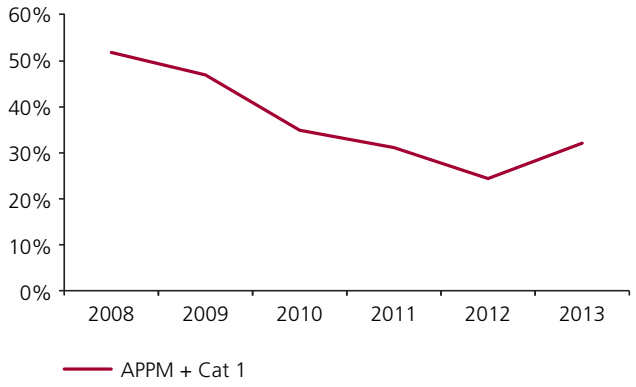
There were two main changes to the disclosure regime from 2007 to 2013. In March 2008, the Takeover Panel published Practice Statement 20.[18] While this was not a change of the rule, it may have resulted in better compliance with the rule (in the same way that FSA/FCA guidance may affect firms). The timing of this change, the fact that it was guidance as opposed to a new rule and the magnitude of the reduction in our market cleanliness measure, suggests that it is does not explain the decline. In September 2011, the rules were changed to require that an announcement of a possible offer must identify any potential offeror with which the offeree company is in talks or from which an approach has been received. We do not expect this change to explain the decline.

Since the Takeover Panel does not use the same statistical methodology to determine if a price movement is significant, it can happen that a price movement shows up as APPM but does not trigger a Category 1 announcement. Similarly, if a price movement triggers a Category 1

18 http://www.thetakeoverpanel.org.uk/wp-content/uploads/2008/11/ps20.pdf

announcement, it may not show up as an APPM because price movement and announcement occur on the same day. If the latter happened more frequently in the second half of the sample period, for example because of a better takeover regime that enforces announcements more swiftly, we would observe a decreasing market cleanliness statistic although markets are not necessarily cleaner. We see in the data that the percentage of Category 1 announcements being classified as APPMs decreased from about 50% in the period from 2008 to 2009 to about 30% in the period from 2010 to 2013. A proper test for intra-day price movements occurring before the official announcement is beyond the scope of this paper. However, as a rough approximation, we calculate a market cleanliness measure based on the assumption that all Category 1 announcements represent suspicious trading behaviour, i.e., we calculate a market cleanliness measure based on a measure that takes value 1 if an event is classified as APPM or as Category 1. The results are shown in Figure 6: the statistic is higher than our standard measure but still exhibits a declining trend in the period 2008 to 2012. As such, we believe that the decline in the statistic is not due to intra-day price movements before Category 1 announcements. More detailed research would, however, be necessary to verify this claim.

Figure 6: Market cleanliness statistic calculated as announcements that are APPM or Category 1.



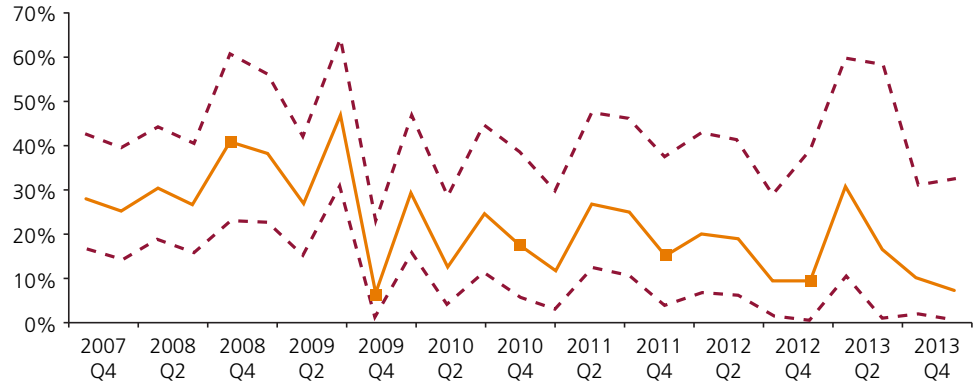
3.3 What else may have caused the decrease in the statistic?

The market cleanliness measure captures abnormal price movements before an announcement as a proxy for insider trading. Our analysis shows that the proportion of announcements that have APPMs has significantly reduced and that the decline is not primarily driven by methodological choices, the change in observable sample characteristics or changes in the volatility of stock returns. As the market cleanliness measure is merely a proxy, however, the question remains whether insider trading has actually decreased and if so, what are the potential drivers that would explain that decrease.

It is possible that those trading on insider information may have better masked their activity. They could have dealt in volumes that would not cause significant price spikes or traded in such a way that they have had less price impact.

Looking at the data quarterly instead of annually, as shown in Figure 7, we see a sharp and sustained drop in APPMs as a percentage of takeover announcements in the last quarter of 2009. The drop suggests that a change in 2009 caused a sustained fall in our measure.

Figure 7: APPM as a % of announcements, quarterly measure



Theoretically, the fall could be caused by any of a number of relevant factors that might have changed in 2009. We only have variation in the market cleanliness measure over time and only one major change, in which the market shifted from being less clean to more clean. So from a technical, econometric viewpoint, it is difficult to isolate which factor might have caused the decrease. Nonetheless, the timing of the drop and its sharpness can suggest which factors are more likely to be the cause.

Apart from insider trading, information being incorporated into stock prices before takeover announcements may be the result of traders successfully and legally identifying that a takeover attempt is imminent. If the market cleanliness statistic decreased because of changes in legal trading, it must either be that the types of firms targeted for takeovers have changed or that the behaviour of traders has changed. In section 3.2 we did control for firm characteristics and, as with Monteiro, Zaman and Leittersdorf (2007), we find that this did not account for the decrease in the proportion of APPMs. However, we acknowledge that there could be some factors for which we do not control.

If, alternatively, the market cleanliness statistic decreased because insider trading decreased, then there are three possible explanations: (i) opportunities for insider trading decreased; (ii) the risk aversion of insider traders increased, or (iii) the perceived risk of insider trading (the chance of getting caught or the consequences) increased. First, as just mentioned, we did control for firm characteristics in our logistic regressions. It seems unlikely, though possible, that the opportunity for insider trading has decreased. Second, market activity has obviously changed markedly since the financial crisis and there could be a change in traders' risk aversion. This could be because previous insider traders are now less willing to take risks, perhaps driven by the difficult job climate in the financial industry (though a difficult job climate could possibly increase the incentives to take risks). Or it could be that career insider traders may have left the financial industry: insider trading only became illegal in 1985 and perhaps traders who began their careers before this time are more likely to insider trade and began to exit the industry following the crisis.

Third, regulatory activity may have increased the perceived risk of getting caught. The first quarter of 2009 saw the first successful criminal sentence for insider trading by the FSA[19] and this has been followed by a continued series of similar high-profile prosecutions against those trading on inside information, as indicated in Table 8.

19 Christopher McQuoid and James William Melbourne , March 2009, http://www.fsa.gov.uk/library/communication/pr/2009/042.shtml

There were also speeches by senior FSA executives highlighting the change in regulatory stance, including a particularly prominent speech in November 2009.[20] It is plausible that the prosecutions and the communication about the new regulatory stance acted as a credible deterrence to insider trading. Previous academic studies show the importance of enforcement, finding that the first prosecution of insider trading is related to a decline in insider trading, but that introducing legislation itself is not.[21]

Table 8: FSA/FCA enforcement activity[22]

Year	Convictions for insider trading	All market abuse fines levied	Total amount of all market abuse fines
2004	-	10	£17,994,000
2005	-	3	£50,000
2006	-	3	£1,750,000
2007	-	-	£0
2008	-	7	£412,326
2009	4	4	£345,785
2010	4	17	£9,359,111
2011	3	8	£8.069.900
2012	10	7	£8,368,795
2013	2	3	£2,147,993

As mentioned, the fact that the market cleanliness statistic declined when high-profile prosecutions began does not prove causality. Whether increased enforcement activity leads to less insider trading and a lower market cleanliness statistic requires further research, data over a longer time horizon and a methodology that can establish the degree of the causal effect of enforcement action on insider trading. If there is variation across different jurisdictions in the degree of enforcement of insider trading, one possible method is to look across countries over time and see if market cleanliness consistently increases as enforcement actions increase. However, there are some caveats in comparing market cleanliness across countries, as discussed in box 1 below.

It is worth mentioning that the FSA/FCA focuses not only on litigation but also on educating market participants. The FSA quickly established its Market Watch publications as a means of communicating its own analysis of the key areas at risk of market abuse. Market Watch also enabled the FSA to describe best practices for mitigating these risks. The FCA has taken a similar stance by publishing a commentary of the market abuse risks identified during the course of relevant enforcement activity. It has also taken a transparent approach to discussing the characteristics of the leads it receives in the form of suspicious transaction reports (STRs), which allows market participants to develop an understanding of the types of market abuse that may be occurring.

20 http://www.fsa.gov.uk/pages/Library/Communication/Speeches/2009/1119_mc.shtml

21 See, for example, Daouk, Bhattacharya, Jorgensen and Kehr (2002).

22 This table does not take into account a small number of instances where a market abuse outcome was achieved but no financial penalty was levied, or was reduced to £0 due to financial hardship. The total amount of all market abuse fines does not include any court-ordered fines, or any confiscation orders made as a result of proceedings. Fines data from 2004 to 2009 could not be verified.

We also note that the market cleanliness statistic is a very narrow measure of insider trading that may not be representative of the overall market since only a small proportion of all trading is linked to takeover announcements. For example, median market capitalisation of our sample stocks is significantly smaller than for FTSE stocks, the stocks of takeover targets are rather thinly traded (Table 4) and often in financial distress (Table 5). While the changes in the statistic cannot be explained by sample characteristics, the level of the statistic may not be representative of the overall markets. Therefore, it is possible that insider trading in other market segments did not decline in the same way it did for takeover events.

Comparing market cleanliness across countries

Care must be taken when comparing the proportion of APPMs before takeover announcements in one country versus another. Different countries have different regimes that determine when announcements occur. Differences in these disclosure regimes make direct comparisons of market cleanliness statistics across countries difficult.

For example, Bhattacharya et al. (2000) studying the Mexican Stock Exchange find that corporate announcements have no effect on prices. They conclude that all information has been leaked far before announcement. In such cases, the FCA measure of market cleanliness would incorrectly rate an unclean market as very clean.

The UK Takeover Code requires companies to make public announcements at a very early stage in the deal-making process, often before formal offers are made. This leaves little time for holders of inside information to misuse that information.

In contrast, in markets with weak (or weakly enforced) disclosure regimes, an announcement is more likely to come only after a formal takeover offer is made. Holders of inside information have, therefore, more time to trade on information. This longer time could enable insiders to space out their actions and reduce the price impact of their trading. In these markets, we would expect the proportion of announcements that are APPMs to be lower, especially for short pre-announcement event windows, such as two days. Leakage for these regimes is less likely to be captured by the standard FCA market cleanliness measure. An event-study market cleanliness statistic with a two-day event window could incorrectly rate an unclean market as very clean.

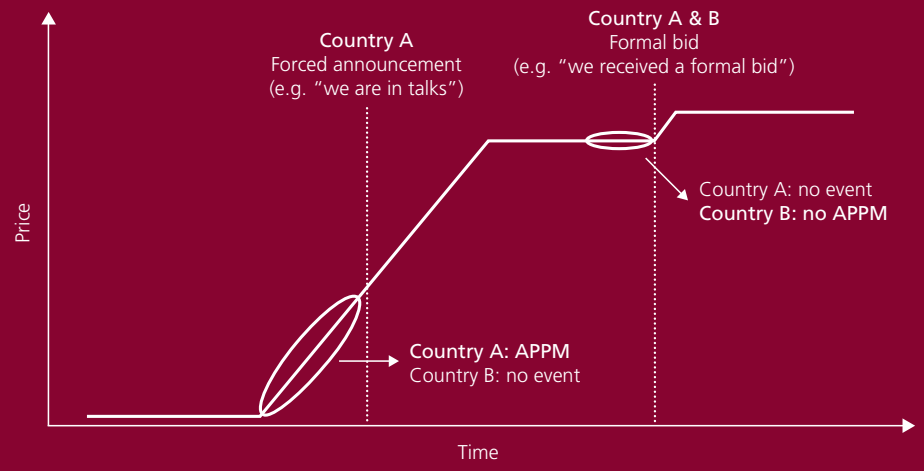
To illustrate the problem further, we refer to countries like the UK where announcements are required early as Country A. We refer to countries that require announcements only when formal offers are put forward, and not to have to announce a 'possible offer' following an untoward share price movement, as Country B.

Table 9: Disclosure regime in stylised Countries A and B

Country	Regime	Announcement timing
A	Requirement/practice to make a 'possible offer' announcement following an untoward share price movement	Early
B	No requirement to announce a 'possible offer' following an untoward share price movement, announcement when formal offers are made	Late

Because announcements in a Country B regime are likely to happen later in the deal, the window for trading on insider information is wider. An event-study with a two-day pre-announcement window is then unlikely to capture most of the suspicious trading. The market cleanliness statistics calculated then may therefore be artificially low.

Figure 8: Illustration of disclosure regime and market cleanliness statistic



Intuition: In regimes where announcements are required at later stage (i.e., country B), leakage could happen much before the announcement and not be captured in a market cleanliness statistical analysis

In contrast, under the strong requirements of Country A, an announcement can be forced precisely because an abnormal price movement is observed. This announcement is, therefore, more likely to show up as a suspicious pre-announcement price movement. As a result, even if the level of insider dealing in Country A and Country B is the same, one can expect Country A to display a higher market cleanliness statistic than Country B. Similar effects would be expected when two countries have similar disclosure requirements but corporate governance mechanisms differ such that the requirements are enforced more strictly in one country than in the other.

One could attempt to mitigate this problem by, for example, extending the window length and moving it to the time of any observable news event instead of restricting it to a short period immediately before the regulatory announcement. Unfortunately, increasing the size of the event window is problematic since it can have a strong negative effect on the reliability of the statistic.[23] Different event window specifications that reflect different disclosure requirements would also reduce the comparability of the measures obtained for different countries. The different specifications may lead to different levels in the measures of market cleanliness, as can be seen in Appendix 1.

Appendix 2 shows a crude approximation of how the UK market cleanliness measure would change under a regime where only formal takeover announcements were required (i.e., similar to a Country B type regime). This illustrates a significant drop in the percentage of APPMs observed.

23 For example, Kothari and Warner (2004) show that the power to detect abnormal performance decreases dramatically with the length of the event window. Moreover, the power to detect abnormal performance decreases when the abnormal performance is not concentrated in the event window.

An alternative is to use announcements where disclosure requirements are likely to be more similar, e.g., earnings announcements. While this has the advantage of allowing a more consistent comparison across countries it introduces certain technical difficulties and does not correct for other country-specific factors.

First, a set of price sensitive announcements would need to be consistently identified across countries. While takeover announcements are very price sensitive, other types of announcements (e.g., earnings) vary in their price sensitivity. An analysis focused on such announcements will be searching for smaller effects (where isolating significant movements from noise becomes more difficult). To bypass this problem a statistical approach may be used to identify significant announcements. It is however difficult to control for potential endogeneity issues.[24] Second, one would still expect country-specific factors like differences in volatility, corporate governance, industry, firm size and market liquidity to affect the size of the market cleanliness measure.

Overall, it is very difficult to devise a common approach for a market cleanliness measure that is valid across countries.

24 Selecting events based on the impact they have on prices may bias the results. See Dubow and Monteiro (2006) for a discussion.

4. Conclusion

After remaining close to 30% for four years, the FSA/FCA market cleanliness statistic for takeovers decreased to 21% in 2010 and 15% in 2013. In this paper we conduct further analyses on the FSA's 2010 to 2013 market cleanliness measure for takeovers to assess possible drivers of this result.

We test for robustness by varying our standard method and recalculating our measure for the years 2007 to 2013. Although these analyses change the results slightly, we find no compelling evidence to suggest that parameter or methodological issues are driving the reduction in the market cleanliness measure.

We look at the composition of firms with takeover announcements over the last five years and do not find evidence that the decrease in the 2010 to 2013 measures is a consequence of changes in sample characteristics.

Overall, we do not find strong evidence of bias in the 2010 to 2013 data or measures. This supports the hypothesis that the recent decrease in the statistic indicates that markets are cleaner.

We recognise that, at this stage, we do not know whether the FSA/ FCA's actions are driving the improvement in the measure but the increase in the FSA's enforcement activity and educational agenda and decrease in the market cleanliness statistic are roughly contemporaneous.

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A. Appendix

1. Are our methodological choices biasing the results?

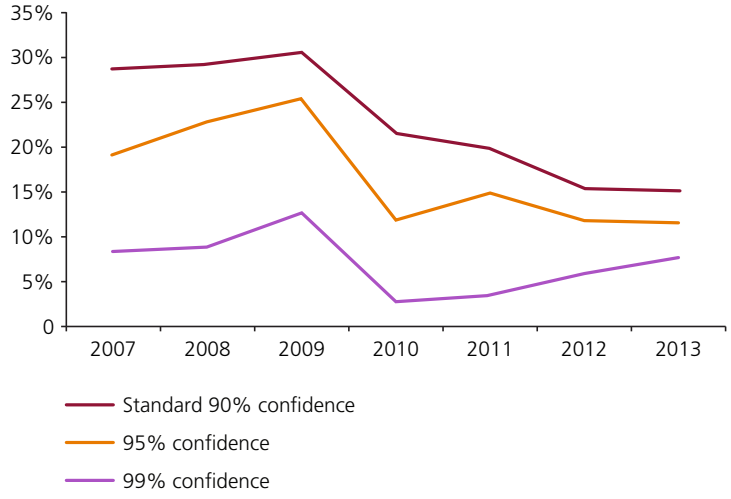
The method for calculating our market cleanliness measure includes a number of parameter choices and other basic methodological choices. To test whether the 2010 to 2011 results are robust to changes in those choices, we recalculate our measure using modified versions of our original methodology. Below we present and discuss the results of changing these parameters.

1.1 Confidence threshold

Choosing a confidence level of 90% means that a stock's pre-announcement cumulative abnormal return (CAR) is deemed significant if it is so large that the probability of abnormal price movements of that scale arising purely by chance is 10%. Around 10% of the time, the methodology will therefore identify a clean event as suspicious. Thus, in a perfectly clean market, one would expect the market cleanliness measure computed at that level of confidence not to be 0% but, on average, 10% across the sample of takeover announcements.

We choose 90% as it is a standard level of confidence and we find that it captures many abnormal price movements. But the choice is essentially arbitrary. Choosing a lower confidence level will reduce the probability that we identify an unsuspicious event as suspicious ('false positive'); but it would increase the probability that we miss a suspicious event and identify it as unsuspicious. We recalculate the figure using a 99% and 95% confidence level.

Figure 9: The market cleanliness statistic 2007-2013 for different confidence thresholds



As shown in Figure 9, during the three years for which the market cleanliness at the 90% level remained close to 30% (2007 to 2009), the measure at the 99%, 95%, and 90% is consistently higher than in the period from 2010 to 2013. Applying a Z-test, as described in section 2.3, for both the 95% and 99% confidence level, Table 9 confirms that the 2007 to 2009 market cleanliness statistic is significantly higher than the 2010 to 2013 statistic. We can conclude that the recent decline in the market cleanliness measure is not dependent on the confidence threshold chosen.

Table 9: Differences of market cleanliness statistic for different confidence thresholds.

	2007-2009			2010-2013			Difference between periods
Model	**Sample**	**APPMs**	**MC statistic**	**Sample**	**APPMs**	**MC statistic**	
Standard (2 Days)	492	145	29.47%	341	63	18.48%	-11.00%***
95% Confidence	449	101	22.49%	316	40	12.66%	-9.84%***
99% Confidence	449	44	9.80%	316	14	4.43%	-5.37%***

*Statistically significant at the 10% level, ** statistically significant at the 5% level, *** statistically significant at the 1% level

1.2 Modelling expected returns

As described in the methodology, we use a simple mean model to calculate expected returns. In this section, we test the robustness of our results to a different model of expected returns. We recalculate abnormal returns using the capital asset pricing model (CAPM) with the FTSE 100 as the market index.

Figure 10: Yearly MC statistic using CAPM model of expected returns.

Table 10: Test for the difference of market cleanliness statistic using the CAPM.

CAPM MC Statistic before and after 2009

Period	Sample	APMs	MC statistic
2007-2009	449	147	32.74%
2010-2013	316	65	20.57%
Difference			12.17%***

As shown in Figure 10, this only changes the market cleanliness measure by a small amount. Using the CAPM slightly shifts the MC measure upwards in most periods but does not change the trend. Table 10 shows that, when using the CAPM, the MC measure between 2007 to 2009 is significantly higher than from 2010 to 2013.

It is not surprising that a change in modelling expected returns does not alter the result greatly. Over a two-day period, stock returns are mostly driven by idiosyncratic factors and not systematic risk. Abnormal returns over a short horizon are unlikely to be greatly affected by the model of systematic risk.

1.3 Event window position and length

Choosing to look for abnormal price movements in the two days immediately prior to announcements can be justified using the experience of FCA enforcement and market abuse specialists. A two-day event window is consistent with Sinha and Gadarowski (2010) but not with the five-day event window of Bulkey and Herreirias (2005). It is possible, however, that from 2010 to 2013 information leaked earlier than from 2007 to 2009 and that our two-day window is not capturing this earlier leakage.

There is clearly not one right window length, so we examine how the length of the event window affects the market cleanliness measure, extending the event window up to ten days before the announcement date. The analysis, shown in Figure 11, confirms that the measure from 2010 to 2013 is substantially lower than those from 2007 to 2009 at all event window lengths that we examine. Table 11 confirms that the differences between the market cleanliness measure from 2007 to 2009 and from 2010 to 2013 are statistically significant at all event window lengths. However, we do see an increase in the statistic for some of the alternative event window lengths in the last two years.

Figure 11: The market cleanliness statistic 2007-2013 calculated for different event windows lengths.

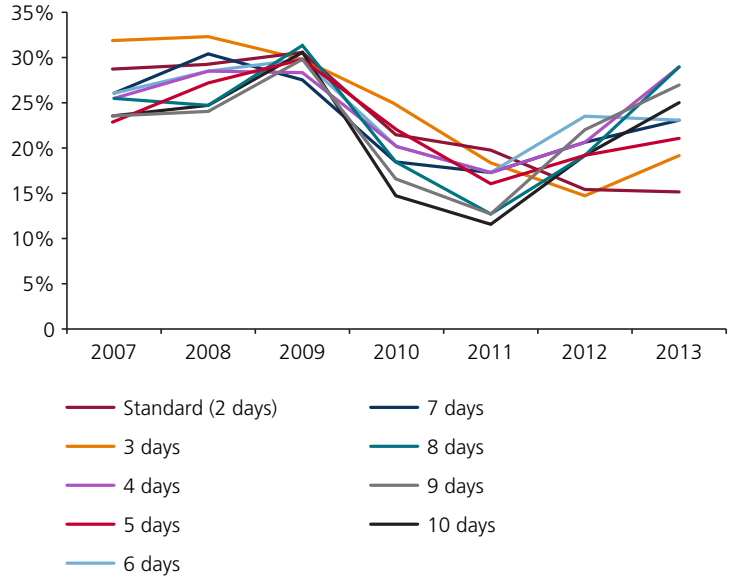


Table 11: Test for the difference of market cleanliness measure using different event window lengths.

	2007-2009			2010-2013			Difference between periods
Model	**Sample**	**APPMs**	**MC statistic**	**Sample**	**APPMs**	**MC statistic**	
Standard (2 Days)	492	145	29.47%	341	63	18.48%	-11.00%***
3 Days event window	449	141	31.40%	316	63	19.94%	-11.47%***
4 Days event window	449	123	27.39%	316	66	20.89%	-6.51%**
5 Days event window	449	119	26.50%	316	62	19.62%	-6.88%**
6 Days event window	449	126	28.06%	316	65	20.57%	-7.49%**
7 Days event window	449	127	28.29%	316	61	19.30%	-8.98%***
8 Days event window	449	121	26.95%	316	59	18.67%	-8.28%***
9 Days event window	449	115	25.61%	316	58	18.35%	-7.26%**
10 Days event window	449	117	26.06%	316	52	16.46%	-9.60%***

We also test for earlier information leakage by keeping a two-day event window but moving it earlier relative to the announcement, e.g., three to four days before the announcement rather than immediately before the announcement. Figure 12 shows the results of this analysis. As expected, we find less information leakage the further away the event window is from the announcement day. The two-day window immediately before the announcement captures the largest number of suspicious events. In addition, 2010 to 2013 still tend to have lower market cleanliness measures than from 2007 to 2009, although some of the measures seem

to have increased at the end of the sample period. Table 12 shows tests of the differences between the MC statistic from 2007 to 2009 and from 2010 to 2013. The later period is significantly cleaner than the earlier period for the first five measures, while being insignificant when moving the window further away from the announcement day. We conclude that a two-day window before the announcement is the least likely to under-report insider trading and that the reduction of our measure from 2010 to 2013 is not driven by our event window choice.

Figure 12: The market cleanliness statistic 2007-2013 calculated for different event window positions.

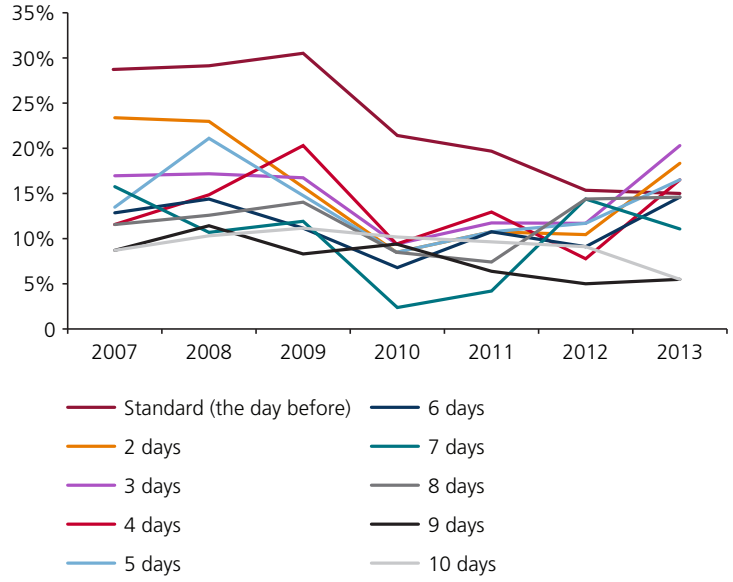


Table 12: Test for the difference of market cleanliness statistic using different event window positions.

	2007-2009			2010-2013			Difference between periods
Model	**Sample**	**APPMs**	**MC statistic**	**Sample**	**APPMs**	**MC statistic**	
Standard (1 Day before)	492	145	29.47%	341	63	18.48%	-11.00%***
2 Days before event	449	97	21.60%	316	37	11.71%	-9.89%***
3 Days before event	449	78	17.37%	316	41	12.97%	-4.40%*
4 Days before event	449	73	16.26%	316	35	11.08%	-5.18%**
5 Days before event	449	76	16.93%	316	37	11.71%	-5.22%**
6 Days before event	449	60	13.36%	316	33	10.44%	-2.92%
7 Days before event	449	59	13.14%	316	24	7.59%	-5.55%**
8 Days before event	449	57	12.69%	316	36	11.39%	-1.30%
9 Days before event	449	44	9.80%	316	24	7.59%	-2.20%
10 Days before event	449	49	10.91%	316	31	9.81%	-1.10%

2. Quantifying the impact of a disclosure regime shift

To illustrate the possible relative upward bias in market cleanliness measures under Country A type regimes, we assess how different the UK market cleanliness statistic would be if the takeover disclosure regime in the UK (which is an example of Country A) required only formal offer announcements. In essence, we try to assess the impact on the market cleanliness statistic of a shift from Country A disclosure regime to Country B. To do this, we re-calculate the 2008 and 2009 FSA market cleanliness measures.[25] But, this time, we only include formal offer announcements in the analysis.

We asked the Takeover Panel for a list of Rule 2.5 (formal offer) announcements for 2008 and 2009 and ran our market cleanliness analysis on this set only. We used a shorter (-250, -50) clean period estimation window because in the case of Rule 2.5 announcements many are preceded by earlier Rule 2.4 announcements. This, very crudely, approximates a regime that only requires disclosure in case of a formal offer. As such it provides a useful illustration of the potential bias. Table A3 shows the MC measure with Rule 2.5 takeover announcements is 12-15 percentage points lower than with the original set of takeover announcements.

Table A3: Market cleanliness measure for Rule 2.5 takeover announcements

Year	Rule 2.5 Announcements	APPMs	Illustrative adjusted Market Cleanliness measure	Original Market Cleanliness measure
2008	85	11	14.1%	29.3%
2009	79	14	19.0%	30.7%

It is important not to take this analysis as a criticism of the UK disclosure regime. The analysis only reveals that market cleanliness measures across countries with different disclosure regimes cannot be reliably compared. The adjustment suggests that market cleanliness statistics for type-B countries under-report abnormal price movements before takeover announcements.[26] In fact, the events taken out in this analysis are precisely takeover announcements where there are good reasons to believe that suspicious activity was going on. This is a very important point. These adjustments are purely made for illustration, not to suggest a race to the bottom to make market cleanliness statistics look smaller. Instead, more research in the area is needed to refine statistical measures of market cleanliness.

We wish to reiterate that these results are illustrative only and should not be taken as a precise measurement of what would be expected under a hypothetical regime where first announcements of takeovers are only required at a formal offer stage. One element of bias introduced here, for example, is where earlier Rule 2.4 announcements have been made prior to formal offer announcements, since some increased probability of eventual takeover will already be reflected in target stock prices. Rule 2.5 announcement may therefore lead to a lower upward price movement relative to a hypothetical regime with no earlier announcement. This lowers the chance of an event being observed as an APPM.

25 We pick two recent years where our statistic shows comparatively high levels.

26 To prevent these earlier announcements from contaminating our clean periods we used a shorter (-250, -50) estimation window. Descriptive statistics reveal that 86% of Rule 2.4 announcements are followed within 50 days by Rule 2.5 announcements, and the average distance between the two announcements is 30 trading days. This indicates that our revised window is appropriate.

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