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Robust Methods in Event Studies: Empirical Evidence and Theoretical Implications

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Abstract: We apply methodology robust to outliers to an existing event study of the effect of U.S. financial reform on the stock markets of the 10 largest world economies, and obtain results that differ from the original OLS results in important ways. This finding underlines the importance of handling outliers in event studies. We further review closely the population of outliers identified using Cook's distance and find that many of the outliers lie within the event windows. We acknowledge that those data points lead to inaccurate regression fitting; however, we cannot remove them since they carry valuable information regarding the event effect. We study further the residuals of the outliers within event windows and find that the residuals change with application of M-estimators and MM-estimators; in most cases they became larger, meaning the main prediction equation is pulled back towards the main data population and further from the outliers and indicating more proper fitting. We support our empirical results by pseudo-simulation experiments and find significant improvement in determination of both types of the event effect − abnormal returns and change in systematic risk. We conclude that robust methods are important for obtaining accurate measurement of event effects in event studies.

Key words: Dodd-Frank Act, event study, financial reform, M-estimator, MM-estimator, outliers, regulation, robust methods, simulation.

1. Introduction

The event study is an important tool in the financial economist's toolkit that can be traced back to the 1930s. Event studies using a market model were popularized by Fama, Fisher, Jensen and Roll (1969) and extended since then. MacKinlay (1997) and Binder (1998), among others, provide a comprehensive guide to the modern event study methodology including technical details, power, and problems with specific techniques. While event study methodology continues to evolve, a commonly used estimation technique in event studies is OLS

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regression. Huber (1973), Yohai (1987) and numerous followers demonstrate that inferences from OLS regressions are sensitive to the presence of outliers and high leverage data points. Brown and Warner (1985) point out that daily stock returns are characterized by non-normality, which implies a significant presence of outliers and high leverage data points. This raises the question to what degree outliers and high leverage data points influence the conclusions drawn by event study researchers. Researchers in the finance and accounting fields often use one of three simple methods for the treatment of outliers: ignore them, trim the sample to remove inconvenient data points by the arbitrary setting of cut-off thresholds for too large or too small observations, or winsorize the largest and/or smallest observations, replacing them with the values of arbitrary selected cut-off points. None of these methods guarantees successful removal of the outliers/leverage points from the dataset, because outliers are identified by the size of the residual from a particular regression model and not by the absolute size of the particular observation of an individual variable. The removal of the problematic points, without proper consideration may improve the accuracy of the inferences, but it may also delete important information from the analysis. We conjecture that without proper treatment, outliers that are located outside event windows may distort the non-event inferences that are used as a benchmark for event effect recognition. When present within event windows, those observations distort the event effect itself. If outliers are ignored, the inferences may be excessively skewed toward outliers leaving a majority of the observations in the sample underrepresented. When they are trimmed away, valuable information is lost, including the effect of interest in our case − the event effect. In our opinion, winsorizing makes the worst choice by adding unambiguously incorrect observations to the dataset.

Robust regression methods for the effective treatment of outliers have been available since at least the early 1970s. However, with only rare exceptions, robust regressions are still uncommon in the finance literature. This study is aimed at demonstrating the particular usefulness and importance of the robust methods when dealing with daily stock returns in event studies. We choose to use the weighted regression (M-estimation) approach of Huber (1973) and the extension of Yohai (1987), MM-estimation. The M-estimation method is robust to outliers, while the MM-estimation method is robust to outliers and high leverage data points. The weighted approaches allow the retention of information contained in outliers by assigning proper weights that result in more accurate inferences as compared to OLS estimates. We compare the results using these robust methodologies to the results using OLS regression, and find inference changes that are statistically and economically significant. The dataset for this analysis is one from a contemporaneous study, Sorokina and Thornton (2011) that examines the effects of the Dodd-Frank Wall Street and Consumer Protection Act of 2010 (Dodd-Frank) on the stock markets of the 10 largest economies in the world as measured by GDP.

The financial crisis of 2007-2009 created serious problems for the global financial system and the economy. Dodd-Frank was passed by the Congress of the United States in July of 2009 in an attempt to correct the flaws in financial institution regulation revealed by the crisis and, thereby, reduce the risk of future crises. Dodd-Frank contains numerous provisions running to more than 2,000 pages in print. Provisions of Dodd-Frank include: establishment of a new consumer protection agency; attempts to control or eliminate "Too Big to Fail" financial institutions; attempts to limit systematic risk; increased regulation of derivatives; increased regulation of mortgage lending; enhancements to corporate governance; changes to the credit ratings system; and, reform of regulatory agencies. The Act mandates changes in regulatory policies, methods and the structure of regulatory organizations. Dodd-Frank only applies directly to financial institutions in the United States. However, we conjecture that due to the broad scope of this legislation, its effects will be felt in the entire economy, not just the financial services industry. It is well known that a smooth-functioning financial system is critical to the growth of a country's economy. This linkage implies that a sweeping change to the financial system, such as Dodd-Frank, will affect all industries to some extent, not just the financial services industry. We also conjecture that the effects will extend beyond the borders of the United States. The United States is the largest economy in the world; it is home to one of the most important financial centers in the world, New York; and, many large, globally important financial institutions are headquartered in the United States. For these reasons, we expect that an important regulatory change in the United States will affect other major economies. If our conjectures are correct, then we expect that the broad stock market in the United States and in other major economies should react to events surrounding the Dodd-Frank legislative process.

The regression equation we use for the study allows us to measure cumulative abnormal returns and changes in systematic risk, as measured by beta, associated with the legislative process for Dodd-Frank (Sorokina and Thornton, 2011). Our results show that the events surrounding the legislative process did affect the broad equity markets. In the United States and Canada we find an increase in systematic risk after the introduction of the Act. This finding is opposite the intended effect of the legislation. We also find negative cumulative abnormal returns in several countries associated with intermediate steps in the legislative process. This finding should be interpreted with caution since the markets in some countries may have been affected by events unrelated to Dodd-Frank that occurred at the same time as some of the legislative steps.

The main focus of our paper is methodological. The results indicate that outliers and high leverage data points do affect inferences from the event study. We review closely the population of outliers identified using Cook's distance, and find that many of these problematic data points occur within the event windows. While it is true that these data points lead to inaccurate regression fitting, they also carry valuable information regarding the event effect. We examine further the residuals of the outliers within event windows and find that the residuals change with application of the M-estimator and the MM-estimator. In most cases they became larger, meaning the main prediction equation is pulled back towards the main data population and further from the outliers, which indicates a better fit of the model to the majority of the data.

In the empirical part of our study we test the reaction of the stock market of the ten world's largest economies to the introduction and enactment of Dodd-Frank as well as key steps in the legislative process. We find that a number of the countries beyond the borders of the United States, including major economies such as the United Kingdom and France, react with negative cumulative abnormal returns. However, using OLS we do not find a similar reaction in two of the largest economies, Japan and Germany. Yet we do find a reaction in smaller economies beset with their own financial troubles, Spain and Italy. These results were puzzling until we implemented robust regressions. Using our robust methodologies we find a significant reaction to the event in Japan and Germany that was not uncovered using OLS. Conversely, the reactions in Spain and Italy lose their significance. In each of these latter markets we find one large outlier that pulls the regression line estimated by OLS away from the majority of the observations.

A finding that robust methodologies improve inferences in one event study dataset is interesting; but, a more important question is whether or not the observed effect stretches beyond our specific case. To shed light on this question, we construct a pseudo-simulation in which the independent variables come from the empirical data. Dependent variable values are generated based on the distribution in our empirical study and contaminated with outliers. Then event effects are artificially induced. We confirm that robust methodology significantly improves recognition of event effects. We find that the MM-estimator performs better in determining abnormal returns; while the M-estimator demonstrates the same or better efficiency in recognizing changes in systematic risk.

This paper contributes to event study methodology by demonstrating that outliers and high leverage data points, particularly those in event windows, can distort inferences. We provide empirical evidence of the efficiency of robust M- and MM-estimation in a specific dataset and use pseudo-simulation to demonstrate that this efficiency gain extends beyond the specific dataset.

The remainder of this paper is structured as follows: We review the development of event study methodology in Section 2. Section 3 lays out data specifics, a description of the methodology, and our hypotheses. In Section 4 we present the results of M- and MM − estimators as compared to OLS regression results and analyze outliers in our data samples, especially in those countries, where results changed significantly from OLS to the robust estimators. The pseudo-simulation methodology and results comprise Section 5. In Section 6 we draw the conclusions.

2. Literature

The methodology of event studies, a recognized procedure in finance, accounting and other disciplines, has constantly evolved since its inception, which is reported to be as early as the 1930s (MacKinlay, 1997). Binder (1998) provides a comprehensive review of event study methodology. He discusses five methods: mean-adjusted, market-adjusted, market model, one-factor normal return estimate (e.g., CAPM), and multifactor normal return estimate, (e.g., APT). MacKinlay (1997) classifies CAPM and APT as economic models. These models are shown to perform approximately as well as a market model by numerous studies, but the market model remains the most commonly used approach.

Binder (1998) is particularly skeptical about event studies of regulation. The fact that events are often anticipated and the legislation period is prolonged makes it difficult to choose the event dates. He cites multiple attempts to simulate the event study effects. These studies find very low power of the tests to reject the null hypothesis when it should be rejected. Binder suggests careful selection of the dates, microeconomic analysis of the regulation impact on the company, and linking it to the firm's abnormal return through cross-sectional regression built upon a specific firm's characteristics. In spite of these concerns, event studies of regulatory actions remain important tools in understanding the effects of regulations on the market. Classic event studies are used to measure the short-term market reaction, which is a reflection of the market expectation. Permanent effects are often studied with long-term event studies that are not a subject of our immediate research.

The most common types of event studies in use currently (market models) can be traced to the seminal paper of Fama, Fisher, Jensen and Roll (1969). They study stock spit announcements using a regression model and measure abnormal returns as residuals. Regression analysis appeared to be more accurate and became a dominant model in event studies. Later improvements include using a regression term to measure abnormal returns and measuring changes in risk using dummy variables (e.g., Gujarati, 1970). This approach, called a modified market model, is the most popular in recent event studies.

Blume (1971) and Gonedes (1973) study the statistical problems of abnormal return estimators. They find that the estimators (1) are prone to cross-sectional correlation in event time; (2) have different variances across firms; (3) are not independent across time for a given firm; and, (4) have greater variance during event time than in surrounding periods. Modern event studies attempt to minimize these problems by aggregating individual stocks into portfolios. Autocorrelation issues are mitigated by using a long overall period of study as compared to the length of the event window(s).

In order to implement a market model event study a researcher must make several decisions. These include the frequency over which returns are measured, the length of the estimation period, and the window used to measure abnormal returns. Early event studies use monthly stock returns, but with the growth of technical opportunities and the knowledge base, since the mid-1980s the use of daily returns has become standard. At the present time, short-term event studies use daily returns almost exclusively. There are even examples of high-frequency event studies (e.g., Mucklow, 1994) where analysis is performed using 15, 30 and 60 minutes returns as well as overnight returns. On the other hand, in long-term studies the use of less frequent, even quarterly, returns is recommended (e.g., Bremer, Buchanan and English II, 2011). There is no uniform agreement on the estimation period. For example, Cox and Peterson (1994) use 100 days, Carow and Kane (2002) use 200 days, and Litvak (2007) uses 500 days. MacKinlay (1997) suggests 250 days. As for the event window, MacKinlay (1997) suggests using $(-1, +1)$. However, other windows are common. For example, Kanas (2005) uses $(-3, +3)$ and Miyajima and Yafeh (2007) use $(-5, +5)$. Longer periods are used for some special cases. For example Cox and Peterson (1994) use $(+4, +20)$.

The landmark simulation-based study of the efficiency of daily stock return implementation in event studies is Brown and Warner (1985). They are concerned with non-normality of the daily returns and non-synchronous trading, but find that these problems are not significant in their tests. They question the accuracy of mean-adjusted and market-adjusted methods under certain conditions. For example if a clear trend is present in the market moves, the results are upward (or downward) biased per Henderson and Glenn (1990). The non-synchronous trading problem and the problem of the market direction bias may be handled using various combinations of lead and lagged estimation (e.g., Scholes and Williams, 1977). Henderson and Glenn (1990) finds that autocorrelation does not create a significant problem. He also suggests that portfolio returns take care of the clustering problems, which are common in event studies of regulation $-$ when the event is the same for a large number of companies. MacKinlay (1997) suggests the use of GMM to control for autocorrelation and heteroscedasticity issues; however, he states that in most cases it is not necessary.

Brown and Warner (1985) point out the non-normal distribution of the daily returns and problems that creates for the event studies. Non-parametric tests such as the generalized sign test and the rank test (Cowan, 1992), without restrictive distribution assumptions, were developed as alternative methods. Non-parametric tests of these types became a useful addition to the parametric tests. Additionally, Cowan (1992) documents the resistance of the sign test to a single large outlier. One prominent example of the application of the outlier-resistant regression methodology is the study of Knez and Ready (1997). They implement a weighted least square regression; and, find that the famous size risk premium, identified by Fama and French (1992), is fully explained by outliers and disappears in their absence. Several other studies, including some very recent ones, demonstrate the importance of the proper consideration of the outliers in financial data samples (e.g., Booth, 1982; Hauser and Booth, 2010; Hauser and Booth, 2011; Kimmel, Booth and Booth, 2010; and, Bhattacharyya, Datta and Booth, 2011). These studies show that robust methods significantly improve results and uncover effects in contaminated samples that are otherwise masked by inappropriate regression fitting. Huber (1973) first introduced M-estimation, a weighted approach, in a regression application. The method is not robust to the high leverage data points but is useful when only outliers are a concern. Yohai (1987) combined M-estimation with S-estimation, which is robust to high breakdown (leverage) points, creating a new method that is robust to both types of contamination. The well-established robust estimation methodology of Huber and Yohai is widely cited in the academic literature and implemented in the major statistical software packages. To our knowledge no studies explore the effect that outliers and high leverage points can have on the inferences of regression-based event study methods. Our study helps to fill this gap in the literature. We show in this paper that robust methods in the presence of outliers have a major effect on the correct interpretation of the event study results.

3. Data, Methodology and Hypotheses

We study the economic impact of Dodd-Frank as measured by the reaction of the broad stock market indices of ten countries with large economies to the major steps in the reform legislative process. We apply the event study methodology to the country-specific stock indices an international multimarket stock index along with other factors that are priced into stocks, as control variables. The approach allows us to identify the reaction of the world's major economies, principal participants of the global financial market, to the changes in U.S. financial regulation. We determined events in the legislative process based on data from the Library of Congress and the Wall Street Journal. A list of the events we identified is presented in Appendix. We study the impact of the legislation on the ten largest world economies as identified by the World Bank GDP data as of the end of 2009. We use major stock indices of the respective countries in our study. The returns on the indices are calculated based on the closing values that were obtained from Yahoo! Finance. We use the MSCI EAFE index as a proxy for the market return. The index closing values for the return calculations were collected from Bloomberg. We obtain the 6 month LIBOR rate from the mortgage information service Mortgage-X and the Major Currency Index from the Federal Reserve web-site and use them as additional control variables on our model.

Summary statistics for our sample are presented in Table 1. The number of calendar days within each sample period is the same for all ten countries; but, the total number of observations varies slightly, depending on the number of non-trading days within the period. We calculate the event period based on the index availability, but then omit without replacement the dates where we find no matching control variable or an index return is missing. If the event falls on a non-trading day in a country, the event window shifts to the available surrounding trading days. The summary statistics of the control variables varies only slightly because we use the same variables in all data samples, the only difference is in the number of trading days in each country.

We develop an extended version of the modified market model similar to Mamun, Hassan and Lai (2004). The model is similar to ones used in other recent event studies of financial regulation. Our model measures the immediate market reaction on and around important milestones in the legislation process as well as changes in market risk at the introduction and enactment of the legislation. Market risk is measured by the slope of the regression line, therefore, a change in market risk is represented by a change in slope, which may be captured by an interaction of the slope and a dummy variable that equals one after the introduction (enactment) of the legislation. Abnormal returns around significant events in the legislative process are captured using dummy variables that equal one in the event windows. Using this model we are able to measure event effects in three timing layers: first (early) stage of the reaction measured as change in beta (market risk) and change in alpha (abnormal returns); second (intermediate) stage of reaction measured as abnormal market returns on the important days in the legislation process; and, third (late) stage of the reaction measured as change in beta and alpha at the enactment of the legislation. Our proxy for the market is a broad international equity market index. We also control for the effects of interest rates and foreign exchange movements. The model is specified as follows:

$$R_i = \alpha_i + \alpha_i' D' + \alpha_{0i} D_0 + \beta_i R_m + \beta_i' D' R_m + \beta_{0i} D_0 R_m + \delta_i R_{rf} + \lambda_i R_{fx} + \gamma_i D + \varepsilon,$$

where

Table 1: Summary statistics

	R_m			R_{rf}			R_{fx}			R_i			Total	Event
	Median	Mean	StdDev	Median	Mean	StdDev	Median	Mean	StdDev	Median	Mean	StdDev	Obs	Obs
U.S.	-0.16	-0.0768	1.4052	0.63	0.8617	0.5265	-0.01	-0.0246	0.6227	0.11	0.0822	1.4867	523	33
Brazil	-0.15	-0.0748	1.3989	0.63	0.8678	0.539	-0.00851	-0.0246	0.6301	0.14	0.1436	1.7365	509	33
France	-0.13	-0.0658	1.4069	0.63	0.8601	0.525	-0.00697	-0.0218	0.6252	-0.01	0.0498	1.6441	516	33
UK	-0.13	-0.0795	1.4075	0.63	0.865	0.5309	-0.00697	-0.0196	0.625	0.08	0.0827	1.3258	514	33
Italy	-0.11	-0.0555	1.4019	0.63	0.8631	0.5325	-0.00601	-0.0215	0.6255	0.085	0.0331	1.824	516	33
Germany	-0.11	-0.0556	1.4006	0.63	0.8665	0.5374	-0.00512	-0.021	0.6249	0.09	0.0801	1.5687	517	33
Canada	-0.11	-0.0797	1.4014	0.63	0.861	0.5285	-0.00691	-0.0199	0.624	0.11	0.1047	1.357	513	33
Japan	-0.1	-0.0578	1.3988	0.63	0.87	0.5449	-0.01	-0.0262	0.6226	0.08	0.0621	1.6138	503	33
Spain	-0.11	-0.0555	1.4019	0.63	0.8631	0.5325	-0.00601	-0.0215	0.6255	0.115	0.0302	1.785	516	33
China	-0.1	-0.0508	1.4111	0.64	0.875	0.5478	-0.00691	-0.0124	0.6152	0.14	0.0792	1.692	499	33

R_i – daily return on the tested index (primary equity index of the country from Yahoo! Finance).

α – index alpha.

α' – difference between index alpha before/after the tested legislation introduced.

α_0 – difference between index alpha before/after the tested legislation enacted.

D' – before/after the tested legislation introduction dummy (0-before, 1-after).

D_0 – before/after the tested legislation enactment dummy (0-before, 1-after).

β – index beta (risk).

R_m – market return (international equity index obtained from Bloomberg).

β' – coefficient of the change in country index beta (risk) after the tested legislation introduced.

β_0 – coefficient of the change in country index beta (risk) after the tested legislation enacted.

δ – risk free rate coefficient.

R_{rf} – risk free rate return (6-month LIBOR from Mortgage-X website).

λ – forex market return coefficient.

R_{fx} – forex market return (MCI index from Federal Reserve).

γ – coefficient of cumulative abnormal returns.

D – dummy variable of the event periods (1 – during the period, 0 – otherwise).

ε – error term.

We estimate the model using data from 120 days before the beginning of the first event window to 120 days after the end of the last event window. The 120 day limit was imposed by the data availability at the time the analysis was performed. MacKinlay (1997) cites 120 trading days as commonly implemented in event studies for the estimation period. We use a $(-1, +1)$ day event window, which is recommended by MacKinlay as most accurate since it allows for spillover effects in surrounding days and does not weaken the power of the test.

We use Cook's distance to identify outliers in the OLS results. Cook's distance was introduced by Cook (1977) for the purpose of identifying outliers; and, it has become the most commonly used estimate of the influence of a data point in a least squares regression. It measures the effect of deleting a given observation and identifies both outliers and high leverage points. We choose a Cook's distance of $4/(n-k-1)$ as the cutoff for identifying an observation as an outlier, where $n =$

number of observations and k = number of independent variables, as suggested by Belsley, Kuh and Welsch (2005).

Brown and Warner (1985) find in their simulation tests that OLS performs as well as more complex regression models; therefore, we begin our tests using OLS. As discussed in Section 2 above, robust methods have been shown to provide valuable insights in other financial datasets. Therefore, we also estimate our model using the M-estimator and the MM-estimator. The M-estimator was introduced by Huber (1973). This is the earliest and simplest robust estimation approach that essentially utilizes median values of the sample and mitigates the influence of outliers by assigning them a weight based on a repeating algorithm until the result is sufficiently improved. The method is heavily utilized in research, but is not robust to leverage points. MM-estimation was developed by Yohai (1987) and combines M-estimation and high breakdown values estimation (S estimation), which was previously developed in Rousseeuw and Yohai (1984). All regression computations were performed in SAS.

We are concerned with proper treatment of the outliers and leverage points that occur at the peaks of the market volatility. We use both robust methods, since M-estimation is well-established for the purpose of financial data analysis and MM-estimation is expected to better deal with extreme values among independent variables. Such leverage points are not uncommon in our data sample.

We summarize our expectations of the results with three hypotheses (stated in null form):

$H_o(1)$ there is no difference between the event effect-related coefficients obtained using OLS and an M-estimator robust to outliers:

$$\begin{array}{ll}
\alpha'_{\mathrm{OLS}} = \alpha'_M, & p(\alpha'_{\mathrm{OLS}}) = p(\alpha'_M), \\
\alpha_{0\,\mathrm{OLS}} = \alpha_{0M}, & p(\alpha_{0\,\mathrm{OLS}}) = p(\alpha_{0M}), \\
\beta'_{\mathrm{OLS}} = \beta'_M, & p(\beta'_{\mathrm{OLS}}) = p(\beta'_M), \\
\beta_{0\,\mathrm{OLS}} = \beta_{0M}, & p(\beta_{0\,\mathrm{OLS}}) = p(\beta_{0M}), \\
\gamma_{\mathrm{OLS}} = \gamma_M, & p(\gamma_{\mathrm{OLS}}) = p(\gamma_M).
\end{array}$$

$H_o(2)$ there is no difference between the event effect-related coefficients obtained using an M-estimator and an MM-estimator robust to outliers

$$\begin{array}{ll}
\alpha'_M = \alpha'_{MM}, & p(\alpha'_M) = p(\alpha'_{MM}), \\
\alpha_{0\,M} = \alpha_{0MM}, & p(\alpha_{0\,M}) = p(\alpha_{0MM}), \\
\beta'_M = \beta'_{MM}, & p(\beta'_M) = p(\beta'_{MM}), \\
\beta_{0\,M} = \beta_{0MM}, & p(\beta_{0\,M}) = p(\beta_{0MM}), \\
\gamma_M = \gamma_{MM}, & p(\gamma_M) = p(\gamma_{MM}).
\end{array}$$

$H_o(3)$ more than one observation within an event window is an outlier.

4. Results

4.1 Model Estimation using OLS

Table 2 presents the results of our estimation of the model. In the table the model is estimated using OLS in Panel A, the M-estimator in Panel B, and the MM-estimator in Panel C. In the panels, the coefficients are presented in two groups separating event effect coefficients from control variable coefficients. We are mostly interested in the event effect parameters section that captures three time layers of effect: change in risk at introduction of the reform, abnormal returns during key event windows throughout the legislation process, and change in risk at reform enactment.

We begin our discussion with the results of the OLS estimation in Panel A of Table 2. In the United States and Canada there is an increase in systematic risk, measured by the beta prime coefficient, at the introduction of the legislation. In France, United Kingdom, Italy and Spain there are significant negative abnormal returns during the intermediate steps in the legislative process. Surprisingly, using OLS, no effect was detected in Japan and Germany. Given the size and importance of these economies we would expect these markets to react to the U.S. legislation if other countries reacted. Note that the Canadian market reacts in a very similar way to the U.S. even though the Dodd-Frank legislation should not directly affect Canada.

The strong reaction of the European markets to the intermediate events in the U.S. legislative process should be viewed with caution. It is likely that events in Europe unrelated to the Dodd-Frank legislative process caused the observed negative abnormal returns. The presence of confounding events; however, does not change the significance of the methodological issues that are the focus of this research. We still observe the distortion of the event effect detection accuracy by the presence of significant outliers outside of the event window. The efficiently of the robust methodology remains important regardless of the economic cause of the observed event effect.

As to the control variables, we find a strong negative relation between foreign exchange market movements and stock market movements for all countries (lambda coefficient). The international equity market index (beta coefficient) is significantly related to local market returns for some countries and close to being significant for others holding the same sign except for Japan and China where the coefficient is of the opposite sign and clearly insignificant. Interestingly, the regression R^2 is very low for these countries as compared to the other countries in the sample. The international equity market index appears to hold significant explanatory power in most markets; but, perhaps the index that we selected as a

Table 2: Dodd-Frank: countries (−120; +120) period (−1; +1) window

$$R_i = \alpha_i + \alpha_i' D' + \alpha_{0i} D_0 + \beta_i R_m + \beta_i' D' R_m + \beta_{i0} D_0 R_m + \delta_i R_{rf} + \lambda_i R_{fx} + \gamma_i D + \varepsilon$$

R_i − daily return on the tested index; α − index alpha; α' − difference between index alpha before/after the tested legislation introduced; α_0 − difference between index alpha before/after the tested legislation enacted; D' − before/after the tested legislation introduction dummy (0-before, 1-after); D_0 − before/after the tested legislation enactment dummy (0-before, 1-after); β − index beta R_m − market return; β' - coefficient of the change in index market beta (risk) after the tested legislation introduced; β_0 − coefficient of the change in index risk after the tested legislation enacted; δ − risk free rate coefficient R_{rf} − risk free rate return; λ − Forex market return coefficient; R_{fx} − Forex market return; γ − coefficient of cumulative abnormal returns; D − dummy variable of the event windows (1 − during window, 0 − otherwise); ε − error term

Panel A: OLS

p-values are reported below the coefficients *** − significant at 1%, ** − significant at 5%, * − significant at 10%

		United States	Japan	China	Germany	France	United Kingdom	Italy	Brazil	Spain	Canada
		Event Effect Parameters									
1	α'	0.0299	0.07012	0.12892	0.04846	0.1581	0.11872	0.2094	−0.21837	0.60007	-0.18285
		0.9246	*0.8548*	*0.7488*	*0.8815*	*0.6475*	*0.6688*	*0.587*	*0.559*	*0.1132*	*0.5418*
	β'	**0.28134*** **	0.02637	-0.0336	0.09438	0.09927	0.09965	0.03485	0.09547	0.03626	**0.22312*** **
		0.0014	*0.8108*	*0.7675*	*0.3063*	*0.3009*	*0.2003*	*0.7483*	*0.3681*	*0.7339*	***0.0075***
2	Γ	-0.37179	-0.41372	-0.01073	-0.33599	**-0.57488** **	**-0.45236** **	**-0.58372* **	-0.19494	**-0.52833* **	-0.18817
		0.1249	*0.1654*	*0.9723*	*0.1843*	***0.0298***	***0.0343***	***0.0505***	*0.4992*	***0.0713***	*0.4073*
	α_0	-0.01523	0.05539	0.06859	-0.04051	-0.02354	0.02101	-0.07173	-0.10716	-0.10955	0.01082
		0.9199	*0.7664*	*0.7259*	*0.7984*	*0.8877*	*0.8762*	*0.7013*	*0.5569*	*0.5507*	*0.9397*
	β_0	0.07002	0.1457	0.03121	-0.04594	-0.08805	-0.05875	0.02675	0.01459	-0.02719	0.08758
		0.606	*0.3882*	*0.8576*	*0.748*	*0.5565*	*0.6228*	*0.8738*	*0.9298*	*0.8694*	*0.4916*
		Control variables and intercept									
	B	**-0.49351*** **	0.05934	0.04831	-0.09566	-0.07772	**-0.1015** **	-0.1003	**-0.37442*** **	-0.10401	**-0.38456*** **
		0.0001	*0.426*	*0.533*	*0.1342*	*0.2416*	***0.0573***	*0.1824*	***0.0001***	*0.1591*	***0.0001***
	Δ	-0.12924	0.08937	0.29756	-0.05419	0.04663	0.04103	0.14165	-0.08174	0.48132	-0.13692
		0.6211	*0.7697*	*0.3534*	*0.8368*	*0.8704*	*0.857*	*0.652*	*0.7866*	*0.1189*	*0.5795*
	Λ	**-0.75283*** **	**-0.4186*** **	**-0.57509*** **	**-1.24376*** **	**-1.31034*** **	**-1.0428*** **	**-1.35813*** **	**-0.94754*** **	**-1.28922*** **	**-0.68731*** **
		0.0001	***0.0003***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***
	A	0.1537	-0.05831	-0.29967	0.09004	-0.10357	-0.04601	-0.2298	0.37275	-0.82257	0.34196
		0.7414	*0.9166*	*0.6095*	*0.8498*	*0.8390*	*0.9102*	*0.6844*	*0.4948*	*0.1387*	*0.4379*
	R^2	0.2376	0.0357	0.0498	0.2531	0.2583	0.2578	0.2333	0.2099	0.2284	0.2008

* 1 − change in risk at introduction of the Act; 2 − abnormal returns measured throughout the legislation process; 3 − change in risk when Act was signed into Law

Table 2 (continued): Dodd-Frank: countries (−120; +120) period (−1; +1) window

Panel B: M-estimator

p-values are reported below the coefficients *** – significant at 1%, ** – significant at 5%, * – significant at 10%

		United States	Japan	China	Germany	France	United Kingdom	Italy	Brazil	Spain	Canada
		Event Effect Parameters									
1	α'	0.1844	0.316	0.4597	-0.1608	-0.077	-0.2221	-0.0534	-0.0272	0.4312	-0.2068
		0.484	*0.3837*	*0.2122*	*0.5833*	*0.8066*	*0.3868*	*0.8796*	*0.9333*	*0.206*	*0.4375*
	β'	**0.1945*** **	-0.0614	-0.0328	0.0564	0.0117	-0.0144	-0.0883	0.0298	0.0182	**0.1456** **
		0.0078	*0.5563*	*0.7524*	*0.4971*	*0.8937*	*0.8412*	*0.3745*	*0.7463*	*0.8494*	***0.0489***
2	Γ	-0.1543	**-0.5916** **	-0.0617	**-0.4787** **	**-0.4887** **	**-0.3934** **	**-0.4745***	-0.1503	-0.2176	-0.1584
		0.4447	***0.036***	*0.8271*	***0.0357***	***0.0419***	***0.0462***	***0.0819***	*0.5484*	*0.409*	*0.4323*
	α_0	-0.0233	0.0743	0.0097	-0.0461	-0.0494	-0.0209	-0.052	-0.1109	-0.1207	0.0049
		0.8539	*0.674*	*0.9569*	*0.7472*	*0.7448*	*0.867*	*0.7614*	*0.4838*	*0.4657*	*0.9694*
	β_0	0.1084	0.1086	0.0773	-0.0091	0.0111	-0.0164	0.0969	0.015	0.0381	0.1008
		0.3385	*0.4971*	*0.6274*	*0.9436*	*0.935*	*0.8817*	*0.5299*	*0.9171*	*0.7981*	*0.3732*
		Control variables and intercept									
	B	**-0.4239*** **	**0.1323***	0.0207	-0.0938	-0.0832	-0.0365	-0.0372	**-0.3309*** **	-0.0992	**-0.313*** **
		0.0001	***0.0608***	*0.7706*	*0.1028*	*0.1679*	*0.4586*	*0.5886*	***0.0001***	*0.1358*	***0.0001***
	Δ	-0.2309	0.2816	**0.5817** **	-0.1708	-0.1888	-0.1937	0.0914	-0.0994	0.3446	-0.0634
		0.2898	*0.33*	***0.0473***	*0.4711*	*0.4677*	*0.3578*	*0.7505*	*0.7046*	*0.2149*	*0.773*
	Λ	**-0.7617*** **	**-0.238** **	**-0.4935*** **	**-1.1671*** **	**-1.2512*** **	**-1.0302*** **	**-1.2679*** **	**-0.7934*** **	**-1.2077*** **	**-0.6637*** **
		0.0001	***0.0297***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***
	A	0.0474	-0.4106	-0.6786	0.361	0.2691	0.4347	0.0418	0.1857	-0.5963	0.335
		0.903	*0.4364*	*0.2061*	*0.3997*	*0.5617*	*0.2494*	*0.9356*	*0.6951*	*0.2331*	*0.3925*

* 1 – change in risk at introduction of the Act; 2 – abnormal returns measured throughout the legislation process; 3 – change in risk when Act was signed into Law

Table 2 (continued): Dodd-Frank: countries $(-120;+120)$ period $(-1;+1)$ window

		United States	Japan	China	Germany	France	United Kingdom	Italy	Brazil	Spain	Canada
		Panel C: MM-estimator									
		p-values are reported below the coefficients *** – significant at 1%, ** – significant at 5%, * – significant at 10%									
		Event Effect Parameters									
1	α'	0.166	0.3517	0.5343	-0.2481	-0.1018	-0.2896	-0.1078	-0.0292	0.3732	-0.1824
		0.5388	*0.3453*	*0.1462*	*0.4181*	*0.7553*	*0.2806*	*0.7696*	*0.9312*	*0.2803*	*0.5268*
	β'	**0.1798****	-0.1104	-0.0342	0.0481	0.0209	-0.0271	-0.1244	0.0103	0.0209	**0.1304***
		0.0176	*0.3068*	*0.7476*	*0.576*	*0.8202*	*0.7192*	*0.2377*	*0.9125*	*0.8352*	***0.0823***
2	γ	-0.1286	**-0.6837****	-0.061	**-0.5486****	**-0.475***	**-0.3738***	-0.4287	-0.1484	-0.1268	-0.1494
		0.5257	***0.018***	*0.8253*	***0.0199***	***0.0602***	***0.0697***	*0.1266*	*0.5518*	*0.6446*	*0.4521*
	α_0	-0.0264	0.067	0.0031	-0.0528	-0.0521	-0.0468	-0.0591	-0.1209	-0.1459	0.0067
		0.8261	*0.6961*	*0.9859*	*0.7032*	*0.7294*	*0.7062*	*0.7276*	*0.4285*	*0.3764*	*0.9553*
	β_0	0.111	0.092	0.0862	0.0023	0.0156	0.0001	0.1209	0.0059	0.0657	0.1043
		0.3025	*0.558*	*0.5797*	*0.9853*	*0.9078*	*0.999*	*0.433*	*0.9655*	*0.6674*	*0.3263*
		Control variables and intercept									
	B	**-0.4083*****	**0.1738****	0.021	-0.0909	-0.0882	-0.0432	-0.0095	**-0.3083*****	-0.1009	**-0.2997*****
		0.0001	***0.0216***	*0.7719*	*0.1331*	*0.1709*	*0.4069*	*0.899*	***0.0001***	*0.1414*	***0.0001***
	δ	-0.2487	0.2557	**0.6784****	-0.2288	-0.1934	-0.2792	0.0906	-0.0954	0.2656	-0.0079
		0.2665	*0.3909*	***0.0217***	*0.3628*	*0.479*	*0.2061*	*0.7634*	*0.7259*	*0.3496*	*0.9733*
	λ	**-0.7673*****	-0.1805	**-0.4722*****	**-1.2021*****	**-1.2873*****	**-1.027*****	**-1.3182*****	**-0.7657*****	**-1.1972*****	**-0.6518*****
		0.0001	*0.1027*	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***	***0.0001***
	α	0.0763	-0.4203	-0.7915	0.4821	0.2977	0.559	0.1056	0.1916	-0.4837	0.2815
		0.8473	*0.4374*	*0.1391*	*0.2828*	*0.537*	*0.1556*	*0.8446*	*0.6968*	*0.3407*	*0.503*

* 1 – change in risk at introduction of the Act; 2 – abnormal returns measured throughout the legislation process; 3 – change in risk when Act was signed into Law

benchmark does not explain the Asian markets as well as Western markets. Interest rates (delta coefficient) are not a significant predictor of equity market returns in our sample.

4.2 Results Using Robust Estimators

Panels B and C of Table 2 provide results of the robust M- and MM-estimators, respectively. The results for the control variables are generally consistent across all three estimators. Hence to conserve space we focus the discussion only on the event related coefficients. To facilitate comparison of the estimators, in Table 3 we show only the statistically significant event related coefficients for all three estimators – OLS, M and MM. From Tables 2 and 3 we see that the results change with the change of method.

Table 3: Summary of the results improvement

	OLS		M		MM	
	β'	γ	β'	γ	β'	γ
United States	**0.28***** ***0.0014***		**0.19***** ***0.0078***		**0.18**** ***0.0176***	
Japan				**-0.59**** ***0.036***		**-0.68**** ***0.018***
China						
Germany				**-0.47**** ***0.0357***		**-0.55**** ***0.0199***
France		**-0.57**** ***0.0298***		**-0.49**** ***0.0419***		**-0.48*** ***0.0602***
United Kingdom		**-0.45**** ***0.0343***		**-0.39**** ***0.0462***		**-0.37*** ***0.0697***
Italy		**-0.58*** ***0.0505***		**-0.47*** ***0.0819***		
Brazil						
Spain		**-0.52*** ***0.0713***				
Canada	**0.22***** ***0.0075***		**0.14**** ***0.0489***		**0.13*** ***0.0823***	

With the robust M-estimator the abnormal returns during the legislative process (gamma coefficient) in Germany and Japan become statistically significant. This is more consistent with the findings in other major economies France and United Kingdom. Major markets usually move synchronously as they react to the major events that drive anticipation of the change in returns, so we would expect all of the markets to react more or less uniformly to an event with broad and uncertain impact. However, the gamma coefficient, a representation of the cumulative abnormal returns of Spain, loses its significance. This may be because the stock market participants of Spain, as a smaller and less influential country, are less likely to anticipate the consequences of U.S. financial reform than investors in larger economies. The gamma coefficient (cumulative abnormal returns) for Italy also looses statistical significance with the MM method. Investors in Spain (and subsequently Italy) may have been forced to devote attention to their internal financial issues and their dependency on the assistance from the European Union to support their financial systems. Therefore, the significance of their role as global market participants may have decayed for those countries. The gamma coefficients in Japan and Germany increase in absolute value and statistical significance with the move from the M-estimator to the MM-estimator. The difference in the magnitude of the reaction between U.S. and Canada in both size and statistical significance becomes more pronounced as we move from OLS to the M-estimator then to the MM-estimator. One would expect that the reaction to foreign regulation, even in a very connected country such as Canada to the U.S. should be weaker than in the domestic market. The robust estimators make this difference more apparent.

Based on the results in Tables 2 and 3 we reject the first and second null hypotheses of coefficients and corresponding probabilities being equal regardless of estimation methodology. Therefore we need to give closer consideration to outliers. We turn to that analysis in the next section.

4.3 Outliers

The assumption of normality is important for the accuracy of the OLS regression results. However, daily stock returns typically are not normally distributed, which raises a question of the validity of the results using OLS. When we obtain the residuals from an OLS regression, we would like to see independence and constant variance to be sure that the prediction equation obtained through the regression analysis is reasonably correct for the majority of the observations in the sample (and inherently in the population). However we usually see some large residual values that suggest some members of the sample do not follow the common pattern (outliers). Researchers that specialize in robust estimation methods (e.g., Yohai, 1987) were able to clearly demonstrate how the outlying observations corresponding to the large residuals are able to pull the whole prediction equation toward them and adversely affect the accuracy of the prediction for the majority of the observations. They also show that proper treatment of the outliers increases overall prediction accuracy.

In an attempt to avoid the problem of outliers, some researchers try to cut out the largest and smallest values in the samples. In addition to loosing potentially valuable information, the exercise is useless for multivariate models. Multivariate outliers are not necessarily the values at the ends of the sorted sample (Barnett and Lewis, 1978). They are values that do not fit well the pattern of the majority of the data. Answering the question why some observations do not fit the pattern may help to discover interesting and useful facts or trends.

Figure 1 provides residual plots for eight of the ten countries under analysis. The countries in the figure are the countries where results changed using the robust estimators, plus the U.S. as a benchmark. Outliers, which are identified using Cook's distance, are marked with an X. All eight of the datasets are heavily contaminated with outliers. We cannot simply exclude outliers from the analysis because many of the outliers comprise our event-related sample and carry important information for our research. Also note that according to the Cook's distance rule we mark some data points that are not very distant from the origin on the vertical axis, but rather on the horizontal axis. These data points are high leverage data points for which residuals are not that large, but the values are on the outskirts of the data range (Cook's distance is not robust to the high leverage points). The existence of those data points suggests not only that a robust estimator is necessary, but that the MM-estimator (Yohai, 1987), which can properly care for the outlying values in both dimensions, is particularly important.

In both Spain and Italy, countries where the gamma coefficients (abnormal returns) lost statistical significance using robust estimators, we see a data point that is very distant from others. This data point occurs on May 10, 2010, the day when the Federal Reserve announced that it would open a currency swap line for the European Central Bank to help fight the sovereign debt crises within the European Union. Although this event is not related to our research interest directly, indirectly it confirms international interconnectivity, detectable by the event study methodology.

Table 4 lists, by country, the outliers among the observations within the event windows. We can't reject our third null hypothesis, since outliers exist in all datasets under review. There were a total of 33 event days in the period under our review (11 event periods, 3 days per event period) and we find that 5-9 of the observations are outliers. We clearly may not ignore these observations by excluding them from the analysis. Nor should we include them without proper handling.

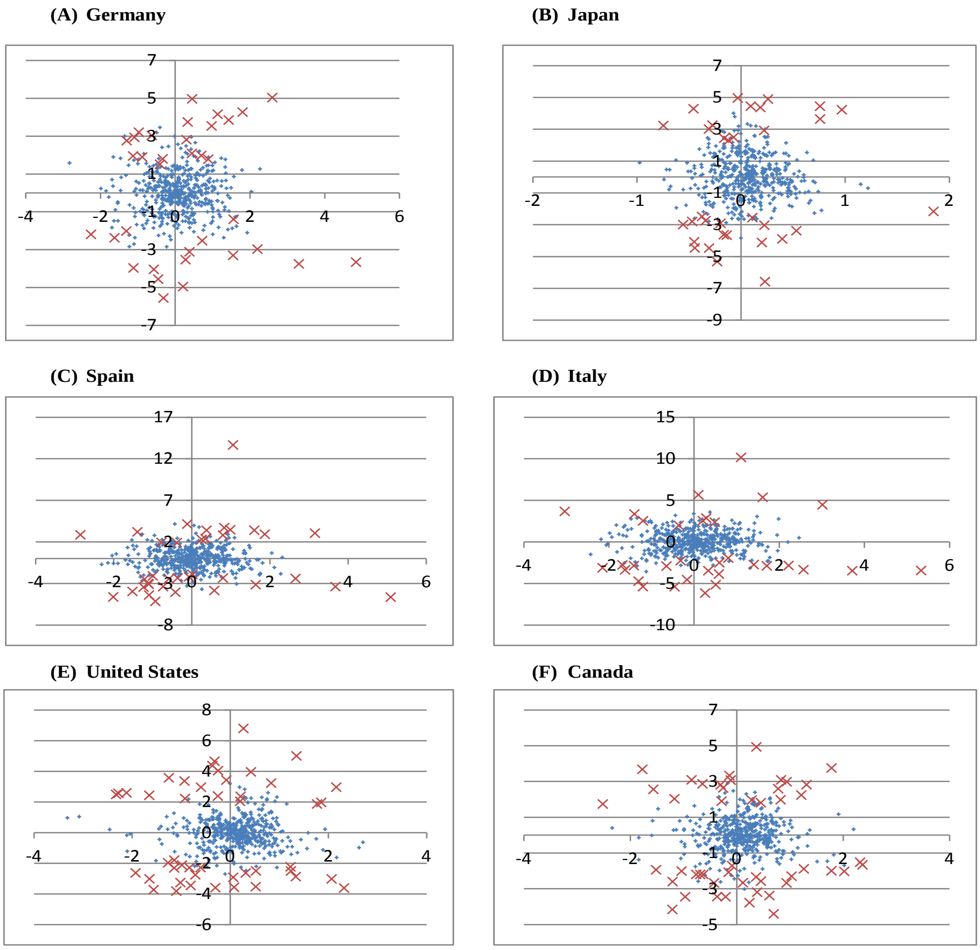

Figure 1: OLS Residuals (horizontal axis – predictions, vertical axis – residuals)

The smallest number of outliers within the event window is in the U.S. sample. The results in the U.S. results do not change significantly with the application of the robust methodology. This is consistent with the idea that outliers distort event effect detection. Additionally, in all countries where results changed significantly the day when legislation was signed into law was an outlier. In Japan, July 23, 2010 was the first business day after the event and the residual on that day is large, just as on some other outliers where Cook's distance just missed the cut-off threshold, thus we are willing to accept it as not being an exception to the rule. We conjecture that the day when legislation is signed into law should be particularly significant for the international markets, since the details of the legislative process may not necessarily be closely followed abroad, while the final event will be the one attracting most of the attention.

Table 4: Outliers in the event windows

Date	Event	OLS Residual	M Residual	MM Residual
Panel A: Germany				
05/29/09	Initial proposal by president Obama	-1.4	-1.27501	-1.29304
06/01/09	Initial proposal by president Obama	3.7465	3.975015	4.061119
12/01/09	Law Proposed in congress	1.9728	2.157526	2.194183
12/11/09	House bill passed	1.9487	1.993302	2.076168
05/12/10	Interchange fees amendment by Durbin	3.0382	3.154942	3.236035
05/13/10	Interchange fees amendment by Durbin	1.5095	1.772241	1.867364
06/29/10	Conference report	-2.0225	-1.91246	-1.80195
07/01/10	Conference report	-2.5305	-2.32817	-2.27936
07/22/10	Signed into Law	2.0844	2.295957	2.362926
Panel B: Japan				
12/01/09	Law Proposed in congress	2.4907	2.812521	2.932834
12/03/09	Law Proposed in congress	4.2856	4.421684	4.484456
12/11/09	House bill passed	3.2253	3.277721	3.307106
05/06/10	Shareholders proxy amendment by Dodd	-2.8013	-2.74379	-2.69207
05/13/10	Interchange fees amendment by Durbin	2.2897	2.478514	2.578859
05/21/10	Senate passed	-2.5595	-2.22226	-2.07793
07/14/10	Senate passed	3.0079	3.161065	3.250317
07/16/10	Senate passed	-2.4861	-2.37159	-2.29395
7/23/10*	Signed into Law	2.4316	2.309579	2.276783
Panel C: Spain				
04/28/10	Shareholders proxy amendment by Dodd	-2.0812	-2.42885	-2.53537
04/29/10	Shareholders proxy amendment by Dodd	2.391	2.144643	2.049957
05/14/10	Interchange fees amendment by Durbin	-4.6085	-4.96408	-5.07121
06/24/10	Conference reconciliation finished	-2.4407	-2.7031	-2.78991
06/28/10	Conference report	1.9254	1.715405	1.629874
06/29/10	Conference report	-3.9504	-4.2843	-4.37819
07/22/10	Signed into Law	2.3112	2.123062	2.080045
Panel D: Italy				
05/29/09	Initial proposal by president Obama	-2.722	-2.77432	-2.89288
06/01/09	Initial proposal by president Obama	2.865	-2.77432	2.704402
12/01/09	Law Proposed in congress	2.3748	2.332686	2.233193
05/14/10	Interchange fees amendment by Durbin	-3.1002	-3.19824	-3.18053
06/29/10	Conference report	-2.757	-2.90096	-2.90901
07/01/10	Conference report	-2.4937	-2.54652	-2.65174
07/22/10	Signed into Law	2.5556	2.508979	2.436981
Panel E: United States				
06/01/09	Initial proposal by president Obama	2.3676	2.207611	2.191586
05/20/10	Senate passed	-3.5887	-3.78937	-3.8129
06/29/10	Conference report	-2.1178	-2.29652	-2.31496
07/16/10	Senate passed	-2.2771	-2.45546	-2.47825
07/22/10	Signed into Law	2.047	1.866787	1.844846
Panel F: Canada				
05/29/09	Initial proposal by president Obama	-1.9899	-1.90944	-1.88842
06/01/09	Initial proposal by president Obama	1.9672	1.882832	1.838155
12/01/09	Law Proposed in congress	1.8158	1.797657	1.799494
01/21/10	Obama proposed Volker's rule	-1.7041	-1.72408	-1.72254
05/12/10	Interchange fees amendment by Durbin	1.9041	1.851042	1.832994
05/20/10	Senate passed	-2.0676	-2.13808	-2.11945
06/29/10	Conference report	-2.1898	-2.2619	-2.29255

* Cook's distance of 0.005 does not let us to name this observation an outlier. See discussion in the text.

To summarize our results so far: We conduct an event study of the reaction of the stock markets in ten major countries to the enactment in the U.S. of Dodd-Frank. The economic importance of our study is that we demonstrate that Dodd-Frank affected the broad stock market in the U.S. and beyond the borders of the U.S. However, the main focus of this study is methodological. In particular, we show that the datasets for all of the countries are heavily contaminated with outliers and high leverage points. Moreover, these outliers and high leverage points cannot be simply removed because many of these problematic data points lie within the event windows under study. We therefore employ the M-estimator, which is robust to the presence of outliers, and the MM-estimator, which is robust to the presence of both outliers and high leverage points. The results using these robust estimators are different from the results using OLS. Clearly the outliers and high leverage points affect inferences in our event study. This finding indicates that event studies using OLS should be interpreted with caution, at least in the datasets used for this study. The question remains as to whether or not the observed effect stretches beyond our specific case. To shed light on this question, in the next section we construct a pseudo-simulation in which the independent variables come from the empirical data, while the base values of the dependent variable, contaminated by outliers, are simulated at various levels in various combinations.

5. Simulation

5.1 Experimental Design

We use the U.S. data sample from the empirical part of our study as a base (523 observations, over the event timeline of the Dodd-Frank Act). We keep the independent variables real and only simulate the dependent variable. We utilize the real timeline when simulating the event effect. In our first step, we generate 100 normally distributed samples using the mean and standard deviation of the base sample (mean 0.0822 standard deviation = 1.4867). In the second step we simulate three different event effects: 1) change in systematic risk at the introduction of Dodd-Frank, measured in our model by β'; 2) abnormal returns on the significant dates in the legislative process, measured in our model by γ; and, 3) change in systematic risk at the enactment of Dodd-Frank, measured in our model by β_0. We test each effect at three strength levels as discussed below. We test the performance of our model for the three types of the event effects separately and combined. The first twelve scenarios are designed to gauge the power of the model in terms of Type II errors. We also test three no-effect scenarios to determine the performance in terms of Type I errors.

In scenarios 1-3 we simulate a change in the market beta (β') at the introduction of the reform. Since market beta is a coefficient representing the slope of a line, we multiply all dependent variable values from the first event-related day to the end of the data sample by the chosen value to simulate a change in slope. We select the mid-level change in slope to be equal to the change in market beta observed using OLS in our empirical study (0.28134). We set the low-level event effect at the value of the lower bound of the 95% confidence interval (0.10936), and the high-level at the value of the upper bound (0.45332). We introduce these three levels of effect effects in 100 samples each. Scenario 1 is based on the low magnitude change, scenario 2 on the mid-level change, and scenario 3 on the high level change. This process yields 300 testable samples.

In scenarios 4-6, similar to 1-3, we simulate 3 levels of the change in market beta (β_0) on the last event day by multiplying all values from that day forward by the simulated event effect. In the empirical study we do not observe a statistically significant. Therefore we do not have an empirical starting point. Based on diagnostic runs, we chose event effect values such that we are able to observe the effect. The values are selected as 1.5 for scenario 4, 2 for scenario 5, and 2.5 for scenario 6. These scenarios add 300 samples to our experiment.

We simulate abnormal returns on the event days in scenario 7-9. Just as in cases 4-6, we do not have empirical observations of the abnormal returns in our earlier tests, so we have to pick suitable values based on diagnostic runs. We set the market index return on each event-related day (significant day and two surrounding days) at -0.5 in scenario 7, -0.6 in scenario 8, and -0.7 in scenario 9, and obtain 300 more samples for the experiment.

In scenarios 10-12 we combine all three effects as described above. For example, the 100 samples of scenario 10 contain the low magnitude changes, i.e., change in market risk at the start of the legislative process of 0.10936; change in country's market beta (risk) at the end of legislative process at 1.5; and, market index return of -0.5 on all event-related days. In scenarios 11 and 12 we insert the mid-level and high level changes, respectively. From this process we obtain another 300 samples.

We keep 100 samples in scenario 13 as they are: clean randomly selected samples with normal distributions, to test the no event effect situation. In scenarios 14 and 15 we simulate one large outlier that changes position in each sample starting with the 1st observation and moving to the next position with the increment of 5 (we have 100 samples and 523 observations, so 5 is a nice increment to cover the whole range of possible positions). For example in the first sample the outlier takes the place of the observation #1, in the second $-$ observation #5, in the 100th $-$ observation #500. We set large outliers at a daily stock return of 20% in scenario 14 and 30% in scenario 15. These values roughly approximate the magnitude of the outliers observed in the Italian and Spanish samples of our empirical study, adjusted by the magnitude of the portfolio volatility in the U.S. as compared to those countries. We obtain the last 200 samples for our experiment that way.

The 15 scenarios described so far result in 1500 normally distributed samples, representing 15 different scenarios of the event effect, with 100 samples in each scenario. In the third step we contaminate all samples with outliers. For that purpose we generate 100 samples with a double exponential distribution by combining exponential and binomial distributions. We randomly assign positions for 46 outliers to the 523 observations in the sample period. We choose 46 as the number of outliers because in our OLS tests of the U.S. portfolio, we identify 46 outliers using Cook's distance. We replace the values in the normal samples with the selected values from the double exponential sample. As a result of this process, we obtain 1500 contaminated samples, a hundred in each of the 15 scenarios described above. Finally we run 4500 tests based on 1500 samples processed with three different estimation methods − OLS, M and MM.

5.2 Simulation Results

The results of our simulation runs are presented in the Table 5. For each scenario we report the number of correct recognitions of the simulated effect per 100 experiments. For most of this discussion we define correct recognition as the presence of statistically significant results at the 95% confidence level, although we also report results at the 90% and 99% confidence levels.

Scenarios 1-3 test for recognition of a change in slope at the beginning of the event period (statistically significant β'). At all three levels of simulated slope change the robust estimators are much better at detecting the change than OLS. Although as the simulated change in slope increases in magnitude (scenarios 2 and 3) successful recognition for all three estimators deteriorates, the robust estimators remain unambiguously better than OLS. We conjecture that the decline in the number of successful detections as the slope increases is due to the proximity of the starting point of the change in slope to the beginning of the data sample.

Comparison of the two robust estimators is more complicated. For this discussion it is important to remember that the M-estimator is robust to outliers only while the MM-estimator is robust to both outliers and high leverage data points. OLS is robust to neither. With a very large slope change (scenario 1) the M-estimator displays somewhat better recognition than the MM-estimator. However, in scenarios 2 and 3, the MM-estimator performs somewhat better than the M-estimator. Our conjecture is that at a very high slope change the MM-estimation does not perform as well because re-weighting the leverage points plays a misleading role.

Table 5: Event effect recognition accuracy

We report a number of event effect recognitions per 100 experiments defined as presence of the statistically significant corresponding regression coefficient. β' − risk (slope) change at the introduction of the reform; β_0 − risk (slope) change at the enactment of the reform; γ − abnormal returns.

	Confidence level	99% ($\alpha = 0.01$)			**95% ($\boldsymbol{\alpha} = \mathbf{0.05}$)**			90% ($\alpha = 0.1$)		
Scenario	**Parameter Name**	**OLS**	**M**	**MM**	**OLS**	**M**	**MM**	**OLS**	**M**	**MM**
1	Count of $p(\beta') <= \alpha$	8	83	77	**15**	**85**	**82**	22	88	84
2	Count of $p(\beta') <= \alpha$	6	43	52	**18**	**55**	**58**	23	61	64
3	Count of $p(\beta') <= \alpha$	6	20	21	**17**	**30**	**33**	22	39	38
4	Count of $p(\beta_0) <= \alpha$	2	5	6	**11**	**15**	**17**	20	21	27
5	Count of $p(\beta_0) <= \alpha$	4	16	18	**20**	**30**	**28**	27	37	37
6	Count of $p(\beta_0) <= \alpha$	9	28	26	**23**	**41**	**39**	30	51	46
7	Count of $p(\gamma) <= \alpha$	4	4	7	**52**	**56**	**63**	75	75	81
8	Count of $p(\gamma) <= \alpha$	30	30	40	**76**	**77**	**84**	94	97	97
9	Count of $p(\gamma) <= \alpha$	60	62	68	**94**	**97**	**97**	99	99	99
10	Count of $p(\beta') <= \alpha$	8	81	74	**16**	**87**	**81**	26	89	85
10	Count of $p(\beta_0) <= \alpha$	0	1	0	**0**	**5**	**4**	1	10	9
10	Count of $p(\gamma) <= \alpha$	0	0	2	**0**	**10**	**17**	0	36	45
11	Count of $p(\beta') <= \alpha$	7	34	34	**14**	**42**	**45**	23	51	54
11	Count of $p(\beta_0) <= \alpha$	0	2	2	**5**	**10**	**12**	6	20	22
11	Count of $p(\gamma) <= \alpha$	0	0	1	**2**	**15**	**29**	6	40	60
12	Count of $p(\beta') <= \alpha$	2	8	12	**6**	**16**	**22**	14	27	29
12	Count of $p(\beta_0) <= \alpha$	3	14	15	**13**	**26**	**25**	20	36	37
12	Count of $p(\gamma) <= \alpha$	0	1	8	**11**	**48**	**60**	48	78	90
13	Count of $p(\beta') <= \alpha$	3	3	2	**11**	**10**	**10**	16	14	15
13	Count of $p(\beta_0) <= \alpha$	1	1	1	**4**	**3**	**5**	8	12	10
13	Count of $p(\gamma) <= \alpha$	0	0	0	**4**	**3**	**2**	9	8	6
14	Count of $p(\beta') <= \alpha$	3	3	2	**6**	**10**	**10**	11	13	15
14	Count of $p(\beta_0) <= \alpha$	4	1	1	**5**	**3**	**5**	10	11	10
14	Count of $p(\gamma) <= \alpha$	1	0	0	**3**	**2**	**2**	8	7	6
15	Count of $p(\beta') <= \alpha$	2	3	2	**8**	**10**	**10**	10	13	15
15	Count of $p(\beta_0) <= \alpha$	3	1	1	**5**	**3**	**5**	7	11	10
15	Count of $p(\gamma) <= \alpha$	3	0	0	**5**	**2**	**2**	5	7	6

In scenarios 4-6 we observe similar trends based on the change in slope of the regression line (systematic risk) on the last event day in our experiment, which is captured by β_0. Remember, we had to use large experimentally selected values to simulate the slope change effect on this tail of our timeline. Most importantly, the two robust estimators perform better than OLS in all three scenarios. Interestingly, here we see that the efficiency of MM-estimation in the recognition of the effect fades as the magnitude of the slope change increases. In scenario 4 MM-estimation performs better than M-estimation, but not in scenarios 5-6. Let us now jump ahead and look at the performance of the change in slope identification in scenarios 10-12 (significant β' and/or β_0), where we apply all three types of the event effect in combination and we find the same patterns, except that combined results are weaker, suggesting that our three different variables capture pieces of the same effect.

We conclude that for changes in slope captured by the dummy variables both robust methods substantially outperform OLS. The results comparing M-estimation to MM-estimation are mixed. The two methods seem to provide similar results.

The improvement of the effect recognition accuracy from OLS to M to MM is obvious and straight forward when we consider abnormal returns in scenarios 7-9. At the mid-level event effect (scenario 8) OLS recognizes the effect in 76 cases out of 100, the M-estimator in 77, and the MM-estimator in 84 cases. The same pattern is present in scenario 7, where we apply a smaller, return effect across event days. With the high level effect in scenario 9 as we get very high event recognition with all three estimators, but we still see an improvement using the robust estimators over OLS. In this scenario there is no distinguishable difference between the M- and MM-estimators at the 95% confidence level, although the MM-estimator is more accurate at the 99% confidence level. A similar pattern occurs for abnormal returns (γ) in the combined effect scenarios 10-12, albeit on a smaller scale.

In scenarios 13-15 we consider type I error $-$ false recognition of the event effects. As we look through the change in risk (slope) recognition results, we admit that they are quite inconclusive. At 95% confidence level M-estimation perhaps performs the best, in that it produces the least number of false recognitions in most of the slope change tests; however, the pattern is not always supported at the 99% and 90% percent confidence level.

In sharp contrast, for abnormal return effects the performance of the robust methods is clearly better than OLS; and, in many cases the MM-estimator performs better than the M-estimator (lower number of the false effect recognitions). There is an interesting effect in the abnormal returns recognition results from the introduction of the single large outliers in the scenarios 14 and 15: the number of false recognitions of the abnormal returns is different and mostly higher in the presence of the outlier when using OLS, but the robust methods, especially the MM-estimator, perform stably in the presence of the outlier at the 90% confidence level.

6. Conclusion

We perform an event study of the reaction of the equity markets in ten of the world's largest economies to the Dodd-Frank Act. The results show that the effects of this U.S. specific regulation extended well beyond the U.S. borders. Interestingly, the initial market reaction to the legislation was an increase in systematic risk as measured by beta. This outcome is opposite the intent of the regulation.

Although these results are interesting as a Finance study, the main thrust of this paper is methodological. We initially estimate our model using OLS, which is commonly used to in event studies. We then compare the OLS results with results from M and MM-estimators that are robust to outliers and high leverage points. In several countries the results using these robust methodologies become more economically meaningful as compared to the results that are obtained using OLS. Further analysis shows that there are a substantial number of outliers within event windows. We conclude that using the robust M and MM-estimators improves inferences from the event study.

We further test the empirical findings using the data obtained using a pseudo-simulation technique. We design the controlled experiment by introducing the event effect to the normally distributed simulated data samples and samples contaminated with outliers. We use event effects in the form of cumulative abnormal returns and changes in systematic risk, as measured by beta. We can see that percentage of correct event effect recognitions increases dramatically for changes in risk effects. In the most extreme cases correct recognitions increase from approximately 15% to 85%, as we switch the estimation method from OLS to robust regressions. We also observe a substantial increase in number of correct recognitions of cumulative abnormal returns, such as increase from 52% correctly recognized by OLS effect to 63% correctly recognized by robust regression, and an even greater improvement, such as increase from 11% to 60% for the samples with both types of event effects applied simultaneously. We also apply our experimental design to the subset of control samples with normally distributed returns, without outliers. Expectedly, we do not observe any improvement between OLS and robust regression methods. Based upon the results from the pseudo-simulation, we conclude that our findings extend beyond the specific case of particular empirical sample. Robust M and MM-estimators improve inferences from the event study model.

As to a comparison of the robust estimators, we find that the MM-estimator works best for the recognition of the abnormal returns on important event dates, while the M-estimator in many cases performs better or at least similar to the MM-estimator in detection of systematic risk changes. Our conjecture is that the reduction of the weight of high leverage data points using the MM-estimator distorts the rotation of the prediction equation line, which measures the change in risk. Additional support to our explanation of the above described effect would come from a fully controlled experiment measuring the influence of high leverage data points on the change in slope of regression line. We leave this question for the future research. In the meantime we suggest using the MM-estimator when abnormal returns are the focus of attention, while a change in systematic risk may be recognized as well, and sometimes better, by the M-estimator.

Our results have important implications for event studies, which are used extensively in Finance and other disciplines. In an event study stock market returns are used to gauge the reaction of investors to releases of new information. Many studies employing the event study methodology use OLS to estimate results. This is problematic since it is well known that stock market returns, are characterized by non-normality, significant outliers and high leverage data points; and, that inferences from OLS can be distorted by outliers and high leverage points. Moreover, it is likely that these problematic data points are found in event windows. Hence, they cannot be simply deleted without the loss of significant information. Our results show that, at least in one sample, these problems lead in some cases to incorrect inferences using OLS. We conclude that researchers should use estimators that are robust to outliers and high leverage points in event studies. At a minimum such estimators should be used as a robustness check for OLS results (Hogg, 1979, Launer and Wilkinson, 1979).

Appendix : Significant Dates in the Dodd-Frank Legislative Process

Event Date	Event Description
6/17/2009	Initial proposal by president Obama
12/2/2009	Law proposed in congress
12/11/2009	House passed
1/21/2010	Obama proposed Volker's rule
4/29/2010	Shareholders proxy amendment by Dodd
5/13/2010	Interchange fees amendment by Durbin
5/20/2010	Senate passed
6/25/2010	Conference reconciliation finished
6/30/2010	Conference report
7/15/2010	Senate passed
7/21/2010	Signed into Law

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