IS EUROPEAN M&A REGULATION PROTECTIONIST? *

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Why do regulatory authorities scrutinize mergers and acquisitions? The authorities themselves claim to be combating monopoly power and protecting consumers. But the last two decades of empirical research has found little supporting evidence for such motives. An alternative is that M&A regulation is actually designed to protect privileged firms. We provide a test of protectionism by studying whether European regulatory intervention is more likely when European firms are harmed by increased competition. Our findings raise a suspicion of protectionist motivations by the European regulator during the nineties. The results are robust to many statistical difficulties, including endogeneity between investor valuations and regulatory actions.

By means of glasses, hotbeds, and hotwalls, very good grapes can be raised in Scotland, and very good wine too can be made of them at about thirty times the expense for which at least equally good wine can be brought from foreign countries.

Adam Smith (*The Wealth of Nations*, Book IV, Chapter II)

1. Mergers and Acquisitions Regulation

Why are mergers and acquisitions (M&A) subject to regulatory scrutiny? Is the answer consumer protection through the maintenance of competition? Letting large producers merge might significantly increase the concentration level in a given industry. This could lead to various anticompetitive practices, such as price increases at the expense of consumers. This phenomenon, called the "market power hypothesis" in the M&A literature, is regularly emphasized by the U.S. Federal Trade Commission (FTC) Chairman, T. Muris, and the European official formerly in charge of competition policy, M. Monti. Still, both authorities recognize that some business combinations are efficient and actually help consumers.

Two decades of empirical research has been unable to find much support for the official position. Ellert (1976), Eckbo (1983), Stillman (1983), and Eckbo and Wier (1985) fail to find support for the market power hypothesis. New tests have emerged during the 1990s. Pesendorfer (1988), studying the M&A wave within the paper industry, Eckbo (1992), comparing US and Canadian experience, Slovin *et al* (1991), focusing on the airline industry, Bittlingmayer and Hazlett (2000), analyzing the Microsoft case, all come to the same conclusion. Fee and Thomas (2004) provide an alternative explanation for the welfare gains from mergers; they argue that suppliers are subject to more pressure by the merging parties, which brings substantial cost

savings. Perhaps more troubling is that competitor firms generally experience positive returns around proposed merger announcement dates. This could be regarded as indirect evidence of greater market power; but Song and Walking (2000) have proposed an alternative explanation. Using data from 1982 through 1991, they argue that these positive returns are due to investor anticipations of further M&A activity in the same industry. Shahrur (2005) examines the impact of horizontal mergers and takeovers on suppliers and customers. He concludes that the average takeover in his sample is driven by efficiency considerations.

All these results leave our initial question open: why does M&A regulation exist? Bittlingmayer and Hazlett (2000) suggest three possible explanations: bureaucratic self-interest, political extraction, and private benefits. We propose in this paper a direct test for one particular private benefit, protectionism[1]; i.e., M&A regulation could shelter local firms from foreign competition. Our proposed test is (1) whether foreign acquirers are subject to more frequent regulatory interventions than domestic ones when (2) local competitors are being harmed. Such a finding would be difficult to explain by anything other than protectionism. Negative domestic competitor returns around the merger announcement date are inconsistent with both increased market power and with a higher likelihood of subsequent acquisition. But such negative returns could be explained by increased competition benefiting consumers. There is however no reason why an acquirer's nationality should influence a regulator's inclination to intervene provided that the regulator is purely motivated by enhancing competition. The combination of (1) and (2) would raise a serious suspicion of protectionism.[2]

We have chosen to test European regulation for several reasons. First, the European Commission is not free of protectionism suspicion. The American financial press cried foul loudly in the General Electric/Honeywell merger case (Priest and Romani, 2001) and in the

Microsoft antitrust case. In its edition of July 23, 2001, *Business Week* says, "Europe also bears responsibility for the surge in protectionism. The EU fought a ridiculous trade war with the U.S. over bananas, of all things… The EU's focus on competitors rather than consumers makes the Bush Administration suspicious. If Europe goes after Microsoft Corp., as it might, there could be a big U.S. reaction." Aktas *et al.* (2004) uncover some troubling traits of European regulators. They find that investors anticipate a far higher cost to the merging parties when the European Commission intervenes against foreign bidders as opposed to domestic (i.e., European) bidders. Such evidence cannot support an unambiguous conclusion of protectionism but it certainly raises suspicion.

The second reason to study European M&A regulation is the fact that it is new, dating only from the beginning of the 1990s. Except for a broadening of intervention criteria in 1997, it has also been strikingly stable through time. Only recently, in 2004, have significant changes been adopted concerning intervention criteria and procedures (see Vickers (2005)). This allows us to cover a long period of regulatory activity (from 1990 to 2000) characterized by a homogenous set of intervention rules and to examine all cases for which data are available. Since an important evolution of the EC competition law came into effect on May 1, 2004, our results are specific to the nineties (and, as explained in Section 2.1, pertain mainly to the end of that decade).

A third reason stems from the European regulatory procedures. Until 2004, those procedures were highly standardized compared to other jurisdictions. For example, all proposed combining firms had to notify the Commission no later than one week after a deal agreement (public announcement of a takeover, an exchange offer, or acquisition of control). The European Commission then had one month to respond. At that point the combination was accepted outright, accepted subject to specific concessions, or postponed during an in-depth investigation

(which can take up to six months).[3] This bureaucratic notification system, as Vickers (2005) terms it, was dismantled in the 2004 reforms but, for us, it represents a research opportunity: each announcement date is well identified, which is critically important for an event study. Thus, our results are obtained in the context of a clear and homogenous legal framework.

Finally, an important feature of European activity is the significant proportion of combinations initiated by non-European bidders (almost 36% in our sample).

For all the above reasons, European regulation seems ideally suited for testing the existence of protectionism. Moreover, any protectionist attitude of the European Commission has prime importance for 350 million consumers living in one the largest and most affluent unified economic zones in the World. While infant industry arguments (Hagen, 1958), strategic behaviour by countries (Krugman, 1987) and specific oligopolistic situations (Krugman, 1999; Rosefielde, 2002) may each justify to some extent protectionist behaviour, attempts to shelter inefficient European firms from foreign competition would probably not only be detrimental to consumers in the long run but would also damage free trade and global prosperity. It would most certainly lead to retaliation from other major commercial zones.

We have collected data about all European Commission regulatory interventions during the period 1990-2000. While more than 1,500 proposed combinations formally notified the Commission of their intention to combine during this period, data availability limits our final sample to only 290 cases. Each of these includes a publicly listed bidder and a listed target for which quoted competitors can be identified (more on this in Section 2). Depending on the identification method, we track the price behaviour of 814 to 1,840 competitors. Of the 290 proposed combinations in our sample, 55 (19%) were challenged by the European Commission; i.e., approved only after concessions or forestalled during an in-depth investigation. This sample

includes in fact almost all large transactions realized during the nineties in Europe, for which concerns about market concentration might have been *a priori* raised. Therefore, if anything, it is biased against the protectionism hypothesis tested here. Our sample is however concentrated on the end of the nineties (27 out of the 30 in-depth investigations initiated by the European regulator occurred during the years 1998-2000). Our results are therefore mostly pertain to this time period.

The main result of our multivariate analysis is both clear: there is a suspicion of protectionism during the 1990s. We employ a probit model to analyze determinants of the probability of regulatory intervention. The joint effect of bidder nationality and European competitors abnormal returns is significant (see conditions (1) and (2) above): for mergers initiated by foreign bidders, the more negative the returns of European competitors around the initial merger announcement date, the higher is the probability of regulatory intervention. As previously mentioned, it is difficult to reconcile such a pattern with anything other than protectionism. Negative competitors' cumulative abnormal returns around the announcement date are in direct contradiction with a reinforcement of monopoly power in the concerned industries.

Our empirical findings are robust to three well known but difficult problems. The first problem concerns possible endogeneity between the probability of regulatory intervention and the observed returns of both the merging parties and the competitors. There is little doubt that investors try to anticipate regulatory actions while regulators simultaneously gauge market price movements in deciding whether to act. This endogeneity problem was pointed out by Eckbo *et al.* (1990) and further analyzed in Aktas *et al.* (2004). We follow here a two-step instrumental variable approach extended to competitors' observed abnormal returns. The Rivers and Vuong

(1988) test clearly confirms the presence of endogeneity. Taking into account endogeneity reinforces the statistical significance of our results.

The second difficulty is known the "weak instrument" problem (Greene, 2003; Wooldridge, 2001). It arises when the original variables are only weakly correlated with their instruments. As shown by Dufour (2003), this might result in poor small sample properties of the two-step estimator. To check for this effect, we amend the Anderson and Rubin (1949) procedure for a discrete dependent variable. Again, the results are robust.

The final potential problem involves generated regressors. Observed abnormal returns are derived from a first stage statistical procedure. Estimation errors at this first stage might have an impact on the validity of inferences drawn in a second stage. As asymptotic results would be difficult to obtain, we explore this issue using a bootstrap scheme (see Moreira *et al.* (2004) for an analysis of bootstrap validity using the Anderson and Rubin (1949) statistic). Our major findings, once again, are essentially immune to this problem.

Our results extend previous studies of European M&A regulation. Neven and Röller (2002) and Duso *et al.* (2003) suggest that a bidder's nationality might have some impact on the European regulatory decisions but they do not provide clear-cut empirical support. Our results also shed light on M&A regulation in general and contribute to answering the initial question posed above about the real reasons for its existence.

The paper is organized into sections as follows. The sample of proposed combinations and the methods used to identify competitors are described in Section 2. Section 3 reports some preliminary univariate evidence that competitors' returns depend on the nationality of the bidder and on the regulatory outcome. Section 4 is devoted to a multivariate investigation of

protectionism, while accommodating the endogeneity between observed returns and regulatory behaviour. Section 5 presents robustness checks and Section 6 concludes.

2. Data and Empirical Procedures

2.1. Data Sources and Sample Selection

From 1990 through 2000 inclusive, 1,573 proposed combinations notified the European Commission[4] of their intention to combine. Among these, 1275 (81%) were approved outright; 72 (4.6%) were approved subject to specific concessions (sales of assets or subsidiaries, etc.) after a one-month review process, and 95 (6%) were subjected to an in-depth investigation, of which 13 were prohibited and 47 were approved subject to concessions.[5] The remaining 131 (8.3%) cases were either withdrawn during the first month of review or resolved by other means (such as referral to the authorities of a single member state).

Our analysis requires market data for both the bidder and the target, which reduces the sample to 439 proposed combinations. Availability of the control variables used in our multivariate analysis, described at Section 4, further restricts the sample to 344 combinations. Finally, identification of European listed competitor firms, a key ingredient of our methodology, has been possible for only 290 combinations. All our multivariate findings are based on this final sample of 290. Some univariate results reported in Section 3 encompass a larger sample, data requirements being less restrictive in this instance.

Table 1 presents a breakdown of the sample. Panel A reports the type of European Commission regulatory decision and Panel B shows the nationality of the bidder. The number of proposed combinations is strongly increasing over time, with a noticeable acceleration after the 1997 broadening of regulatory criteria, which made more proposed combinations subject to European Commission review. Twenty-seven of the 30 in-depth investigations occurred in 1998-

2000, the latest three years in our sample. Our results are clearly specific to the second half of the decennia.

In our final sample of 290 proposed combinations, EC regulators approved 81% outright after a one-month review period; this is lower than the 90% outright approval rate among all 1,573 proposed combinations. Evidently, a somewhat larger fraction of outright approvals involve small companies that do not enter our sample because they are not listed on an exchange. In this sense and as emphasized in Section 1, our sample is, if anything, biased against the validation of our protectionism hypothesis. It is indeed for large deals that, *a priori*, the market power hypothesis should be the most probable motivation behind regulatory authorities' intervention. It is also worthwhile to note that:

- these 290 deals include almost all significant European deals that took place during the nineties. Many of the excluded operations involve small non listed target;
- the 55 cases subject to either "Approval after concessions" or "In depth investigation", beyond the direct regulatory power of the European Commission that they put into light, might have had also a real deterrence effect (as defined in Eckbo (1992)) on the European M&A market.

Our primary source of information is the European Commission Internet site, where final decision reports are freely available. These reports identify the firms involved in all proposed combinations. They also provide several control variables, as described in Section 4. Initial announcement dates have been cross-checked in several sources (the above-mentioned final decision reports, the financial press – *Les Echos*, *Financial Times*, *Wall Street Journal*, …, the SDC Database.[6] Market data (prices, dividends, exchange rates, market indexes, …) are from Datastream. The SDC Database also provides several control variables.

For foreign acquirers, we do not take into account the home country regulatory authorities' attitude for two reasons:

- in the logic of our protectionism hypothesis, foreign regulatory authorities are themselves not free of protectionism suspicions;
- the rules governing the regulatory authorities' intervention vary too much from country to country to allow a direct comparison.

2.2. Abnormal Return Estimation

Following Aktas *et al.* (2004), prices of firms listed in different countries are converted into U.S. Dollars. "Normal" returns are generated with the standard market model using a broad local market index in each country:

$$r_{i,t} = \alpha + \beta r_{m,t} + \varepsilon_{i,t}\,, \tag{1}$$

where $r_{i,t}$ is the return of firm i at time t and $r_{m,t}$ is the return of the market index at time t. Coefficients of the market model are estimated using 200 daily observations during a period that ends thirty days before the initial announcement of the proposed combination. The 30-day insulation period is designed to mitigate potential information leakage. The event window extends from five days before to five days after the announcement day. Abnormal returns are the difference between the observed returns during the event window and the return predicted by the market model:

$$AR_{i,t} = r_{i;t} - (\hat{\alpha} + \hat{\beta} r_{m,t})\,, \tag{2}$$

where $\hat{\alpha}$ and $\hat{\beta}$ are the estimated values of the market model parameters and $AR_{i,t}$ are the abnormal returns of firm i at time t. The cumulative abnormal returns are then simply obtained by summation on the event window:

$$CAR_i = \sum_{t=-5}^{5} AR_{i,t} \ , \tag{3}$$

where CAR_i stands for cumulative abnormal return. Aktas *et al.* (2004) document that these choices are robust to several variations (e.g., using the Scholes and Williams (1977) method or local currency returns, *inter alia*). The 11-day event window, used in many previous studies (see Malmendier and Tate (2005) for a recent reference) limits the potential calendar overlap between operations. This immunizes the results from the *non-random sample* bias that could potentially affect them (Kothari and Warner, *forthcoming*). M&As are indeed *non random*, as is any other voluntary corporate event. Firms undertaking these operations might therefore share common features, generating cross sectional correlation among contemporaneous returns. Choosing a short event window reduces drastically the risk of calendar overlap. As returns are known to be almost uncorrelated through time, the risk of cross sectional correlation is controlled for. It is also worthwhile to note that using a short event window and neutralizing 30 days before it allow us capturing the very specific informational effect (in the sense of investors' anticipations revision) of deal announcements. This also plays an important role in our methodology, limiting the risk that unrelated events will be taken into account when measuring value effects. Recent empirical contributions in the field of M&A follow the same path (Fuller *et al.*, 2003; Moeller *et al.*, 2004).

When studying M&A, inferences about cumulative (average) abnormal returns are exposed to several econometric problems. Abnormal returns are often both non-Gaussian and auto-correlated. Mergers cluster in time and generate event-induced volatility. Solutions to these problems have been studied extensively (see, e.g., Salinger, 1992). Our methods for handling these problems include:

- following Jaffe (1974) and Mandelker (1974), we analyze proposed combinations by forming value-weighted portfolios of the merging parties using as weights the merging parties' market values on the last day of the estimation window (thirty days prior to the initial announcement). The same weighting schemed is employed to construct a portfolio of competitor firm returns.
- as suggested by Boehmer *et al.* (1991) and by Ruback (1982), the estimated variance of abnormal returns is adjusted to take into account, respectively, event-induced variance and first order autocorrelation.
- all reported *p*-values are obtained from a percentile-t bootstrap procedure (Horowitz, 2001): we resample case by case the original sample with replacement. For each bootstrap sample, the student statistic of interest is estimated under the tested null hypothesis (focusing on studentized statistics is important to achieve bootstrap efficiency gains). The reported *p*-values are then the number of occurrences for which the bootstrapped statistics exceed the observed one.

2.3. Identification of Competitor Firms

To make sure that the results are not sensitive to the identification of competitor firms, we employ and compare three distinct approaches:

- *case-by-case* identification of the target firm's competitors using Hoover's Online Database, the European Commission Web Site, the Datastream table of listed firms by sector and nationality, and the financial press. This provides a small portfolio of direct competitors (six competitors on average per proposed combination) coming from 814 <u>different</u> competitor firms. While requiring a large amount of work, this procedure

allows us to identify, case by case, European competitors of foreign acquirers and is therefore an essential ingredient of our approach.

- *same industry, same country* identification, i.e., automatic selection of all listed competitors in the same industry as the target and the same country as the bidder. As the European Commission provides NACE industry classifications while Datastream uses SIC codes, we were obliged to construct a table of sector equivalents. This approach produces about nine competitors per proposed combination from 1,021 different firms.
- *same industry, same geographic zone,* automatic selection of firms from three large geographic zones: Americas, Europe and Asia. The average number of competitors per proposed combination is about 38, involving 1,840 different identified competitors.

Some comparisons with previous studies are interesting. Song and Walkling (2000) use the Value Line industrial classification. They identify 2,459 Value Line competitors, associated with 141 takeover targets, giving an average competitor portfolio size of 15. Fee and Thomas (2004), working with four-digit SIC codes, obtain an average competitor portfolio size of about 75. This suggests that our third procedure lies somewhere between the Song and Walking (2000) and the Fee and Thomas (2004) methods. Our first two procedures lead to far smaller competitor portfolio sizes. The best choice is not obvious. More firms reduce the competitor portfolio's variance, but perhaps at the cost of including firms that are only distant competitors.

To investigate whether the competitor portfolio is a material issue, Table 2 provides a comparison of competitor portfolio cumulative average abnormal returns (CAARs) around the announcement date. The three procedures display significant negative CAARs during the eleven day event window, -0.24% for *case-by-case* identification, -1.11% for *same industry, same country* and -0.68% for *same industry, same geographic zone*. Since all three procedures give

negative CAARs, we hereafter report results only with *case-by-case* identification of rivals, which is the least statistically significant in Table 2 and thus the most conservative. Moreover, as already mentioned, since we are interested in studying the potential protectionist dimension of the European Commission regulatory behaviour, focusing on European competitors of non European acquirers is essential. The second and third identification procedures, being based on countries or geographic zones, do not allow this specific identification.

3. Return Reactions to M&A Announcements

For each proposed combination, we assign the role of bidder to one firm and the role of target to a second firm. Most of the time, bidders and targets are specifically identified in the decision report of the European Commission. If this is not the case, we consult the financial press and make our best effort to ascertain each firm's role. In addition, for each proposed combination we construct a portfolio of competitor firms as described in Section 2.3. This section reports the stock price movements of bidders, targets, bidders plus targets, and competitors.

3.1. The Initial Announcement of a Proposed Combination

Table 3 gives initial announcement CAARs for bidders, targets, combinations (bidders plus targets weighted by their respective market values on the last day of the estimation window), and competitors. Our results generally confirm the pattern reported in past studies[7]; a large and statistically significant abnormal price increase for target firms. The target CAAR over the event window is 9.05%, which is somewhat lower than in previous studies for American combinations. For example, Mulherin and Boone (2000) find a target CAAR of 20.2% during 1990-1999 and Andrade *et al.* (2001) report 15.9% during 1990-1998. Our target CAAR is closer to those

reported by Campa and Hernando (2004) and by Goergen and Renneboog (2004), 3.92% and 12.96% respectively, over short event windows for European mergers.

Our bidding firms have significant negative returns from days –5 through –2. However, over the 11-day window, the bidder CAAR is 0.10% and is not significant. This result seems also to contrast slightly with previous finding in the literature. Mulherin and Boone (2000) and Andrade *et al.* (2001) document insignificant CAARs of -0.37% and -1%, respectively. For European combinations, Campa and Hernando (2004) and Goergen and Renneboog (2004) report bidder CAARs of 0.44% and 0.118% respectively, only the latter being significant.

For combined firms, (value-weighted bidder plus target), we find significant positive returns on the announcement date itself and on the previous (-1) and all following (0 to +5) days. Over the event window, the combined CAAR is 0.88% (as compared with 3.51% in Mulherin and Boone (2000) and 1.4% in Andrade *et al.* (2001)).

As already reported in Table 2, our competitor sample shows a negative impact of the proposed combination. This suggests that M&A announcements were bad news on average for our sample of rival firms, a result that contrasts with previous studies. In past literature, acquisition activity within an industry was found to have a positive impact on the stock price of rival firms. For example, Eckbo (1985) finds that horizontal competitors of target firms earn significantly positive abnormal returns of 0.58% over the seven-day period surrounding the M&A announcement. Eckbo and Wier (1985) report similar announcement period abnormal returns. More recent studies give the same result. For example, Song and Walkling (2000) report an 11-day abnormal return of 0.56% for rival firms. Fee and Thomas (2004) find similar results.

Table 3 presents also dollar value effects (in billion). The results show a large value destruction for bidders and for combinations. This is consistent with Moeller *et al.* (2005), who

study the same period in the US. As suggested by the CAAR, targets' shareholders capture more than 100% of the value creation. It is worth noting that:

- the effect at the combination level (which requires both the target and the bidder to be quoted) is not the sum of the effect at the bidder and target levels. This is likely due to the unavoidable exclusion of private firm acquisitions at the combination level. Such acquisitions are known to be more value creating for bidders (Fuller *et al.*, 2002);
- the value effect on competitors seems to be very large with respect to the reported CAAR but is spread over a portfolio of 6 firms on average (see above).

3.2. The Initial Announcement and Regulatory Outcome

Table 4 reports the initial announcement stock price impact classified by eventual regulatory outcome, for combinations and competitors. As shown by Aktas *et al.* (2004), market participants appear to consider eventual antitrust procedures at the time of the initial announcement. But it seems plausible also that regulators themselves are influenced by the initial price response to a proposed deal. For example, suppose on occasion there really is some monopoly rent to be gained from a merger. If the market correctly assesses this possibility, there should be a larger than average price rise of both bidder and target around the initial announcement and also a significant stock price increase for competitors, who would benefit by the reduction in competition. But if regulators are genuinely encouraging competition, they would react with a more vigorous investigation.

The results in Table 4 are consistent with this idea. Combinations that are eventually subjected to an in-depth investigation by regulators have a large abnormal price increase around the initial announcement date (1.66%) and the abnormal return of competitors over the same

period is a positive 0.06%, though insignificant. Both are possibly understatements of true rents because investors would recognize the threat of regulatory intervention and bid up prices less in the first place. This suggests that announcement date returns should be interpreted cautiously as indicators of market power because they are influenced by an endogenous relation between market participants' evaluations and regulators' decisions.

When regulators authorize the combination subject to concessions, Table 4 shows that the announcement price impact is negative for both the combining parties and for competitors, (but is significant for rivals only at a 10% level). Outright authorization, however, has a significant positive effect on the combination but a negative, though statistically insignificant, impact on rivals. One possible explanation is that no firm within the industry is strong enough to be a price leader and the market consequently anticipates increased competitiveness. Again, these results contrast with previous empirical studies in the U.S. context. Eckbo (1985) finds a significant CAAR of 0.48% for rivals of challenged US combinations while Fee and Thomas (2004) document a significant CAAR of 1.13%.

3.3. Initial Announcement Effects by Home Countries of Bidders and Competitors

Suspicions about European Commission motives has been frequently raised in the non- European press, but is there really empirical evidence that European regulators are biased against non-European firms? To answer this question definitively, we need a multivariate setting, which is provided in Section 4 below. First, however, we take a simple look at competitors' abnormal returns while controlling for their home country <u>and</u> the home country of the bidder.

Table 5 Panel A presents results for combinations that are approved outright. They affect European competitors negatively, which is consistent with increased competition (CAAR=-

0.53%, *p*-value =0.11). This negative effect is considerably larger in magnitude when the bidder is from the EC, though the difference is not statistically significant.

When the bidder is from the EC, outright approval is granted even though the impact on external competitors is positive, (CAAR=0.66%, *p*-value=0.16), which indicates either increased market power from the combination or a greater probability of further acquisitions, (Song and Walkling, 2000). The result is not strongly significant, however. For competitors outside the EC, the sign of the CAAR is reversed when the bidder's home country is also outside the European Community (CAAR=-1.19%, *p*-value=0.13). The difference is strongly significant. This suggests that European regulators tend to ignore the accumulation of market power <u>outside</u> the European Community when the bidder is from <u>inside</u> while they grant outright authorization readily to non-European bidders when competition is greater for non-European competitors.

Table 5, Panels B and C give results for proposed combinations challenged by the European regulator, either by imposing concessions (Panel B) or subjecting the parties to a thorough investigation (Panel C). The most striking results are the followings:

- the price impact is always negative for non-European competitors and is larger and marginally significant (*p*-value=0.08) when the bidder is also from outside the EC;
- when both the bidder and competitors are domiciled within the EC, the price impact is positive and significant (*p*-value=0.03) when the combination is subject to an in-depth investigation.

The univariate results above lack statistical power except in a few instances. In some cases, the sample sizes are rather low while in other cases power might be lost because of uncontrolled important determinants of the announcement date returns. In an effort to increase the power of

these tests and also to take other determinants into account, we now turn to a multivariate approach.

4. A Direct Test for Protectionism

Testing for protectionism must account for various determinants of European Commission regulatory intervention. Broadly speaking two general approaches could have been followed to tackle this issue:

- a difference in difference (DID) estimator: DID estimators are particularly suited to the analysis of treatment effects (Wooldridge, 2001; Bertrand *et al.*, 2004) and have been applied many times to analyze policy interventions. The DID estimator presents many advantages including control for unobserved variables, (using a control group to neutralize their effects). Defining the EC regulator intervention as the "treatment," such an approach could have been used in our specific case. It would, however, have been subject to two major weaknesses. (1) Using a DID estimator would have required the calculation of acquirers' performances before and after regulator interventions. Using long-term abnormal returns as performance indicator would have exposed us to a mis-specification biases affecting these methods (Fama, 1998). Using accounting-based economic performance indicators would have raised the problem of the compatibility of accounting standards for acquirers coming from many countries. (2) The expected impact of regulator intervention on the dependent variable (some performance measure) under the null hypothesis (no protectionism attitude) would have been quite ambiguous, the regulator's intervention being a possible signal of (or motivated by) high value creation.

- a direct study of the probability of intervention determinants: the alternative approach, a direct analysis of the probability of intervention determinants, allow us to get around these problems. Short term abnormal returns are known to be robust (see Section 2.2). Under the null hypothesis of no protectionism, the combined effect of acquirer nationality and European competitors' impact should have no impact on the probability of regulator intervention. This approach exposes us however to the problems of (1) missing variables and (2) endogeneity between the probability of intervention and the observed market reactions at the announcement. We try to tackle theses issues as carefully as possible in this section.

The type of European Commission intervention is qualitative by nature: outright authorization at the end of a month-long review, authorization subject to concessions at the end of a month, or an in-depth investigation. The second and third outcomes are burdensome to the combining parties, the first of these because the concessions often involve spin-offs of divisions or other actions that the firms would not have voluntarily elected and the second because of the delay and the implication that something about the proposed combination is objectionable to the regulators. Hence, we decided to distinguish outright authorization from the two other potential regulatory outcomes, which leads to a binary qualitative dependent variable model. Such a model has several advantages. It reduces to a minimum the number of parameters to be estimated, an important consideration given the limited data (290 proposed combinations). It also allows us to employ an extensive set of econometric methods that has been designed to deal with endogeneity problems within the framework of non-linear models (see Wooldridge (2001) for an extensive review).

Thus, our model has the following form:

$$\Pr(EC\ Intervention) = \Phi(\mathbf{X'\beta}) \tag{4}$$

wherein the dependent variable, *EC Intervention*, is 1.0 in case of authorization subject to concessions or an in-depth investigation and zero in the case of outright authorization, **X** is a vector of explanatory variables (including a constant), **β** is a vector of coefficients and Φ is the normal cumulative density function. As usual with a standard probit model, estimation is by maximum likelihood.[8]

Because the sample size is limited, all statistical tests are bootstrapped. We follow the percentile-t bootstrap procedure of Efron and Tibshirani (1993). See Horowitz (2001) for a verification of the bootstrap's advantages. The bootstrap procedure is described at Section 2.2.

In the analyses to follow, we employ a number of explanatory variables; they are described in Appendix. *Non-EC Bidder* and *Large EC Bidder Country* are dummy variables for assessing the impact of bidder nationality. *Target Size*, *Deal Value*, and *Bidder Size* are control variables for the potential impact of the proposed combination on the industry's concentration level. *Target to Bidder Size Ratio* gives an idea of the importance of the proposed combination from the bidder's point of view. *Bidder/Target Correlation* and *Competitors/Bidder Correlation* are proxies for pre-combination relatedness of, respectively, the bidder and target and the bidder and competitors. *Tender Offer*, *Cash Offer* and *Stock Offer* measure specific features of the proposed combination. *Bidder Past Performance* tracks bidder returns prior to the deal announcement. *Rumour* indicates whether the proposed combination has been anticipated and, finally, *Competitors' Relative Size* is a proxy for the market power of competitors relative to that of the bidder. We are not really interested in all of these variables *per se* but they are helpful in dealing with endogeneity between our dependent variable (the regulatory decision) and the observed market reaction at the initial announcement date.

4.1. Determinants of the Probability of European Commission Intervention

We propose a multivariate test of protectionism to answer the following question: Is the probability of European regulator intervention higher when a non European bidder announces an operation that hurts European competitors? This is the key test of our protectionism hypothesis. The test relies on the following explanatory variables:

- *Target Size* and *Deal Value*: both variables are proxies for the potential impact of the proposed combination on industry concentration. These are important variables to control for. Operations realized by foreign bidders might be systematically bigger, naturally triggering the attention of European regulatory authorities. Our results are in this sense *on top of* this potential determinant of the intervention probability.
- *Bidder Size*: one might very well anticipate that takeovers initiated by large bidders would more likely attract regulator attention.
- *Bidder/Target Correlation*: the more related the bidder and target, the higher should be the probability of intervention if regulators are striving to promote competitiveness;
- *Proposed combination CAR (cumulative abnormal return)*: the European regulator could use the market reaction on the announcement date as a gauge of wealth creation by the proposed combination (Eckbo *et al.*, 1990; Aktas *et al.*, 2004).

To test for protectionism, we include three other variables: *Non-EC Bidder* (the nationality of the bidder), *EC Competitors' CAR* (the impact of the proposed combination on European competitors, as perceived by investors) and the product of these two variables to capture their joint effect on the probability of intervention.

Table 6 presents the results. Not surprisingly, the coefficient of *Deal Value* is positive and highly significant: the probability of intervention is greater for larger combinations. *Non-EC*

Bidder and EC Competitors' CAR is negative and significant (*p*-value=0.08), which indicates that European regulators are more likely to intervene when the bidder is foreign and the proposed combination has a negative impact on European competitors. This answers our main question. However, European regulators may not be against competition in general because *EC Competitors' CAR* is positive and significant. In other words, when the bidder is from the European community, regulators would be more likely to intervene when they perceive an indication of increased market power.[9] As shown in the next section, this result (reflecting a curious duality of regulatory responses depending on whether the competition is coming from inside or outside the European Community) is not robust to the treatment of endogeneity.

Notice also that the variable *Non-EC Bidder* is statistically significant by itself; i.e., European regulators are about as likely to intervene when the bidder is foreign, *ceteris paribus*. European Commission scrutiny increases when foreign bidders are involved. This increase is reinforced if the operation harms European firms.

Target Size and *Bidder/Target Correlation* are both positive but are not significant at usual levels of confidence in Table 6. These variables, which are intended as proxies for possible market power, seem less powerful motivating factors for European regulators. Surprisingly, the coefficient of *Bidder Size* is negative and significant. Everything being equal, large bidders are less challenged by EC regulators. This might be attributable to either strategic behaviour or to more informed bidder decision making processes.

Though suggestive, the results presented in Table 6 might be influenced by econometric problems. The probit model is consistent if the explanatory variables are exogenous, but the observed CARs for the merging parties and for the competitors around the announcement date cannot reasonably be presumed exogenous. At the deal announcement, investors are anticipating

the potential value creation (or destruction) for the target, bidder and competitors. They know also that European Community regulation might come into play. At the same time, regulators are looking at market price reactions to assess potential monopoly rents or increased competition along with the benefits and/or harm that the proposed combination might generate for all affected parties. Clearly, the CAR and European regulator decisions are fundamentally endogenous. We explore this issue and its consequences for the results above in the next sub-section.

4.2.. Endogeneity between Regulatory Intervention and Announcement CARs

Dealing with endogeneity requires the formation of instrumental variables. We have opted for a standard two-step method (Greene, 2003; Wooldridge, 2001). The first step regresses potentially endogenous variables on a set of genuine exogenous variables. Then the fitted OLS values are used as instruments in the probit model.[10] The sets of exogenous variables are[11]:

- for the proposed combination CAR: *Non-EC Bidder, Large EC Country Bidder, Deal Value, Target Size, Bidder Size, Target to Bidder Size Ratio, Bidder/Target Correlation, Tender Offer, Cash Offer, Stock Offer, Rumour, Bidder Past Performance*;
- for competitors' CAR: *Non-EC Bidder, Large EC Country Bidder, Deal Value, Bidder/Target Correlation, Competitors Relative Size, Competitors/Bidder Correlation.*

These variables have been reported in numerous previous empirical studies as contributing to explain observed abnormal returns around M&A announcements. They are therefore natural candidate to form a valid instrument.

We first perform the Rivers and Vuong (1988) test for the existence of endogeneity between CARs and regulator actions. This test proceeds in two steps. First, OLS regressions are calculated with the observed CARs (one regression for proposed combination CARs and another for competitor CARs) as dependent variables and exogenous variables described above as

explanatory variables. Second, residuals from the first step regressions are included as explanatory variables in the probit. Under the null hypothesis of no endogeneity, the coefficients of the residuals should be insignificantly different from zero. The results, presented in Table 7, clearly reject the null of no endogeneity. This result has implications beyond our paper. It shows that observed abnormal returns as explanatory variables in causal models must be interpreted with great care if endogeneity has not been explicitly taken into account.

Given the presence of endogeneity, we re-estimate the model presented in Table 6, but this time using instrumental variables formed in the first step OLS regression described above in place of the observed combination and competitor CARs. The results are reported in Table 8. The main conclusions are:

- the previous result concerning the joint impact of bidder nationality and European Competitors' CAR is reinforced. The coefficient is negative and significant (*p*-value=0.04). Again, the more negative the impact of the proposed combination on European Community domiciled competitors, the higher is the probability of an European Commission regulatory intervention, provided that the bidder is <u>not</u> from the European Community.
- *Non-EC Bidder* looses its significance. This result is consistent with the result reported in our earlier paper (Aktas *et al.*, 2004): bidder nationality, by itself, is not sufficient to arouse EC regulator intervention. European Commission scrutiny increases only when foreign bidders harm European firms.
- *EC Competitors' CAR* is reversed. It is now negative and significant. This surprising result reveals the importance of dealing explicitly with endogeneity. Indeed, it invalidates the conclusion drawn in the previous subsection that European regulators

might be intent on fostering competition so long as it comes from within the EC. It now appears, to the contrary, that they are more likely to examine <u>any</u> proposed combination that appears to harm European Community firms (by increasing competition) whether or not the bidder is foreign or domestic.

- On the other hand, *Bidder/Target correlation* is now positive and significant. This seems contrary to the tentative deduction just above because it implies a higher likelihood of intervention when the two parties of the proposed combination are more related, *ceteris paribus*. This appears to be consistent with a notion that European regulators are more concerned when the merging parties are similar, holding constant the impact of the proposed combination on other firms.

5. Robustness Checks

5.1. Weak Instruments

When resolving endogeneity issues by using a two-step instrumental variable approach, the quality of the instruments is important. If the instruments are poorly correlated with the original variables (the "weak instruments" problem), asymptotic *p*-values might be seriously misleading (Wooldridge, 2001; Dufour, 2003). Could our results be affected by this condition? It should be emphasized that all our *p*-values are from the bootstrap (and are not asymptotic) and, as such, should be more robust to the weak instruments problem. Also, the joint impact of bidder nationality and European competitors' CAR has the same sign (and a comparable level of significance) in Table 6 (direct estimation) and in Table 7 (two-step instrumental variable estimation). Nonetheless, we would be remiss to bypass the weak instrument issue without some direct evidence.

Table 9 presents the first step OLS regressions used to form the instrumental variables, Panel A for proposed combination CARs and Panel B for competitor CARs. In Panel A, the regression R^2 is 9% and the Fisher test rejects the null hypothesis that all coefficients are zero. In Panel B, the results are not as good. The R^2 is low (around 2%) and the null hypothesis of all zero coefficients is not rejected. These estimations are in line with recent results published in the M&A fields (see, e.g., Moeller *et al.* (2004), Table 5). The source of these difficulties is clear; cumulative abnormal returns are very noisy, so just about any instrument will be "weak."

To study the consequences, we implement a modified version of the Anderson and Rubin (1949) procedure. Inferences drawn using bootstrap applied to this test procedure are shown to be valid in Moreira and Porter (2003) in the framework of linear structural models. The test is designed so that if the coefficients of all endogenous variables are truly zero, specification of the first-step instrumental variable regression is immaterial. Because the Anderson and Rubin test is not intended for a qualitative dependent variable, we have modified the procedure as follows:

- the first step OLS regressions remains unchanged;
- instead of a probit model in the second step, we use a linear probabilistic model. As a classification threshold, we select a value that minimizes the number of classification errors;
- *p*-values are estimated by a percentile-t bootstrap (with 2500 replications), which is necessary because asymptotic values might be invalid.

This approach amounts to an approximation of the second step qualitative dependent model by a linear one. It is accurate near the means of the explanatory variables and, in practice, is known to be quite robust (Wooldridge, 2001).

This test rejects the null hypothesis that all endogenous variables are jointly equal to zero (t-statistic=3.28, *p*-value=9.1%).

In conclusion, although the instruments are relatively weak, the adapted Anderson/Rubin indicates that they are still strong enough to provide significant results.

5.2. Generated Regressors

The final statistical trap we investigate involves "generated regressors." The CARs are statistical estimates, not error-free variables. Potentially, sampling noise could have an impact on reported *p*-value.

To investigate this issue, we modify the bootstrap procedure as follows:

- We assume that each proposed combination's CAR and each competitors' CAR is a random Gaussian variable with mean equal to the estimated CAR and variance obtained with the modified Boehmer *et al.* (1991) method.
- To keep matters simple, we also assume that the CAR random variables are independent of each other. This is not an overly strong assumption because CARs are computed from different periods, so their estimation errors should not be very closely related.
- We then generate 2,500 replications of the full two-step probit analysis presented in Table 8, replacing the observed CAR in each replication by a random draw from its distribution.

The resulting *p*-values are given in Table 10. They are almost unchanged from those presented in Table 8. Arguably, the results could conceivably change if CARs are strongly correlated but this seems unlikely given that the 290 proposed combinations in the sample are spread over many countries and years.

6. Conclusions

The impact of a proposed business combination can be measured by the price reactions of rival firms immediately around the initial announcement date of the combination. If the combination were believed by investors to create monopoly power in the industry, rival firms should display price increases around the announcement date. In fact, they display price decreases on average in our sample. This suggests that, on average, our proposed combinations enhance industry competitiveness.

European M&A regulators claim to be fostering competition and thereby protecting European consumers. But, using a sample of 290 proposed acquisitions screened by the European regulator during the nineties, we find that the more harm suffered by European rival firms when the acquirer is coming from outside the European Community, the greater the likelihood of European regulatory intervention against the proposed combination.[12] These results are robust to a variety of empirical problems including endogeneity between announcement date returns and regulatory intervention and they raise a clear suspicion of protectionist behaviour. It is hard to reconcile the actual pattern of EC regulatory intervention with consumer protection. It must however be emphasized that these results cannot be extrapolated to the current period, because significant changes of the M&A regulatory principles (concerning both the criteria and the procedures of intervention) were adopted by the European Commission in May 2004.

Faced with the empirical facts, a cynical observer might doubt the good intentions of European regulators. If, during the nineties, they were actually bent on protecting European firms from domestic competitive pressure and even more anxious to forestall competition from foreigners, they could not have behaved more appropriately. Perhaps 1990s regulators were engaged in strategic behaviour, in the spirit of Krugman (1987). Their actions protected

European firms. Did they harm European consumers? That depends on whether one believes that free-trade remains the best way to improve the *wealth of nations.*

To conclude, we would like to point out what could appear as an historical paradox. Quoting an anonymous referee, "In the mid-1980s, the EC changed its antitrust policy from one of emphasizing ex post effects of large corporate combinations, to the US tradition of prohibiting such combinations ex ante. The earlier ex post ("wait and see") policy allowed an empirical approach to examine whether a corporate combination in fact were going to be detrimental to things like product prices and quality. In contrast, US policy is traditionally based on the so-called "market concentration doctrine". This doctrine says that measures of industry concentration are empirically useful proxies for industry market power. As a result, antitrust authorities disallow mergers that are expected to generate a minimum increase in industry concentration. The problem, of course, is that the theory behind the ex ante approach is deeply flawed. As is well known, one can generate a positive relationship between industry concentration and profitability either via a competitive scale-economies argument, or via a monopoly argument. Thus, the paradox is that after having "exported" its own and possibly flawed antitrust policy to the EC, the very same policy now hits back at US bidders."

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Appendix

Variable	Description	Source
Non-EC Bidder	A dummy variable equal to 1.0 if the home country of the bidder is outside the European Community	European Commission Final Decision Report
Large EC Country Bidder	A dummy variable equal to 1.0 if the home country of the bidder is one of the large European Community countries (Germany, France, Spain, Italy or UK)	European Commission Final Decision Report
Target Size	The market value of the target evaluated at the end of the estimation period	Datastream database
Bidder/Target Correlation	The correlation coefficient of target and bidder returns during the estimation period (an indicator of sector and geographic proximity of the target and the bidder)	Datastream database
Deal Value	The deal value in millions of dollars	Securities Data Corporation database
Bidder Size	The market value of the bidder evaluated at the end of the estimation period	Datastream database
Target to Bidder Size Ratio	The target to bidder size ratio, each measured by the market value at the end of the estimation period	Datastream database
Tender Offer	A dummy variable equal to 1.0 if the combination is a public offering	Securities Data Corporation database, Financial Press
Cash Offer	A dummy variable equal to 1.0 if the combination is 100% cash paid	Securities Data Corporation database, Financial Press
Stock Offer	A dummy variable equal to 1.0 if the combination is 100% stock paid.	Securities Data Corporation database, Financial Press
Rumour	A dummy variable equal to 1.0 if there have been rumours in the financial press during the 6 months preceding the combination	Financial Press
Bidder Past Performance	The accumulated bidder performance during the estimation period	Datastream database
Competitors Relative Size	The ratio of the average competitors'/bidder size on the last day of the estimation window (an indicator of the relative market power of competitors and bidder)	Datastream database
Competitors/Bidder Correlation	The correlation coefficient between competitors' portfolio returns and bidder returns, evaluated during the estimation period (an indicator of the relatedness of competitor and bidder activities)	Datastream database

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[1] To avoid any ambiguity about the concept of protectionism, we define it as "any state policy action which is adopted for the purpose of improving the competitive position of local firms" (Paramithiotti, 2002).

[2] As in any empirical work, we can not definitively reject the occurrence of type 1 error.

[3] A detailed discussion of European M&A regulations can be found either in Aktas *et al.*. (2001) or in Aktas *et al.* (2004).

[4] See http://europa.eu.int/comm/competition/index_en.html.

[5] The remaining 35 cases were either approved without concessions or withdrawn before the end of the in-depth investigation.

[6] The Securities Data Corporation Database provided by Thompson Financial.

[7] See, for example, the review paper by Jensen and Ruback (1983), Mulherin and Boone (2000) or Andrade *et al.* (2001).

[8] A logit model gives virtually the same results as the probit model.

[9] We are assuming that EC regulators believe this positive effect arises from market power and not from an increased likelihood of EC competitors becoming targets in subsequent acquisitions.

[10] We also have conducted tests using Full Information Maximum Likelihood procedures. As the parameter space dimension is high (31 dimensions, using the simplifying assumption that residuals of proposed combination CAARs and competitor CAARs are independent), numerical convergence is difficult to achieve and depends on the starting values.

[11] The classical order and rank conditions necessary to assure identification of the equation system (Greene; 2003) are satisfied by such a specification.

[12] An alternative explanation for competitor firm price declines might be that proposed mergers harm competitors because they could be driven out of business by a larger, ravenous, and dominant predatory rival. We have,

however, controlled for bidder size so even under this explanation, European regulators seem to be protecting local firms, though their motivations would be more laudable.

Table 1
The Sample of Proposed Business Combinations

	1990	*1991*	*1992*	*1993*	*1994*	*1995*	*1996*	*1997*	*1998*	*1999*	*2000*	Total
						Panel A. Regulatory decision						
Outright approval	5	3	5	9	10	18	18	33	35	47	52	235
(%)	1.7	1.0	1.7	3.1	3.4	6.2	6.2	11.4	12.1	16.2	17.9	81.0
Approval after concessions	2	1	2	0	0	2	1	2	2	8	5	25
(%)	0.7	0.3	0.7	0.0	0.0	0.7	0.3	0.7	0.7	2.8	1.7	8.6
In depth investigation	0	0	0	0	0	1	0	2	8	9	10	30
(%)	0.0	0.0	0.0	0.0	0.0	0.3	0.0	0.7	2.8	3.1	3.4	10.3
						Panel B. Nationality of bidder						
EC Bidder	4	3	6	7	4	13	10	27	28	40	44	186
(%)	1.4	1.0	2.1	2.4	1.4	4.5	3.4	9.3	9.7	13.8	15.2	64.1
Foreign Bidder	3	1	1	2	6	8	9	10	17	24	23	104
(%)	1.0	0.3	0.3	0.7	2.1	2.8	3.1	3.4	5.9	8.3	7.9	35.9

The sample period is 1990–2000 inclusive. The 290 proposed business combinations are those with available market data for bidder and target, a complete set of control variables for the multivariate analysis, and an identifiable set of European competitor firms. Proposed combinations are reported by the year of notification to EU regulators, by the type of EU regulatory decision, and by the nationality of the bidder (European or foreign).

Table 2
Comparison of Methods for Identifying Competitor Firms

	Day relative to the announcement date										
	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5
	Case-by-case identification, N=511										
CAAR (%)	-0.004	-0.048	-0.21	-0.25	-0.07	-0.07	-0.06	-0.08	-0.23	-0.31	-0.24
Bootstrap *p*-value	0.94	0.35	0.03	0.03	0.39	0.26	0.27	0.3	0.07	0.03	0.08
	Same industry, same country, N=528										
CAAR (%)	-0.25	-0.23	-0.31	-0.31	-0.39	-0.40	-0.58	-0.76	-0.91	-0.99	-1.11
Bootstrap *p*-value	0.00	0.03	0.13	0.18	0.13	0.25	0.07	0.01	0.01	0.00	0.00
	Same industry, same geographic zone, N=628										
CAAR (%)	-0.17	-0.28	-0.32	-0.36	-0.33	-0.33	-0.36	-0.42	-0.53	-0.58	-0.68
Bootstrap *p*-value	0.02	0.00	0.00	0.02	0.02	0.03	0.03	0.00	0.00	0.00	0.00

CAARs (cumulative average abnormal returns) are presented from day minus 5 to day plus 5 relative to the initial announcement (day zero) of the proposed business combination. The three competitors' identification methods are *case-by-case* (for each proposed combination, quoted competitors is identified by hand from data sources and the financial press), *same industry, same country* (competitors are listed firms with the same nationality as the bidder and active in the same industry as the target) and *same industry, same geographic zone* (competitors are listed firms from the same geographic zone – Europe, America or Asia – as the bidder and active in the same industry as the target). Reported *p*-values are obtained using percentile-t bootstrap procedure; see Section 2.2. N denotes the number of competitor firms that are identifiable by the method indicated.

Table 3
Price Reaction to Initial Announcement

	Day relative to the announcement date										
	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5
						Bidders, N=579					
CAAR (%)	-0.25	-0.21	-0.30	-0.38	-0.02	0.19	0.27	0.24	0.23	0.09	0.10
Boostrap *p*-value	0.00	0.04	0.05	0.01	0.52	0.06	0.08	0.09	0.14	0.29	0.25
USD (Billion)	-37.5	4.3	-19.1	-62.8	-66.3	-91.9	-79.6	-94.9	-101.0	-132.5	-148.0
						Targets, N=482					
CAAR (%)	0.63	0.98	1.40	2.18	5.31	8.17	8.72	8.87	8.99	9.01	9.05
Boostrap *p*-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
USD (Billion)	27.4	31.9	67.8	84.9	133.6	197.0	192.7	168.2	185.9	193.2	174.0
						Combinations, N=439					
CAAR (%)	-0.04	0.04	0.00	0.01	0.58	1.09	1.05	1.10	1.08	0.91	0.88
Boostrap *p*-value	0.54	0.83	0.95	0.25	0.00	0.00	0.00	0.00	0.00	0.00	0.00
USD (Billion)	17.8	65.0	51.4	11.1	32.3	57.5	17.6	-29.2	-6.3	-31.7	-84.8
						Competitors, N=511					
CAAR (%)	-0.004	-0.048	-0.21	-0.25	-0.07	0.07	-0.06	-0.08	-0.23	-0.31	-0.24
Boostrap *p*-value	0.94	0.35	0.03	0.03	0.39	0.26	0.27	0.3	0.07	0.03	0.08
USD (Billion)	-57.4	-87.2	-178.5	-217.7	-113.0	-67.7	-45.6	-8.0	-133.2	-269.6	-241.9

This table presents CAARs and cumulated value effects around the initial announcement date (day 0) of proposed combinations for bidders, targets, combinations (bidders plus targets weighted by their respective market values on the last day of the estimation window), and competitors. The competitors are identified using the *case-by-case* method described in Section 2.3. Estimation is by the market model with local indexes converted into US dollars. Reported *p*-values are obtained from a percentile-t bootstrap based on the modified Boehmer *et al.* (1991) method as described in Section 2.2. N denotes the number of firms in each category.

Table 4
The Initial Announcement Effect and The Eventual Regulatory Outcome

	N	CAAR (%)	Bootstrap *p*-value
		Outright authorization	
Combinations	365	0.93	0.00
Competitors	422	-0.19	0.26
		Authorization after concessions	
Combinations	39	-0.27	0.86
Competitors	44	-1.03	0.10
		In-depth investigation	
Combinations	35	1.66	0.00
Competitors	45	0.06	0.57

Initial announcement CAARs for combinations and competitors over the 11-day event window are classified below by the ultimate outcome of regulatory intervention. Three regulatory outcomes are possible: (1) outright authorization from the European Commission (EC) at the end of a one-month review period, (2) authorization subject to concessions after the one-month review, and (3) an in-depth investigation. The CAAR is estimated using the market model with local indexes converted into US dollars; *p*-values are from a percentile-t bootstrap based on the modified Boehmer *et al.* (1991) method as described in Section 2.2. N denotes the number of combinations or competitors

Table 5
Initial Announcement Effects and Nationality

	N	CAAR (%)	Bootstrap *p*-value
Panel A. Outright authorization			
EC Competitors	384	-0.53	0.11
EC Competitors with EC Bidders	248	-0.72	0.12
EC Competitors with Non-EC Bidders	136	-0.19	0.33
Difference EC vs. Non-EC Bidders			0.52
Non-EC Competitors	272	-0.24	0.97
Non-EC Competitors with EC Bidders	139	0.66	0.16
Non-EC Competitors with Non-EC Bidders	133	-1.19	0.13
Difference EC vs. Non-EC Bidders			0.01
Panel B. Authorization after concessions			
EC Competitors	43	-0.11	0.71
EC Competitors with EC Bidders	32	0.51	0.54
EC Competitors with Non-EC Bidders	11	-1.92	0.26
Difference EC vs. Non-EC Bidders			0.27
Non-EC Competitors	38	-1.31	0.12
Non-EC Competitors with EC Bidders	28	-0.73	0.81
Non-EC Competitors with Non-EC Bidders	10	-2.92	0.08
Difference EC vs. Non-EC Bidders			0.06
Panel C. In-depth investigation			
EC Competitors	40	1.38	0.01
EC Competitors with EC Bidders	21	1.55	0.03
EC Competitors with Non-EC Bidders	19	1.20	0.14
Difference EC vs. Non-EC Bidders			0.41
Non-EC Competitors	37	-0.97	0.06
Non-EC Competitors with EC Bidders	17	-0.65	0.40
Non-EC Competitors with Non-EC Bidders	20	-1.24	0.08
Difference EC vs. Non-EC Bidders			0.30

Panel A presents CAARs for combinations authorized outright by the European Commission (EC) regulators. Panel B presents CAARs for combinations receiving authorization after concessions. Panel C presents CAARs for combinations subjected to an in-depth investigation. CAARs are estimated using the market model with local indexes converted into US dollars; *p*-values are from a percentile-t bootstrap based on the modified Boehmer *et al.* (1991) method as described in Section 2.2. N denotes the sample size.

Table 6
Determinants of The Probability of EC Regulatory Intervention
Standard Probit

Explanatory Variable	Estimated Coefficient	Bootstrap *p*-value
Proposed combination announcement CAR	0.64	0.43
Target Size	0.3 E-5	0.16
Bidder Size	-0.3 E-5	0.10
Bidder/Target Correlation	0.51	0.17
Deal Value	0.05 E-3	0.00
Non-EC Bidder	-0.13	0.31
Non-EC Bidder AND EC Competitors' CAR	-4.43	0.08
EC Competitors' CAR	3.34	0.02
LR Statistic	56.88	0.00
Pseudo R^2	20.19	

The dependent variable is equal to zero if European regulators approve a proposed business combination outright after one month. It is 1.0 if approval is given after concessions or if the EC conducts an in-depth analysis. The independent variables are fully described in Appendix. Estimation is by maximum likelihood. The LR Statistic provides a likelihood ratio test for the null hypothesis that all independent variables are jointly insignificant. *P*-values are obtained by a bootstrap percentile-t procedure, using 1,000 replications, as described in Section 2.2.

Table 7
Rivers and Vuong (1988) Endogeneity Test

Explanatory Variable	Estimated Coefficient	Bootstrap *p*-value
Target Size	0.1 E-5	0.77
Bidder Size	-0.6 E-5	0.01
Bidder/Target Correlation	2.48	0.00
Deal Value	0.05 E-3	0.00
Non-EC Bidder	0.41	0.03
EC Competitors' CAR	-29.39	0.01
Residuals from first-stage OLS, EC Competitors' CAR	31.24	0.01
Proposed combination CAR	-15.54	0.02
Residuals from first-stage OLS, Proposed combination CAR	16.30	0.02
LR Statistic	61.57	0.00
Pseudo R^2	*21..9*	

The Rivers and Vuong (1988) endogeneity test is applied to both the proposed combination CARs and the competitors' CARs. Instruments are formed for both variables in a first-stage OLS estimation, as described in Section 4. The dependent variable is equal to zero if European regulators approve a proposed business combination outright after one month. It is 1.0 if approval is given after concessions or if the EC conducts an in-depth analysis. The independent variables are fully described in Appendix. Estimation is by maximum likelihood. The LR Statistic provides a likelihood ratio test for the null hypothesis that all independent variables are jointly insignificant. *P*-values are obtained from a bootstrap percentile-t procedure, using 2,500 replications, as described in Section 2.2. Endogeneity is indicated by significant coefficients for the first-stage residuals.

Table 8
Determinants of The Probability of EC Regulatory Intervention
Two-Stage Instrumental Variable Probit

Explanatory Variable	Estimated Coefficient	Bootstrap *p*-value
Proposed combination CAR Instrument	-13.85	0.02
Target Size	0.2 E-5	0.46
Bidder Size	-0.6 E-5	0.00
Bidder/Target Correlation	2.56	0.00
Deal Value	0.05 E-3	0.00
Non-EC Bidder	0.32	0.14
Non-EC Bidder AND EC Competitors' CAR Instrument	-53.56	0.04
EC Competitors' CAR Instrument	-18.67	0.10
LR Statistic	62.04	0.00
Pseudo R^2	0.22	

The dependent variable is equal to zero if European regulators approve a proposed business combination outright after one month. It is 1.0 if approval is given after concessions or if the EC conducts an in-depth analysis. The independent variables are fully described in Appendix. Estimation is by maximum likelihood. The LR Statistic provides a likelihood ratio test for the null hypothesis that all independent variables are jointly insignificant. *P*-values are obtained from a bootstrap percentile-t procedure, using 2,500 replications, as described in Section 2.2. Proposed combination CAR and competitors' CAR instruments are fitted values from a first-stage OLS estimation, as explained in Section 4.

Table 9
First-Stage OLS Instrumental Variable Formation

Explanatory Variable	Estimated Coefficient	Asymptotic *p*-value
Panel A. Dependent variable is proposed combination CAR		
Non-EC Bidder	0.01	0.33
Large EC Country Bidder	0.00	0.77
Deal Value	0.00	0.31
Target Size	0.00	0.31
Bidder Size	0.00	0.10
Target to Bidder Size Ratio	-0.35 E-4	0.65
Bidder/Target Correlation	0.08	0.00
Tender Offer	0.01	0.40
Cash Offer	0.01	0.76
Stock Offer	0.30 E-4	0.99
Rumour	-0.14 E-2	0.01
Bidder Past Performance	0.86 E-2	0.54
Fisher Statistic	2.27	0.00
Adjusted R^2	0.09	
Panel B. Dependent variable is EC competitors' CAR		
Non-EC Bidder	0.02	0.18
Large EC Country Bidder	-0.16 E-3	0.98
Deal Value	0.00	0.79
Bidder/Target Correlation	0.80 E-2	0.75
Competitors Relative Size	0.82 E-4	0.57
Competitors/Bidder Correlation	0.03	0.18
Fisher Statistic	0.83	0.54
Adjusted R^2	0.02	

The instruments used in Table 8 are obtained from the OLS regressions reported below. The dependent variable is the CAR during an 11-day window around the initial announcement of a business combination. Independent variables are described in Appendix. The Fisher statistic provides a test that all independent variables are jointly insignificant

Table 10
Determinants of The Probability of EC Regulatory Intervention
Two-Stage Instrumental Variables Probit with Generated Regressor Adjusted p-values

Explanatory Variable	Estimated Coefficient	Bootstrap *p*-value
Proposed combination CAR Instrument	-13.85	0.03
Target Size	0.2 E-5	0.49
Bidder Size	-0.6 E-5	0.00
Bidder/Target Correlation	2.56	0.00
Deal Value	0.05 E-3	0.00
Non-EC Bidder	0.32	0.16
Non-EC Bidder AND EC Competitors' CAR Instrument	-53.56	0.03
EC Competitors' CAR Instrument	-18.67	0.10
LR Statistic	62.04	0.00
Pseudo R^2	0.22	

This table repeats the probit reported in Table 8 but accounts for the fact that some explanatory variables, the CARs, are statistically generated estimates. The dependent variable is equal to zero if European regulators approve a proposed business combination outright after one month. It is 1.0 if approval is given after concessions or if the EC conducts an in-depth analysis. The independent variables are fully described in Appendix. Estimation is by maximum likelihood. The LR Statistic provides a likelihood ratio test for the null hypothesis that all independent variables are jointly insignificant. *P*-values are obtained from a bootstrap percentile-t procedure, using 2,500 replications adapted for possible estimation error in proposed combination and competitors' CARs (see Section 5). Proposed combination CAR and competitors' CAR instruments are fitted values from a first-stage OLS estimation, as explained in Section 4.