Event Study under Disturbed Estimation Period

by

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Abstract

Since the formalized approach adopted by Fama, Fisher, Jensen and Roll (1969), event studies have become an important reference tool for empirical research in Finance. The original methodology has been improved in order to tackle numerous problems such event-date uncertainty, event clustering, event-induced variance phenomenon… Somewhat surprisingly, the determination of the estimation period has attracted less interest. It remains most frequently routinely determined as a fixed window prior to the event announcement day, during which it is supposed that no other significant events have happened. In practice, in large sample studies, validation of this assumption on a case-by-case basis is out of reach, despite the fact that it is known to be violated for some specific corporate events. The case of merger and acquisitions, in particular the behavior of bidders who make repetitive acquisitions (and acquisition attempts), is a typical example. We propose in this work an adaptation of the basic methodology by explicitly taking into account the likely existence of firm-specific events during the estimation period. We first carry out a standard specification and power analysis, following the Brown and Warner (1980, 1985) scheme. We then show that the proposed method significantly changes the inferences using a sample of around 580 merger and acquisition operations concerning the bidders' abnormal returns.

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1. INTRODUCTION

Since Fama, Fisher, Jensen and Roll (1969), hereafter FFJR, event studies have become a standard empirical research methodology in Finance. Here we will take for granted that the FFJR approach is well known to the reader[1]. Applications have been so numerous that it would be hopeless even to try to list them exhaustively. Numerous suggestions have also been put forward to improve the basic FFJR scheme. Without any ambition of completeness, we can quote the works of Brown and Warner (1980, 1985), where the specification and power of several alternative modifications of the FFJR scheme are analyzed; Ball and Torous (1988), who explicitly take into account the uncertainty about the event dates; Corrado (1989), who introduces a robust test of significance; Boehmer et al. (1991), who adapt the methodology in order to tackle the event-induced variance phenomenon; and Salinger (1992) who systematically defines the correct abnormal returns standard errors, with and without event clustering, taking into account the correction factor due to the forecasting nature of the estimated abnormal returns during the event window.

Yet, and maybe somewhat surprisingly, the estimation period, used to fit the parameters of the chosen return-generating process, has been less extensively analyzed. It is most often defined as a period situated before the event, sufficiently long to properly estimate what should happen in the absence of an event. In studies using daily data, a window going from day -250 to day -30 relative to the event day is classically chosen, somewhat mechanically. A shorter period can then be used between the end of the estimation period and the beginning of the event period, if we wish to neutralize the impact of information leakages (or rumors) before the announcement. This mechanical choice of the estimation period is however not without raising problems, particularly in the framework of large sample empirical studies, which are currently more and more frequent (see e.g. Fuller et al. (2002), Mitchell and Stafford (1999)…). When compiling the data for several thousands of observations, it becomes out of reach to analyze the estimation period ,on a case-by -case basis, in order to be sure that it corresponds to a normal period, without any

[1] Numerous well-done presentations have been published, among which Chapter 4 of Campbell et al. (1997) deserves to be quoted.

other significant firm-specific disturbing events. As we will show in the sequel, there exists a significant risk to bias the analysis.

Let us take the case of mergers and acquisitions (M&A) as a practical example. Imagine that the bidder has, during the months preceding a specific operation, realized other operations, as appears to be frequently the case[2]. The existence of such firm-specific events in the estimation window will definitively affect the estimated values of the return-generating process and, hence, the estimated normal returns during the event window. Maybe more importantly, it will also have a significant impact on the return-generating model residuals variance, frequently used to test the statistical significance of the observed abnormal returns around the event day[3]. As our results clearly show, this reduces the power of the event study methodology (even when including the recent refinements proposed by the literature). In other words, it limits its ability to detect abnormal returns when there are abnormal returns.

Our paper addresses this issue. We first suppose that, essentially for practical reasons, it is almost impossible to verify *manually* and systematically on a case-by-case basis and for all the observations included in the sample that there are non-firm specific events disturbing the estimation period. We then propose an adaptation of the event study methodology in such a way that it automatically takes into account the potential presence of firm specific disturbances in the estimation period. Our approach is essentially based on a combination of the well-established market model of Sharpe (1963) and the more recent Markov Switching Regressions models (MSR), widely introduced and developed by Hamilton (1989, 1994) and significantly extended in Krolzig (1997). Our initial intuition is simple: the occurrence of firm-specific events has a significant impact on the firm's return-generating process, in particular on the variance of the generated returns. We will try to capture this disturbance by a switching regime model. We will then use the estimated parameters of the normal regime (which should correspond to the estimation window as initially defined by FFJR, this is to say, to a period without extraordinary events) as parameters used to conduct the statistical analysis of abnormal returns during

[2] Malatesta and Tompson (1985) already point out that bidders frequently follow a strategy of acquisitions' program and suggest an adapted methodology to evaluate its global impact. The recent contribution of Fuller et al. (2002) emphasizes the importance of this point.

[3] See Salinger (1992) for a systematic analysis of the ways to build a correct test of significance.

the event window. It makes some sense to understand our approach as a statistical filter of the data, allowing us to neutralize disturbing events present in the estimation period, without requiring a manual case-by-case analysis of all the samples observation. Another way to interpret our proposition is to see it as a better specified return-generating model, which takes into account the occurrence of the probability of firm-specific events, and therefore, leads to better specified and more powerful statistical tests. In fact, the way that we propose to model the normal return is somewhat in line with Roll's (1987) intuition. According to Roll, the true return generating process seems to be better described by a mixture of two distributions, the first one corresponding to a state of information arrival and the other one to the normal return behavior.

The approach adopted in this paper is now classical in the field of event study methodology and follows that of Brown and Warner (1980, 1985) or Boehmer et al. (1991), to quote only the most classical references. In a first step, using a large sample of firms (around three thousands quoted US firms included in the RUSSELL 3000 index), we first carry out a specification and power analysis of several alternatives to the classical event study approach under disturbed estimation period. We specially highlight the fact that, when disturbing the estimation period by simulated abnormal returns, the results obtained using the standard approaches quickly become biased. We then show that our proposition seems to be more robust to such alterations. We finally illustrate that, using a real and large dataset in the field of M&A, indeed, for bidders and as previous studies anticipated(see in particular Fuller et al. (2002)), taking into account the repetitive nature of the bidders behavior significantly modifies the inferences from the bidders abnormal returns. In the light of the generally accepted result that, on average and on a global sample, bidders do not undergo significant abnormal returns, this is without doubt the main contribution of our paper

The next section of this document is devoted to a short presentation of the most classically proposed event study approaches. Using the same set of notations, we also introduce in this section our approach and summarize some of the features of the MSR family of models. Section 3 is dedicated to the simulation work. We present the dataset used, the methodology followed to realize specification and power analyses and our results. Section 4 presents the results obtained when applying our propositions to a real sample of M&A operations from the European context. We finally conclude in Section 5.

2. EVENT STUDY METHODOLOGY

The seminal contribution of FFJR was the starting point of an impressive diffusion of the event study methodology in empirical finance. Its constitutive steps are well-known: determination of the event, determination of its announcement date, determination of the event and estimation windows, estimation of the return generating model parameters with the estimation window data set, computation of the abnormal returns (the residuals of the normal model during the event window), and if required, computation of the cumulative abnormal returns, averaging of the abnormal returns on the sample and construction of a statistical test of their significance. We will, in this section, focus on the choice of the return-generating process and the construction of the statistical test of significance, the two key points as far as our work is concerned.

2.1. Return-generating model

In the classical framework of the event-study methodology, abnormal returns are defined as the forecast errors of a specific normal return-generating model. The classical Market Model (MM), introduced by Sharpe (1963), is the most frequently chosen model[4]:

$$R_{jt} = \alpha_j + \beta_j R_{mt} + \varepsilon_{jt} \tag{1}$$

where R_{jt} and R_{mt} are the return of asset j and the return of the market index at time t respectively. The residuals, ε_{jt}, provide the estimation of the abnormal returns. Initially, the residuals were classically supposed to be identically and independently Gaussian distributed (NIID). Numerous contributions have dealt with violations of these

[4] Brown and Warner (1980, 1985) have shown that the results of an event study are not sensitive to the choice of a specific return generating process. These results are confirmed by Cowan and Sergeant (1996), who show that the use of Williams and Scholes (1977) approach does not add a lot, and more recently, Aktas et al. (2002) find that there is no significant difference when comparing the results obtained with the constant mean modem return, the market model or the Williams and Scholes approach on a European sample of business combination announcements.

hypotheses. For example, Rubacq (1982) suggests an easy to implement way to cope with the existence of first-order auto-correlation in asset returns. As our work focus specifically on the NIID hypotheses, we will dedicate to it section 2.2. The residuals' variance during the estimation period is denoted by σ_j^2, while their variance during the event period is denoted by $\sigma *_j^2$ [5]. The estimation of the MM parameters is realized by OLS.

2.2. Statistical test of significance

In order to introduce the different approaches that will be submitted to disturbances during the estimation period, we will use the same set of notations as in Boehmer et al. (1991):

- N: number of firms in the sample;
- A_{jE}: abnormal return of firm j at the event date;
- A_{jt}: abnormal return of firm j at date t;
- T: number of days during the estimation period;
- $\overline{R}_m$: average return of the market index during the estimation period;
- $\hat{S}_j$: standard deviation of firm j abnormal returns during the estimation period
- SR_{jE} : standardized abnormal returns of firm j at the event date, corrected in order to take into account the forecasting nature of the estimated abnormal returns (see Boehmer et al. (1991) or Salinger (1992)). They are computed as:

$$SR_{jE} = A_{jE} / \hat{S}_j \sqrt{1 + \frac{1}{T} + \frac{(R_{mE} - \overline{R}_m)^2}{\sum_{t=1}^{T} (R_{mt} - \overline{R}_m)^2}} \quad (2)$$

The Boehmer et al. (1991) standardized residuals test (BSR)

[5] The variance of the forecast errors is denoted by $\hat{\sigma}_j^2$. It is by definition superior to σ_j^2, as clearly

As for each of the following tests, the authors test the null hypothesis of no cross-sectional average (cumulative) abnormal returns around the event date. The BSR test is similar in spirit to the Patell (1976) one, but the authors propose the use of estimated cross-sectional variance of standardized abnormal returns instead of the theoretical one. This adaptation of the Patell (1976) approach allows us to take into account the observed increase in the cross-sectional abnormal returns variance around the event date (called by the authors the event-induced variance phenomenon and which is due to firm specific reactions to the event announcement). The BSR test takes the following form:

$$Z=\frac{\frac{1}{N}\sum_{j=1}^{N}SR_{jE}}{\frac{1}{N(N-1)}\sqrt{\sum_{j=1}^{N}(SR_{jE}-\sum_{i=1}^{N}\frac{SR_{iE}}{N})^{2}}} \tag{3}$$

Boehmer et al. (1991) show that, defined as such, the test is well specified for a sample firms selected in the NYSE-AMEX universe under the null hypothesis (no average abnormal returns), even if the cross-sectional variance increases during the event period. When there is no such increase, the BSR is moreover as powerful as the classical Patell (1976) standardized residuals test. Campbell and Wasley (1993) show us the specification problem that affect the Pattel (1976) test in case of event clustering (absence of event independence). For the same reason, the same limit affects, for the same reason the BSR test. The BSR method has been so frequently used in empirical tests since its introduction that it has become the reference. We use it in the sequel as the benchmark.

The Corrado (1989) rank test (RANK)

Corrado (1989) introduces a test based on the ranks of abnormal returns, releasing in this way any hypotheses in the form of the distribution of abnormal returns (except the independence one among the observations). For each firm in the sample, the RANK test merges the estimation and event windows abnormal returns in a unique time series.

highlight in Salinger (1992).

Abnormal returns are then sorted and a rank is attributed to each day. Let K_{jt} be the rank attributed to firm j abnormal return on day t. By convention, rank one is attributed to the lowest observed abnormal returns. By construction, the mean rank is half the number of days in the constituted series of abnormal returns (the number of the days of the estimation period plus the number of days of the event period) plus one half. We note it $\overline{K}$. The RANK test takes then the following form:

$$T = \frac{\frac{1}{N}\sum_{j=1}^{N}(K_{jE} - \overline{K})}{S(K)} \tag{4}$$

where the standard error, *S(K)*, is :

$$S(K) = \sqrt{\frac{1}{250}\sum_{t=1}^{250}\left(\frac{1}{N}\sum_{j=1}^{N}(K_{jt} - \overline{K})\right)^2} \tag{5}$$

As usual, the use of ranks neutralizes the impact of the form of the abnormal returns distribution (skewness, kurtosis, outliers, …). Corrado (1989), Corrado and Zivney (1992) and Campbell and Wasley (1993) show using simulations that the test is generally well specified and robust. Campbell and Wasley (1993) also reports that the test seems to be robust to event clustering among observations, when using the market model as return-generating process and an equal-weighted market index. As the RANK test has also been classically used as a robust alternative to the BSR one, we will also use it in the sequel as a benchmark.

Other suggestions

Numerous other modifications of the initial FFJR methodology have been proposed. We quickly quote some of them because they could appear at first sight interesting alternatives to our propositions.

Ball and Torous (1988) study the case of event date uncertainty. Using a maximum likelihood estimator, they simultaneously estimated, for each day of the event window, the abnormal returns, their variance and the probability of an event. Using simulations, the authors show that their approach is more powerful (more frequently detect simulated abnormal returns) than the classical ones when the event date is uncertain. Conceptually, such an approach could be adapted to our problem (the potential presence of disturbing events during the estimation period). The approach seems attractive, even at first sight. While Ball and Torous (1996) estimate the probability of an event during the event window, we could, using the same approach, estimate the probability of an event during the estimation window. Days for which this probability is too high could be neutralized. In practice however, a thorough examination of the Ball and Torous approach reveals that the increase in the number of days in the study period[6] and of the number of potential events[7] makes it computationally non tractable.

Nimalendran (1994) introduces an approach based on a return generating process that jointly combines a Poisson process (in order to capture jumps in the distribution of returns) and a standard Brownian motion. The author's intuition is quite close to ours: the Poisson process allows dissociation of the normal behavior (the Brownian motion) from the days of information arrival. His findings put forward that, for event with multiple announcements spread over a significant period of time, the proposed approach is more powerful than the classical ones. Again, a careful examination of the Nimalendran propositions reveals that it is not well adapted for short event period such the one on which we focus. Either the number of observations is too short to realize any estimation or the event window becomes too large to attribute the observed abnormal returns to a single event with confidence.

The Markov Switching Regression test (MSR)

The intuition on which the MSR test is based is quite straightforward: we anticipate that firm specific events will, during the event period, change the return variance. It could be argued that it is in contradiction with the semi-strong form of efficiency hypothesis, under which the prices should adjust immediately to any public information announcement. It is in fact not so, if we take into account the uncertainty attached to

[6] The estimation period is classically far longer than the event one.
[7] We cannot exclude the presence of several firm specific events during the estimation window.

firm-specific events. Let us take again the example of M&A. The announcement of a takeover does in no way guarantee its success. The initial announcement could be followed by the announcement of competitors' bids, bid price revisions, target initiatives to block the operations, anti-trust authorities interventions… All of this will generate a strong increase in the variance during the event period. Let us now imagine that such firm-specific events occur during the chosen estimation period. Classical tests, such as the BSR or the RANK ones, will in fact overestimate the abnormal returns variance during the estimation period, leading to a downward bias (less powerful) in the test of significance during the event window.

To deal with this bias, the MSR test uses the Markov Switching Regression approach, widely introduced and developed by Hamilton (1989, 1994). We will suppose that the return generating process can be adequately modeled using a two-regime process[8], one regime with normal variance and one regime with high variance (firm-specific event regime). In both regimes, the MM parameters are assumed to be the same. The return-generating process is therefore the following:

$$
\begin{aligned}
&R_{jt} = \alpha_j + \beta_j \cdot R_{m,t} + \varepsilon_{j,1,t} \ \textit{if} \ S_t = 1 \ \textit{with} \ \varepsilon_{j,1,t} \ \textit{following} \ N(0,\sigma_{j,1}) \\
&R_{jt} = \alpha_j + \beta_j \cdot R_{m,t} + \varepsilon_{j,2,t} \ \textit{if} \ S_t = 2 \ \textit{with} \ \varepsilon_{j,2,t} \ \textit{following} \ N(0,\sigma_{j,2}) \\
&\textit{and} \ \sigma_{j,2} > \sigma_{j,1}
\end{aligned} \tag{6}
$$

where S_t is an indicator variable taking value 1 if we are in the low variance regime and 2 if we are in the high variance regime. The proposed model is a direct and parsimonious extension of the classical MM. As regime state variable S_t is not directly observable (recall that we assume the practical impossibility to verify *manually* on a case-by-case basis the presence of firm-specific events during the estimation period), we have to specify its statistical properties. We rely for this on the Markov Switching Regression approach: S_t is described by a first-order Markov process (S_t depends only on S_{t-1}). The Markov chain is therefore defined by four transition probabilities:

[8] This hypothesis is supported by unreported results where, using the formal equivalence between ARMA models and some families of Markov Switching Auto Regressive models developed in Krolzig (1997), we find a two regimes model (three regimes in the case of the presence of strong outliers) is an adequate representation of the return generating process.

$$\begin{aligned}
P(S_t = 1 | S_{t-1} = 1) = p_{11} \\
P(S_t = 1 | S_{t-1} = 2) = p_{12} \\
P(S_t = 2 | S_{t-1} = 1) = p_{21} \\
P(S_t = 2 | S_{t-1} = 2) = p_{22}
\end{aligned} \tag{7}$$

where p_{ij} is the probability of regime *i* in *t-1* and *j* in period *t*. All probabilities have to be positive and, for *i* equal to 1 and 2, $p_{i1}+p_{i2}$ must be equal to one. The model we propose is therefore based on the estimation of six parameters (α, β, σ_1, σ_2, p_{11} *and* p_{21}) and, while for more flexibility than the classical MM one (as we will stress in the next paragraph), remains really parsimonious.

The estimation of Markov Switching Regression models is fully presented in Hamilton (1994). It is based on a maximum likelihood approach, for which an efficient estimation algorithm has been developed[9]. The three central relations on which the estimation process is built are the following. The density of the observations is:

$$f(R_{jt} | S_t = s, \Omega_{t-1}; \theta) = \frac{1}{\sqrt{2\pi}\sigma_{j,s}} \exp\left\{ \frac{-(y_{jt} - \alpha_j - \beta_j \cdot R_{m,t})^2}{2\sigma_{j,s}^2} \right\} \tag{8}$$

where Ω_t is the information set at period *t*. As we cannot observe S_t, the unconditional (relative to the regime) density is (in our two-regime case):

$$f(R_{jt} | \Omega_{t-1}; \theta) = f(R_{jt}, S_t = 1 | \Omega_{t-1}; \theta) + f(R_{jt}, S_t = 2 | \Omega_{t-1}; \theta) \tag{9}$$

Using the definition of the conditional probability, we obtain:

[9] Hamilton (1994) (p. 688-689) presents an Expectation – Minimisation algorithm that proves to be numerically very efficient.

$$f(R_{jt}|\ \Omega_{t-1};\theta)=\left[f(R_{jt}|\ S_t=1,\Omega_{t-1};\theta)\times P(S_t=1|\ \Omega_{t-1};\theta)\right]+\left[f(R_{jt}|\ S_t=2,\Omega_{t-1};\theta)\times P(S_t=2|\ \Omega_{t-1};\theta)\right] \tag{10}$$

Estimation of the density of the observations therefore necessitates the estimation of the probability of being at date *t* in regime *j* ($P(S_t=s|\ \Omega_{t-1};\theta)$). This is realized by a filtering process. Hence, the estimated probability of being in a specific state at a specific date is one of the interesting by-products of the approach advocated. It allows confirmation, on a sub-sample, on a case-by-case basis, of the validity of the inferred presence of firm-specific events. In other words, by looking at period of high probability of regime of high variance, it is possible to verify that these clusters of variance have indeed been generated by firm-specific events.

Our MSR test is thus a straightforward adaptation of the BSR test obtained by modifying the standardization procedure of the abnormal returns (equation 2). We now divide the estimated abnormal returns by their estimated standard deviation in regime 1 (low variance), taking into account, as in the BSR test, the forecasting nature of the abnormal returns during the event window. Standardized residuals therefore take the following form:

$$SR_{jE}=A_{jE}/\hat{S}_{j,1}\sqrt{1+\frac{1}{T}+\frac{(R_{mE}-\bar{R}_m)^2}{\sum_{t=1}^{T}(R_{mt}-\bar{R}_m)^2}} \tag{11}$$

where $\hat{S}_{j,1}$ is the estimated standard deviation in the low variance regime. Contrary to what could be expected at first sight, the use of the low regime standard deviation does not automatically produce higher cross-sectional student statistics, as demonstrated in the homoscedastic case in appendix 1. Note also that, for specification analysis, we use the average standard deviation of the two regimes with the probabilities of each regime as weight[10].

The use of this Markov Switching Regression model provides numerous interesting features. Besides the above-quoted possibility to obtain the estimated probability of being in a specific regime at a specific date, this specification is in-line with Roll's (1987) intuition. In this Presidential Address of the American Finance Association, he clearly stress the fact that the true return generating process is better described by a mixture of two distributions, the first one corresponding to a state of information arrival and the other one to the normal return behavior[11]. The specification has other attractive features. The Gaussian conditional distribution of return (equation 8) could be misleading. While it imposes a Gaussian assumption for return-distribution in each state, as shown in Hamilton (1994) and Krolzig (1997), it allows to capture the skewness and kurtosis in the unconditional distribution (equation 9). The Markov Switching Regression framework also allows for conditional heteroscedasticity without imposing a specific form on the conditional dependence of the variance (as in the (G)ARCH framework[12]). Finally, the estimation process provides us simultaneously the abnormal returns, the estimated variance in each regime, the probability of being in a specific regime at a specific date and the estimated transition probabilities[13].

3. SPECIFICATION AND POWER TESTS

The investigation of the specification and power of the MSR test follows the procedure introduces in Brown and Warner (1980,1985) and classically used since then (see e.g. Boehmer et al. (1991), Corrado (1989, 1992), Cowan (1992), Cowan et Sergeant (1996),

[10] Specification analysis is done under the null hypothesis of no event. As we are working on real data and we do not know whether there has been an event at the selected date, the weighted average standard deviation indedd represents its expected value.

[11] It should however be emphasized that the author also suspect that no-news day is featured by a mixture of distribution, which is not taken into account here.

[12] Hilliard and Savickas (2000) propose an event study methodology built on the GARCH framework. Aktas et al. (2001) is another example of the use of GARCH models to estimate the variance of the abnormal returns.

[13] Following the advice of Professor Hamilton, all estimations presented in this paper have been realized under the Ox econometric software, using the Krolzig MSVAR package, freely downloadable at http://www.nuff.ox.ac.uk/Users/Doornik/index.html.

…). In contrast with Monte-Carlo simulation, in the framework of which data are generated using a theoretical return generation process specification, the authors build their data samples from real returns computed from the CRSP[14] database. They randomly pick firms in the NYSE/AMEX/NASDAQ universe. For each firm, they randomly determine an event date and generate simulated events by injecting pertubations in the real price series. Proceeding in this way avoids any assumption concerning the return-generating process and allow to be as close as possible to the situation faced when using event studies on a real dataset.

3.1. Data

Our firms' universe is composed of the RUSSELL 3000 index, which includes a large sample of quoted US firms from the NYSE, AMEX and NASDAQ. This index encompasses around 98% of total US capitalization. The mean individual firm market value is 4 billion US $ while the median one is 0.7 billion US $. The minimum and maximum are respectively 0.128 billion US $ and 309 billion US $. For all the firms included in this index, we use daily prices from 1/1/1990 to 3/1/2002. We use as market portfolio the Standard & Poors 500 index. The data were obtained from Datastream accessed at the Université de Lille II.

3.2. Sample generation

All firms and event dates are randomly chosen with replacement. For each simulation, we build 250 samples of 50 firms. The estimation window length is 200 days and the event date is situated at day 250. To be included in a sample, the firm must have at least 50 available prices during the estimation window and no missing prices in the 30 days around the event date (from day 230 to day 260).

3.3. Event generation

[14] Center for Research in Security Prices (Chicago University).

The aim of our simulation work is to study the specification and power of the MSR test, as compared to the BSR and RANK tests, when disturbing the estimation window. The mean simulated abnormal returns injected into the estimation window will be 0%, +/- 1%, +/- 2% and +/- 4%. We generate both positive and negative abnormal returns in order to take into account the unknown nature of the events likely to disturb the estimation window. These disturbances will be stochastic. As in Boehmer et al. (1991), the variance of the Gaussian distribution will have the form $k\ \sigma_j^2$ where σ_j^2 is the estimated variance form firm j during the undisturbed estimation window. k will take value 1 and 2 (doubling and tripling the variance).
The number and nature of events during the estimation window is determined in a two-step process. First, a random drawing in a Poisson distribution with mean 2 was made. It will represent the number of events during the estimation window. Then, the number of days during which these events will affect the firm is also randomly determined by a drawing in Poisson distribution (but with mean 4 this time). Such an approach allows generation of random events during the estimation window. Figure 1 presents a typical result obtained using this procedure. The plain line is the initial returns times series and the dotted line is the returns time series obtained after event-generation. Four events (average of 1% and k=2) are generated at dates T=37, 50, 68 and 124. The initial estimation variance is 1.37E-04 and, after event generation, becomes 1.73E-04.

We generate abnormal returns at the event day as in Brown and Warner (1980, 1985). A constant is added to the observed day 0 return for each security. The abnormal performance simulated are 0%, + 0,5%, + 1% et +2%. To produce stochastic abnormal return (the event-induced variance), each security's day 0 return, $R_{i,0}$, is transformed to double (k'=1) or triple (k'=2) its variance. For k'=1, the following transformation is realized:

$$R'_{i,0} = R_{i,0} + (R_{i,X} - \overline{R_i}) \qquad (12)$$

where $R'_{i,0}$ is the transformed return, $R_{i,X}$ a security return randomly extract from the estimation period of security i and $\overline{R_i}$ the security average return in the estimation period. For k'=2, we randomly add two security returns. This procedure is equivalent to simulating a situation where the abnormal performance differs across sample firms but is, on average, zero; based on one cross-section of day 0 returns, such a situation cannot be distinguished from a variance increase.

3.4. Results

Our simulations are too numerous to be analyzed on a case-by-case basis. The details are given for the specification analysis in appendix 2 and for the power analysis in appendix 3. These appendices are organized as follow:

- Appendix 2 – panel A: specification analysis without simulated event during the estimation period.
- Appendix 2 – panel B: specification analysis with simulated event during the estimation period (with average simulated abnormal returns of 0%, 1%, -1%, 2%, -2%, 4% and -4%).
- Appendix 3 – panel A: power analysis without simulated event during the estimation period.
- Appendix 3 – panel B: power analysis with simulated event during the estimation period (with average simulated abnormal returns of 0%, 1%, -1%, 2%, -2%, 4% and -4%).

To interpret all these results, we propose the following regression analysis: the dependent variables will be either the error of specification percentage (absolute value of the difference between the chosen confidence level (5%) and the obtained proportion of samples with significant abnormal returns) or the power (percentage of samples where the generated abnormal returns are correctly detected). As our dependent variables are percentages, we have to taken into account the inherent heteroscedastic nature of our regression models in order to build correct inferences. We follow the procedure presented in appendix 4, leading us to use as dependent variable either the logistic transform of the specification error or the logistic transform of the power. The independent variables represent the conditions of the simulation. We use the following ones:

- DRANK : dummy variable equal to one for RANK test ;
- DMSR : dummy variable equal to one for MSR test ;

- VAREvent : multiplicative coefficient applied to variance of abnormal returns generated at the event date (k' coefficient introduced at section 3.3) ;
- AREst : average abnormal return generated during the estimation window ;
- VAREst : multiplicative coefficient applied to variance of abnormal returns generated during the estimation window (k coefficient introduced at section 3.2) ;
- SAREst : dummy variable equal to one if AREst is negative ;
- RT : dummy variable equal to one if specification is evaluated in the right tail.

Note that our regressions are built in such a way that we compare the RANK and MSR test to the BSR. Coefficients of DRANK and DMSR variables must therefore to be interpreted as deviation from the BSR results.

Specification analysis

Results for specification analysis are summarized in table 1. The main results are the following ones:

- regression 1 compares RANK and MSR tests to BSR on the whole set of simulations. The significant positive sign of DRANK (0.43) shows that the RANK test is worse specified than the BSR and the MSR ones.
- regression 2 focuses on simulations where the variance of the generated abnormal returns at the event date is increased. We reach the same result (coefficient of DRANK * VAREvent is positive (0.72) and significant at 1%).
- regression 3 focuses on simulations where the estimation period is disturbed, the main point of our analysis. The specification error of the BSR test clearly increases (coefficient of AREst positive (24.53) and significant at 1%). We see not specific impact of the RANK test (DRANK * AREST coefficient not significantly different from zero). The MSR test is clearly better specified than the other two (negative sign of DMSR * AREST (-16.25), significant at 1% level).
- regression 4 focuses on simulations where the variance of AREst is increased. The RANK is clearly worse specified than the BSR one (DRANK * VAREst coefficient positive (0.25) and significant at 1%). The MSR test appears to be slightly better specified than the BSR one (negative coefficient of DBSR * VAREst significant at only 10% level).

- regression 5 presents the results for the sign of generated average abnormal returns during the estimation period. No significant differences appear.
- regression 6 test whether the specification results are asymmetric. No clear results emerges.

The main conclusion is that the MSR test clearly dominates the BSR and RANK test as regards the specification when the estimation period is disturbed. As in Cowan and Sergeant (1996), we find that the RANK test is globally misspecified.

Power

Results for power analysis are presented in three tables. Table 2 summarizes results for low event date generated abnormal returns (0.5%), table 3 for medium ones (1%) and table 4 for large ones (2%).

The main results in table 2 (0.5% generated abnormal returns) are:

- regression 1 compares RANK and MSR tests to BSR on the set of simulations with no event induced variance. The significant positive sign of DMSR (0.44) above the significant positive sign DRANK (0.27) shows that the MSR test is more powerful than the RANK and the BSR one in this case.
- regression 2 compares RANK and MSR tests to BSR on the whole set of simulations. The significant positive sign of DRANK (0.44) and DMSR (0.32) significant at the 1% level show that the RANK test and the MSR test are more powerful than the BSR one in general case.
- regression 3 focuses on simulations where the variance of the generated abnormal returns at the event date is increased. The rank test and the MSR test are more powerful than BSR test when the event induced variance increased (coefficient of DRANK * VAREvent is positive (0.35) and significant at 1% and coefficient of DMSR * VAREvent is positive (0.16) and significant at 1%).
- regression 4 focuses on simulations where the estimation period is disturbed, the main point of our analysis. Under disturbed estimation window, detection of the low abnormal return decreases for the BSR test (AREst coefficient negative (-11.22) and significant at the 1% level). The rejection rate of the null hypothesis is

more frequent with the MSR test and the RANK test than BSR test (DRANK * AREST coefficient is positive (15.21) and significant at the 1% level and DMSR * AREST is positive (10.45) and significant at the 5% level).

- regression 5 focuses on simulations where the variance of AREst is increased. The BSR is clearly less powerful than the MSR one (VAREst coefficient negative(-0.17) , DMSR * VAREst coefficient positive (0.20) and significant at 1%). The RANK test appears to be slightly more powerful than the MSR one (positive coefficient of DRANK * VAREst significant at the 1% level and superior to the MSR one).
- regression 6 presents the results for the sign of generated average abnormal returns during the estimation period. A general asymmetry seems to appear (SAREst coefficient positive and significant, DMSR*SAREst and DRANK*SAREst non significantly different from zero).

The main conclusion is that the MSR test clearly dominates the BSR and RANK test when there is no event induced variance. The MSR test also clearly dominates the BSR test in all the other situations (event induced variance, estimation period disturbed…). The RANK test seems more powerful than MSR one, but is misspecified as demonstrated in the previous paragraph.

Analyses of tables 3 (1% generated abnormal returns) and 4 (2% generated abnormal returns) reveal:

- for medium generated abnormal returns, the MSR test continues to dominate the BSR one, not always as significantly as in table 2. The RANK test appears to be the most powerful (but is misspecified).
- for large generated abnormal returns, MSR and BSR tests reach the same level of power except for the case of regression 1 and 3 where the MSR test significantly dominates the BSR). Conclusions concerning the RANK do not change.

The results in section 3 allow us to conclude that the RANK test is generally not well-specified, that the BSR test becomes not well-specified under disturbed estimation window and that the MSR rest remains well-specified even in this case. As regard power, the MSR test clearly dominates the BSR for low generated abnormal returns. The differences progressively disappear when the generated abnormal returns' size increases.

4. THE CASE OF MERGERS AND ACQUISITIONS

4.1. Data

Our data provide from Statistics about actions by the DGC (Directorate General for Competition of the European Commission). Table 5 provides summary information on proposed combinations notified to the DGC since the inception of the EC *n°4064/89* regulation[15] up to December 2000. The entries after the last column show the number of outcomes by type of decision. As of December, 2000, 78 proposed mergers and acquisitions were taken through Phase II by the Commission (detailed investigation). Among them, 15 were approved without conditions, 47 were approved subject to various conditions, and 13 were declared incompatible with EU conditions and were therefore forbidden. Another three cases were resolved by a different type of decision (partial referral to an individual EC member state or restoration of effective competition).

Market data were obtained from Datastream accessed at the *Université de Lille* II. For announcement dates, four separate sources were checked: Reuters, Bloomberg (through Dexia bank), the SDC Database edited by Thomson Financial and, depending on the country, the financial press (*Les Echos, Financial times, Wall Street Journal*, etc.). The SDC Database and the financial press were also used to collect supplementary information such the size of the deal, the means of payment, the type of combination, the presence of rumors in the months preceding the combination, etc.

Because the firms involved were traded on various national exchanges, it was necessary to collect local market information about each exchange and to select a market index (used in the usual way to construct abnormal returns.) The countries involved, the stock market indexes selected, and the local currencies are listed in Table 6. We also collected

[15] EC n°4064/89 set up the notification obligation for European dimension operations. A detailed presentation of the notification criteria can be found in Aktas et al. (2002).

currency exchange rates, short-term interest rates (we use the UK Cash Deposit USD 1 month for some robustness checks) and MSCI World Price Index data from Datastream.

It usually takes quite a while after the intervention for the EC to file an official report on its web site. Consequently, we were obliged to restrict our analysis to notifications from 1990 through 2000 inclusive; later cases were mostly incomplete. The total number of notified combinations during this period was 1573 (see Table 2).

Of these 1573 notifications, 1560 final decisions, comprised of 1505 major decisions and 55 “other” decisions[16], were reached by the end of 2000[17]. We study only the major decisions. Many proposed business combinations involved small or closely-held firms with no readily available market price information, so they could not be included in this study. Thus, to be included in our sample, at least one of the subject firms must be quoted on a national stock exchange; 874 of the 1505[18] major decisions, involving 1535 different firms, satisfy this requirement. Among these 1535 different firms 582 are bidders and 486 targets, the remaining being firms involved in Joint Venture operations.

4.2. Results

Table 7 displays the results for the bidders. Among the 582 sample bidders, 514 converge with the MSR method (circa 88% of convergence). The significance of the SCAR (Standardized Cumulative Abnormal Return) over the period that goes from day -3 to day +2 relative to the announcement day increases with the MSR approach. The resultant t-stat is 1.95 (p-value: 5.1%) for the MSR approach while being 1.66 (p-value: 9.5%) with the BSR test. This corresponds to a 17% increase in the significance of the statistic. This result confirms to a large extent our initial intuition in the sense that firm specific events during the estimation period biased the significance of the abnormal return of the bidders when we use standard tests (RANK or BSR). The bidder result obtained with the MSR method contrasts with the prior findings in the literature (cf. the literature review of

[16] As described in the footnote of Table 1.
[17] Thirteen were carried over for resolution into calendar year 2000.
[18] 1990-2000 inclusive.

Jensen and Ruback (1983)). Prior studies have documented that the abnormal gain to bidder companies around the announcement day of the m&a operation is not or low significant. Taking into account the probability of disturbed estimation window with the MSR test, the observed abnormal returns become largely more significant. This leads us to think that previous published results understate the real value impact of M&A for bidders.

Are our results trivial? In other words, does the MSR test automatically generate a more significant cross-sectional student test? Table 8 displays the results for the targets. Among the 486 sample targets, 436 converge with the MSR method (circa 89% of convergence). The significance of the two tests (BSR and MSR) is comparable; the t-stats are 10.70 and 10.89 for the BSR and MSR respectively. These results are consistent with appendix 1. The increase in the statistical significance in not an automatic by-product of the MSR test.

5. CONCLUSION

The recent contribution of Fuller et al. (2002) stresses one important dimension of the bidder's behavior in the field of mergers and acquisition: they are often repetitive acquirers. This has already been pointed out by Malatesta and Thompson (1985), who propose a methodology adapted to evaluate the wealth effects of acquisitions programs. Such repetitive acquisitions (attemps) potentially create significant disturbances during the estimation window used in the classical event study methodology. Could it be that such behavior would have some influence on inferences drawn concerning the wealth effect of M&A for bidders? In other words, do potential disturbances during the estimation window affect statistical inferences drawn in the classical event study framework?

We first study the specification and power of three alternative cross-sectional tests of abnormal returns build upon the seminal contribution of Fama et al. (1969). The first one is the Boehmer et al. (1991) proposition which is known to tackle the event-induced variance phenomenon. The second one is the Corrado rank test (1989), which is robust for departures from the Gaussian abnormal returns hypothesis. We introduce a third one, in the spirit of the Roll (1987) results. The author shows in it presidential address to the American Finance Association that the return generating process could be better described as a mixture of Gaussian distributions, describing a two-state return-generating process. The first would correspond to a no new regime and the second, to an information arrival one. Our test builds on this result to propose a two-regime market model generating process, with state-dependent variance and stochastic regime transition model (described by a Markov chain). The main results of our simulation work are that the Corrado (1989) rank is generally miss-specified (this has already been pointed out in previous contributions) and that the Boehmer et al. (1991) becomes misspecified as disturbance in the estimation window appear. Our test reveals itself to be robust for this problem and, moreover, more powerful than the Boehmer and al. (1991) one (at least for small generated abnormal returns).

We finally applied the proposed methodology to a sample of real M&A operations. Our sample includes 582 bidders and 486 targets involved in operations notified to the European Commission during the 1990-2000 period. One major result emerges from this analysis. Bidders' abnormal returns, while only marginally significant using the classical Boehmer et al. (1991) methodology, become significant at the 5% level when taking into account the probability of a disturbed estimation window.

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Figure 1

Event generation during the estimation window

Figure 1 presents a typical result obtained using the procedure described in section 3.3 (estimation window simulated events). The plain line is the initial returns times series and the dotted one is the returns time series obtained after event generations. Four events (average of 1% and k=2) are generated at dates T=37, 50, 68 and 124. The initial estimation variance is 1.37E-04 and becomes, after event generation, 1.73E-04.

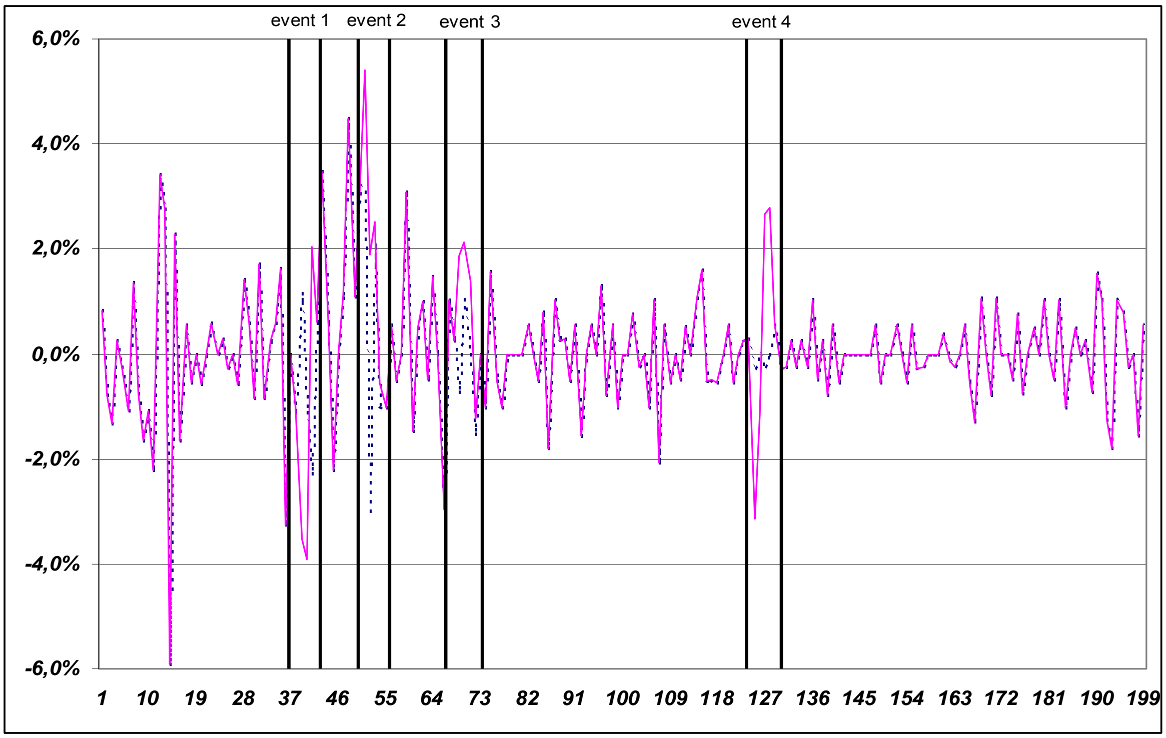

Table 1

The estimated model is a weighted olsq. The dependent variable is the logit of the error of spécification (5%-reject proportion). We use the following independent variables: DRANK : dummy variable equal to one for RANK test ; DMSR : dummy variable equal to one for MSR test ; VAREvent : multiplicative coefficient applied to variance of abnormal returns generated at the event date (k' coefficient introduced at section 3.3) ; AREst : average abnormal return generated during the estimation window ; VAREst : multiplicative coefficient applied to variance of abnormal returns generated during the estimation window (k coefficient introduced at section 3.3) ; SAREst : dummy variable equal to one if AREst is negative ; RT : dummy variable equal to one if specification is evaluated in the right tail.

Specification Error | 5%- H0's Reject |

	weighted OLSQ : dependant variable Logit (ESPEC)											
	1		2		3		4		5		6	
	Coef,	t-Stat,	Coef,	t-Stat,	Coef,	t-Stat,	Coef,	t-Stat,	Coef,	t-Stat,	Coef,	t-Stat,
C	-4,25 ***	-37,07	-4,10 ***	-44,67	-4,57 ***	-40,77	-4,07 ***	-26,11	-4,21 ***	-50,64	-4,04 ***	-48,27
DRANK	0,43 ***	2,94										
DMSR	-0,28	-1,58										
VAREvent			-0,10	-0,95								
DRANK*VAREvent			0,72 ***	6,97								
DMSR*VAREvent			-0,05	-0,35								
AREst					24,53 ***	4,90						
DRANK*AREst					2,75	0,56						
DMSR*AREst					-16,25 ***	-2,71						
VAREst							-0,10	-0,85				
DRANK*VAREst							0,25 ***	2,59				
DMSR*VAREst							-0,21 *	-1,82				
SAREst (1 if AREst is negative)									-0,07	-0,34		
DRANK*SAREst									0,37	1,55		
DMSR*SAREst									-0,10	-0,39		
RT (1 if right tail)											-0,37 *	-1,93
DRANK*RT											0,30	1,32
DMSR*RT											-0,12	-0,49
Number of Observations	270		270		270		270		270		270	

* significant at 10% level
** significant at 5% level
*** significant at 1% level

Table 2

The estimated model is a weighted olsq. The dependent variable is the logistic transform of the power rate (0.5% generated abnormal returns).We use the following independent variables: DRANK : dummy variable equal to one for RANK test ; DMSR : dummy variable equal to one for MSR test ; VAREvent : multiplicative coefficient applied to variance of abnormal returns generated at the event date (k' coefficient introduced at section 3.3) ; AREst : average abnormal return generated during the estimation window ; VAREst : multiplicative coefficient applied to variance of abnormal returns generated during the estimation window (k coefficient introduced at section 3.3) ; SAREst : dummy variable equal to one if AREst is negative.

Study of average rejection rates for abnormal return of +0,5%

	weighted OLSQ : dependant variable Logit (Rejection rates)																	
	1			2			3			4			5			6		
	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,
C	-1,08	***	-15,25	-1,62	***	-21,80	-0,92	***	-23,45	-1,32	***	-20,05	-1,36	***	-13,50	-0,96	***	-17,39
DRANK	0,27	***	2,80	0,44	***	4,40												
DMSR	0,44	***	4,62	0,32	***	3,15												
VAREvent							-0,63	***	-12,30									
DRANK*VAREvent							0,35	***	6,04									
DMSR*VAREvent							0,16	***	2,69									
AREst										-11,22	***	-2,78						
DRANK*AREst										15,21	***	3,48						
DMSR*AREst										10,45	**	2,34						
VAREst													-0,17	**	-2,10			
DRANK*VAREst													0,27	***	4,10			
DMSR*VAREst													0,20	***	2,92			
SAREst (1 if AREst is negative)																0,24	*	1,93
DRANK*SAREst																0,10		0,62
DMSR*SAREst																0,13		0,82
Number of Observations	45			135			135			135			135			135		

* significant at 10% level
** significant at 5% level
*** significant at 1% level

Table 3

The estimated model is a weighted olsq. The dependent variable is the logistic transform of the power rate (1% generated abnormal returns).. We use the following independent variables: DRANK : dummy variable equal to one for RANK test ; DMSR : dummy variable equal to one for MSR test ; VAREvent : multiplicative coefficient applied to variance of abnormal returns generated at the event date (k' coefficient introduced at section 3.3) ; AREst : average abnormal return generated during the estimation window ; VAREst : multiplicative coefficient applied to variance of abnormal returns generated during the estimation window (k coefficient introduced at section 3.2) ; SAREst : dummy variable equal to one if AREst is negative.

Study of average rejection rates for abnormal return of +1%

	weighted OLSQ : dependant variable Logit (Rejection rates)																	
	1			2			3			4			5			6		
	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,
C	0,96	***	12,15	0,00		0,02	1,15	***	21,02	0,38	***	3,21	0,34	*	1,90	1,20	***	16,10
DRANK	0,75	***	5,98	0,57	***	3,27												
DMSR	0,40	***	3,39	0,27		1,56												
VAREvent							-1,04	***	-18,80									
DRANK*VAREvent							0,31	***	5,12									
DMSR*VAREvent							0,13	**	2,13									
AREst										-14,27	**	-2,18						
DRANK*AREst										18,90	**	2,52						
DMSR*AREst										8,63		1,17						
VAREst													-0,21		-1,54			
DRANK*VAREst													0,34	***	2,94			
DMSR*VAREst													0,16		1,36			
SAREst (1 if AREst is negative)																0,10		0,59
DRANK*SAREst																0,64	**	2,47
DMSR*SAREst																0,16		0,68
Number of Observations	45			135			135			135			135			135		

* significant at 10% level
** significant at 5% level
*** significant at 1% level

Table 4

The estimated model is a weighted olsq. The dependent variable is the logistic transform of the power rate (2% generated abnormal returns).We use the following independent variables: DRANK : dummy variable equal to one for RANK test ; DMSR : dummy variable equal to one for MSR test ; VAREvent : multiplicative coefficient applied to variance of abnormal returns generated at the event date (k' coefficient introduced at section 3.3) ; AREst : average abnormal return generated during the estimation window ; VAREst : multiplicative coefficient applied to variance of abnormal returns generated during the estimation window (k coefficient introduced at section 3.2) ; SAREst : dummy variable equal to one if AREst is negative.

Study of average rejection rates for abnormal return of +2%

	weighted OLSQ : dependant variable Logit (Rejection rates)																	
	1			2			3			4			5			6		
	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,	Coef,		t-Stat,
C	5,24	***	16,54	3,17	***	12,51	4,99	***	24,40	3,97	***	11,43	3,96	***	7,25	6,52	***	17,08
DRANK	3,48	*	1,91	1,71	***	2,71												
DMSR	1,14	*	1,77	0,48		1,17												
VAREvent							-1,74	***	-15,86									
DRANK*VAREvent							0,33	***	6,20									
DMSR*VAREvent							0,09	*	1,86									
AREst										-31,57	**	-2,41						
DRANK*AREst										64,40	**	2,49						
DMSR*AREst										17,36		1,20						
VAREst													-0,46		-1,34			
DRANK*VAREst													1,02	**	2,49			
DMSR*VAREst													0,19		0,77			
SAREst (1 if AREst is negative)																-1,37	***	17,08
DRANK*SAREst																4,06		1,30
DMSR*SAREst																1,98	*	1,69
Number of Observations	45			135			135			135			135			135		

* significant at 10% level
** significant at 5% level
*** significant at 1% level

Table 5

Table 2 presents summary statistics on combinations that have been notified to the EC since the inception of the regulation in 1990 through the latest month in our data sample (December 2000).
The entries after the last column show the number of outcomes by type of decision. Phase I cases are those for which a decision is taken at the end of a first one-month investigation period. Phase II cases are subjected to an in-depth investigation.

	90	91	92	93	94	95	96	97	98	99	00	Total
Number of cases notifying the EC	**12**	**63**	**60**	**58**	**95**	**110**	**131**	**172**	**235**	**292**	**345**	**1573**
Cases withdrawn – Phase I			**3**	**1**	**6**	**4**	**5**	**9**	**5**	**7**	**8**	**48**
Termination after Phase I	**7**	**55**	**57**	**54**	**86**	**102**	**118**	**131**	**229**	**260**	**328**	**1427**
Outside EC jurisdiction	2	5	9	4	5	9	6	4	6	1	1	52
Approved without conditions	5	47	43	49	78	90	109	118	207	236	293	1275
Approved subject to conditions		3	4		2	3		2	12	19	28	73
Other decisions after Phase I[19]			1	1	1		3	7	4	4	6	27
Phase II proceedings initiated		**6**	**4**	**4**	**6**	**7**	**6**	**11**	**12**	**20**	**19**	**95**
Cases withdrawn – Phase II				**1**			**1**		**4**	**5**	**6**	**17**
Decision after Phase II		**5**	**4**	**3**	**5**	**7**	**7**	**11**	**9**	**10**	**17**	**78**
Approved		1	1	1	2	2	1	1	3	0	3	15
Approved subject to conditions		3	3	2	2	3	3	7	4	8	12	47
Prohibited		1			1	2	3	1	2	1	2	13
Other decisions of Phase II[20]								2		1	0	3
Other decisions[21]	**1**	**2**	**2**	**4**	**1**	**3**	**4**	**6**	**14**	**13**	**5**	**55**

Source: DGC, "Merger Task Force"

[19] Partial or full referral to an individual EC member state.
[20] Partial referral to an individual EC member state or restoration of effective competition.
[21] Previous decision revoked, imposition of fines, or relief from prior suspension.

Table 6

Table 2 presents a breakdown by year of notification, final decision type and home countries for the 889 combinations and 1535 firms in our initial sample. Panel C also reports the local market index and currency of each country.

Panel A. Year of notification

Year	90	91	92	93	94	95	96	97	98	99	00	Total
N	12	44	37	40	53	66	59	86	105	150	222	874

Panel B. Final decision

Final decision	Prohibition	Conditions	Approval	Referral	Total
N	9	102	759	4	874

Panel C. Home Country, local market index and currency

Country	N	Index	Currency22
Australia	5	S&P ASX 200	Dollar
Austria	8	Weiner Boerse Index	Schilling*
Belgium	24	Brussels all Shares	Franc*
Bermuda	2	MSCI World Price Index	Dollar
Canada	21	Toronto 300	Dollar
Denmark	11	Copenhagen SE	Danish Kröne
Finland	24	HEX	Finish Markka*
France	221	CAC40	Franc*
Germany	267	DAX Kurs Price Index	Mark*
GrECe	2	DJ Euro Stoxx Price Index	Euro
Hong Kong	1	Hang Seng	Dollar
Ireland	2	Ireland SE	Punt*
Italy	74	Milan Comit	Lira*
Japan	35	NIKKEI 225	Yen
Luxembourg	1	Luxembourg SE 13	Franc*
Netherlands	88	CBS All Share	Guilder*
Norway	11	Oslo SE General	Norwegian Kröne
Portugal	3	DJ Euro Stoxx Price Index	Euro
Singapour	1	Singapore DBS 50 Price Index	Dollar
Sout Africa	7	JSE Industrial	Rand
Spain	26	Madrid SE General	Peseta*
Sweden	61	Affarsvarlden weighted all shares	Swedish Kröne
Switzerland	57	Swiss Market Index	Franc
UK	250	FTSE 100	Pound
USA	334	S&P 500	Dollar
Total	1535		

[22] Since January 1, 1999, euroland countries indicated by an asterisk have maintained fixed exchange rates with the euro (and hence with each other).

Table 7

Table 7 presents the Standardized Abnormal Return (SAR) and Standardized Cumulative Abnormal Return (SCAR) for Bidders (Among the 582 sample bidders, 514 converge with the MSR method (circa 88% of convergence)) around the initial announcement date. RANK is the Corrado's test(1989), BSR is the Boehmer and al test (1991) and MSR is the Markov Switching Regression test (see section 2.2).

BSR	***-3***	***-2***	***-1***	***0***	***1***	***2***
SAR	-0,0038	-0,0468	0,2387	0,2084	0,0478	0,5042
SE	0,0534	0,0580	0,0894	0,1051	0,0558	0,5436
T-Stat	-0,0713	-0,8070	2,6705	1,9834	0,8569	0,9276

SCAR (-3;+2)	0,9485
Var(SCAR)	0,5690
T-Stat	1,6669

MSR	***-3***	***-2***	***-1***	***0***	***1***	***2***
SAR	0,0578	-0,0265	0,4030	0,1907	0,1067	0,5301
SE	0,0811	0,0846	0,1487	0,1694	0,0872	0,5886
T-Stat	0,7120	-0,3133	2,7097	1,1257	1,2238	0,9005

SCAR (-3;+2)	1,2619
Var(SCAR)	0,6471
T-Stat	1,9502

Table 8

Table 8 presents the Standardized Abnormal Return (SAR) and Standardized Cumulative Abnormal Return (SCAR) for Targets (Among the 486 sample targets, 436 converge with the MSR method (circa 89% of convergence)) around the initial announcement date. RANK is the Corrado's test-(1989), BSR is the Boehmer and al test (1991) and MSR is the Markov Switching Regression test (see section 2.2).

BSR	-3	-2	-1	0	1	2
SAR	0,2017	0,3537	1,8501	1,3269	0,1028	0,0894
SE	0,0800	0,0776	0,2443	0,2261	0,0794	0,0705
T-Stat	2,5226	4,5585	7,5718	5,8685	1,2940	1,2690

SCAR (-3;+2)	3,9247
Var(SCAR)	0,3668
T-Stat	10,7007

MSR	-3	-2	-1	0	1	2
SAR	0,3803	0,4804	2,6354	1,9153	0,2705	0,1504
SE	0,1235	0,1191	0,3750	0,2991	0,1234	0,1126
T-Stat	3,0788	4,0337	7,0279	6,4046	2,1929	1,3361

SCAR (-3;+2)	5,8324
Var(SCAR)	0,5361
T-Stat	10,8796

Appendix 1 – Standard deviation estimation and cross-sectional student test

The proposed MSR test relies on a two-state decomposition of the return generating process with state dependent standard error estimation. As we use the estimated standard deviation of the low regime variance to standardize the abnormal return, we might think that we automatically increase the cross-sectional student t-stat. However, this is no true. We propose a proof of this in the sequel. More precisely, we study here, in the homoscedastic case, the consequences of using a systematically biased standard deviation on the behavior of the cross-sectional student test. We show that this is without any influence on the cross-sectional student t-stat, the increase in the cross-sectional standard deviation being strictly compensated by the increase in the cross-sectional average abnormal returns.

Assume that AR_i corresponds to the abnormal return of firm i on the event day.

- We assume that the variance (σ^2) of the AR is the same for each sample firm (homoscedasticity).

  The cross-sectional test of Boehmer et al. (1991) is implemented in the following way :

  First we standardize the abnormal return by dividing it with its standard deviation: $t_i = \frac{AR_i}{\sqrt{\sigma^2}}$

  The cross-sectional average of the standardized abnormal return is given the following expression:

  $$\bar{t_i} = \frac{\sum_{i=1}^{n} t_i}{n}.$$

  The variance of the average standardized abnormal return corresponds to:

  $$\sigma^2_{\bar{t_i}} = \frac{\frac{\sum_{i=1}^{n}(t_i - \bar{t_i})^2}{n-1}}{n} = \frac{\sigma^2_{t_i}}{n}$$

  The t-stat following the Boehmer et al. (1991) approach is given by:

  $$T - stat = \frac{\bar{t_i}}{\sqrt{\sigma^2_{\bar{t_i}}}}$$

  $$\Leftrightarrow T - stat = \frac{\frac{\sum_{i=1}^{n}\frac{AR_i}{\sqrt{\sigma^2}}}{n}}{\sqrt{\frac{\frac{\sum_{i=1}^{n}\left(\frac{AR_i}{\sqrt{\sigma^2}} - \frac{\sum_{i=1}^{n}\frac{AR_i}{\sqrt{\sigma^2}}}{n}\right)^2}{n-1}}{n}}}$$

$$\Leftrightarrow T - stat = \frac{\frac{1}{\sigma} \cdot \overline{AR}}{\frac{1}{\sqrt{n \cdot (n-1)}} \cdot \frac{1}{\sigma} \cdot \sqrt{\sum_{i=1}^{n} (AR_i - \overline{AR})^2}} = \frac{\overline{AR} \cdot \sqrt{n \cdot (n-1)}}{\sqrt{\sum_{i=1}^{n} (AR_i - \overline{AR})^2}}$$

where $\overline{AR} = \frac{\sum_{i=1}^{n} AR_i}{n}$, and therefore $\bar{t}_i = \frac{\overline{AR}}{\sigma}$.

- Suppose that the estimation of the abnormal return variance is biased downward with a constant c and corresponds to $\sigma^{2*} = \sigma^2 - c$. The previous expressions can be written in the following way:

$$t_i = \frac{AR_i}{\sqrt{(\sigma^2 - c)}}$$

$$\Leftrightarrow T - stat = \frac{\frac{\sum_{i=1}^{n} \frac{AR_i}{\sqrt{(\sigma^2 - c)}}}{n}}{\sqrt{\frac{\sum_{i=1}^{n} \left(\frac{AR_i}{\sqrt{(\sigma^2 - c)}} - \frac{\sum_{i=1}^{n} \frac{AR_i}{\sqrt{(\sigma^2 - c)}}}{n}\right)^2}{\frac{n-1}{n}}}}$$

$$\Leftrightarrow T - stat = \frac{\frac{1}{(\sigma - c)} \cdot \overline{AR}}{\frac{1}{\sqrt{n \cdot (n-1)}} \cdot \frac{1}{(\sigma - c)} \cdot \sqrt{\sum_{i=1}^{n} (AR_i - \overline{AR})^2}} = \frac{\overline{AR} \cdot \sqrt{n \cdot (n-1)}}{\sqrt{\sum_{i=1}^{n} (AR_i - \overline{AR})^2}}$$

where $\overline{AR} = \frac{\sum_{i=1}^{n} AR_i}{n}$, and therefore $\bar{t}_i = \frac{\overline{AR}}{\sigma - c}$

The result is the same as the previous one. The estimation of the cross-sectional variance compensated the bias applied to the variance of the abnormal returns.

Appendix 2 – Specification analysis

Average rejection rates for various test statistics (RANK (Corrado - 1989), BSR (Boehmer and al - 1991) and MSR (Markov Switching Regression test – see section 2.2)) for 250 portfolios of 50 securities at significance level of 5%. The test is realized under the null hypothesis of no abnormal returns. Securities are randomly chosen in the Russell 3000 Index universe during the period 1990/2000. Panel A presents the case of undisturbed estimation window. Panel B introduces disturbances during the estimation window. *k* represents the multiplicative factor applied to the variance during the estimation window and *k'*, the one applied during the event window. *k* and *k'* are used to generate abnormal returns respectively during the estimation period and the event window. *AGAR* is the level of average generated abnormal returns during the estimation period (0%, +/- 1%, +/- 2%, +/-4%). Specifics of the simulation procedure are described at section 3.

Panel A – Specification analysis without estimation window disturbances

	k'=0		*k'=1*		*k'=2*	
	left tail	right tail	left tail	right tail	left tail	right tail
RANK	2.4%	4.8%	10.0%	8.0%	10.8%	8.0%
BSR	2.8%	6.0%	6.0%	5.6%	3.6%	5.6%
MSR	2.0%	8.8%	4.0%	6.4%	4.4%	7.2%

Panel B – Specification analysis with estimation window disturbances

AGAR=0%

	k'=0		*k'=1*		*k'=2*	
	left tail	right tail	left tail	right tail	left tail	right tail
k=1						
RANK	4.4%	5.6%	7.2%	2.8%	14.0%	7.2%
BSR	6.8%	6.0%	3.2%	2.0%	5.6%	4.4%
MSR	4.4%	8.8%	3.6%	2.8%	5.2%	5.6%
k=2						
RANK	4.0%	2.4%	10.4%	4.8%	13.6%	7.6%
BSR	7.2%	4.0%	4.4%	5.2%	7.2%	5.2%
MSR	4.0%	5.2%	4.4%	4.4%	7.2%	4.4%

AGAR = 1%

	k'=0 left tail	k'=0 right tail	k'=1 left tail	k'=1 right tail	k'=2 left tail	k'=2 right tail
k=1						
RANK	5.2%	5.2%	10.8%	7.2%	12.8%	6.0%
BSR	6.4%	4.0%	6.0%	4.0%	5.2%	5.6%
MSR	4.0%	5.6%	4.0%	6.8%	4.0%	4.4%
k=2						
RANK	4.4%	3.6%	11.2%	6.0%	14.8%	6.8%
BSR	8.4%	2.8%	6.8%	5.2%	5.6%	4.8%
MSR	5.6%	6.4%	6.0%	4.8%	7.2%	5.2%

AGAR = -1%

	k'=0 left tail	k'=0 right tail	k'=1 left tail	k'=1 right tail	k'=2 left tail	k'=2 right tail
k=1						
RANK	4.0%	4.4%	7.2%	6.8%	10.8%	9.6%
BSR	2.4%	4.0%	3.2%	5.6%	6.8%	7.2%
MSR	2.8%	5.2%	2.8%	6.4%	5.2%	8.0%
k=2						
RANK	4.8%	5.6%	8.4%	7.2%	9.6%	9.6%
BSR	6.8%	5.2%	5.2%	4.8%	5.2%	4.0%
MSR	4.8%	6.0%	2.8%	4.4%	4.0%	5.2%

AGAR = 2%

	k'=0 left tail	k'=0 right tail	k'=1 left tail	k'=1 right tail	k'=2 left tail	k'=2 right tail
k=1						
RANK	5.2%	2.8%	10.8%	3.2%	12.0%	7.6%
BSR	8.4%	3.6%	6.8%	5.6%	4.8%	5.6%
MSR	4.4%	5.2%	4.4%	4.4%	4.4%	6.4%
k=2						
RANK	6.0%	3.6%	11.6%	5.6%	16.8%	4.0%
BSR	10.0%	2.0%	8.4%	3.2%	7.2%	2.8%
MSR	5.6%	5.2%	6.8%	4.0%	6.0%	2.4%

AGAR = - 2%

	k'=0 left tail	k'=0 right tail	k'=1 left tail	k'=1 right tail	k'=2 left tail	k'=2 right tail
k=1						
RANK	2.8%	7.2%	9.2%	7.6%	11.2%	8.4%
BSR	6.4%	10.0%	3.6%	8.4%	3.6%	6.4%
MSR	3.2%	12.0%	4.0%	8.0%	5.2%	6.0%
k=2						
RANK	6.0%	5.2%	10.0%	6.8%	9.2%	7.2%
BSR	6.0%	6.8%	6.4%	3.6%	2.4%	6.0%
MSR	7.2%	8.0%	5.6%	3.2%	2.8%	4.8%

AGAR = 4%

	k'=0 left tail	k'=0 right tail	k'=1 left tail	k'=1 right tail	k'=2 left tail	k'=2 right tail
k=1						
RANK	6.0%	2.8%	18.0%	2.0%	19.2%	1.6%
BSR	13.6%	2.0%	12.0%	1.6%	8.0%	1.6%
MSR	4.4%	4.4%	6.8%	2.8%	7.6%	2.0%
k=2						
RANK	8.4%	2.4%	14.8%	2.8%	18.8%	6.4%
BSR	15.6%	2.4%	10.4%	2.0%	10.8%	3.6%
MSR	7.2%	6.0%	7.6%	4.0%	7.6%	4.4%

AGAR = - 4%

	k'=0 left tail	k'=0 right tail	k'=1 left tail	k'=1 right tail	k'=2 left tail	k'=2 right tail
k=1						
RANK	4.8%	7.2%	5.6%	11.2%	8.0%	8.4%
BSR	4.4%	10.4%	4.4%	10.4%	2.8%	5.6%
MSR	6.0%	9.2%	3.2%	8.4%	2.4%	3.6%
k=2						
RANK	1.6%	4.8%	6.4%	11.2%	7.6%	10.4%
BSR	2.4%	8.8%	1.6%	8.8%	2.8%	6.0%
MSR	2.8%	7.2%	4.0%	7.2%	3.6%	3.6%

Appendix 3 – Power analysis

Average rejection rates for various test statistics (RANK (Corrado - 1989), BSR (Boehmer and al - 1991) and MSR (Markov Switching Regression test – see section 2.2)) for 250 portfolios of 50 securities at significance level of 5%. The test is realized under the null hypothesis of no abnormal returns. Securities are randomly chosen in the Russell 3000 Index universe during the period 1990/2000. The abnormal performances generated at the event day have an average of 0.5%, 1% or 2%. Panel A presents the case of undisturbed estimation window. Panel B introduces disturbances during the estimation window. *k* represents the multiplicative factor applied to the variance during the estimation window and *k'*, the one applied during the event window. *k* and *k'* are used to generate abnormal returns respectively during the estimation period and the event window. *AGAR* is the level of average generated abnormal returns during the estimation period (0%, +/- 1%, +/- 2%, +/-4%). Specifics of the simulation procedure are described at section 3.

Panel A – Power analysis without estimation window disturbances

	k'=0			*k'=1*			*k'=2*		
	0,05%	1%	2%	0,05%	1%	2%	0,05%	1%	2%
RANK	34,0%	89,2%	100,0%	21,2%	62,0%	98,0%	20,4%	47,2%	91,6%
BSR	29,6%	73,2%	100,0%	15,6%	51,6%	93,6%	13,6%	37,2%	85,2%
MSR	37,6%	83,2%	100,0%	21,6%	57,2%	94,8%	15,6%	38,0%	88,8%

Panel B – Power analysis with estimation window disturbances

AGAR = 0%

	k'=0			*k'=1*			*k'=2*		
	0,05%	1%	2%	0,05%	1%	2%	0,05%	1%	2%
k=1									
RANK	27,2%	82,8%	100,0%	18,0%	52,0%	98,8%	14,4%	42,4%	91,2%
BSR	24,0%	73,2%	98,8%	11,6%	40,0%	96,8%	10,4%	32,0%	82,8%
MSR	33,6%	79,6%	99,2%	13,2%	46,8%	95,2%	13,6%	38,4%	85,2%
k=2									
RANK	32,4%	85,2%	100,0%	22,8%	57,2%	95,2%	18,8%	46,8%	88,4%
BSR	26,4%	71,2%	100,0%	14,8%	46,4%	92,8%	11,2%	34,8%	80,0%
MSR	36,4%	76,4%	100,0%	20,0%	51,6%	93,2%	13,6%	40,0%	82,0%

AGAR = 1%

	k'=0			*k'=1*			*k'=2*		
	0,05%	1%	2%	0,05%	1%	2%	0,05%	1%	2%
k=1									
RANK	32,4%	86,0%	99,6%	19,2%	49,6%	96,4%	20,4%	42,0%	90,4%
BSR	25,2%	70,8%	98,8%	14,8%	36,8%	92,8%	11,2%	34,4%	84,0%
MSR	35,2%	82,8%	99,2%	18,8%	45,2%	94,4%	14,8%	37,6%	84,8%
k=2									
RANK	26,0%	87,2%	100,0%	19,6%	57,2%	96,0%	20,4%	45,2%	90,4%
BSR	20,8%	73,2%	97,2%	13,2%	42,4%	93,2%	12,4%	34,0%	81,6%
MSR	34,8%	80,8%	99,2%	20,0%	51,2%	94,0%	14,8%	39,6%	83,6%

AGAR = -1%

	k'=0			*k'=1*			*k'=2*		
	0,05%	1%	2%	0,05%	1%	2%	0,05%	1%	2%
k=1									
RANK	36,0%	88,4%	100,0%	18,0%	62,0%	98,4%	20,4%	45,2%	86,8%
BSR	30,0%	76,0%	98,8%	12,8%	52,4%	96,4%	10,4%	33,6%	83,2%
MSR	38,0%	82,8%	100,0%	17,2%	53,6%	94,8%	13,2%	38,0%	83,6%
k=2									
RANK	31,2%	84,4%	100,0%	24,4%	56,0%	97,2%	20,4%	49,6%	92,4%
BSR	27,6%	76,4%	99,6%	17,6%	41,6%	96,0%	13,2%	37,6%	87,2%
MSR	34,0%	80,8%	99,6%	19,6%	46,8%	93,2%	16,4%	41,2%	86,4%

AGAR = 2%

	k'=0			*k'=1*			*k'=2*		
	0,05%	1%	2%	0,05%	1%	2%	0,05%	1%	2%
k=1									
RANK	23,6%	80,8%	100,0%	14,4%	54,0%	96,4%	21,6%	42,8%	89,2%
BSR	16,8%	65,2%	98,8%	11,6%	41,2%	91,2%	12,8%	36,4%	82,4%
MSR	29,6%	78,4%	99,2%	14,4%	53,6%	94,8%	16,0%	41,2%	84,8%
k=2									
RANK	28,0%	81,6%	100,0%	17,6%	49,6%	96,8%	20,0%	41,6%	89,2%
BSR	21,6%	67,6%	99,2%	12,0%	41,6%	92,4%	12,0%	30,4%	80,8%
MSR	34,8%	78,4%	99,6%	19,2%	47,6%	95,6%	16,8%	36,4%	84,4%

AGAR = - 2%

	k'=0			*k'=1*			*k'=2*		
	0,05%	1%	2%	0,05%	1%	2%	0,05%	1%	2%
k=1									
RANK	36,8%	86,8%	100,0%	25,6%	54,0%	98,4%	21,2%	47,2%	95,2%
BSR	36,4%	76,4%	98,8%	17,6%	46,8%	95,6%	14,4%	32,4%	88,0%
MSR	36,8%	79,6%	100,0%	20,0%	48,8%	95,2%	15,6%	36,0%	89,2%
k=2									
RANK	36,0%	87,6%	100,0%	26,4%	58,4%	98,4%	26,0%	47,2%	89,6%
BSR	32,8%	79,2%	99,6%	19,2%	45,6%	95,6%	15,2%	34,4%	84,4%
MSR	40,8%	83,2%	99,2%	24,8%	46,4%	95,6%	16,0%	38,8%	84,8%

AGAR = 4%

	k'=0			*k'=1*			*k'=2*		
	0,05%	1%	2%	0,05%	1%	2%	0,05%	1%	2%
k=1									
RANK	25,6%	71,6%	100,0%	15,2%	44,4%	95,2%	15,2%	32,4%	80,4%
BSR	14,8%	49,6%	97,2%	7,6%	34,4%	85,6%	10,8%	23,2%	70,4%
MSR	30,0%	68,8%	98,4%	16,8%	48,0%	92,8%	14,8%	32,8%	80,0%
k=2									
RANK	27,6%	74,0%	99,6%	12,8%	44,8%	95,6%	15,2%	36,8%	82,0%
BSR	15,6%	60,8%	98,4%	10,0%	29,6%	88,8%	8,0%	24,0%	72,0%
MSR	35,2%	75,2%	99,6%	14,4%	41,6%	95,2%	11,6%	33,6%	82,4%

AGAR = - 4%

	k'=0			*k'=1*			*k'=2*		
	0,05%	1%	2%	0,05%	1%	2%	0,05%	1%	2%
k=1									
RANK	32,4%	89,2%	100,0%	28,8%	64,0%	98,8%	25,6%	55,6%	92,8%
BSR	34,4%	79,2%	99,6%	22,4%	54,0%	96,4%	15,2%	40,8%	88,4%
MSR	32,8%	80,0%	100,0%	18,8%	48,4%	94,8%	15,2%	39,6%	88,0%
k=2									
RANK	36,8%	84,4%	100,0%	21,6%	64,4%	99,6%	22,0%	52,0%	88,4%
BSR	35,2%	81,2%	99,6%	14,8%	53,2%	94,8%	12,4%	35,6%	78,8%
MSR	31,2%	77,6%	99,6%	12,8%	53,2%	92,4%	13,6%	32,4%	78,8%

Appendix 4 – Proportion data regression

The dependent variable of section 3 regressions is the proportion (P_i) of the 250 simulations, which rejects the null hypothesis. The regression analysis of P_i, as shown in Greene (2000, p. 835), raises a concern of heteroscedasticity. The observed P_i is an estimate of the population quantity, $\pi_i = F(\beta' X_i)$. If we treat this problem as sampling from Bernoulli population, then we have:

$$P_i = F(\beta' X_i) + \varepsilon_i = \pi_i + \varepsilon_i \qquad \text{(A2.1)}$$

where:

$$E[\varepsilon_i] = 0\, , \; Var[\varepsilon_i] = \frac{\pi_i(1-\pi_i)}{n_i} \qquad \text{(A2.2)}$$

This heteroscedastic regression format suggests that the parameters could be estimated by a nonlinear weighted least squares regression. But the author proposes a simpler way to proceed. Since the function $F(\beta' X_i)$ is strictly monotonic, it has an inverse.

$$F^{-1}(P_i) = F^{-1}(\pi_i + \varepsilon_i) \approx \beta' X_i + \frac{\varepsilon}{\int_i} \qquad \text{(A2.3)}$$

This equation produces an heteroscedastic linear regression:

$$F^{-1}(P_i) = Z_i = \beta' X_i + u_i \qquad \text{(A2.4)}$$

where:

$$E[u_i] = 0\, , \; Var[u_i] = \frac{F_i(1-F_i)}{n_i \int_i^2} \qquad \text{(A2.5)}$$

The inverse function for the logistic model is easy to obtain. If

$$\pi_i = \frac{\exp(\beta' X_i)}{1+\exp(\beta' X_i)} \qquad \text{(A2.6)}$$

then:

$$Ln\left(\frac{\pi_i}{1-\pi_i}\right) = \beta' X_i \qquad \text{(A2.7)}$$

Weighted least squares regression produced the minimum χ^2 estimator of β. Since the weights are function of the unknown parameters, a two-step procedure is called for.

Simple least squares at the first step produces a consistent but inefficient estimates. Then the weights for the logit model based on the first step estimates are then:

$$W_i = n_i \Lambda_i (1 - \Lambda_i) \text{ with } \Lambda_i = \frac{\exp(\beta X_i)}{1 + \exp(\beta Xi)} \quad \text{(A2.8)}$$

and can be used for weighted least squares in the second step procedure.