

Updated Measurement of Market Cleanliness

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UPDATED MEASUREMENT OF MARKET CLEANLINESS

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FSA Occasional Paper

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Biographical note

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1. Summary

1.1 Introduction

The FSA has a Statutory Objective to maintain confidence in the financial system. The FSA's Business Plan for 2006/07 notes that all that we do, whether in wholesale or retail markets, reflects our belief that efficient, orderly and fair markets are the most efficient way of delivering value to both users and providers of financial services. The FSA has primary responsibility for tackling market abuse in the UK and we consider the successful reduction of market abuse to be one of our highest priorities.

In March 2006 the FSA published Occasional Paper 23 (OP23) which reported on work to develop a measure of 'market cleanliness'. The work was motivated by the FSA's commitment to evaluate its overall performance.

The measure of market cleanliness was based on the extent to which 'informed price movements' are observed ahead of 'significant' (i.e. price-sensitive) regulatory announcements made by issuers to the market. These price movements could indicate insider trading. OP23 examined two kinds of announcements – 'trading statements' made by FTSE350 issuers and public takeover announcements made by companies to which the takeover code applies.

The scope of OP23 was limited in at least three ways. First, given the data and techniques at our disposal it focused only on insider trading – which is just one form of market abuse. Second, it only considered 'cash' equities, rather than derivative instruments, such as, for example, contracts for difference.¹ Third, it did not seek to measure the benefit of insider trading regulation but noted that financial markets are built on trust and insider trading erodes that trust.

¹ Mindful that insider dealing, and other forms of market abuse, can occur in the non equity markets, which are not covered by this study, the FSA undertook work for the credit markets in 2006 and reviewed cases of potentially suspicious trading in instruments such as Contracts for Differences (CFDs) and spread bets. In CP06/4 in March 2006, the FSA consulted on the need for enhanced disclosure of interests in CFDs.

OP23 examined changes in the measure for periods both before and after the introduction of the Financial Services and Markets Act (FSMA) in 2001. Introducing a civil regime for prosecuting market abuse is more straightforward and quicker in terms of procedure and enables the FSA to take action for a broader range of misconduct.² Also, the Disclosure Rules³ have tightened up the obligations on listed issuers to disclose inside information and have introduced unlimited fines for firms which do not make timely, accurate and full disclosures to the market. Both these changes should further deter market misconduct, including informed trading ahead of regulatory announcements.

OP23 showed that for announcements by FTSE350 listed companies there was no change in market cleanliness after FSMA was implemented. For public takeover announcements there was a statistically significant increase in the measure of informed trading between 2000 and 2004. OP23 concluded that the new powers given by FSMA had not, to date, resulted in an improvement in the level of market cleanliness. The paper considered some possible explanations for this including that, while FSMA had been introduced in 2001, no major enforcement action had taken place in the period under examination⁴. OP23 also noted that changes in the measure might be due to factors other than the level of insider trading (although it did not draw the conclusion that they were) and sought feedback on the statistical methods employed⁵.

This paper extends the original analysis in three ways:

1. We have gathered and analysed data for additional years to extend the dataset, including 2005 data.
2. We have revised our method following feedback on OP23 and discussions with external practitioners and academics. This has resulted in a revision to the numbers we published in OP23.

2 However, importantly, a prosecution under the civil regime for market abuse requires an evidential hurdle similar to that for criminal prosecutions.

3 These are the United Kingdom Listing Authority (UKLA) disclosure rules, which can be found in the FSA website in <http://fsahandbook.info/FSA/html/handbook/DTR>.

4 Bhattacharya and Daouk (2002) found that it is enforcement of the regime which has an effect, not merely the introduction of rules. The analysis of FTSE350 announcements contained only data up to 2003 – before this no enforcement action under the new FSMA regime had taken place at all.

5 Further detail of the feedback received on OP23 is provided in Annex 1.

3. We have extended our analysis in two ways. First, we investigated the extent to which changes in our measure can be identified with changes in the level of insider trading, rather than other factors. Then, we analysed trading volume surrounding news announcements and whether volume can be an indicator of informed trading.

The results for the takeovers analysis still show a significant increase in the measure of informed trading between 2000 and 2004, as reported in OP23. We also see a decline in the measure between 2004 and 2005, but the level of the measure remains high and is not lower than it was in 2000 before FSMA came into force. For the FTSE350 analysis, the measure of informed trading is very low in the years 2004 and 2005 and is statistically significantly lower than in the period 1998-2000 before FSMA was introduced. This could indicate that markets have become cleaner.

We have examined the extent to which our measure depends on factors such as the size, trading liquidity and reliance on research and development (R&D) of firms in our sample. The aim was to assess the extent to which changes in these factors between time periods were driving any changes in our measure, resulting in 'sample-specific effects'. We have found no evidence that our results are being driven significantly by sample-specific effects.

The analysis of volumes surrounding announcements by FTSE350 listed companies largely reinforces the conclusions of our analysis of price movements. For the takeover announcements, we see a different pattern of year-on-year changes but the two broad conclusions of the price analysis appear to stand. These are that a significant proportion of takeover announcements appear to be preceded by 'pre-event abnormal volume' (PAVs), and the level of PAVs does not appear to differ significantly between the pre-FSMA and post-FSMA periods. We have also found that only a minority of 'informed price movements' (IPMs) are PAVs and vice-versa. We have investigated this and concluded that the different year-on-year pattern, rather than suggesting different conclusions about the level of informed trading, may be explained by other factors such as changes in level of (uninformed) speculative position-taking.

The analysis in this study, and the earlier work set out in OP23, has been helpful in informing the FSA's work programme in tackling market abuse.

Most of the issues dealt with in this paper are technical, so the body of this paper is aimed at readers with a solid grounding in statistical and econometric concepts. The remainder of this section provides a non-technical summary of the main methodological issues and results.

1.2 The measure of market cleanliness

OP23 described a measure of market cleanliness based on the extent to which share prices move ahead of the regulatory announcements which issuers are required to make to the market. We examined two kinds of announcement: announcements relating to public takeover bids using data for 2000 and 2004 and announcements about the trading performance of FTSE350 listed issuers using data between 1998 and 2003. Share price movements ahead of such announcements may reflect insider trading.

We estimated statistical relationships between the returns to individual stocks and the return to the market as a whole. These relationships were used to identify whether the returns to an individual stock during key periods were 'abnormal' in a statistical sense. We interpreted a large abnormal return around the time of a regulatory announcement as indicating that the announcement contained important news about the stock's value; news which could be of interest to an insider trader. We referred to these regulatory announcements as 'significant announcements'.

Our measure of market cleanliness was based on the proportion of significant announcements where the announcement was preceded by an 'informed price movement' (IPM). We defined an IPM as an instance where there is an abnormal stock return before an announcement. That return would be positive in the case of a good news announcement and negative in the case of a bad news announcement. We explained that IPMs can indicate insider trading, while asserting neither that most insider trading gives rise to IPMs, nor that IPMs arise only as a result of insider trading.

Our updated method (set out in Section 3) is again based on the ratio of IPMs to significant announcements but we have made two main changes. The first change is to the way we estimate statistical relationships between the returns to individual stocks and the return to the market as a whole in order to identify 'abnormal' returns.⁶ The change reduces the risk that we incorrectly conclude that an IPM has occurred in situations where volatility has increased in the period leading up to a regulatory announcement, or where the correlation between returns in nearby days is strong. Equally, the change reduces the risk that we fail to spot an IPM in situations where volatility has decreased over time.

⁶ Technically speaking, we have changed our regression model and normalized abnormal returns to reduce further the impact on our measure of changing volatility over time or serial correlation in returns.

The second change is to the way we define which announcements are ‘significant’. We now treat all takeover announcements as significant, rather than just those which lead to an extremely large price movement once the announcement is made. This approach makes sense because all announcements in the takeover sample may arguably be considered economically significant. Furthermore, our old approach caused us to eliminate from our analysis many announcements which clearly had a big impact on prices but nonetheless did not meet the very high statistical threshold we set for considering announcements to be significant. The approach also eliminates the need we faced in OP23 to adjust the results obtained for an upward bias which arose from the way in which we had used price movements around the time of the announcement to identify whether they are significant.

For the FTSE350 analysis we still need to use price movements around the time of the announcement to identify whether they contain significant news. This is because many of these announcements contain no significant news. However, we now identify IPMs for significant FTSE350 announcements in a way that reduces their number in order to eliminate the potential for bias in our measure (and the need to adjust for that bias).

The results for the takeovers analysis are presented in Table 1 below. We can see that the changes in our method affect the measure calculated for individual years.⁷ However, with both methods we see a significant increase between 2000 and 2004, as reported in OP23. We also see a decline between 2004 and 2005, but the measure overall is statistically not lower than in 2000 before FSMA came into force.

Table 1: The measure of market cleanliness for the takeovers analysis

Time Period	Number of announcements	Number of IPMs	Measure using revised method	Measure using OP23 method
2000	183	44	24.0%	28.1%
2002	147	37	25.1%	10.0%
2003	160	22	13.8%	19.9%
2004	102	33	32.4%	35.9%
2005	177	42	23.7%	30.0%

⁷ We now consider more announcements as significant, the level of the measure calculated with the new method is generally lower. It also changes considerably for some years (e.g. 2002) as we found evidence of informed trading ahead of announcements that were previously deemed as non-significant.

These results suggest that leaks of inside information about public takeovers are higher than we would expect in a clean market.⁸

Table 2: The measure of market cleanliness for the FTSE350 analysis

Time Period	Number of announcements	Number of significant announcements	Number of IPMs	Measure using revised method	Measure using OP23 method
Before FSMA (1998/1999/2000)	487	51	10	19.6%	29.9%
After FSMA (2002/2003)	734	54	6	11.1%	30.7%
After Enforcement (2004/2005)	927	49	1	2.0%	1.4%

For the FTSE350 analysis we see in Table 2 that, with both methods, the measure of market cleanliness is very low (1%-2%) in the years 2004 and 2005 and is significantly lower than in the period 1998-2000 before FSMA was introduced. The finding that there are very few instances where prices move ahead of FTSE350 announcements appears to be a robust statistical result which does not depend on the method used⁹. This could indicate that markets have become cleaner, which may result from improved market discipline and/or the impact that FSMA and enforcement had on firms' management and disclosure of price-sensitive information.

A couple of caveats are worth noting. First, we draw our results from a relatively small sample of significant announcements which suggests we should be cautious in drawing conclusions that the market has changed unless we continue to see the same pattern in future years. Second, it was pointed out to us that our results may reflect a weakening in the link between insider trading and prices, since insiders may now be more easily able to disguise their trades and so minimize the impact they have on prices. This may follow from recent developments in the market such as an increase in liquidity, a higher proportion of automatic trading and an increased use of non-equity instruments. We have, however, no evidence to support this alternative hypothesis.

8 In 2006 we launched a significant project to review the controls over such information to enhance our understanding and consider ways to tighten the flow of information. We published a short article on the project in December 2006.

9 See Annex 2 for a wider set of results based on alternative research parameters.

1.3 Sample specific effects and the ‘identification problem’

Movements in our measure of market cleanliness need to be interpreted with care. They may reflect changes in our sample over time rather than changes in market cleanliness. OP23 referred to this difficulty with drawing inferences from changes in our measure as the “identification problem”.

In order to “identify” changes in our measure with changes in market cleanliness, we need to identify and control for sample specific factors that may influence our results. In OP23, we provided some descriptive statistics of potential sample specific factors which may or may not have affected our measure of market cleanliness. These did not point to any obvious problem.

In this paper, we also present a formal econometric analysis of the extent to which variations in our measure of market cleanliness may be explained by sample specific factors (see Section 4). We consider a number of potential sample specific factors, including the size of firms, their R&D activity, the volatility and liquidity of their shares, and the extent of the absolute cumulative abnormal returns around an event window. These factors may either affect the significance of announcements or the likelihood that they are preceded by an IPM.

Our results are, on the whole, reassuring. We find limited evidence that changes in sample specific effects materially influence our measure of market cleanliness. We note, however, that our analysis focuses only on a small set of observable variables and is constrained by features of our data.

1.4 Analysis of trading volumes

The final extension to our analysis in OP23 was to examine changes in trading volumes ahead of regulatory announcements, on the basis that abnormally high volumes can in some circumstances be indicative of informed trading (see section 5).

The method used is similar in outline to our analysis of prices. We examine the extent to which abnormally high trading volumes are observed ahead of takeover announcements and other announcements by FTSE350 issuers which were surrounded by abnormally large trading volumes (ALV).¹⁰

10 Rather than abnormally large price movements, as it was the case for ‘significant announcements’.

To some extent our analysis of volumes reinforces the conclusions of our analysis of price movements. In Table 3 we can see that in the period covering 2004 and 2005 relatively few significant announcements by FTSE350 issuers are preceded by abnormally large volumes. Like the price analysis, we also find that this proportion has declined over time and is lower than in the period 1998-2000 before FSMA was introduced.

Table 3: Volume results for the FTSE350 analysis

Time Period	Number of announcements	Number of ALV announcements	Number of PAVs	Measure	Measure for price analysis
Before FSMA (1998/1999/2000)	487	39	7	17.9%	19.6%
After FSMA (2002/2003)	734	80	12	15%	11.1%
After Enforcement (2004/2005)	927	101	6	5.9%	2.0%

When we look at the takeover announcements, we see a different pattern of year-on-year changes in the volume measure. However, the two broad conclusions of the price analysis are mirrored in the volume analysis: first, a significant proportion of takeover announcements appear to be preceded by PAVs; and second the level of PAVs does not appear to differ significantly between the pre-FSMA and post-FSMA periods.

Table 4: Volume results for the takeovers analysis

Time Period	Number of announcements	Number of PAVs	Measure	Measure for price analysis
2000	126	39	30.9%	24.0%
2002	124	45	36.3%	25.1%
2003	125	33	26.4%	13.8%
2004	86	16	18.6%	32.4%
2005	159	43	27.0%	23.7%

Finally, we examine the relationship between those announcements where we identify PAVs and those announcements where we identify IPMs. We find that for both the takeovers and FTSE350 analyses only a relatively small proportion of IPMs are PAVs and vice-versa. In Section 5 we conclude that abnormal volumes are not on their own likely to provide a robust indication of the overall level of informed trading although they may provide useful additional information, for example, to the analysis of individual instances of informed trading.

We also provide evidence that, in addition to changes in informed trading, the measure obtained in the volume analysis may be explained by other factors. These include changes in level of (uninformed) speculative position-taking ahead of a scheduled announcement (e.g. trading statements), legitimate position-taking by an offeror ahead of a takeover announcement or the arbitrage activity of rational traders betting that takeover rumours are false.

2. Extending and updating the dataset

We have extended and updated our datasets of both FTSE350 trading statements and takeover announcements.

We obtained FTSE350 trading statements for 2004 and 2005 from the ELMS¹¹ database.¹² In our analysis below we examine these two years together and refer to them as the 'After Enforcement' period. This reflects the fact that the first insider trading enforcement case brought by the FSA under the new FSMA regime occurred only in 2004 and is in line with the evidence provided by Bhattacharya and Daouk (2002) that enforcement is required to make insider trading regulation effective.

These new data for the 'After Enforcement period' therefore extend the data in OP23 for 1998, 1999, 2000 – a period that we called 'Before FSMA' – and data for 2002 and 2003 – a period that we called 'After FSMA'.

11 ELMS stands for Electronic Listing Management System and is an FSA database that derives its content from the data provider AFXnews.com.

12 Data for the earlier years reported in OP23 were obtained from the Factiva search engine and database. As in OP23 we have included announcements under the heading 'trading update', 'contract award' or 'drilling report', as price-sensitive information will often (but not always) appear under these headings.

Table 5: Sample of FTSE350 announcements

<i>Number of announcements</i>	1998	1999	2000	2002	2003	2004	2005	Total
	99	135	251	369	367	500	427	2148

We have also extended our data on takeovers. The FSA's Markets Division has obtained data from the Takeover Panel for the years of 2002, 2003 and 2005 in addition to the list of takeover announcements we analysed before for the years 2000 and 2004. These are announcements by companies to which the Takeover Code applies and which led to the commencement of an 'offer period' (as defined in the Takeover Code¹³).

Table 6: Sample of takeover announcements

<i>Number of announcements</i>	2000	2002	2003	2004	2005	Total
	183	147	160	102	177	769

3. The measure of market cleanliness

3.1 Changes to the original method

At the time of the publication of OP23 we committed to update our analysis by extending our sample of announcements. We sought and have since received feedback on our method, some of which led to improvements to our measure of market cleanliness. In this section we explain the changes in our method and we focus only on the feedback that led to these changes. We try to describe these changes as plainly as possible given the technical nature of some of the feedback. Further detail and discussion of other feedback received is provided in Annex 1 and we set out the details of our method using mathematical notation in Annex 5.

13 The Takeover Code refers to the 'City Code on Takeovers and Mergers', which can be found in the Takeover Panel website in <http://www.thetakeoverpanel.org.uk/new/codesars/DATA/code.pdf>. The definition of 'offer period' may be found in section C1 of the Takeover Code.

3.1.1 Choice of 'event' window

As before there are two periods of interest to us for each announcement. One is the period before the announcement which is used to determine whether an IPM has taken place – the 'pre-announcement window'. The other is the period after the announcement (the 'post-announcement window') which, together with the period before the announcement, is used to determine whether the announcement contains 'significant' news.

The combined length of these two periods (the 'event window') is a critical decision which reflects a number of factors. We discussed these in OP23¹⁴ and decided to set the pre-announcement window as the two trading days before the day of the announcement. The choice of the post-event window is not problematic and we again use a two-day window comprising the day of the announcement itself and the day afterwards.

However, we acknowledged in OP23 (and it was also pointed out to us in the feedback received) that the pre-event window would both begin too late and end too soon to cover all instances of insider trading. To respond to this we have, in addition to a two day pre-announcement window, replicated our analysis using a five day pre-announcement window. The findings from this analysis are provided in Annex s and confirm the robustness of our results.

3.1.2 Modelling expected returns

As in OP23, the abnormal return (AR) on a given day is the difference between the expected return from our model and the actual return and by adding together abnormal returns over time we calculate cumulative abnormal returns (CARs).

Equation 1: Abnormal Returns and Cumulative Abnormal Returns

$$AR_{it} = R_{it} - E(R_{it})$$

where i refers to the firm, t to the day and where $E(R_{it})$ is the expected value of the return of firm i in day t .

$$CAR_i(\tau_1, \tau_2) = \sum_{t=\tau_1}^{\tau_2} AR_{it}$$

14 For more detail on these factors please see section 5.2.2 of OP23.

Our approach is then to identify significant announcements and IPMs by summing on-day abnormal returns for periods of four-days, $CAR_i(-2, +1)$, and two-days, $CAR_i(-2, -1)$, respectively.

Identifying abnormal returns therefore requires us to define what expected returns are. For the FTSE350 analysis, we calculate the expected return by estimating a statistical relationship between the stock and the market return, using daily data on the stock return and the FTSE350 return over an estimation window of 240 trading days ending ten days before the announcement.

Equation 2: Daily stock return according to the market model used in OP23

$$\text{Daily Stock Return} = \alpha + \beta \text{ Daily Market Return} + \varepsilon$$

In OP23, we estimated this statistical relationship using the above simple form of the market model. The parameter β captures the extent to which the stock return depends on the market return over that period while α represents the expected value of the daily return to that stock in addition to any market-driven movements over that period. This simple model assumes that ε (the estimation error), which represents the abnormal return over one-day, has standard statistical properties¹⁵.

In the feedback received it was pointed out that these assumptions may not hold for the underlying returns data, namely that the variance of daily abnormal returns may change over time ('heteroskedasticity') and the abnormal returns on nearby days may not be independent ('serial correlation'). So, using a model that relies on these assumptions could lead us to estimate abnormal returns wrongly and undermine our results. We have now tested the validity of these assumptions in our data and, where they do not hold (e.g. where there is heteroskedasticity), we have changed the way in which we model expected returns.

Heteroskedasticity could not only affect our estimates of abnormal returns but also the likelihood of abnormal returns showing up as significant. For example, when modelling an issuer's expected returns in a period where the volatility (i.e. the variance) of abnormal returns increased, if we fail to adjust for this increase, we will conclude that abnormal returns of a given size are statistically significant more often than we should.

15 In particular, the estimation errors are assumed to have an expected value of zero, a constant variance over time and are assumed to be independent from the estimation errors of nearby days.

To control for the presence of heteroskedasticity in the returns data of some issuers we have extended the market model. We have done this by assuming that the variance of the estimation errors is not constant over time but a result of a GARCH(1,1)¹⁶ process. According to this process the variance of the error on a given day, instead of constant, is a function of the size of the error term and its variance in the day before, according to the following equation:

Equation 3: Daily variance of the estimation error according to a GARCH(1,1) process

$$\begin{array}{lcl} \text{Expected Variance} & & \text{Square of} \\ \text{of Estimation} & = & \text{Estimation} \\ \text{Error in day } t & a + b & \text{Error in day } t-1 \\ & + c & \text{Expected Variance} \\ & & \text{of Estimation Error} \\ & & \text{in day } t-1 \end{array}$$

In this extended version of the market model we therefore calculate not only one-day abnormal returns, but also their variance using the returns of the 240 days in the estimation window.¹⁷ After doing this, we normalize the abnormal returns by dividing them by the estimate of the variance. Returning to our example, the idea is that if the abnormal returns in the event window (the period over which CARs are calculated) are higher than those in the estimation window, we can make them comparable with the ones in the estimation window if we divide them by their higher variance.

If the estimation errors of nearby days are not independent this may again lead to incorrect estimates of α and β and, more importantly, affect our judgement about the significance of abnormal returns in the event window. As noted above, we select significant announcements and IPMs by looking at the total four-day CARs and the two-day pre-announcement CARs, respectively. In assessing their significance we compare them with the sum of abnormal returns on four or two days drawn randomly from the estimation window, which are therefore not necessarily close to the announcement.

This approach is problematic if the returns to an issuer's stock on one day tend to be correlated with the returns to that stock on nearby days – i.e. the returns are serially correlated. However, drawing returns randomly from the estimation window will not produce a returns series which is serially correlated. This means that in assessing the

16 GARCH is the acronym for Generalized Autoregressive Conditional Heteroskedasticity. (1,1) means that we are using a first order process, i.e., using the variance and size of the error of the previous day.

17 We exclude returns of days outside the estimation window because they may evidence changes in variance that are abnormal due to insider trading and so could contaminate the model.

significance of CARs we would not be comparing like with like and, where there is serial correlation, we will overestimate the number of significant events and IPMs.

We control for the presence of serial correlation in the returns data of some issuers through an extended market model. We calculate the statistical relationship between the stock and the market, also using the lagged values of both stock and market returns, as a way of proxying the error in the previous day and so of discounting the effect it may have on the contemporaneous stock return.

Equation 4: Daily stock return according to an extended market model that controls for serial correlation in estimation errors

$$\begin{array}{ccccccc} \text{Stock} & & \text{Market} & & \text{Stock} & & \text{Market} \\ \text{Return} & = & \alpha + \beta_1 & \text{Return} & + & \beta_2 & \text{Return} & + & \beta_3 & \text{Return in} & + & \varepsilon \\ \text{in day } t & & & \text{in day } t & & & \text{in day } t-1 & & & \text{day } t-1 & & \end{array}$$

Since in the returns data for a given issuer both heteroskedasticity and serial correlation may be present, we have used a procedure to detect and control for their presence that takes the following steps:

1. For each announcement, we estimate the relationship in Equation 1 above, using data on daily stock and market returns for the 240 trading days estimation window ending ten days before that announcement. We then calculate the abnormal returns for each of the 240 days as the difference between the expected and the actual return for each day.
2. We have used the series of abnormal returns for those 240 days to test for changes in their variance and serial correlation over time.
3. According to the results of the previous tests we can choose one of four ways to estimate the market model in the estimation window:
 - a. If neither heteroskedasticity nor serial correlation are present in the data we use the simple market model used in OP23.
 - b. If only changes in variance over time are detected, we extend the market model by also estimating the abnormal returns' variance according to Equation 3.
 - c. If only serial correlation over time is detected, we extend the market model to include lagged variables according to Equation 4.

- d. If both features are present in the data, we use a combination of b. and c. and calculate the expected returns using Equation 4 and the estimated variance of the estimation errors using Equation 3.

Detail on this procedure is provided in Annex 5. To examine the degree to which this procedure is effective, we used the alternative Durbin-Watson test of serial correlation and the Engle's test of heteroskedasticity. The alternative Durbin-Watson test indicates that by using Equation 1, we find serial correlation for 47% of our announcements. After employing the procedure described above that figure falls to 4%. The Engle test indicates that by applying only Equation 1, we find heteroskedasticity in 36% of our announcements but when the procedure described above is used, we find it in fewer than 12%¹⁸.

For the takeovers analysis, since some of these stocks trade relatively infrequently there is less value in using a more sophisticated model. So, we retain a simpler model where expected returns are assumed to be the average stock return over the 240 trading day estimation window. We have, however, changed our method to account for some of the features of the returns data mentioned above in a way set out in section 3.1.4.

3.1.3 Identifying 'significance'

In OP23 we described the 'bootstrap' method we used to assess whether the observed pre-announcement CAR and total CAR for each announcement in our sample are statistically significant. This method generates detailed information about the 'distribution' of the total CAR and the pre-announcement CAR for each announcement, from the limited number of four- and two-day CARs we have data for.

In OP23 we also mentioned that our method of selecting significant announcements tends to bias upward our estimate of the number of IPMs due to circularity in our approach. The intuition is that a large pre-announcement price movement can contribute to a finding that over the four-day period before and after the announcement the price movement is significantly abnormal and that the

18 We note that the reduction in heteroskedasticity is not as significant as in serial correlation. We have analyzed some of the remaining heteroskedastic series and concluded that this resulted from the presence of outliers rather than or in addition to remaining conditional heteroskedasticity. This is consistent with the literature, see Carnero, Peña and Ruiz (2001). We could have further reduced heteroskedasticity by using heavier-tailed or higher-order GARCH models. However, each series is different, and it would be time consuming to individually choose the most appropriate distributional form in each case. Therefore, we have only used a GARCH(1,1) because a balance needed to be struck between the time spent and the possible benefits from more modelling.

announcement is therefore ‘significant’. So ‘significant’ announcements are more likely to have large pre-announcement price movements than other announcements, simply as a result of the way we have selected them and regardless of whether insider trading has occurred.

Previously, we took two approaches to correcting this bias. Both approaches involved identifying and eliminating ‘false’ significant announcements and IPMs from our analysis. However, it was pointed out to us in the feedback that we could avoid this bias arising in the first instance by amending our bootstrap. Our original bootstrap used the unconditional¹⁹ distribution of the pre-announcement CAR to assess the significance of pre-announcement CAR. By instead using the distribution of the two-day CAR conditional on the fact the four-day CAR for that announcement was significant we could eliminate the bias at source.

To implement this new approach we now take the following steps:

1. For each announcement we calculate the abnormal returns for each of the 240 days as the normalized difference between the actual return and the expected return obtained using the procedure described at the end of section 3.1.2.
2. We draw four one-day abnormal returns from the set of 240 at random and sum them to calculate a ‘simulated’ four-day CAR.
3. We repeat this exercise 50,000 times yielding 50,000 random simulated four-day CARs.
4. We compare the actual total CAR associated with the announcement with these 50,000 simulated total CARs. We deem the actual total CAR to be statistically significant at the 1% level if it is less than or equal to the 250th most negative simulated total CAR or greater than or equal to the 250th most positive simulated total CAR.
5. To obtain the conditional distribution of the two-day CAR we repeat steps 1 to 3 and of the 50,000 random simulated four-day CARs we focus only on the

19 i.e. the distribution of a two-day CAR regardless of the CARs over the four-day period being significant.

four-day CARs that are less than or equal to the 250th most negative previously simulated total CAR or greater than or equal to the 250th most positive previously simulated total CAR.

6. From this limited set of 500 four-day CARs we calculate the CAR on the first two days to obtain the two-day CAR distribution conditional on the fact that the total CAR was deemed as significant at the 1% level.
7. We compare the actual two-day CAR associated with the announcement with the 500 simulated conditional two-day CARs. We deem the actual two-day CAR to be statistically significant at the 10% level²⁰ if it is less than or equal to the 50th most negative simulated conditional two-day CAR, if the four-day CAR was negative, or greater than or equal to the 50th most positive of the simulated conditional two-day CAR, if otherwise.

Since this procedure eliminates the upward bias in our estimate of the number of IPMs, we expect the number of announcements that are classified as IPMs to be lower than the number reported in OP23.²¹

3.1.4 Approach to takeovers sample

As stated above, since some of the stocks in the takeovers analysis trade relatively infrequently there is less value in using a more sophisticated method to calculate abnormal returns. However, we have changed our method to some extent, to take account of some of the issues we refer to above. In particular, we have eliminated the potential for upward bias in the selection of IPMs and we have controlled for serial correlation.

To eliminate the potential for upward bias in the selection of IPMs we have dropped the notion of 'significant' takeover announcements, since arguably all takeover announcements can be deemed as economically significant. This removes the circularity in our approach and at the same time increases the number of announcements in the denominator of our measure which should reduce its level

20 This significance level refers to a one-tailed test where as the previous referred to a two-tailed test.

21 In practice, the new approach leads to high critical values which increase the chances of a Type II error, i.e., incorrectly not classifying announcements as IPMs when they should be. This is the price we pay to eliminate the positive selection bias and prevent changes over time in that bias from affecting our measure. By applying a uniform procedure to all announcements in all time periods, we assume that the incremental level of type II error that we are introducing remains constant over time. However, we can not test this assumption.

compared to the level we reported before. As noted above, this change also has the benefit of allowing us to include in our analysis a large number of takeover announcements which clearly affected prices but not as much as to pass the very high threshold we set for considering announcements to be significant in OP23.

To control for serial correlation, we have changed our method of drawing return observations when bootstrapping in the takeovers analysis. Instead of forming sets of random two-days by drawing one day at a time we now draw two consecutive days in order to reflect the serial correlation we see in the underlying data²².

3.2 Results

3.2.1 Calculating the measure

The results are presented in the tables below

Table 7: The measure of market cleanliness for the FTSE350 analysis

Time Period	Number of announcements	Number of significant announcements	Number of IPMs	Measure	Measure using OP23 method	Number of IPMs using OP23 method	Number of significant announcements using OP23 method
Before FSMA (1998/1999/2000)	487	51	10	19.6%	29.9%	18	54
After FSMA (2002/2003)	734	54	6	11.1%	30.7%	20	56
After Enforcement (2004/2005)	927	49	1	2.0%	1.4%	7	47

Table 8: The measure of market cleanliness for the takeovers analysis

Time Period	Number of announcements	Number of IPMs	Measure	Measure using OP23 method	Number of IPMs using OP23 method	Number of significant announcements using OP23 method
2000	183	44	24.0%	28.1%	29	100
2002	147	37	25.1%	10.0%	9	70
2003	160	22	13.8%	19.9%	8	31
2004	102	33	32.4%	35.9%	22	57
2005	177	42	23.7%	30.0%	33	107

²² We used the alternative Durbin-Watson to test for serial correlation in the returns series. The test found evidence of serial correlation for 49% of takeover announcements.

The results of the FTSE350 analysis suggest that instances of insider trading have dropped sharply since 2004. In addition, the results for the 'after FSMA' period are now lower than for the 'before FSMA' period, which suggests a downward trend in the level of informed trading ahead of these announcements. We find the same results when repeating the analysis with a different event window and different significance thresholds²³ which makes us confident that this is a robust statistical finding.

However, two caveats should be noted when interpreting these results as suggesting market cleanliness has improved. On the one hand, the sample of significant announcements is small. Changes in that small sample might not be representative of general trends in the amount of insider trading. We try, to some extent, to control for these in the next section but the size of our sample may imply that our results are subject to wide variability over time.

On the other hand, in our meetings with practitioners, it was suggested that for more liquid FTSE350 stocks, prices tend to be more stable, there is an increasing amount of automatic trading and there is more scope to trade on inside information using other instruments e.g. contracts for difference (CFDs). It was suggested that these factors may contribute to a weaker link between insider trading and equity prices, allowing insiders to more easily disguise their trades and minimize their impact on prices. We have, however, no evidence that this is actually happening or about the mechanisms through which this would happen²⁴. Hence, these hypothesis remains to be formally tested.

The results for the takeovers analysis show that the average level of informed trading in the 2004 and 2005 period (26.9%) is not lower than that in the 2000 and 2002 one (24.5%) and follows a dip in 2003. There is also a decrease in our measure from 2004 to 2005 which is statistically significant at a 5% significance level.

These changes in the measure of market cleanliness may, to some extent, be due to changes over time in the sample of firms for which takeover announcements were made and not to changes in the level of insider trading. This problem is potentially greater for the takeovers analysis than for the FTSE350 analysis, where there is greater continuity between the firms in the sample from year to year. In section 4, we describe the method and provide results of extending the analysis to control for changes in the sample of firms.

23 We have used a 5% instead of a 1% significance threshold to identify significant announcements and a five-day instead of a two-day event window. The results of these two analyses can be found in Annex 2.

24 We expect insider trading to show up in prices even where insider traders trade in derivative instruments. This is because arbitrage transmits changes in the price of one instrument to the other. Jayaraman, Frye, and Sabherwal (2001) provide evidence of this mechanism in the options market.

3.2.2 Are these results statistically significant?

We used further bootstrap analysis to understand the variation in the measure calculated above. This allowed us to test whether there is evidence of informed trading (i.e. whether the level of the measure is significantly different from what it would be in the absence of informed trading) and whether there is evidence that the change in the statistics from one period to the next is significant. Further details of these bootstrap procedures and results (including confidence intervals) are provided in Annex 5. The conclusions from this analysis can be summarised as follows:

- For all periods but 2003 in the takeovers analysis, and for the 'Before FSMA' and 'After FSMA' periods in the FTSE350 analysis, the measure is above the level required for it to be considered significantly different (at the 1% level) from the level it would have been if there was no informed trading. This provides evidence that informed trading is taking place.
- For 2003 in the takeovers analysis and the 'After Enforcement' period in the FTSE350 analysis, the measure cannot be considered (at the 5% level) significantly different from the level it would have been if there was no informed trading. However, this does not provide evidence that informed trading did not take place in this period. This is because, as we explained in OP23, informed trading may not show up in changes in prices. Furthermore our analysis only considers instances where there is a particularly large change in prices.
- The drop in the measure over the three periods in the FTSE350 analysis is, at the 5% level, statistically significant.
- Finally, the examination of the statistical significance of the change in the measure between periods, at the 5% level for the takeovers analysis, reveals that:
 - The measure in 2003 is significantly lower than the measure for any other year.
 - The measure in 2004 is still significantly higher than the measure for any of the previous years²⁵, which confirms the result obtained in OP23.
 - The drop in the measure from 2004 to 2005 is statistically significant, which provides evidence that, despite still being high, the level of informed trading may have decreased for takeover announcements in 2005.

²⁵ Except for 2002, in relation to which the measure in 2004 is significantly higher only at a 10% level.

4. Analysis of Sample Specific Effects

4.1 The identification problem

Movements in our measure of market cleanliness need to be interpreted with care. It is possible that changes in our sample over time lead to changes in the measure of market cleanliness without changes in the level of informed trading. We refer to this as the ‘identification problem’. It means that it may not be appropriate to ‘identify’ changes in our measure with changes in the level of insider trading.

The possibility of sample specific effects was noted already in OP23. We have now attempted to control for some potential sample specific effects of particular concern. The table below provides an overview of the potential sample specific factors we considered:

Table 9: Sample specific factors

Factor	Proxy	Effect on measure of market cleanliness
Size of firms	Market value of equity (MVE)	Larger firms might disclose price-sensitive information more punctually and control insiders more effectively than smaller firms, though improved market discipline could have reduced potential differences in reporting behaviour and insider control between firms of different sizes.
Stock volatility	Variance of returns	The market model estimates and our bootstrapping tests are less reliable in periods of high volatility.
Liquidity	Volumes traded/ outstanding shares	The estimate of β in the market model may be biased downwards for illiquid stocks. This means expected returns are lower and measured abnormal returns are higher, increasing the probability that announcements pass our test for significance or for informed price movements. However, illiquidity may make it more difficult to disguise insider trading.
Innovativeness of firms	R&D/Sales	Innovative firms may be difficult to value for most investors. This may increase the price effect of announcements. It may also create opportunities for trading on inside information which relate to innovations or discoveries made by the firm.
Absolute size of the mean return in the event window around an announcement	Mean absolute abnormal returns ²⁶	The higher the absolute mean return in the event window, the greater are the potential gains from insider trading.
Industry affiliation of firms	Industry dummy variables	The effect of announcements on shareholders’ expectations about the value of a company, and hence, stock prices may vary across industries. Some industries may be more vulnerable to insider trading than others because there are more opportunities for insider trading, insider trading is more profitable or detection more difficult.

26 We were not able to control for this factor by using the four-day absolute mean CAR, as this term would derive its significance largely from our earlier tests for significance and IPMs. We tentatively included the two-day post-event CAR in some of our models instead, though this term is less informative. Specifically, the two-day post-event CAR may have a negative coefficient if it reflects the likelihood of an announcement taking the market by surprise rather than proxying the potential gains from insider trading.

The next sections set out our method for controlling for these sample specific effects and our main results, for both the FTSE350 sample and the takeovers sample. Detailed descriptive statistics and regression outputs can be found in Annex 3.

4.2 Sample specific effects in the takeovers analysis

In the takeovers sample, we analyse whether the variation in the probability that an announcement is preceded by an IPM is explained by sample specific factors. We use a Logit model for this analysis.

Our Logit model has the following specification:

Equation 5: The odds ratio of an IPM according to a Logit model

$$\begin{aligned} Lipm &= \ln (P_{ipm} / (1 - P_{ipm})) \\ &= \beta_0 + \beta_1 MVE + \beta_2 R\&D/Sales + \beta_3 volatility + \beta_4 liquidity \\ &\quad + \beta_5 absolute\ mean\ post\text{-}announcement\ 2\text{-}day\ CAR \\ &\quad + \beta_6 2002dummy + \beta_7 2003dummy + \beta_8 2004dummy + \beta_9 2005dummy \end{aligned}$$

In the equation above, the dependent variable $\ln (P_{ipm} / (1 - P_{ipm}))$ is the log of the odds ratio of the probability of occurrence to the probability of non-occurrence of an IPM. An odds ratio of one indicates the probability of occurrence of an IPM is equally likely to that of non-occurrence. An odds ratio greater/less than one indicates the probability of occurrence is more/less likely than that of non-occurrence.

The model contains two types of independent variables:

- Sample specific factors: the model includes five potential sample specific factors, MVE, R&D/sales, volatility, liquidity and the absolute mean two-day post-announcement CAR. We exclude sector dummies from the model as preliminary analysis suggests they do not improve its fit.
- Additive year dummies: the model includes dummies for each year in our sample. These dummies increase the flexibility of the model, allowing for potential structural changes (in the constant of the fitted logistic distribution function), caused perhaps by marked changes in the number of IPMs in our sample over time.

The question the model allows us to ask is whether the probability of an IPM depends on factors other than those considered in our earlier test for the presence of IPMs.

The regression results (see Table A7) show that the model had a poor fit (pseudo-R² of 0.0374). The 2003 dummy was significant at the 1% level and negative, reflecting the lower number of IPMs in the sample in this year. None of the sample specific factors had a significant effect on the odds in favour of an IPM. These results do not point to any sample specific factors which may affect our measure of market cleanliness in the takeovers sample.

4.3 Sample specific effects in the FTSE350 sample

The analysis of sample specific factors is more complex in the FTSE350 sample than in the takeovers sample. In the FTSE350 sample, we cannot assume that all announcements are significant, as in the takeovers sample. So we have to analyse whether variations in the probability that an announcement in the FTSE350 sample is significant may be explained by sample specific effects. As in the takeovers sample, we also have to analyse whether the variation in the probability that an announcement is preceded by an IPM may be caused in part by sample-specific factors.

4.3.1 Sample specific factors and significance of announcements

We estimate a Logit model, with the following specification:

Equation 6: The odds ratio of a significant announcement according to a Logit model

$$\begin{aligned} L_{ipm} &= \ln (P_{ipm} / (1 - P_{ipm})) \\ &= \beta_0 + \beta_1 MVE + \beta_2 R\&D/Sales + \beta_3 volatility + \beta_4 liquidity \\ &\quad + \beta_5 post-FSMA dummy + \beta_6 after-enforcement dummy \end{aligned}$$

Like equation 5, this equation includes a number of variables to identify sample specific effects as well as period dummies. However, neither the four-day absolute CAR nor the two-day absolute post-CAR are included in this model, as these terms would derive their significance from our tests for determining the significance of an announcement in the FTSE350 sample.

The results (see table A8) show that the fit of the model is poor, with a pseudo-R² of 0.0195. The period dummies have a large negative impact on the odds in favour of an IPM, reflecting changes in the number of IPMs over time. The market value of equity has a statistically significant but negligible effect on the odds in favour of an IPM. None of the other factors significantly affects the odds in favour of an IPM.

We extend the above model to include sector dummies, to capture potential effects of sector affiliation. While one sector (energy) turns out to have a significant effect, this removes significance from the market value of equity. This suggests that the statistically significant but negligible effect of the market value of equity is actually due to sector affiliation.

In sum, our model does not suggest that the significance of announcements in the FTSE350 sample is materially affected by changes in the sample specific factors under consideration.

4.3.2 Sample specific factors and IPMs

There are only 17 IPMs in the FTSE350 sample. This is not sufficient to run a robust Logit model with the log of the odds in favour of an IPM as the dependent variable. We therefore have to adopt an alternative approach.

We note that the odds in favour of an IPM depend on what we called the ‘excess ratio’, i.e. the ratio of the two-day pre-CAR to the threshold (derived from our bootstrap), which the two-day pre-CAR must exceed to be considered an IPM. If the excess ratio is greater than one for a particular announcement, there is an IPM; if it is less than one there is not. The excess ratio is a continuous variable that underlies the discrete variable we are actually interested in – the probability of an IPM. We therefore set up an OLS model to explore to what extent sample-specific factors might explain the variation in the excess ratio. While not ideal, this model is informative.

Our model specification is as follows:

Equation 7: The pre-announcement CAR according to an OLS model

$$\begin{aligned} \text{Excess Ratio} = & \beta_0 + \beta_1 \text{ absolute 2-day post-announcement CAR} + \beta_2 \text{ MVE} + \beta_3 \text{ R\&D/Sales} \\ & + \beta_4 \text{ volatility} + \beta_5 \text{ liquidity} + \beta_6 \text{ post-FSMA dummy} \\ & + \beta_7 \text{ after-enforcement dummy} \end{aligned}$$

The model again includes a number of variables to test for sample specific effects (including, this time, the two-day post-CAR as it is unrelated to our tests for determining the presence of IPMs in the FTSE350 sample, which focus on the two-day pre-CAR), as well as period dummies.

Given the small sample size, it is difficult to control effectively for heteroskedasticity. However, while not entirely reliable, the results of the model (see table A9) are perhaps encouraging. Of the sample specific factors, only the two-day post-announcement CAR has a significant though small effect, suggesting that the likelihood that an announcement takes the market by surprise increases with size of the post-announcement CAR. In addition, the after-enforcement dummy is significant and negative. This leaves open the possibility that movements in our measure of market cleanliness reflect changes in regulatory and enforcement policy rather than sample specific factors.

4.4 Conclusion

We attempted to identify sample specific effects that may affect our measure of market cleanliness in the takeovers and the FTSE350 samples. We looked at the size of firms, stock volatility, liquidity, innovativeness of firms, the absolute size of the mean return after an announcement (in some models), and the industry affiliation of firms (in some models). We found little evidence that changes in these variables materially influence our measure of market cleanliness, which is reassuring. We caution, however, that our analysis focused only on a limited set of variables and was constrained by the quality of the data available.

5. Volume Event Study

In addition to stock return data, trading volume surrounding news announcements can also be an indicator of insider-trading activities. Investors have an incentive to identify price sensitive information and take positions before announcements. This may lead to increased trading in the period before announcements.

We replicate the event study for both FTSE350 and takeover announcements with trading volume data instead of price data. The volume event study method is largely the same as that used for the returns event study, but there are some significant differences. The approach used is elaborated below.

5.1 Method

5.1.1 Approach to FTSE350 sample

For FTSE350 announcements our volume analysis is similar to the returns analysis. To investigate trading volume effects, we test for evidence of unusual trading volume before announcements. The volume measure chosen is the daily number of shares traded. In two ways our method diverges from our analysis of returns: firstly our measure of trading volume differs in the way it is constructed, and secondly the way we model expected volume is different.

The remainder of the volume event study method is the same as that used for the returns event study. Abnormal volumes are calculated for every announcement for each of the 240 days as the difference between the expected and the actual volume traded on each day. We use an event window covering two days before and two days after the announcement. Finally, we use the bootstrap method, described above for the analysis of returns, to determine significance of pre-announcement and total cumulative abnormal volumes.

Measure of trading volume

Ajinkya and Jain (1989) and Cready and Ramanan (1991) find that raw trading volume data has undesirable statistical properties but that a natural log-transformation yields trading volume measures that are approximately normally distributed and so well-behaved. Following these studies we transform our raw trading volume measure to $v_{it} = \ln(1 + \text{volume}_{it})$ where \ln is the natural logarithm

and 1 is added to volume to handle the problem of log transformation in the event of zero volume. The ensuing analysis is performed for this transformed data. Annex 4 details the characteristics of our raw trading volume data and the effect of the log-transformation performed.

Modelling expected volumes

The day-of-the-week anomaly in stock market volume and volatility has been widely studied in the finance literature. Kiyamaz & Berument (2003), for example, find that the lowest trading volumes occur on Mondays and Fridays for Japan, the United Kingdom and the United States, and the highest trading volume occurs on Thursday for each market. Following this and event studies like Sanders and Zdanowicz (1992), Meulbroek (1992) and Wong (2002) we incorporate day-of-the-week dummies in our analysis of volumes. As with the returns analysis, we also include an aggregate market volume variable m_t . We estimate a market model of the stock trading volume on the aggregate market volume using data over the 240 trading days ending ten days before the announcement.

Equation 8: Daily stock trading volume on aggregated market volume and day-of-the-week dummies

$$V_{it} = \beta_0 + \beta_1 V_{mt} + \beta_2 \text{Monday}_{it} + \beta_3 \text{Tuesday}_{it} + \beta_4 \text{Wednesday}_{it} + \beta_5 \text{Thursday}_{it} + \varepsilon_{it}$$

Instead of Ordinary Least Squares (OLS), the model is estimated using Generalised Least Squares (EGLS) to correct for serial correlation²⁷. In the presence of serial correlation the OLS estimates would be unbiased, but not efficient, and the literature documents serial correlation for both trading volume and residuals. To examine the degree of such a problem in our data, and to see if the corrective procedure of employing EGLS is effective, we use the Durbin-Watson d test. The test indicates that in 88% of our original 2148 data series positive serial correlation is detected. In 10% of cases we can neither reject nor accept a hypothesis of no serial correlation. In only the remaining 2% do we not detect any serial correlation. After employing EGLS we discover an almost exactly opposite situation where no serial correlation is detected in 91% of our regressions. Some 7% of instances lead to no concrete finding either way, while in only less than 2% of all cases do we now detect serial correlation. So our remedial measure appears to work well.

27 The generalised least squares procedure transforms the original variables of the model by assigning weights based on variances. For further description of the EGLS method, see Maddala (2001).

5.1.2 Approach to takeovers sample

For the takeovers sample we also transform our data using natural log to take account of the non-normality of raw trading volume data identified in the section above. In the returns analysis above we described the 'bootstrap' method that we used to assess whether the observed pre-announcement CAR and total CAR for each announcement in our sample are statistically significant. We employ the same method to determine statistical significance for our analysis of volumes.

To identify abnormal trading volume we need to define what expected volumes are. For the analysis of returns for the takeovers sample, we calculated the expected return as the average daily return in the estimation window. However, the period of 240 days ending ten days before the announcement may not be the most appropriate estimation window for the takeovers section as explained in the next section.

Change in Estimation Window

A recent study of the Canadian market, King & Padalko (2005), observed long volume run-ups before takeover announcements. We hypothesise that, if in our calculation of average or expected volumes we experience rising volume several weeks before announcements, this may 'contaminate' our measure²⁸:

- if it makes it more difficult to identify statistically significant abnormal trading volumes in the two days before an announcement because the expected volume calculated over the 240 day estimation window is biased upward; or
- if announcements are falsely identified as associated with abnormal volumes, due to sustained upward movements in volume unrelated to announcements (a trend over time not linked to the announcement).

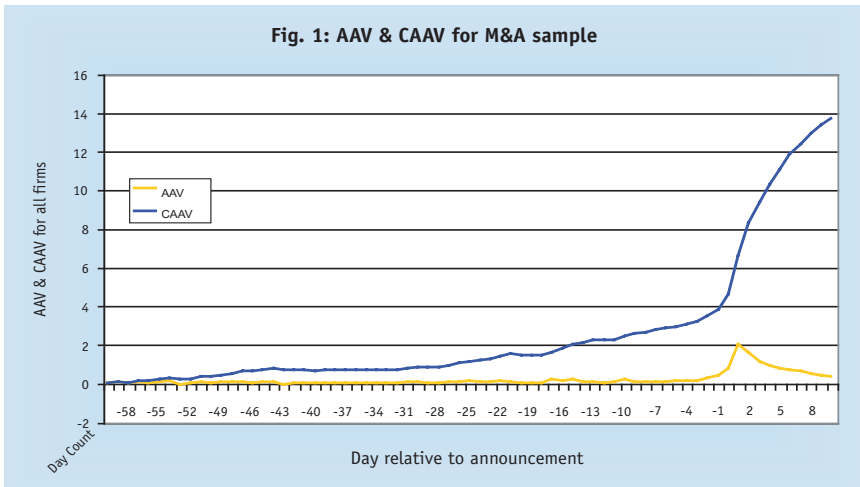
These are opposing effects, and we cannot determine in advance which impact may dominate. To establish whether we need to control for such impacts we examine the volume of shares traded starting from a period several weeks before announcements of interest to determine if pre-bid run-ups are apparent in the sample we are studying. To do this we first aggregate individual abnormal volumes across announcements to generate average abnormal volumes (AAVs), and then

28 Volume run-ups appear to be characteristic of takeover announcements. We do not observe such run-ups for FTSE350 sample of trading statement/update announcements, and therefore do not need to control for this. An analysis of the data also reveals there are no run-ups of prices.

aggregate AAVs over a time period to calculate the cumulative average abnormal volume (CAAV) for all announcements in our sample. The mathematical description of these is detailed in Annex 4.

Figure 1 shows the AAV and CAAV over the window $(-60, 10)$ for our entire takeovers sample. Visually we notice irregular days with positive AAVs during the days before announcement. A larger increase is seen on the day before the announcement, and a clear spike is seen on the day of the announcement, demonstrating the significance of these announcements.

Examining CAAVs we see a run-up in abnormal volume beginning several weeks ahead of announcement. The graph depicts a steady run-up which starts to accelerate closer to announcement dates. This indicates that an unaltered measure of expected volume is likely to be biased. On the day of the announcement itself we see a large increase, and we continue to see abnormal trading volume in the days directly after indicating increased trading once information is confirmed and uncertainty reduced.



To control for the presence of this feature in the volumes data of some issuers in our calculation of expected volume we introduce a trend variable to capture the effect of a sustained upward movement over time:

Equation 9: Daily volume traded as function of time trend

$$\text{Daily Volume Traded} = \alpha + \beta \text{ Time Trend} + \varepsilon$$

The model is estimated using daily data on volume traded over the 180 trading days ending 70 days before the announcement. The parameter β captures the extent to which there is a sustained upward movement in the behaviour of volume over time. Since our estimation window stops 70 days before the announcement we expect to capture only upward trends in volume unrelated to the announcement. α represents the expected daily volume traded in addition to any sustained time-associated movements over the period. By limiting the estimation window to 180 days ending 70 days before the announcement, we eliminate the possibility of capturing the effect of the sort of announcement-driven volume run-ups we observe above in figure 1.

Using this model over the limited estimation window will help prevent us from identifying an abnormal volume as being statistically significant in instances where volume appears to move significantly but only because volume run-ups unrelated to announcements are the cause. The model also helps prevent us from rejecting an abnormal volume as being statistically significant in instances where volume has moved significantly but we fail to identify this among announcement-associated volume run-ups in the weeks before announcement.

Having run this regression for the entire sample of takeover announcements, we identify announcements where the time trend variable is not found to be statistically significant. It is apparent that volume run-ups unrelated to announcements are not a characteristic of the data for these announcements. So, for these series we revert to calculating a simple model of expected volume, where we take the average daily volume traded for the 180 days ending 70 days before the announcement. We do not extend our estimation window to 240 days because this would reintroduce the difficulty of identifying instances where volume has moved significantly in the two-day pre-announcement window, but we fail to identify this among announcement-associated volume run-ups in the weeks before the announcement.

5.2 Results

Our analysis of trading volume for FTSE350 announcements reveals a similar overall pattern to the analysis of returns. There are significantly fewer events of abnormal pre-announcement volumes (PAVs) observed after enforcement compared to before.

However, the measure obtained using volumes is higher than that observed for returns (5.9% compared to 2%). We also notice in the intermediate period after FSMA there is a significantly smaller drop seen in the number of PAVs compared to IPMs.

Table 10: Volume results for the FTSE350 analysis

Time Period	Number of announcements	Number of ALV announcements	Number of PAVs	Measure
Before FSMA (1998/1999/2000)	487	39	7	17.9%
After FSMA (2002/2003)	734	80	12	15%
After Enforcement (2004/2005)	927	101	6	5.9%

Our analysis of trading volume for the takeovers sample reveals a pattern slightly different to that observed in our analysis of returns. The first two years of the analysis show high and similar levels of pre-announcement abnormal volume, followed by a dip observed in 2003. This is similar to the pattern observed with returns. However, the percentage of pre-announcement abnormal volume observed in 2004 is significantly lower than any other year of our study. This is directly opposite to our results using prices where the level of IPMs in 2004 is observed to be the highest among all years.

This may partly be explained by limitations in our data. Our price information covers 769 takeover announcements, traded volume information is only robustly available for 620 announcements, and information on both covers only 598 announcements. Repeating the analysis where information is available for both, we find the percentage of IPMs in 2004 to be lower than the percentage calculated using the complete sample, but this does not change the measure enough to be lower than the other years of our study.

Table 11: Volume results for the takeovers analysis

Time Period	Number of announcements	Number of PAVs	Measure
2000	126	39	30.9%
2002	124	45	36.3%
2003	125	33	26.4%
2004	86	16	18.6%
2005	159	43	27.0%

5.2.1 Are these results statistically significant?

As with the analysis of returns, we use bootstrap analysis to understand the variation in the results reported above. This allows us to test whether the level of the measure estimated for each period is significantly different from what it would be in the absence of informed trading, and whether the change of the numbers from one period to the next is statistically significant. The details of the results from these bootstrap procedures are provided in Annex 5. The conclusions from this analysis can be summarised as follows:

- For all time periods, in the takeovers analysis, the measure is above the level required to be significantly different, at the 1% level, from the one it would have if there was no informed trading. For the FTSE350 this is also true, at a 5% level, except for the 'After-Enforcement' period.
- For the FTSE350 sample, the measure in the period after enforcement is significantly different from the measure calculated for the period before FSMA at the 1% level, and from the measure after FSMA at the 5% level. But the change in the measure between the periods before and after FSMA is not statistically significant.
- The measures for the takeover announcements in 2000 and 2002 are significantly different from the measure calculated in 2004 at the 1% level. Measures in 2003 and 2005 are not statistically different from 2004 at the 1% level, but are so at the 5% level. In addition the measures in the years 2003 and 2005 are also statistically different from the measure in 2002 at the 5% and 1% level, respectively.

5.2.2 Price-Volume Relationship

We proceed in this section to examine the relationship between abnormal price and volume movements. Several studies analyse returns and volume data around announcements (e.g. Meulbroek 1992; Cornell and Sirri 1992, Wong 2002). Such empirical studies regularly find some evidence of rises in share price and increased trading volume before announcements. But there is considerably less research examining the relation between price increases and abnormal volume of trading. Two studies that do investigate this link are Gao and Oler (2004) and King and Padalko (2005). We draw on these findings to inform our analysis of the price-volume relationship we observe in our data.

Table 12: Price-Volume results relationship for the takeovers analysis

	PAVs	Not PAVs
IPMs	56	75
Not IPMs	113	354

In the takeovers sample about 43% of IPMs are also PAVs. To explain the table, the first row of data for example shows that of the 131 announcements we identify as IPMs 56 are also identified as PAVs while 75 are not. Interpretation of announcements where there is a match by both measures, the cells highlighted in grey, is clear. This is when announcements which are identified as (not) leading to abnormal pre-announcement volume are also identified as (not) leading to abnormal pre-announcement price movements. We attempt to explain the main remaining deviations below.

Some 113 announcements shown in table 12 are identified with pre-announcement abnormal volume but no significant abnormal movement in prices. There exists an incentive for investors to identify takeover targets and take positions before announcements. This may lead to increased trading in the period before announcements. Since takeover premiums are generally positive why, despite heavy trading, would information not quickly be incorporated into price? According to Gao and Oler (2004) this may occur because active selling, when we may expect informed investors to buy, may reflect the arbitrage activity of rational traders betting that takeover rumours are false.

Using a sample of rumours published in the Wall Street Journal they demonstrate that a simple strategy of selling when a rumour is published can produce significantly positive returns. Such finding depends on the market over-reacting to takeover rumours on average, and a large number of rumours not materialising into actual takeovers. However, since there are fairly strict rules leading to announcement requirements in the wake of published takeover rumours in the UK, it is not certain how valid this explanation may be for the market we are examining.

Some 75 announcements in the table are observed to be IPMs but not PAVs. Since we have already adjusted for volume run-ups in our method, potential bias created by such trends cannot explain this observation. One potential explanation for these cases could be the immediate adjustment of expectations of the value of a security through the quick spread of inside information which is viewed as credible by investors. We hypothesise that this may, for example, be likely in circumstances where a stock is relatively illiquid and held by market makers, committed principals or a small number of (institutional) investors. We do not have supporting evidence to back this hypothesis.

Table 13: Price-Volume results relationship for the FTSE350 analysis

	PAVs	ALVs	Non-ALVs
IPMs	2	5	10
Significant P	3	55	87
Non-significant P	22	165	1738

Further examination of our FTSE350 results shows that while a similar overall pattern over the periods is observed for both measures, only a very small percentage of announcements identified as significant or depicting abnormal movements by one measure (returns or volume) are identified as significant or showing abnormal movements by the other. Table 13 shows this price-volume relationship categorised by announcement type.

To explain, the first column of data for example shows that of the 25 announcements we identify as PAVs 22 are found to be non-significant and three are found to be significant in our price analysis, of which two are also identified as IPMs. Similarly, the first row of data shows 15 announcements we identify as IPMs, ten of which are found to be non-ALV and five are identified in our volume analysis as ALV announcements, of which two are also identified as PAVs. Further conclusions from this analysis are summarised below.

We observe 87 announcements with significant price movements but no associated abnormally large volumes. This depicts a desired regulatory outcome, with no observed leak before the announcement, and instantaneous adjustment in the market of expectation of the value of a stock after an announcement.

Ten out of a total of 15 IPMs are found to be non-ALV announcements. This may partially be explained by immediate adjustment of expectations of the actual value of a stock through the wide and quick spread of inside information viewed as credible by investors. Our hypothesis is that this explanation might hold in a scenario where there are few investors involved who may adjust expectations quickly. This may be the case for securities which are comparatively illiquid, traded mainly by market makers, committed principals and/or a limited number of other institutional investors. Since we would expect securities of this nature to be small in size, we checked the average size (in terms of market value of equity) of the firms responsible for these ten announcements where an IPM is detected but there is no abnormal change in volume traded. We find, in implicit support of our

hypothesis, that this is seven times smaller than the average size of a security in our total sample, and eight times smaller than the other five securities where IPMs are identified and where significant volume movements are also detected.

Twenty two out of 25 PAVs are associated with insignificant price movements. One inference could be that these announcements are not indicative of insider trading, since announcements here are not seen to be price sensitive. This could be explained by investors trading on rumours with little validity. Investors may know that an announcement is likely but believe it is equally likely to be positive or negative. In fact, these announcements may contain little price-sensitive information. Or investors may be trading in response to scheduled announcements which actually contain no new price-sensitive information. An investigation of these 22 announcements shows more than 60% are regular or potentially scheduled announcements like quarterly trading results. This would suggest that on its own, abnormal volume traded before announcements is not a good measure of insider trading. We know from our takeovers sample though that more than 40% of PAVs are also IPMs, so this inference would not necessarily seem to hold across all data samples or all types of announcements²⁹.

We observed that 165 out of 210 abnormally large volume announcements had no associated abnormal returns. This suggests the known fact that trading volume may increase in response to several non-price-sensitive announcements, for example CEOs appearing in the media, or aesthetically pleasing reports being published by firms.

5.2.3 What can we infer from the analysis of trading volumes?

The analysis of volumes at an aggregate level appears to reinforce the conclusions of our analysis of price movements. For example, for FTSE350 listed companies the analysis shows a small drop in the measure from the period before FSMA to the period after FSMA, and then a much more significant drop in the period after enforcement. This pattern follows closely the results of the returns analysis.

29 There may of course be other explanations for this observation. It is for example possible, though highly unlikely, that the volume-price lag means even post-announcement the time it takes for prices to adjust completely is not captured in our event window for some of these announcements. Alternatively, there may be price movements associated with these PAVs, too small to be captured by our levels of significance.

When we look closely at the relationship between price and volume movements of individual announcements we observe that only a relatively small proportion of IPMs are PAVs and vice-versa. This suggests that the different year-on-year pattern may be explained by other factors such as changes in level of (uninformed) speculative position-taking ahead of a scheduled announcement (e.g. trading statements), legitimate position-taking by an offeror ahead of a take-over announcement or the arbitrage activity of rational traders betting that takeover rumours are false.

We learn from this analysis, given the significant proportion of announcements identified as PAVs but not as IPMs, that abnormal volumes are not on their own likely to provide a robust indication of the level of informed trading. Only when supplemented by price information does volume data provide further use³⁰.

30 One example of an insight that may be gained from examining trading volume data for events identified as IPMs is to infer if traders may have taken sizeable advantage of inside information or not.

Annex 1 – Detail and comments on feedback received on OP23

Following the publication of OP23 we received further feedback from several sources: academics, market analysts and participants at seminars where we presented its results. The most important issues raised in this feedback were technical and have been addressed in the main body of this report – for example the suggestion that we look at volume as well as price movements. However, there are three questions that commentators raised more than once, and that we discuss here.

Are the price movements identified ahead of takeover announcements really indicative of insider trading, or could there be innocent explanations?

It was suggested that, in the days before a take-over announcement there may be signals that such an announcement is due, for example rumours in the press or meetings between key executives and advisers. Market participants may trade on these signals without breaching insider trading laws.

However, we maintain that the price movements we observe ahead of takeover announcements are signs of an unclean market. To the extent pre-announcement price increases are caused by rumour, the price increases which occur after the public announcement indicate that there was some truth to these rumours. This itself could suggest that information was leaked in contravention of the law.

Of course, from the research we have done, we cannot rule out that market participants are merely ‘piecing together’ various innocent signals to conclude that a takeover announcement is imminent. But only a close examination of the actual trades occurring ahead of the announcement will allow an assessment of the hypothesis that they are all purely innocent.

Why are only those announcements where the pre-announcement price change is in the same direction as the post-announcement one, considered to be potentially indicative of insider trading?

As stated in our original report, we do not claim that all insider trading affects prices. An insider may trade (e.g. buying ahead of a takeover announcement) without affecting the price which may remain unchanged or even fall. But we do state that in some instances insider-trading will affect prices. There are empirical reasons to believe this. In instances where the information possessed by insiders is factored into stock prices through their trading, the price of the stock must move in the direction implied by the information possessed by the insider trader, i.e.

prices should rise if the inside information is 'good news' and fall if the inside information is 'bad news'.

To be absolutely clear we are not interested in situations where traders who believe they have inside information in fact have wrong or, in the words of some economists, 'partial' information. Regardless of the legal position, in economic terms the effect of this kind of trading would rather be to dissuade the putative insider trader from seeking to engage in such conduct in future and would not appear to damage anyone other than the trader themselves.

Have similar studies been conducted in other countries and, if so, how does the UK compare?

We have found some academic studies that are based on the event study approach. However, most of those studies have a different aim, do not employ exactly the same method as ours and/or refer to a different time-period, which means that results are not directly comparable with ours.

However, there are a couple of other studies which provide some useful context for the results. The New York Times (2006) reported the results of the study it commissioned from consultancy Measured Markets. The study examined mergers over the previous 12 months and concluded that there was suspicious trading in the stocks of 41% of the offeree companies in the days and weeks before the bid.

Bhattacharya et al (2000) examined announcements on Mexico's Bolsa de Valores between 1994 and 1997. They found that stock prices failed to react to the public announcements that were made and conclude that unrestricted insider trading ahead of the announcement was the cause. While their method is very different from our own, it would suggest that the current UK market is far cleaner than this. For example, most takeover announcements in the UK lead to price movements which are extremely large in statistical terms³¹. This indicates that whatever information leakage does occur is in fact relatively limited, as markets generally are taken by surprise when the announcement is made.

31 For more than 60% of takeover announcements, price movements after the announcement are significant at the 5% level.

Annex 2 – Results obtained using alternative research parameters

5-days instead of a 2-days pre-announcement window

Table A1: The measure of market cleanliness for the FTSE350 analysis using a 5-days pre-announcement window.

Time Period	Number of announcements	Number of significant announcements	Number of IPMs	Measure	Measure using 2-days window
Before FSMA (1998/1999/2000)	487	51	8	15.6%	19.6%
After FSMA (2002/2003)	734	56	6	10.7%	11.1%
After Enforcement (2004/2005)	927	50	3	6.0%	2.0%

Table A2: The measure of market cleanliness for the takeovers analysis using a 5 days pre-announcement window.

Time Period	Number of announcements	Number of IPMs	Measure	Measure using 2-days window
2000	183	18	9.8%	24.0%
2002	147	13	8.8%	25.1%
2003	160	6	3.8%	13.8%
2004	102	10	9.8%	32.4%
2005	177	13	7.3%	23.7%

Table A2 shows that, for the takeovers analysis, the measure falls significantly when a 5-days pre-announcement window is used. This is because more than 70% of the IPMs identified using a 2-day window do not meet the higher thresholds required to be classified as an IPM when using a 5-day window. This happens for two reasons:

- The average absolute level of 5-day pre announcement CAR³² required for those announcements to be classified as an IPM is (22.3%) roughly 5 times higher than the level required for the 2-day pre-announcement CAR,³³ (4.4%).
- The average absolute value of CAR(-5,-1) (8.4%) for those announcements is very similar to the average absolute value of CAR(-2,-1) (8.6%).

This indicates that most of informed trading seems to take place in the two days before the announcement which means that the event window we used is appropriate. It may also indicate that when informed trading occurs ahead of takeovers, an announcement tends to follow shortly thereafter, which may be due to the Takeover Panel's requirement for an announcement to be made under Rule 2 of the Takeover Code.

5% threshold for significant announcements

Table A3: The measure of market cleanliness for the FTSE350 analysis using a 5% threshold for significant announcements

Time Period	Number of announcements	Number of significant announcements	Number of IPMs	Measure	Measure using 1% threshold
Before FSMA (1998/1999/2000)	487	81	17	21.0%	19.6%
After FSMA (2002/2003)	734	103	11	10.7%	11.1%
After Enforcement (2004/2005)	927	119	3	2.5%	2.0%

³² CAR(-5,-1)

³³ CAR(-2,-1)

Annex 3 – Descriptive statistics and regression results for sample-specific effects

The tables below provide descriptive statistics of the variables and the regression results underlying the analysis of sample specific effects in Section 4.

Table A4 - Descriptive statistics for the takeovers sample by time period – all announcements

Time period	2000	2002	2003	2004	2005
Market value of equity (Mean (s.d.), £ Million)	888 (3915)	222 (836)	194 (668)	1240 (6299)	282 (938)
R&D/Sales (Median (s.d.)) ³⁴	1.7% (203.0%)	21.8% (840.0%)	6.4% (3205.4%)	2.0% (2896.8%)	2.9% (234.4%)
Volatility (variance of returns) (Mean (s.d.))	0.00136 (0.00240)	0.00395 (0.00621)	0.00416 (0.01682)	0.00104 (0.00120)	0.00212 (0.00901)
Liquidity (volumes/(MVE/price)) (Mean (s.d.))	253573 (281675)	2187167 (6977261)	309199 (653562)	4161105 (2.66e+07)	1.31e+07 (5.43e+07)
2-day mean (s.d.) absolute abnormal return after the announcement	10.3% (9.0%)	10.3% (10.5%)	9.5% (14.6%)	10.0% (9.2%)	8.2% (9.0%)

Table A5 - Descriptive statistics for the FTSE350 sample by time period – all announcements

Time period	Pre-FSMA	Post-FSMA	Post-enforcement
Market value of equity (Mean (s.d.), £ Million)	8214 (26434)	6205 (22258)	3227 (10590)
R&D/Sales (Median (s.d.)) ³⁵	0.01% (0.13%)	0.01% (0.19%)	1.6% (6.67%)
Volatility (variance of returns) (Mean (s.d.))	0.00087 (0.00088)	0.00083 (0.00090)	0.00032 (0.00036)
Liquidity (MVE/(volumes/price)) (Mean (s.d.))	7172940 (4.22e+07)	5.81e+07 (7.04e+08)	1.15e+08 (8.18e+08)

34 Medians and standard deviations are only for the 141 reported values for R&D/sales in the sample (18% of all announcements).

35 Medians and standard deviations are only for the 732 reported values for R&D/sales in the sample (34% of all announcements in the sample).

Table A6 - Descriptive statistics for the FTSE350 sample by time period – significant announcements only

Time period	Pre-FSMA	Post-FSMA	Post-enforcement
Mean (s.d.) ratio of 2-day mean absolute return before the announcement and the IPM return threshold	0.30 (0.31)	0.24 (0.21)	0.13 (0.12)
Market value of equity (Mean (s.d.), £ Million)	2994 (5389)	2814 (10368)	2248 (4824)
R&D/Sales (Median (s.d.)) ³⁶	0.01% (0.04%)	0.04% (0.07%)	4.10% (5.17%)
Volatility (variance of returns) (Mean (s.d.))	0.00072 (0.00061)	0.00120 (0.00145)	0.00028 (0.00027)
Liquidity (MVE/(volumes/price)) (Mean (s.d.))	1318243 (2507038)	2180633 (4111482)	1665711 (6422205)
2-day mean (s.d.) absolute abnormal return after the announcement	7.4% (5.2%)	12.0% (10.1%)	6.3% (4.4%)

Table A7: Regression results for sample specific effects and the odds ratio of an IPM (takeover sample) according to a Logit model³⁷

	Logit coefficients	Odds ratios
Market value of equity	0.000 [1.19]	1.00 [1.19]
R&D/sales	0.000 [0.83]	1.000 [0.83]
Volatility	13.646 [0.88]	843,854.44 [0.88]
Liquidity	0.000 [1.56]	1.000 [1.56]
Two-day post-announcement CAR	0.017 [0.45]	1.017 [0.45]
2002 dummy	0.152 [0.54]	1.165 [0.54]
2003 dummy	-0.936 [2.84]***	0.392 [2.84]***
2004 dummy	0.423 [1.39]	1.526 [1.39]
2005 dummy	-0.06 [0.21]	0.941 [0.21]
Constant	-1.221 [5.16]***	
Observations: 645		

Absolute value of z statistics in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%

36 Medians and standard deviations are only for the 48 reported values for R&D/sales in the sample (31% of all significant announcements in the sample).

37 The results are presented both in terms of the ordinary Logit (log of the odds ratio) coefficients and antilog of those coefficients (the odds ratio). The odds ratio presentation is more intuitive as it can be interpreted by means of a rule of thumb: if you subtract 1 from the odds ratio and multiply the result by 100, you will get the percent change in the odds for a unit increase in the j^{th} regressor.

Table A8: Regression results for sample specific effects and the odds ratio of a significant announcement (FTSE350 sample) according to a Logit model³⁸

	Logit coefficients	Odds ratios
Market value of equity	-0.0000229 [1.78]*	0.9999771 [1.78]*
R&D/sales	-0.022 [0.57]	0.979 [0.57]
Volatility	69.497 [0.60]	1.52E+30 [0.60]
Liquidity	0.000 [0.73]	1.000 [0.73]
Post-FSMA dummy	-0.459 [2.12]**	0.632 [2.12]**
After-enforcement dummy	-0.681 [2.93]***	0.506 [2.93]***
Constant	-2.118 [10.86]**	
Observations: 2069		

Absolute value of z statistics in brackets. * significant at 10%;
** significant at 5%; *** significant at 1%

Table A9: Regression results for sample specific effects and the pre-announcement CAR (FTSE350 sample) according to an OLS model

absolute two-day post-announcement CAR	-0.008 [2.40]**
market value of equity	0 [0.02]
R&D/sales	0 [0.06]
volatility	1.689 [0.06]
liquidity	0 [0.98]
post-FSMA dummy	-0.031 [0.52]
after-enforcement dummy	-0.153 [3.05]***
constant	0.325 [6.17]***
Observations: 139	
R-squared: 0.1	

Absolute value of t statistics using 'robust' standard errors in brackets.
Three outliers in the input variables were removed, as the sample size
is very small and the model therefore sensitive to these outliers. *
significant at 10%; ** significant at 5%; *** significant at 1%

38 See footnote 37.

Annex 4 – Volume Data Characteristics and Construction of AAV & CAAV

Characteristics of Raw Traded Volume & the Effect of Log Transformation

Table A10 shows the mean, median, standard deviation, skewness and kurtosis for volume traded across firms and over time (around 560,000 observations). For a normal distribution we would expect skewness to have a value of 0, and kurtosis to be equal to 3. The measure of skewness indicates the distribution is right skewed, while the kurtosis value of 31.79 indicates that it is considerably leptokurtic (with a high centre peak or long-tailed). We also note that median and mean values are not close together. These indicators demonstrate that the daily trading volume without any transformation is not distributed normally.

Table A10: Summary statistics of FTSE350 daily trading volume without and with log transformation

	Mean	Median	Skewness	Kurtosis
Volume traded	4,139,944	1,099,392	3.89	31.79
Log transformed volume	13.33	13.92	-0.60	6.21

The frequency distribution of traded volume is plotted in Fig. A1 overlaid with the normal density function. The figure depicts a distribution highly skewed to the right. The natural log transformed distribution is plotted in Fig. A2 also overlaid with the normal distribution. This figure demonstrates how close the transformed distribution is to the normal distribution. Summary statistics in table A10 confirm the mean value to be close to the median, and skewness (-0.60) and kurtosis (6.21) statistics to be much closer to those of a normal distribution following the log transformation.

Fig. A1: Density of Trading Volume

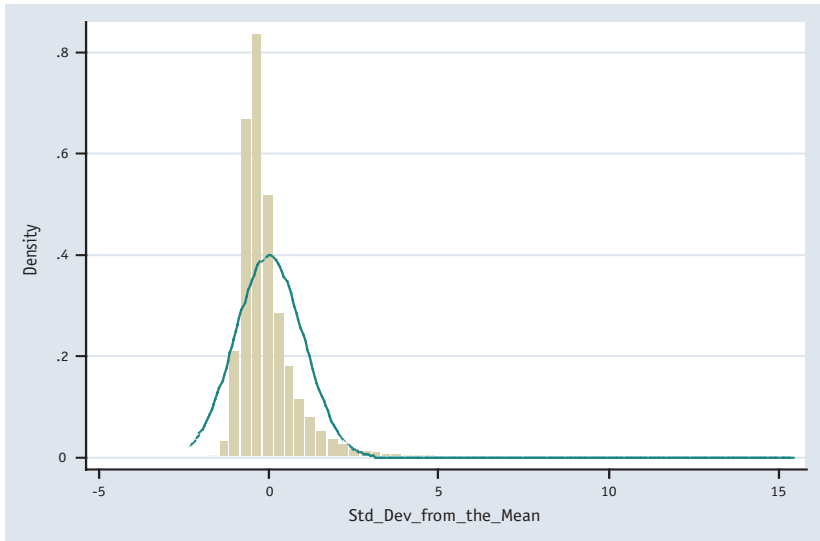
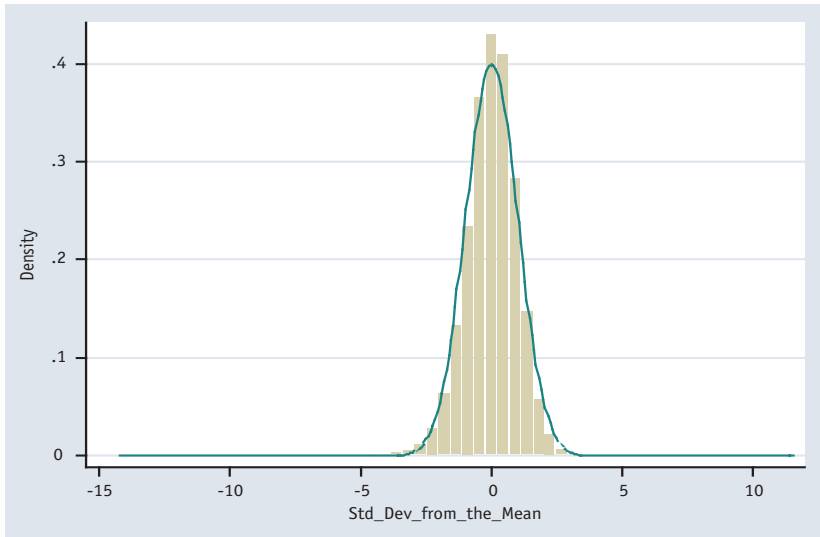


Fig. A2: Density of Log Transformed Trading Volume



Construction of AAV & CAAV

First, we aggregate individual abnormal volumes across announcements (normalised through dividing by individual means over the time period we are examining) to generate average abnormal volumes (AAVs). For our total of N announcements we calculate³⁹:

$$AAV_t = 1/N \sum_{i=1}^N AV_{it} / \overline{AV_i}$$

Then, we aggregate the AAVs over a time period (T1, T2) to calculate the cumulative average abnormal volume (CAAV) for all announcements in our sample:

$$CAAV (T_1, T_2) = \sum_{t=T_1}^{T_2} AAV_t$$

39 $\overline{AV_i}$ denotes mean of abnormal volume for event i.

Annex 5 – Mathematical specification of model and tests

Price Event Study

For each FTSE350 announcement we computed the firm's abnormal return, which is defined mathematically as:

$$AR_{it} = R_{it} - E(R_{it}) \quad (1)$$

where i refers to the firm, t to the day and where $E(R_{it})$ is the expected value of the return of firm i in day t .

To obtain the expected returns $E(R_{it})$ we begin by estimating the following linear relationship:

$$R_{it} = \alpha_i + \beta_i \cdot R_{mt} + \varepsilon_{it} \quad (2)$$

where R_{mt} is the FTSE350 return in day t and α_i, β_i are estimated for firm i by regressing the stock return on the index return in the estimation window – the 240 trading days, $t = -250$ to $t = -10$, ending 10 days before the announcement day (where the announcement day $t = 0$).

Then, we compute Durbin's alternative test against the null hypothesis that there is no first order serial correlation using the residuals from (2) to fit the following auxiliary regression:

$$\hat{\varepsilon}_{it} = \varphi \cdot \hat{\varepsilon}_{it-1} + \beta_i \cdot R_{mt} + \mu \quad (3)$$

Durbin's alternative test is computed by performing a Wald test to determine whether the coefficients of $\hat{\varepsilon}_{it-1}$ are different from zero.

We also compute the Engle's LM test for ARCH(1) effects by again using the residuals from (2) this time to fit the auxiliary regression below and obtaining $n.R^2$ as the test statistic.

$$\hat{\varepsilon}_{it} = \varphi_0 + \varphi_1 \cdot \hat{\varepsilon}_{it-1}^2 + \mu \quad (4)$$

For each announcement, if the null hypothesis is rejected for both tests we estimate, for firm i in the 240 trading day's estimation window, the following model:

$$R_{it} = \alpha_i + \beta_{1i} \cdot R_{mt} + \beta_{2i} \cdot R_{it-1} + \beta_{3i} \cdot R_{mt-1} + \varepsilon_{it} \quad (5)$$

and

$$\delta_{\varepsilon_t}^2 = a + b \cdot \hat{\varepsilon}_{it-1}^2 + c \cdot \delta_{\varepsilon_{t-1}}^2 + \mu \quad (6)$$

Giving us an expected return defined as:

$$E(R_{it}) = \alpha_i + \beta_{1i} \cdot R_{mt} + \beta_{2i} \cdot R_{it-1} + \beta_{3i} \cdot R_{mt-1} \quad (7)$$

Then, we compute a normalized abnormal return equal to:

$$nAR_{it} = \frac{R_{it} - E(R_{it})}{\sqrt{E(\delta_{\varepsilon_t}^2)}} \quad (8)$$

where,

$$E(\delta_{\varepsilon_t}^2) = a + b \cdot \hat{\varepsilon}_{it-1}^2 + c \cdot \delta_{\varepsilon_{t-1}}^2 \quad (9)$$

for the returns in the estimation window, and

$$E(\delta_{\varepsilon_t}^2) = a + b \cdot \delta_{\varepsilon_{t-1}}^2 \quad (10)$$

for those in the post-estimation window.

For those announcements where we only reject the null hypothesis of Durbin's alternative test we obtain the expected returns using OLS to estimate the model in (5) to obtain the expected returns as set out in (7) and then the normalized abnormal return using (8) where $\hat{\varepsilon}_{\varepsilon_i}^2$ is the constant estimate of the variance of the error obtained in the OLS estimation.

For those announcements where we only reject the null hypothesis of LM test for ARCH(1) effects we obtain the expected returns by estimating a model made by (2) and (6) to obtain the expected returns as:

$$E(R_{it}) = \alpha_i + \beta_i \cdot R_{mt} \quad (11)$$

and then the normalized abnormal return using (8), (9) and (10).

Finally, for announcements where we do not reject the null hypothesis of both tests we obtain the expected returns using OLS to estimate the model in (2) to obtain the expected returns as set out in (11) and then the normalized abnormal return using (8) where $\hat{\delta}_{e_i}^2$ is the constant estimate of the variance of the error obtained in the OLS estimation.

For takeover announcements the expected return is simply the average over the estimation window.

$$E(R_{it}) = \frac{\sum_{t=-250}^{-10} R_{it}}{240} \quad (12)$$

Aggregating (normalized) abnormal returns over several days results in the cumulative (normalized) abnormal return (CAR^{40}), which is defined mathematically as:

$$CAR_i(\tau_1, \tau_2) = \sum_{t=\tau_1}^{\tau_2} AR_{it} \quad (13)$$

We examine two variables: a pre-announcement CAR $\tau_1 = -2$ and $\tau_2 = -1$ and a total CAR where $\tau_1 = -2$ and $\tau_2 = +1$ for each announcement i , where τ_1 and τ_2 are the first and the last day of the period under analysis.

To obtain the distribution of 4-days CARs we generate for each firm 50,000 random samples of size 4 drawn from \hat{F} , the sample of abnormal returns for stock i in the estimation window.

$$\hat{F}_i = (AR_{i,-250}, \dots, AR_{i,-10}) \quad (14)$$

where,

$${}^4CAR_i^*(-2, +1) = S(AR_{i1}^*, AR_{i2}^*, AR_{i3}^*, AR_{i4}^*) \quad (15)$$

with

$$S = \sum_{t=1}^n AR_{it} \quad (16)$$

Note that when we use * in the notation the indexes do not refer to the actual sample but rather to the randomized version of AR_{it} .

40 From this point on we will use CAR for both the cumulative normalized abnormal returns used in the FTSE350 analysis and the cumulative abnormal returns used in the takeovers analysis.

To obtain the conditional distribution of 2-days CARs we select only the 4-days CARs draws that were statistically significant at the 1% level – i.e., less than or equal to the 250th most negative or greater than or equal to the 250th most positive of the 50,000 draws – and then take the CAR for the first two-days.

$${}^2CAR_i^*(-2,-1) = S(AR_{i1}^*, AR_{i2}^*) \Big| {}^4CAR_i^*(-2,+1) \neq 0_{at 1\% level} \quad (17)$$

Our measure p^k , for period k , is then a proportion of m^k :

$$p^k = \frac{r^k}{m^k} \quad (18)$$

m^k is the number of announcements containing significant information:

$$m^k = \sum_{i=1}^{N^k} Y_i^k \quad (19)$$

and Y_i^k equals: 1 – if the announcement is considered a ‘significant announcement’, i.e., for which the four-day CAR is less than or equal to the 250th most negative or 250th most positive simulated CAR for that announcement; and

0 – otherwise.

Note that in the takeovers analysis we are now considering all the announcements as significant and so $m^k = N^k$ the total number of announcements in period k

r^k is the number of announcements containing significant information and with suspicious price movements before the announcement:

$$r^k = \sum_{i=1}^{N^k} X_i^k \quad (20)$$

and X_i^k equals: 1 – if the announcement is considered a ‘significant announcement’ as well as an ‘IPM’, i.e., pre- announcement CARs is significant at the 10% level and the direction of the CAR is the same as that for the total CAR associated with that announcement, i.e., if it is less than or equal to the 50th most negative simulated conditional two-day CAR, when the four-day CAR was negative, or greater than or equal to the 50th most positive of the simulated conditional two-day CAR, otherwise; and

0 – otherwise.

To obtain the statistical significance of our results we combine the actual two-day post-announcement CAR for each announcement ($\tau_1 = 0$ and $\tau_2 = 1$) with a random two-day pre-announcement CAR, obtaining:⁴¹

$$^{2+2}CAR_i^*(-2,+1) = S(AR_{i1}^*, AR_{i2}^*, AR_{i0}, AR_{i1}) \quad (21)$$

where AR_{i1}^*, AR_{i2}^* are derived from \hat{F}_i in a similar way to $^4CAR_i^*(-2,+1)$ but using only 2-days samples and AR_{i0}, AR_{i1} are the real values of the abnormal return for firm i in the day and the day after the announcement.

We then apply (19) and (20) above, in order to obtain an individual observation of m^k and r^k , which we call m_{noIT}^k and r_{noIT}^k indicating that it is an estimate of r^k in the absence of informed trading since it results from a purely random process. The process is repeated 10,000 times to obtain the distribution of our measure in the absence of insider trading:

$$P_{noIT}^k = \frac{r_{noIT}^k}{m_{noIT}^k} \quad (22)$$

Finally we compare the actual measure obtained in (18) with this distribution to test if it is significantly different from what it should be in the absence of insider trading.

Table A11: Tests of measures' difference from value under H_0 – FTSE350 sample

Period	Measure	Measure under H_0 (mean, s.d.)	99% confidence interval of measure under H_0	95% confidence interval of measure under H_0
Before FSMA	19.6%	2.6%, 2.5%	[0%, 10.4%]	[0%, 8.2%]
After FSMA	11.1%	3.1%, 2.4%	[0%, 10.5%]	[0%, 8.5%]
After Enforcement	2.0%	3.4%, 2.3%	[0%, 10.3%]	[0%, 8.5%]

41 We cannot identify announcement-related CARs that are guaranteed to be uncontaminated (i.e. for which there was no insider trading). We are therefore unable to exactly replicate the conditions under the null hypothesis. As a result the bootstrap procedure that we used is only an approximation to what the market conditions would be in the absence of insider trading.

Table A12: Tests of measures' difference from value under H_0 – takeovers sample

Period	Measure	Measure under H_0 (mean, s.d.)	99% confidence interval of measure under H_0	95% confidence interval of measure under H_0
2000	24.0%	10.2%, 1.9%	[0.6%, 15.3%]	[0.7%, 14.2%]
2002	25.1%	10.5%, 2.2%	[0.6%, 16.6%]	[0.7%, 15.1%]
2003	13.8%	13.2%, 2.1%	[0.8%, 19.4%]	[0.9%, 17.6%]
2004	32.4%	11.1%, 2.5%	[0.6%, 17.6%]	[0.7%, 16.7%]
2005	23.7%	10.2%, 1.9%	[0.6%, 13.9%]	[0.7%, 13.9%]

From the distribution of the measure under H_0 for each period, we generate for each firm a random sample of size 2 drawn from $\hat{G}_j^{k_1}, \hat{G}_j^{k_2}$ where:

$$\hat{G}_j^{k_1} = (p_{TS,1}^{k_1}, \dots, p_{TS,10000}^{k_1}) \quad (23)$$

$$\hat{G}_j^{k_2} = (p_{TS,1}^{k_2}, \dots, p_{TS,10000}^{k_2}) \quad (24)$$

We then form pairs of the random drawings of the measure and apply the difference between the 2 values, such that:

$$DIF^* = T(p_{TS}^{k_1*}, p_{TS}^{k_2*}) \quad (25)$$

where,

$$T = p_{TS}^{k_2} - p_{TS}^{k_1} \quad (26)$$

By repeating this process 10,000 times we obtain the distribution of the difference in the measure between two periods, under the null hypothesis that there is no change in the level of informed trading.

Table A13: Tests for differences in measure between periods – FTSE350 sample

Periods	Actual value of difference	99% confidence interval of measure under H_0	95% confidence interval of measure under H_0
After FSMA – Before FSMA	-8.5%	[-9.3%, 8.9%]	[-7.0%, 6.6%]
After Enforcement – Before FSMA	-17.5%	[-9.1%, 8.6%]	[-6.8%, 6.5%]
After Enforcement – After FSMA	-9.1%	[-9.0%, 8.6%]	[-6.4%, 6.5%]

Table A14: Tests for differences in measure between periods – takeovers sample

Periods	Actual value of difference	99% confidence interval of measure under H_0	95% confidence interval of measure under H_0
2002 – 2000	1.4%	[-6.9%, 7.8%]	[-5.1%, 5.9%]
2003 – 2000	-10.2%	[-4.1%, 10.2%]	[-2.3%, 8.6%]
2004 – 2000	8.2%	[-6.8%, 8.9%]	[-4.8%, 7.1%]
2005 – 2000	-0.7%	[-7.0%, 6.8%]	[-4.8%, 5.1%]
2003 – 2002	-11.6%	[-4.6%, 10.6 %]	[-3.1%, 8.6%]
2004 – 2002	6.8%	[-7.3%, 9.5%]	[-5.5%, 7.1%]
2005 – 2002	-2.2%	[-7.8%, 6.9%]	[-5.9%, 5.3%]
2004 – 2003	18.5%	[-10.5%, 6.0%]	[-8.4%, 4.1%]
2005 – 2003	9.4%	[-10.3%, 3.9%]	[-8.6%, 2.2%]
2005 – 2004	-9.0%	[-9.0%, 7.0%]	[-7.1%, 5.1%]

Volume Event Study

For each FTSE350 announcement we computed the abnormal volume for the firm, which is defined mathematically as:

$$AV_{it} = V_{it} - E(V_{it}) \quad (27)$$

where i refers to the firm, t to the day and where $E(V_{it})$ is the expected volume traded of firm i in day t .

To obtain the expected volume $E(V_{it})$ we begin by estimating the following linear relationship:

$$V_{it} = \beta_{0i} + \beta_{1i} V_{mt} + \beta_{2i} Mon_{it} + \beta_{3i} Tue_{it} + \beta_{4i} Wed_{it} + \beta_{5i} Thu_{it} + \beta_{it} \quad (28)$$

where V_{mt} is the FTSE350 volume traded on day t and Mon-Thu are day-of-the-week dummy variables. β s are estimated for firm i by regressing the stock volume on the index volume and the day-of-the-week dummies in the estimation window – the 240 trading days, $t = -250$ to $t = -10$, ending 10 days before the announcement day (where the announcement day $t = 0$).

EGLS is used to estimate the model in (28). This gives us expected volume defined as:

$$E(V_{it}) = \beta_{0i} + \beta_{1i} V_{mt} + \beta_{2i} Mon_{it} + \beta_{3i} Tue_{it} + \beta_{4i} Wed_{it} + \beta_{5i} Thu_{it} \quad (29)$$

For each takeovers announcement we similarly calculate abnormal volume as defined in (27). To estimate expected volume we begin by calculating the following relationship:

$$V_{it} = \alpha_i + \beta_i \cdot TimeTrend + \varepsilon_{it} \quad (30)$$

where $TimeTrend$ is a trend variable capturing a sustained movement in the behaviour of the volume of a stock over time, and α, β are estimated for firm i by regressing the stock volume on the time trend in the estimation window – the 180 trading days, $t = -250$ to $t = -70$, ending 70 days before the announcement day (where the announcement day $t = 0$).

We use OLS to estimate the model in (30). We then compute t-statistics to confirm/reject the null hypothesis that there is no linear trend in volume.

For those announcements where we reject the null hypothesis we obtain the expected volume:

$$E(V_{it}) = \alpha_i + \beta_i \cdot TimeTrend \quad (31)$$

For announcements where we do not reject the null hypothesis, expected volume is simply calculated as the average over the estimation window:

$$E(V_{it}) = \frac{\sum_{t=-250}^{-60} V_{it}}{180} \quad (32)$$

Aggregating abnormal volumes over several days results in the cumulative abnormal volume (CAV⁴²), which is defined mathematically as:

$$CAV_i(\tau_1, \tau_2) = \sum_{t=\tau_1}^{\tau_2} AV_{it} \quad (33)$$

We examine two variables: a pre-announcement CAV $\tau_1 = -2$ and $\tau_2 = -1$ and a total CAV where $\tau_1 = -2$ and $\tau_2 = +1$ for each announcement i , where τ_1 and τ_2 are the first and the last day of the period under analysis.

To obtain the distribution of 4-days CAVs, the conditional distributions of 2-day CAVs, and then to identify abnormal volumes' announcements and calculate confidence intervals under H_0 , we follow exactly the same process as defined in equations (14) – (22) for abnormal returns.

Table A15: Tests of volume measures' difference from value under H_0 – FTSE350 sample

Period	Measure	Measure under H_0 (mean, s.d.)	99% confidence interval of measure under H_0	95% confidence interval of measure under H_0
Before FSMA	19.0%	5.3%, 4.6%	[0%, 20.0%]	[0%, 16.0%]
After FSMA	16.1%	5.2%, 3.5%	[0%, 16.2%]	[0%, 13.2%]
After Enforcement	6.5%	4.3%, 2.7%	[0%, 12.5%]	[0%, 10.4%]

Table A16: Tests of volume measures' difference from value under H_0 – takeovers sample

Year	Measure	Measure under H_0 (mean, s.d.)	99% confidence interval of measure under H_0	95% confidence interval of measure under H_0
2000	30.9%	6.5%, 2.2%	[1.6%, 12.7%]	[2.4%, 11.1%]
2002	36.3%	7.5%, 2.3%	[2.4%, 14.3%]	[3.2%, 12.1%]
2003	26.4%	8.8%, 2.5%	[3.2%, 16.0%]	[4.8%, 14.4%]
2004	18.6%	7.8%, 2.8%	[1.2%, 15.1%]	[2.3%, 14.0%]
2005	27.0%	8.4%, 2.1%	[3.8%, 13.8%]	[4.4%, 12.6%]

42 From this point on we will use CAV for both the cumulative abnormal volumes used in the FTSE350 analysis and the cumulative abnormal volumes used in the takeovers analysis.

From the distribution of the test statistic for each period, the distribution of the difference in the measure between two periods is generated as defined in equations (23) – (26).

Table A17: Tests for differences in volume measure between periods – FTSE350 sample

Periods	Expected value of difference	99% confidence interval of difference under H_0	95% confidence interval of difference under H_0
After FSMA – Before FSMA	-2.9%	[-14.3%, 17.3%]	[-10.8%, 12.5%]
After Enforcement – Before FSMA	-12.6%	[-10.8%, 17.6%]	[-8.2%, 12.8%]
After Enforcement – After FSMA	-9.6%	[-10.0%, 13.5%]	[-7.1%, 10.2%]

Table A18: Tests for differences in volume measure between periods – takeovers sample

Periods	Expected value of difference	99% confidence interval of difference under H_0	95% confidence interval of difference under H_0
2002 – 2000	5.3%	[-8.9%, 7.1%]	[-7.3%, 4.7%]
2003 – 2000	-4.6%	[-11.2%, 6.3%]	[-8.8%, 3.9%]
2004 – 2000	-12.3%	[-10.4%, 7.6%]	[-8.3%, 5.7%]
2005 – 2000	-3.9%	[-9.2%, 6.1%]	[-7.7%, 4.0%]
2003 – 2002	-9.9%	[-10.3%, 7.3%]	[-8.0%, 5.7%]
2004 – 2002	-17.7%	[-9.9%, 9.0%]	[-7.6%, 6.6%]
2005 – 2002	-9.3%	[-8.7%, 7.3%]	[-6.8%, 5.5%]
2004 – 2003	-7.8%	[-8.7%, 10.9%]	[-6.4%, 8.5%]
2005 – 2003	0.6%	[-7.5%, 9.0%]	[-5.5%, 6.9%]
2005 – 2004	8.5%	[-9.2%, 8.9%]	[-7.2%, 6.5%]

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