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EARLY SETTLEMENT IN EUROPEAN MERGER CONTROL*

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We analyse the determinants of early settlement between merging parties and the European Commission over remedies that remove concerns of anticompetitive effects. This extends the previously narrow range of econometric literature on early settlement. Consistent with the theory of early settlement, our results confirm the importance of delay costs and of uncertainty, measured by the complexity of the economic analysis required for each merger. We also find a non-monotonic effect of agency resourcing, which raises questions about the Commission's efficiency in times of high case load. Econometrically, we select a sample of merger decisions in which the European Commission intervened due to concerns of anticompetitive effects, and our selection model provides estimates of the factors determining intervention by the Commission. Conclusions are drawn for public policy.

I. INTRODUCTION

THE PURPOSE OF MERGER CONTROL IS TO PREVENT FIRMS from acquiring assets that would otherwise impede competition, while allowing the efficient reallocation of ownership of other assets. Impediments to competition can be resolved by prohibiting the implementation of anticompetitive mergers. However, few mergers are completely blocked in this way, because typically only a subset of the affected markets raise concerns of anticompetitive effects. Instead, the merging parties and the competition agency responsible

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for merger control can agree to 'remedies' that modify the merger in order to prevent any loss of competition post-merger. For example, such remedies may require the merging parties to divest assets that would reduce competition if merged under common ownership. An efficient policy achieves this with minimum feasible regulatory costs. This paper is concerned with understanding a significant component of these regulatory costs.

Like most competition agencies, the European Commission (EC) adopts a two-phase approach to merger control. During a relatively short Phase I investigation, it establishes whether or not there is a *prima facie* case that a merger is likely to impede competition. If there is no such case, the merger is cleared without any intervention ('non-intervention'). If the merger raises anti-competitive concern, the EC will intervene and a settlement short of prohibition may be agreed. The formal merger regulation (ECMR) allows the merging parties to propose a final remedy offer. If the EC agrees that the remedy would eliminate the anticompetitive effects, the modified merger is cleared to proceed without further delay ('early settlement'). Otherwise, the merger is referred for a longer in-depth Phase II investigation ('referral'). In this second phase, the EC develops a greater understanding of the competitive effects, which may change its opinion of whether or precisely how the merger impedes competition. The merging parties also have the right to propose new remedies, which may involve a greater or lesser modification to the merger than was proposed in Phase I. If the EC finds that there is no impediment to competition or that the remedies proposed are sufficient to eliminate the anticompetitive effects, the merger is cleared subject to any agreed remedies. Otherwise, the entire merger is prohibited.

The speed and accuracy of a competition agency's decision are major components of regulatory transaction costs. While there is an increasing amount of empirical research that investigates the accuracy of remedy settlements,[1] there is an absence of research into the speed of settlement. We begin to fill this gap by analysing the determinants of early settlement conditional on intervention by the EC for a sample of European merger decisions. Econometrically, we apply a bivariate probit with sample selection. A selection probit models the determinants of intervention by the EC (i.e., non-intervention versus early settlement and referral). This selects the mergers where remedy negotiation would have occurred. The outcome probit models the determinants of early settlement for this selected sample (i.e., early settlement versus referral).[2]

Our analysis extends the previously narrow range of econometric literature on early settlement. Consistent with insights from the theory of bargaining

[1] Examples of various methodologies include: Duso *et al*. [2007] for an event study; Ivaldi and Verboven [2005] for a merger simulation; Ashenfelter and Hosken [2010] for a difference-in-difference approach. See Davies and Ormosi [2012] for a review.

[2] This distinction is not made in the econometric literature on merger decisions which typically pools non-interventions and early settlements. See our review in section 6.

and related literatures (e.g., Bebchuk [1984]; Baker and Mezzetti [2001]), we show that the probability of early settlement is decreasing in *ex ante* uncertainty as to the agency's findings, and increasing in the cost of delay to the merging parties. Of wider interest is that our findings on the importance of uncertainty, which is fundamental to the theoretical literature, are stronger than has previously been found in econometric studies of other settings, such as settlement in tort (e.g., see Fournier and Zuehlke [1996]; Kessler [1996]; and Fenn and Rickman [1999]). Moreover, in contrast to these empirical studies of private action, we are able to investigate how the resourcing available to the agency affects the probability of early settlement. We find evidence of a non-monotonic relationship that includes a higher probability of early settlement when resources become very stretched. This suggests there may be administrative pressure to settle early when resources are tight.

The lack of previous research into the timing of remedy settlements is also an important gap in the competition policy literature for two reasons. First, the speed of settlement is a major component of the transaction costs associated with merger control, and an efficient policy design requires an understanding of what affects the balance between the costs and benefits of an extended investigation. The main benefit of delay is that merger decisions made in Phase II are likely to be more accurate than those made in Phase I, due to the additional time and resources available for in-depth analysis of the competitive effects. Thus, Phase II remedy settlements are less likely to be either excessive in that they reduce potential efficiencies ('Type 1 errors') or insufficient in that they do not eliminate all anticompetitive effects ('Type 2 errors'). On the other hand, the costs of not reaching a settlement in Phase I can be considerable. The merging parties incur direct costs in the form of diverted management time and fees to external advisers, and they incur opportunity costs due to the postponement of investment, possible efficiency gains and product development.[3] These delays may result in lower consumer surplus for the duration of the second phase, and there may also be longer term welfare effects if uncertainty over the regulatory outcome leads to key personnel moving to other jobs, thus eroding core competences in the firms. Furthermore, the agency has to allocate more resources to an investigation if the merger enters Phase II, and the associated opportunity cost may be particularly high if this diverts resources from proactive cartel and other antitrust work (including Phase I merger investigations).

The second reason why understanding early settlement is important is that it may indirectly reveal information about the incentives that determine the size of the merging parties' remedy offer. This casts some much

[3] A study of 50 companies involved in over 500 mergers worldwide, conducted by PricewaterhouseCoopers [2003], found: 'With internal costs added in (management time and staff time), deals involving in-depth reviews are eight to ten times more expensive that those subject only to initial review (an average €6m-plus per deal)' (p.42).

needed light on when a remedy settlement is more likely to be a Type 1 or a Type 2 error, which, unlike early settlement, is difficult and costly to observe directly. For example, suppose the merging parties anticipate that a competition agency is less willing to refer a merger to Phase II when the agency's resources are considerably stretched (as our empirical results imply). In such a case, they have a strategic incentive to propose a smaller modification to the merger than they would have otherwise, because there is a greater chance that the agency will clear the merger in Phase I. As a result, we may infer that a merger cleared in Phase I when the agency's resources are stretched may be subject to a less extensive remedy than if the agency had greater resources available (i.e., the remedy settlement may either be more of a Type 2 error or less of a Type 1 error). Thus, our results may provide an indicator of which remedy settlements in Phase I are more likely to involve Type 2 errors. Moreover, if this link is confirmed by targeted evaluations, our findings would also indicate the institutional improvements necessary to address them (e.g., greater resourcing for the agency during merger booms).

The rest of the paper is organised as follows. In Section II, we review the theory of early settlement in different settings. We develop how the core insights of these settings apply in the context of merger control to enable us to specify and interpret our empirical model. Section III presents our econometric methodology. Section IV discusses the data and our measurement of the independent variables, and Section V presents the results. Section VI discusses our findings in relation to the wider empirical literatures on tort settlement and merger control. Section VII concludes.

II. THEORY OF EARLY SETTLEMENT

In this section, we begin by reviewing three related theoretical literatures that model the causes of early settlement versus delay in different settings. We then consider how they relate to the particular setting of European merger control. The first two literatures share an interest in investigating the joint determination of the size of offer and the probability of the offer's being accepted. While some detailed results are sensitive to the specifics of the individual model (e.g., who makes the last offer or who has imperfect information), other findings are not unduly sensitive to precise assumptions or institutional setting and we focus on these more robust predictions below. The essential ingredients of these models are costs of delay (which provide an incentive for early settlement) and uncertainty over the other party's reservation price (which makes early settlement less likely). In our merger setting, we also expect that agency resourcing will be important, but this has received less theoretical analysis probably because the literature mostly considers private actions. The third literature, on plea bargaining, provides some guidance and we develop this intuitively in Section II(ii).

II(i). *Related Theoretical Literatures*

II(i)(a). *Bargaining under Uncertainty.* The theory of bargaining under uncertainty investigates the price agreed between a single buyer and seller. Offers by one party are either accepted or rejected by the other. In such models the agreed price does not affect the size of the 'cake' that is being shared, but the cake shrinks if agreement is delayed. Agreement may not be immediate due to uncertainty as to the other side's preferences. For example, building on Fudenberg and Tirole [1983] and Sobel and Takahashi [1983], Cramton [1984] develops an infinite horizon bargaining model in which each party is uncertain as to the true 'type' of the other. Each offer reveals information about the offerer's type and this signal is rationally interpreted by the other party. He finds that delay in reaching agreement is increasing in uncertainty and decreasing in delay costs.

II(i)(b). *Out-of-Court Settlement.* The theory of out-of-court settlement, typically a claim for damages over which liability is disputed, introduces the possibility of a costly trial if agreement is delayed too long.[4] The truth (e.g., liability, magnitude of harm) is revealed in court and there is a chance that the defendant may not have to concede anything (e.g., she may not be found liable in a tort claim). Bebchuk [1984] was the first to study optimal settlement offers in this context. Both parties know what the court would award if it found against the defendant, but there is uncertainty as to whether the plaintiff would win. The defendant has private information as to her liability and the plaintiff can make a take-it-or-leave-it pre-trial offer (demand). If the plaintiff demands more compensation, this obviously increases her utility conditional on the offer's being accepted, but it reduces the probability of it's being accepted by the defendant. The optimal offer balances these two effects. Bebchuk shows that the likelihood of settlement out of court (i.e., early settlement) is increasing in litigation costs and decreasing in uncertainty, as measured by the spread of types. He also finds that a spread-preserving increase in the amount that would be awarded to the plaintiff in court does not affect the probability of settlement out of court—it simply raises the plaintiff's demand commensurately.[5] Friedman and Wittman [2006] develop the Bebchuk model to include two-sided uncertainty and they use a different bargaining protocol. Consistent with Bebchuk, they still find that the probability of settlement out of court is increasing in litigation (delay) costs and decreasing in uncertainty.

[4] Dougherty and Reinganum [2005] and Spier [2007] provide comprehensive reviews of this literature.

[5] Spier [1992] provides a dynamic extension to Bebchuk [1984]. Fixed costs of preparing for litigation favour immediate agreement, but the advantage of being able to make the final take-it-or-leave-it offer favours brinkmanship in the form of agreements 'on the courtroom steps'. Taken together, she finds that settlement is most likely either very early or at the last minute, but unlikely at intermediate times.

In a different approach to Bebchuk [1984], Priest and Klein [1984] suppose that failure to settle is driven by divergent expectations between the parties over going to court.[6] This approach is developed by Perloff *et al.* [1996] to analyse private antitrust settlement (as distinct from public agency settlement) in the presence of a common belief about the probability of winning at trial. They show that settlement out of court is more likely, for any given risk aversion and costs of going to court, the lower is the plaintiff's expected gain from the court relative to the defendant's expected loss. Settlement is also more likely for higher trial costs and for greater uncertainty about the trial outcome (i.e., closer to 50:50 chance of winning at trial). Note that this is uncertainty about the outcome of the trial (the second phase), so it is not the same as and has the opposite effect to uncertainty over pre-trial negotiations (the first phase) on settlement.

II(i)(c). *Plea Bargaining.* In the theory of plea bargains in criminal cases, a prosecutor may offer a reduced penalty in return for a guilty plea from the defendant prior to a costly trial. The introduction of an independent prosecutor/agency with a separate utility function (e.g., preferences for a correct verdict and not losing in court) breaks away from the zero-sum payoff structure of the literatures discussed above. Grossman and Katz [1983] and Reinganum [1988] investigate how the prosecutor can screen between the innocent and the guilty by offering a reduced sentence in return for a guilty plea. A feature of these models is that the innocent always elect to go to trial and some of the guilty settle. Baker and Mezzetti [2001] introduce prosecutorial resources which make it more likely that the innocent are identified before going to court. They find a semi-separating equilibrium in which, for a sufficiently severe crime, some of the guilty but none of the innocent accept the prosecutor's offer, and the remainder of the guilty plus all of the innocent elect to go to trial. The proportion of the guilty accepting the offer (i.e., settling early) is increasing in prosecutorial resources, but unrelated to the defendant's court costs.

II(ii). *Application to European Merger Control*

Our empirical context of European merger control shares some of the key features of these literatures.[7] The European Community Merger Regulation

[6] Many of the predictions on the settlement rate are similar to Bebchuk's [1984] asymmetric information model, but there are some key differences in relation to the proportion of the 'guilty' going to trial. Waldfogel [1998] compares the predictions of the asymmetric information approach with Priest and Klein's [1984] divergent expectations model. His main focus is on differences between these two models in their predictions of the probability of success conditional on going to trial. His empirical work focuses on non-antitrust cases including tort and civil rights.

[7] Bourjade, Rey and Seabright [2009] provide a rare theoretical application of settlement theory to a non-merger area of competition policy. Kühn [2010] provides a brief review of issues in merger policy design, particularly with respect to merger remedies.

(ECMR) has a statutory timetable giving two deadlines for remedy offers, one at the end of each phase,[8] and the merging parties face a tradeoff between costs of delay and trying to keep a larger share of the assets. This set up is similar to the two phase process modelled by the out-of-court settlement and plea bargaining literatures, where there is an expectation that the costly second phase would reveal some of the information that is initially hidden. In fact, the court in these models can be thought of as a costly second phase investigation. Furthermore, one of the parties in the plea bargaining models is a prosecutor with a utility function who has features similar to a competition agency, but these models focus on decision errors ('do the innocent or guilty settle early?') which are not easily observed, so their predictions are hard to test. The out-of-court settlement literature, on the other hand, has a particular focus on the timing of agreement, which is also our empirical interest. Given the robustness of the theoretical predictions to different settings, these similarities indicate that the same ingredients that are important in the above models are also likely to be important in our context.

Nevertheless, some other features of merger control are not incorporated in the above models. In particular, out-of-court settlement and plea bargaining models are built around a past harm that is not expected to continue, and the settlement is zero sum at the time of agreement—damages payments are a simple transfer of wealth. In contrast, merger appraisal anticipates a potential harm (and possible efficiencies), so the optimal merger settlement generates a larger social 'cake' than one with either excessive or insufficient remedies. Although an exact comparison with out-of-court settlement models would arise only if the merger 'remedy' were a fine that did not alter the anticompetitive harm, we have no reason to expect this to change the central positive predictions from simpler models.

Another difference between our merger context and the theoretical literature relates to the agency resource constraints. This constraint is explicitly modelled by Baker and Mezzetti [2001] but we expect that their model tells only part of the story for merger control. The reason is that in their model the prosecutor's resources are assumed to be separate from those of the court. However, this is not true for an agency whose resources must be spread across both phases of multiple investigations, as is the case in our merger context. This introduces a new effect that is likely to kick in when resourcing is particularly tight. For example, suppose the resources available to a competition agency to analyse all mergers are fixed and consider

[8] Until 2004, Phase I had to be completed within one month, extended to six weeks if remedies had been offered. Phase II had to be completed within four months (with or without a remedy offer). The revised merger regulation of 20 January, 2004, extended the Phase I period to 25 working days or 35 if remedies are offered. The regulation requires Phase II to be completed in 90 working days or 105 if remedies are offered, though it is possible to 'stop the clock' under some circumstances.

the effect of increasing the case load of the agency starting from a low level. A higher case load leads to a thinner spread of resources, so a Phase I investigation is less likely to come to a confident assessment (i.e., there is greater uncertainty) which decreases the likelihood of early settlement. However, referral imposes a substantial burden on the agency's resources which compounds the problem, so there may be administrative or political pressure on the agency to settle early when resources are very tight.

Strategic firms may also respond to limited agency resourcing. If the case load is perceived to be initially low and firms observe a tightening of resourcing, they may make a more generous offer of remedies in Phase I in order to achieve the same risk of referral (even though this incurs a Type 1 error if accepted). However, if the case load is high, then firms may respond to a further tightening of the resource constraint by offering a *smaller* modification to the merger, while retaining the same risk of referral to Phase II as they would otherwise have had.[9] This would suggest that early settlements in periods of very high case load may be more likely to be Type 2 errors (i.e., anticompetitive mergers) or less likely to be Type 1 errors, particularly for mergers that are complex for the agency to analyse.

Finally, we note two features that these strategic models do not find important although a naive intuition may suggest they could matter. These are the scale of the merger and the total anticompetitive harm that would result from an unremedied merger. If the scale of a merger does not add to uncertainty, it should not affect the timing of settlement. Only to the extent that scale adds to complexity, for example by adding more potentially harmful markets to consider, should it reduce the likelihood of early settlement. Similarly, delay need not be the consequence of a merger which would involve the creation of monopoly in some markets, because this could immediately be seen as anticompetitive and so be addressed by an early divestiture offer.

We explain how we operationalise these theoretical insights after setting out our econometric methodology.

III. ECONOMETRIC METHODOLOGY

As discussed in the introduction, a European merger decision can fall into three groups: non-intervention (where the merger can proceed without any remedies in Phase I); early settlement (where the merger is cleared in Phase I subject to implementation of remedies); or referral (where the merger is referred to Phase II). We use 'intervention decisions' to refer to both early settlements and referrals, because on the evidence available in Phase I, the EC decided that it should intervene in these mergers due to potential

[9] Firms may alternatively trade-off some risk of referral against size of remedy offer.

anticompetitive effects. The remaining Phase I clearances, which do not require remedies, are 'non-intervention decisions.'

Consider the set of intervention decisions. We observe $y_i = 1$ if merger i was an early settlement (i.e., if the potential anticompetitive effects were settled quickly) and $y_i = 0$ if it was a referral (i.e., if the potential anticompetitive effects were not settled quickly). However, y_i is observed only if the EC intervenes in the merger because its Phase I investigation finds evidence of potential anticompetitive effects ($z_i = 1$). We do not observe y_i for non-interventions decisions ($z_i = 0$).

We specify the latent intervention (or selection) equation:

$$(1) \qquad z_i^* = w_i\gamma + \varepsilon_i, \quad \text{where } z_i = \begin{cases} 1 & \text{if } z_i^* > 0 \\ 0 & \text{otherwise.} \end{cases}$$

z_i^* is the underlying propensity for the merger to require an intervention, w_i is a vector of independent variables that determine intervention and ε_i is an error term. The latent early settlement (or outcome) equation is:

$$(2) \qquad y_i^* = x_i\beta + u_i, \quad \text{where } y_i = \begin{cases} 1 & \text{if } y_i^* > 0 \text{ and } z_i^* > 0 \\ 0 & \text{if } y_i^* \leq 0 \text{ and } z_i^* > 0 \\ - & \text{otherwise.} \end{cases}$$

y_i^* is the underlying propensity for the merger to be settled in Phase I, x_i is a vector of independent variables that determine early settlement (e.g., delay costs and uncertainty) and u_i is an error term. The variables in x and w may overlap. We assume both error terms are standard normal.

There is a possibility of selection bias when estimating (2). Some mergers that appear straightforward according to measured characteristics w may have unmeasured complexity captured by a high positive error ε. This introduces a disproportionate sample of mergers with high unmeasured complexity into the selected intervention sample for which y_i is observable. If ε and u are correlated, this would introduce a correlation between unmeasured complexity and x in the selected sample, even if they are uncorrelated in the population. The result would be inconsistent estimates and a possible underestimation of β.[10] However, it remains possible that there would be no selection bias if the unmeasured factors in each equation are not in fact correlated, or if every variable affecting intervention is controlled for in the settlement equation.

[10] For example, in terms of one of the variables we use to measure complexity (defined in section 4.2.2), some mergers with a low value of *#unsafe* but high unmeasured complexity would be in our sample (but none with low value of *#unsafe* and low unmeasured complexity). This dilutes the true effect of *#unsafe*.

In order to correct for possible selection bias, we jointly estimate (1) and (2) by maximum likelihood. Given bivariate normal errors with covariance ρ, the log-likelihood function is:

$$\ln L = \sum \left\{ \begin{array}{c} y_i z_i * \ln \Phi_2(x_i\beta,\ w_i\gamma;\ \rho) \\ +[1-y_i]z_i * \ln[\Phi(w_i\gamma) - \Phi_2(x_i\beta,\ w_i\gamma;\ \rho)] \\ +[1-z_i] * \ln \Phi(-w_i\gamma) \end{array} \right\}$$

where the three terms are indicators of the three types of merger decision (early settlement, referral, and non-intervention) multiplied by their respective probabilities.[11] $\Phi(.)$ is the cumulative normal distribution and $\Phi_2(.)$ is the bivariate cumulative normal distribution. In order for the model to be clearly identified, it is desirable to have at least one variable in w that is excluded from x.

IV. DATA AND MEASUREMENT

Our dataset is constructed from merger decisions of the EC which were notified between 1999 and 2006 inclusive. Although explicit merger regulation was first introduced at the EU level in 1989, we do not collect data prior to 1999 for two reasons. First, there was uncertainty as to the legality of remedy settlements in Phase I until a clarification in the regulations in June, 1997.[12] Second, the information available in decisions is less systematic prior to 1999.

IV(i). *Sample by Type and Timing of Merger Decision*

There were 2,348 mergers notified during our period. We exclude 48 mergers that were fully or partially referred to a national competition authority. Another 61 mergers were withdrawn by the merging parties, so the EC was not required to make a decision.[13] Thus, the EC made 2,239 merger decisions in our period. The vast majority of these are non-intervention decisions (92%). Of the 184 intervention decisions, 59% were early settlements and the remainder were referrals. The referrals can be further broken down

[11] This follows van de Ven and van Praag [1981].

[12] See revision to Art.6(1) in Council Regulation (EC) No 1310/97 of 30 June 1997 amending Regulation (EEC) No 4064/89 on the control of concentrations between undertakings (OJ L 180, 9.7.1997, p.1–6).

[13] 41 were withdrawals during Phase I which are likely to have been for reasons other than merger regulation (e.g., shareholder disagreement). The remainder were withdrawn in Phase II. Unfortunately, no information is available on withdrawals, as no report is published. While the withdrawals in Phase I can reasonably be expected to be random in relation to competition issues, some of the withdrawals in Phase II may have been in anticipation of a prohibition decision. This may result in an under-representation of such mergers in our sample. Withdrawals are further discussed in the robustness section V(iii)(b).

TABLE I
TYPE AND TIMING OF MERGER DECISIONS 1999–2006

	total number of decisions			number in our sample		
	Ph. I	Ph. II	Total	Ph. I	Ph. II	Total
Intervention decisions						
prohibitions	—	10	10	—	10	10
settlements	108	48	156	100	41	141
clearances	—	18	18	—	16	16
Non-intervention decisions	2055		2055	206		206
Total	2163	76	2239	306	67	373

into their eventual outcomes: 26% of the intervention decisions were settlements in Phase II, 10% were clearances after a Phase II investigation (where the merger proceeded without remedies), and the remaining 5% were prohibitions (where the entire merger was blocked). Table I provides the total number of each outcome during our period, and the numbers in our sample.

Our full sample covers 373 merger decisions. This includes 167 intervention decisions (*intervention* = 1) and a random sample of 206 non-intervention decisions (*intervention* = 0), which we refer to as the 'intervention sample' and the 'non-intervention sample', respectively. The intervention sample is the selected sample for which we estimate the early settlement equation (2), which determines whether settlement is reached in Phase I (*settle* = 1) or not (*settle* = 0). The full sample allows us to correct for possible selection bias by jointly estimating equations (1) and (2).

The non-intervention sample accounts for 10% of non-intervention decisions during the period.[14] The intervention sample is based on all 184 intervention decisions, although 17 intervention decisions had to be excluded for one of two reasons. First, the data required to construct some of the variables is not available in 15 decisions. This is either because the decision report is heavily censored to prevent sensitive information from being in the public domain or because the analysis of the merger differs from the norm.[15] Second, two early settlements were mergers in which the only modifications required were the implementation of the remedy settlements of previous decisions. Consequently, these decisions are effectively non-intervention decisions.

IV(ii). *Specification of the Early Settlement Equation*

Our primary source of data is the standardised decision reports published by the EC for each merger decision. Since these reports do not allow us to

[14] All European mergers are allocated a consecutive case number according to date of notification. We used computer-generated random numbers to choose a 10% sample in the relevant range of case numbers.

[15] For example, in *OMYA/J.M. Huber* it was argued that market shares were not a good approximation of market power. Consequently, they were not reported.

observe the theoretically important determinants of early settlement directly, we need to construct appropriate proxy variables. Where possible, we focus on objectively quantifiable measures and aim to avoid qualitative opinions that could be biased towards *ex post* justifications. The theoretical models reviewed in Section II suggest that there should be three main determinants of the probability of early settlement: delay costs of the merging parties, uncertainty over the agency's findings, and resourcing of the agency. We discuss the proxies that we have constructed for each of these in turn, and Table III, which is presented at the end of Section IV, provides summary descriptions of all variables.

IV(ii)(a). *Resourcing of the Agency.* An agency is better able to understand competitive effects and appropriate remedies in a short time frame if it has the benefit of a well-resourced investigation. The competitive effects of some mergers are easily understood and resolved in Phase I. Others have idiosyncratic complexities that could not be resolved in Phase I. In between these extremes, there is a greater chance of the agency's understanding competitive effects in the relevant markets and of the parties offering acceptable remedies if more resources are available to the agency in Phase I. This suggests a positive relationship between resourcing and early settlement. However, the agency resource constraint bites hard when resources are already very tight, so the opportunity cost of Phase II is high for the agency. This increases the incentive for the agency to reach an early settlement. In other words, the relationship between early settlement and resourcing is expected to be non-monotonic (U-shaped).

Resourcing per case depends on the agency's budget, or more precisely the number of case officers available to the agency, and the number of other cases at the time of the Phase I investigation (which is the relevant period for early settlement). We measure pressure on the agency's resources by the number of mergers being investigated per 100 case officers employed by the EC: *caseload*. This is calculated for each day of the Phase I investigation then averaged over the same period. Case officers include economists, lawyers and senior support staff available for case work in the EC Competition Directorate.[16] Following the argument in the previous paragraph, we expect the probability of early settlement to be decreasing in *caseload* up to a point beyond which the agency-wide effect bites and the relationship may become increasing. We include the square of *caseload* to

[16] We thank DG Competition for providing us with annual observations of the number of case officers, measured in October of each year. This number increased from 255 in October, 1998 to 495 in October, 2007. We assume that the number of case officers changes in October and is constant throughout the rest of the year. Our empirical results are robust when we instead assume that the annual increase in case officers occurs at a constant monthly rate over the year.

allow for this possibility: *caseloadsq*. We expect the estimated coefficient on *caseload* to be negative and on *caseloadsq* to be positive.

IV(ii)(b). *Uncertainty over the Agency's Findings.* If there is uncertainty over the agency's findings, then it is difficult for the merging parties to make an acceptable remedy offer in Phase I. The agency may also be cautious in appraising such offers if it recognises that its early findings may be imprecise. This suggests a negative relationship between uncertainty and early settlement. We expect that uncertainty in merger appraisal derives fundamentally from the inherent complexity of the proposed merger, the subtleties of the theories of harm or defences to be investigated, and the lack of experience of the agency.

Mergers investigated by the EC often affect a large number of product and geographic markets. We suggest that the complexity of the merger is increasing in the number of contentious markets to be investigated ('contentious' is defined below). Each extra market adds to the analysis to be done and raises the likelihood of randomly coming across a difficult market. Limited resources mean that there is also likely to be diminishing analysis per market. When there is a large number of contentious markets, the agency is therefore less likely to discover key evidence either identifying or dismissing competitive concerns. This raises the degree of uncertainty. Our first measure of uncertainty is therefore a simple count of the number of contentious markets: *#unsafe* (i.e., those for which a remedy may be required). We expect *#unsafe* to be negatively related to the probability of early settlement.

Merger appraisal only considers increments to market power, so a horizontal merger can only be contentious (i.e., may impede competition) if the merging parties were previously competing. We define a contentious market as one in which the market shares of more than one of the merging parties are positive pre-merger and their combined market share is greater than or equal to 35%. Combined market share is the most important filter for competition analysis in European merger control.[17] We adopt the 35% threshold for a number of reasons. Recital 32 of ECMR [2004] states that market shares that do not exceed 25% 'are not liable to impede effective competition.' The horizontal merger guidelines further state that combined market shares in the range of 40–50% 'may also raise competition concerns' and 'in some cases' a share of less than 40% can raise concerns. Less

[17] This contrasts with merger control in the U.S. which commonly uses the Hirschman-Herfindal Index (HHI). The European Horizontal Merger Guidelines prioritise market share and the HHI has very much a secondary role. The HHI is rarely reported in European merger decisions. The reasons for this may include the use of a formal 'dominance test' prior to the revised 'significant impediment to effective competition' test in the new ECMR. Another reason may be that the EC considers a larger rival to be a more effective competitor than one with a small market share in its unilateral effects analysis.

formally, it is often claimed that 40% is an important threshold for consideration. Our intervention equation estimates, as summarised in Figure 1 below, help substantiate this claim. In practice, exact combined market shares are not usually reported in published decisions, but are replaced by a 10% range for confidentiality reasons. We adopt the convention of using the mid-point of the range, so record 35% for a market share reported as [30–40]%. Our econometric results do not significantly change if we reduce the 35% threshold to 30% or raise it to 40%. We discuss further robustness checks in Section V(iii)(a), including making an allowance for incremental market share and coordinated effects.

The theory of harm the EC investigates may also increase uncertainty. The most frequent and standard theory of harm is unilateral effects. This lies behind the above discussion of market share and contentious markets. Alternative theories of harm are usually more complex to analyse, not least because they require a greater understanding of the incentives for and effects on other firms. There are three main alternative theories of harm: 'coordinated effects' where a merger may increase the likelihood of tacit collusion;[18] 'vertical effects' where for example a merger may foreclose rivals; and 'conglomerate effects' where the portfolio of products brought together by the merger could foreclose rivals without a full product range. The market features likely to trigger regulatory interest in these theories are difficult to characterise in a quantifiable way, so we construct three dummy variables depending on whether or not there is assessment of such theories in the decision report: *coordinated*, *vertical*, and *conglomerate*. We expect the probability of early settlement to decrease when the EC investigates these theories because, as compared with an investigation that does not analyse such effects, there will be greater uncertainty over Phase I findings and suitable remedies.[19]

[18] The uncertainty associated with finding harm due to coordinated effects is likely to have been influenced by two cases in which the Commission was successfully challenged in the Court. In particular, *Airtours*/*First Choice* in 1999 was the first coordinated effects prohibition for '4 to 3' firms, but it was overturned by the Court in 2002. In 2005, the clearance of *Sony*/*BMG* (in a '5 to 4' market) was successfully challenged by a third party, Impala, in Court.

[19] A referee suggested that market definition and theories of harm may be endogenous. EC market definition usually follows the precedent of prior mergers in the industry. For example, in the *Glaxo Wellcome*/*Smithkline Beecham* decision (p.3): 'The Commission has on many occasions dealt with the definition of the relevant market in the case of pharmaceutical products and has established a number of principles in its previous decisions. On the basis of these decisions, product markets in the pharmaceutical industry can be grouped into pharmaceutic specialities, active substances and future products.' Even if the merger is the first in the industry, the EC will want to get the principles of market definition correct as they affect market definition in future mergers. More generally, institutional reputation and the threat of appeal make market definition quite inflexible so it is unlikely to be endogenous in the sense of being chosen to justify a decision based on other factors. Note that pricing pressure tests were not used by the EC during this period. There is a similar emphasis on precedent and consistency in the application of theories of harm, though see the previous footnote for the influence of the Court in relation to coordinated effects.

Other complexities can occur when the merging parties use a defence that differs from the norm. The standard line of defence is that the merger does not enhance market power despite high market shares because, for example, there are low barriers to entry or the products are weak substitutes in a differentiated market. More rarely, there are two alternative forms of defence that are inherently more complicated to assess. These are the 'efficiency defence', where it is claimed that marginal cost savings would incentivise lower prices; and the 'failing firm defence' where one of the merging parties is in financial distress and claims it would exit in the absence of a merger. We construct two dummy variables that capture whether there is assessment of these defences in the decision report: *efficiency* and *failing*. Both raise the complexity of the analysis and are expected to decrease the probability of early settlement.

To construct these five dummy variables, we undertook a word search of each decision report for the terms in Table II. When such terms were found, a close reading of the report determined whether it was mentioned only in a cursory manner or whether there was serious discussion of the theory. A dummy variable equals 1 for the latter as it shows clear evidence that an assessment of the theory was required.

Our final proxies of uncertainty relate to time-dependent factors. Merger control at the European level only began seriously in 1990. The EC had much to learn as did merging firms and their advisers. We expect that accumulated experience should reduce uncertainty. We separate specific industry experience from wider institutional experience. Industry-specific knowledge is accumulated from prior investigations. When the EC is unfamilar with an industry, it faces new challenges, including basic market definition and understanding the mode of competition. Moreover, there is little guidance for the merging parties' legal and economic advisors as to what might constitute acceptable remedies. We measure this form of industry-specific experience by the number of EC merger investigations since 1990 in the same industry (3-digit NACE): *experience*. In our econometric model we take the natural log of *experience* to reflect diminishing returns. We expect this to be positively associated with early settlement. We

TABLE II
IDENTIFICATION OF ALTERNATIVE THEORIES OF HARM AND DEFENCE

variable	words searched
coordinated	coordinat[ed], collusi[on], collective
vertical	vertical, foreclos[ure]
conglomerate	conglomerate, bundl[ing], portfolio
effciency	efficien[cy], cost saving[s], synerg[y]
failing	failing firm, reseue merger

Notes: For words with [.], we only searched for the term before the square brackets so the word search also identified plurals and similar words (e.g., coordinat[ed] and coordinat[ion]).

discuss robustness with respect to the construction of this variable in Section V(iii).

A second dimension of experience relates to a more general (non-specific) learning effect that is accumulated over the years. We model this general experience simply by a time trend calibrated for each merger on the day it was notified: *time*. In contrast to industry-specific experience, this captures much wider effects including the evolving confidence of the institution. It also picks up incremental regulatory and institutional reforms. For example, the EC suffered severe criticism by the Court in 2002 when three merger decisions were overturned. These fed into a review of procedures and a major set of reforms was introduced by Commissioner Monti over the following two years.[20] We include the square of *time* in order to allow for a non-monotonic effect of this increasing confidence in the early years (e.g., in requiring tougher remedies) followed by reforms that may have enhanced predictability: *timesq*. This quadratic time trend allows for a smooth evolution of any trend in early settlement, but it is not suitable to capture a single event with lasting impact (i.e., a structural break). We highlight the formal revision to the merger regulation, which came into effect in May, 2004, as a potentially significant event.[21] Among other things, this increased the time available for investigation, particularly when remedies are being proposed, and modified the substantive test for a merger. We test for a structural break in early settlement at this time by constructing a dummy variable for mergers that were notified under the new regulation: *newecmr*.

IV(ii)(c). *Delay Costs of Merging Parties.* The merging parties risk referral to Phase II if they haggle over the remedies for contentious markets. As already discussed in the introduction, the additional costs to the firms of referral to Phase II include diverted managerial time, external adviser costs and uncertainty which may result in key staff finding alternative employment. Referral also delays the achievement of synergies in uncontentious markets for the duration of the Phase II investigation. Consequently, the opportunity cost of haggling over remedies for contentious markets is increasing in the number of uncontentious markets exposed to delay. Thus, we measure the opportunity cost of delay to the merging parties by the number of uncontentious markets: #*safe* (i.e., markets that are safe from remedy). We expect #*safe* to be negatively related to the probability of early settlement.

We define an uncontentious market as one in which the market shares of more than one of the merging parties are positive pre-merger and their

[20] These included the creation of a chief economist's team, greater internal challenge to economic arguments, horizontal merger guidelines and a revised ECMR. See Lyons [2009] for further detail.

[21] See Council Regulation (EC) No 139/2004 of 20 January, 2004, on the control of concentrations between undertakings (the EC Merger Regulation) (OJ L 24, 29.01.2004, p. 1–22).

combined market share is (strictly) less than 35% but above 15%.[22] As we discussed in relation to *#unsafe* above, a combined market share below 35% is sufficiently small that it is unlikely to raise concerns of anticompetitive effects. We exclude markets with combined market shares (strictly) less than 15% in order to avoid a potential reporting bias due to inconsistent reporting across decision reports of such markets.[23] Our robustness checks in Section V(iii) confirm that our results are not sensitive to these exact thresholds.

IV(ii)(d). *Other Control Variables.* It is sometimes claimed that merger control is used as a political measure to protect European industry.[24] By far the largest source of non-EU domiciled mergers involved U.S. firms (27% of intervention decisions included at least one U.S. firm). We construct four dummy variables that identify whether the merging parties were each domiciled in Europe, in the U.S. a combination of Europe and the U.S., or any other nationality mix: *eeaonly*, *usonly*, *eeaus*, and *otherhome*. We use *eeaonly* as the base variable. If, for example, the EC were particularly obstructive against mergers involving only U.S. domiciled firms (as compared with mergers between only European firms), this would result in a negative coefficient on *usonly*.

IV(iii). *Specification of the Intervention Equation*

The probability of intervention is expected to be determined by characteristics that raise concerns of anticompetitive effects. We begin by discussing an exclusion restriction (i.e., a variable which is excluded from the settlement equation and so facilitates its econometric identification), then we discuss how other variables that are included in the settlement equation may affect intervention.

IV(iii)(a). *Exclusion Restriction.* As discussed in Section IV(iii)(a), the merging parties' combined market share is the main indicator of unilateral effects used by the EC, and such effects are more likely to be of concern for markets with high combined market shares. Thus, we use the merging

[22] This implies that the sum of *#safe* and *#unsafe* equals the number of markets with a combined market share above 15%.

[23] When the merging parties' combined market shares are sufficiently small, such that anticompetitive effects are highly unlikely to arise, markets are often not discussed in detail in the decision reports. This is particularly an issue for referrals. In general, there seemed to be a *de facto* threshold between 15% and 25%. For example, it was stated in *Hoechst/Rhône-Poulenc* (p.21) that 'there is only one affected market, where the share of the parties exceeds 15%,' and in *Pfizer/Warner Lambert* (p.10) that 'In 11 national markets, the operation does not give rise to competition concerns because the aggregated market share of the parties remains below 25%.' In relation to potential synergies for the merging parties, we expect these to be relatively minor for very small market shares (i.e., less than 15%), at least in the context of intervention decisions.

[24] For example, see Aktas *et al*. [2007].

parties' maximum combined market share across all affected markets as our exclusion restriction: *maxshare*. Focusing on the market with the largest combined market share is appropriate because a single anticompetitive market is sufficient for the EC to intervene in a merger. Importantly, *maxshare* is also not expected to influence early settlement because it does not proxy any of the three main determinants highlighted by the theoretical models reviewed in section 2.[25] Consistent with the construction of #*unsafe*, we set *maxshare* equal to zero if there is no market with a combined market share greater than or equal to 15%.[26]

We expect *maxshare* to raise the probability of intervention in a nonlinear manner. For example, the marginal effect of raising *maxshare* from, say, 20% to 30% on the probability of intervention should be very small for reasons given in Section IV(ii)(b). Likewise, raising *maxshare* from, say, 60% to 70% is also expected to have a small marginal effect, because intervention should already be highly likely. Consequently, the marginal effect is likely to be largest when *maxshare* is in an intermediate range, say, between 40% and 50%. This suggests a cubic is an appropriate functional form, so we also include variables of *maxshare* squared and cubed: *maxsharesq* and *maxsharecub*. We expect the former to have a positive coefficient and the latter, a negative coefficient.

In principle, our exclusion restriction could be improved by taking into account other factors that are important for anticompetitive effects, such as barriers to entry or rivals' market shares. However, this is not possible because such factors are hard to measure objectively and/or are only partially and inconsistently reported. Some other papers have simply assumed that barriers to entry are low if they are not reported (see Section VI for examples). We believe this is inappropriate and biased not least because barriers to entry should be a necessary condition for a market to raise concerns of anticompetitive effects, yet there are many such markets for which entry barriers are not reported. Furthermore, there appears to be a substantial reporting bias between Phase I and Phase II decisions. Of the markets that raised concerns of anticompetitive effects, (high) entry barriers are explicitly reported in only 35.3% of such markets in early settlements, 56.2% in settlements in Phase II and 100% in prohibitions.[27] Therefore, we avoid using such data.

[25] It could be argued that *maxshare* might proxy the potential harm caused by the merger, but, even if this were the case, Bebchuk [1984] shows that greater potential harm should not affect early settlement.

[26] While market share is used in the construction of #*safe* and #*unsafe*, it is important to stress that such variables are fundamentally different to *maxshare*. The former are counts of the number of markets below and above 35%, respectively, whereas the latter takes the value of the maximum combined market share. As a result, the inclusion of #*safe* and #*unsafe* in the settlement equation does not imply that *maxshare* cannot be used as the exclusion restriction.

[27] Notice that this pattern is consistent with legal drafting concerns in increasing anticipation of an appeal (judicial review of the decision) as we move from Phase I to Phase II and prohibition.

TABLE III
DESCRIPTION OF VARIABLES

variable	description
#safe	the number of markets where the market shares of more than one of the merging parties are positive pre-merger and the combined market share is greater than or equal to 15% but less than 35%
#unsafe	the number of markets where the market shares of more than one of the merging parties are positive pre-merger and the combined market share is greater than or equal to 35%
caseload	mean number of mergers under investigation by the EC (excluding non-intervention decisions and withdrawals) per day of the merger's Phase I investigation per 100 case officers
caseloadsq	the square of *caseload*
coordinated	1 if there is an assessment of coordinated effects; 0 otherwise
vertical	1 if there is an assessment of vertical effects; 0 otherwise
conglomerate	1 if there is an assessment of conglomerate effects; 0 otherwise
efficiency	1 if there is an assessment of an efficiency defence; 0 otherwise
failing	1 if there is an assessment of a failing firm defence; 0 otherwise
experience	number of mergers in the merger's 3-digit industry (NACE rev 2) since merger control was introduced, up to and including the current merger (excluding non-intervention decisions and withdrawals); when a merger falls into two or more 3-digit industries, the minimum is used
time	X/365 where X equals 1 if the merger was notified on 1 January, 1999, 2 if notified on 2 January, 1999, and so on
timesq	the square of *time*
newecmr	1 if the merger was notified under Council Regulation 139/2004; 0 otherwise
eeaonly	1 if the merging parties' domiciles were in the EEA only; 0 otherwise
usonly	1 if the merging parties' domiciles were in the U.S. only; 0 otherwise
eeaus	1 if the merging parties include domiciles in each of the EEA and U.S.; 0 otherwise
otherhome	1 if the merging parties' domiciles do not fall in *eeaonly, usonly* or *eeaus*; 0 otherwise
maxshare	the maximum combined market share of all markets in which the market shares of more than one of the merging parties are positive pre-merger, divided by 100; if there are no such markets with a combined market share greater than or equal to 15%, the value is 0
maxsharesq	the square of *maxshare*
maxsharecub	the cube of *maxshare*

IV(iii)(b). *Other Control Variables.* In our results reported below, we include as control variables in the intervention equation all of the remaining variables that are in the early settlement equation. Some of these variables are clearly expected to influence the probability of intervention. In particular, the dummy variables of *coordinated*, *vertical* and *conglomerate* may each pick up concerns of anticompetitive effects arising from their respective alternative theories of harm. So we expect that the likelihood of intervention will be positively associated with these dummy variables. For others, we do not have an *a priori* belief of whether or how they may affect the likelihood of intervention. For example, we have no reason to expect the number of markets to affect the decision to intervene, because one problematic market should be sufficient, but we include *#safe* and *#unsafe* in the controls for consistency. Finally, since we did not find any non-monotonicity in *caseload* or *time*, we present the specifications that only include the linear variable. In general, our results for the early

TABLE IV
DESCRIPTIVE STATISTICS OF THE INTERVENTION SAMPLE

	early settlements (*settle* = 1, obs = 100)				referrals (*settle* = 0, obs = 67)			
variable	mean	st dev	min	max	mean	st dev	min	max
#safe	4.610	7.336	0.000	57.000	3.030	4.526	0.000	20.000
#unsafe	8.150	11.087	0.000	53.000	11.015	20.296	0.000	129.000
caseload	2.480	1.402	0.726	5.734	2.529	1.280	0.295	5.607
coordinated	0.170	0.378	0.000	1.000	0.299	0.461	0.000	1.000
vertical	0.370	0.485	0.000	1.000	0.522	0.503	0.000	1.000
conglomerate	0.150	0.359	0.000	1.000	0.224	0.420	0.000	1.000
efficiency	0.010	0.100	0.000	1.000	0.060	0.239	0.000	1.000
failing	0.000	0.000	0.000	0.000	0.060	0.239	0.000	1.000
experience	9.310	9.912	1.000	42.000	9.254	7.999	1.000	39.000
time	3.723	2.518	0.038	7.978	3.360	2.330	0.321	7.833
newecmr	0.330	0.473	0.000	1.000	0.224	0.420	0.000	1.000
eeaonly	0.620	0.488	0.000	1.000	0.701	0.461	0.000	1.000
usonly	0.070	0.256	0.000	1.000	0.119	0.327	0.000	1.000
eeaus	0.220	0.416	0.000	1.000	0.090	0.288	0.000	1.000
otherhome	0.090	0.288	0.000	1.000	0.090	0.288	0.000	1.000

Notes: The minimum value for *#unsafe* is zero because some mergers raised concerns of anticompetitive effects due to vertical effects only.

settlement equation are robust regardless of the detailed specification of the intervention equation.

V. RESULTS

Since our focus is on early settlement, we discuss our results relating to the early settlement equation (2) in Section V(i), before returning to the intervention equation (1) in Section V(ii). We discuss robustness checks in Section V(iii).

V(i). *Early Settlement*

V(i)(a). *Descriptive Statistics on Early Settlement versus Referral.* We begin by presenting descriptive statistics in Table IV where the intervention sample is split between early settlements and referrals.

Consistent with our cost of delay argument, the mean of *#safe* shows that there is a larger number of markets with a combined market share below 35% in early settlements than in referrals. Similarly, consistent with our complexity arguments, the mean of *#unsafe* shows there is a smaller number of markets with a combined market share above 35% in early settlements. The mean of *caseload* for both early settlements and referrals is very similar to the intervention sample mean of 2.5 mergers per 100 case officers per day of a Phase I investigation. However, this is not particularly informative since we do not expect a monotonic relationship between *caseload* and early settlement. We return to this in the following section.

The most common alternative theory of harm (i.e., other than horizontal unilateral effects) for the intervention sample is vertical effects, which is assessed in 72 intervention decisions. Coordinated effects are assessed in 37 intervention decisions, and conglomerate effects in 30. In contrast, the alternative defences are much rarer, as efficiency and failing firm defences are assessed in only five and four intervention decisions, respectively. Table IV shows that the proportion of decisions that required an assessment of the alternative theories is greater for referrals than for early settlements. This is consistent with such alternative theories of harm making early settlement more difficult. Similarly, only one early settlement had assessment of an efficiency defence and all decisions with assessment of a failing firm defence were referrals.

The EC's sectoral experience ranged from being the first substantial merger investigation in the industry to the fourty-second.[28] The mean of *experience* is very slightly higher for early settlements than for referrals, but the difference is negligible. There are 48 intervention decisions that were notified post Monti reforms (*newecmr* = 1) and the proportion of such decisions is greater for early settlements than for referrals. Finally, the mean of *time* is only marginally larger for early settlements and the following econometrics suggests a more subtle evolution over time.

Regarding the domiciles of the merging parties, nearly two-thirds of intervention decisions were between European firms, but at least one U.S. firm was involved in approximately one quarter of intervention decisions. The proportion of early settlements is much the highest for mergers between European and U.S. domiciled firms, and there are slightly elevated proportions of referrals for mergers between firms domiciled in the U.S. only and EEA only.

V(i)(b). *Probability of Early Settlement.* Our estimations of (2) are presented in Table V and the marginal effects in Table VI. We report three specifications for the early settlement equation. Each is estimated jointly with the intervention equation. The first specification includes only our most central and objective variables (*#safe*, *#unsafe*, *caseload* and *caseloadsq*). As discussed in Section IV(ii), we interpret these respectively as proxies for the merging parties' opportunity cost of delay, uncertainty due to the complexity of the merger, and the extent to which the EC's resources are stretched. The second specification introduces the dummies of the alternative theories that capture other forms of complexity (*coordinated*, *vertical*, *conglomerate*, *efficiency* and *failing*). Since *failing* is perfectly correlated

[28] The EC had no previous experience of the industry under investigation for 23 intervention decisions. The industry in which it had most experience as of 31st December, 2006, was C20—Manufacture of chemicals and chemical products (which excludes pharmaceuticals).

TABLE V
ESTIMATIONS OF EARLY SETTLEMENT EQUATION (2)

dependent variable: *settle*	A	B	C	D
constant	1.264	2.313	2.745	2.168
	(0.499)	(0.598)	(0.937)	0.949
#safe	0.061**	0.053**	0.058**	0.061**
	(0.028)	(0.025)	(0.026)	0.027
#unsafe	−0.025***	−0.024**	−0.030***	−0.026**
	(0.009)	(0.010)	(0.011)	0.010
caseload	−0.701*	−1.004**	−1.009**	−0.839*
	(0.364)	(0.397)	(0.466)	(0.493)
caseloadsq	0.112**	0.145***	0.149**	0.127*
	(0.057)	(0.061)	(0.067)	(0.071)
coordinated	—	−0.678***	−0.880***	−0.843***
		(0.260)	(0.266)	(0.268)
vertical	—	−0.589***	−0.724***	−0.714***
		(0.216)	(0.229)	(0.233)
conglomerate	—	−0.301	−0.388	−0.310
		(0.288)	(0.286)	(0.281)
efficiency	—	−1.316**	−1.796***	−1.877***
		(0.638)	(0.636)	(0.655)
ln(*experience*)	—	—	0.048	0.077
			(0.113)	(0.111)
time	—	—	−0.488**	−0.475**
			(0.224)	(0.230)
timesq	—	—	0.066*	0.069**
			(0.035)	(0.035)
newecmr	—	—	0.293	0.126
			(0.652)	(0.639)
usonly	—	—	−0.502	−0.426
			(0.356)	(0.364)
eeaus	—	—	0.900***	0.939***
			(0.342)	(0.338)
otherhome	—	—	0.077	0.139
			(0.385)	(0.393)
# of (uncensored) obs	167	163	163	163
log pseudolikelihood	−208.7	−180.3	−168.3	−86.2
estimate of ρ	−0.488**	−0.496*	−0.448	—
pseudo r-squared	—	—	—	0.208

Notes: *, **, *** signify significance at the 10%, 5% and 1%, respectively. A, B and C estimate (2) using the bivariate probit with sample selection, and D estimates (2) using the univariate probit. Robust standard errors are in brackets, beneath the estimated coefficients.

with referral, four observations are dropped.[29] The third provides the full specification, which adds the time-dependent variables (*experience*, *time*, *timesq*, *newecmr*) and the domiciles of the merging parties (*eeaus*, *usonly*, *otherhome*).

We also have three specifications for the intervention equation, each complementing the logic of the respective early settlement specifications. These results are discussed in Section V(ii)(b). In the first two specifications, the estimates of the covariance between the errors, ρ, have a consistent negative sign and a Wald test confirms the importance of taking

[29] Our results are robust when these four observations are included without controlling for the failing firm defence.

TABLE VI
MARGINAL EFFECTS

x = variable	A	B	C	D
		dPr(*settle* = 1|*intervention* = 1)/dx		
safe	0.024**	0.018**	0.022**	0.024**
unsafe	−0.010***	−0.008**	−0.012***	−0.010**
caseload	−0.271*	−0.327**	−0.383**	−0.327*
caseloadsq	0.045**	0.049***	0.059**	0.049*
coordinated†	—	−0.233***	−0.322***	−0.321***
vertical†	—	−0.200***	−0.268***	−0.276***
conglomerate†	—	−0.080	−0.134	−0.123
efficiency†	—	−0.482**	−0.542***	−0.539***
ln(*experience*)	—	—	0.024	0.030
time	—	—	−0.188**	−0.185**
timesq	—	—	0.026*	0.027**
newecmr†	—	—	0.101	0.048
usonly†	—	—	−0.189	−0.169
eeaus†	—	—	0.274***	0.289***
otherhome†	—	—	0.034	0.053
w = variable		dPr(*intervention* = 1)/dw		
maxshare	0.048	−0.268	−0.112	−0.077
maxsharesq	1.216**	3.335***	2.250***	2.136**
maxsharecub	−0.947**	−2.379***	−1.647***	−1.553***
#*safe*	0.000	−0.001	−0.002	−0.001
#*unsafe*	0.001	0.000	0.000	0.000
caseload	0.026***	0.051***	0.052***	0.053***
coordinated†	—	0.087***	0.056***	0.056***
vertical†	—	0.081***	0.054***	0.055***
conglomerate†	—	0.102***	0.064***	0.065***
efficiency†	—	0.069	0.018	0.002
ln(*experience*)	—	—	0.019	0.022*
time	—	—	0.016	0.018
newecmr†	—	—	−0.038	−0.057
usonly†	—	—	0.038	0.037
eeaus†	—	—	−0.041	−0.039
otherhome†	—	—	0.017	0.021

Notes: Pr(*intervention* = 1) is the univariate predicted probability of intervention and Pr(*settle* = 1|*intervention* = 1) is the predicted probability of early settlement conditional on intervention. The latter is calculated by dividing the bivariate predicted probability of *intervention* = 1 and *settle* = 1 by Pr(*intervention* = 1). Marginal effects are calculated with continuous variables at the means of the intervention sample and dummy variables equal to 0. †is for a discrete change of dummy variable from 0 to 1. Stars signify the significance of the estimated coefficients. See notes to Table V.

selection into account. This implies that the unmeasured factors that are associated with an increased likelihood of intervention are also associated with a decreased likelihood of early settlement. As more variables are added, the significance of the correlation reduces slightly. In the third specification, the estimate of ρ is slightly above the 10% significance threshold, so there is no compelling evidence of selection bias. It is never possible to disprove a selection effect but we also estimate the third specification using the univariate probit. The results are very similar, both in terms of

coefficient size and apparent statistical significance, so we do not need to rely on one estimation method over the other.

Overall, our results provide significant support for our settlement theory approach. The results for individual variables are robust across different specifications, so our discussion is arranged by variable and focuses on specification D.[30]

The estimated coefficients on *#safe* and *#unsafe* are of the expected sign and they are consistently significant at better than the 5% level across all specifications. The marginal effects imply that an increase of one standard deviation in the number of uncontentious markets (i.e., $\Delta\#safe = 6.4$) is associated with a 15% point *increase* in the probability of early settlement. In contrast, an increase of one standard deviation in the number of contentious markets (i.e., $\Delta\#unsafe = 15.5$) would *reduce* the probability of early settlement by (coincidentally) 15% points. The former is consistent with a reduced willingness of the merging parties to risk delay when the merger covers a larger number of uncontentious markets. The latter is consistent with an increase in the complexity facing the agency in assessing a merger with multiple contentious markets. Together these results confirm that there is no simple merger scale effect on early settlement because the total number of markets does not matter (*#safe* and *#unsafe* have opposite signs).

The coefficient estimates on *caseload* and *caseloadsq* are negative and positive, respectively, and they are consistently significant at or close to the 5% level. This implies that during periods of relatively low merger activity, the probability of early settlement falls with an increase in the number of mergers under investigation relative to the number of case officers. However, there comes a point at which this investigations-to-staff ratio is so large that a further increase is associated with a higher probability of early settlement. Using the marginal effects of regression D, this turning point is estimated to be at 3.34 mergers per 100 case officers. To illustrate the quantitative significance of this, the mean of *caseload* for the intervention sample is 2.50 and it follows from the symmetry of a quadratic that there is a higher probability of early settlement when *caseload* is greater than 4.18 than when it is at the intervention sample mean, other things equal. This is the case for 13.2% of the intervention sample. In terms of the probability of early settlement conditional on intervention, this increases by 9% points if *caseload* is either increased or decreased by one standard deviation from the turning point.[31]

This U-shaped relationship is consistent with the following. At a below average pressure on resources, an increase in *caseload* reduces the precision

[30] Evaluating the continuous variables at their means of the intervention sample and assuming dummy variables are zero, specification D estimates that the probability of early settlement conditional on intervention is equal to 0.59. This compares with 60% early settlement in our sample (see Table I).

[31] Because of the non-linearity, an increase of 1.5 standard deviations would have a 20% point effect.

of the Phase I investigation, which increases uncertainty and compromises the likelihood of early settlement. During very busy periods, however, an increase in *caseload* may mean that the EC tries to limit resource-draining Phase II investigations by settling earlier than it would otherwise. The latter implies that the merging parties will be in a strong bargaining position during busy periods, so they may propose a less extensive remedy. If so, early settlements during very busy periods are more likely to have been Type 2 errors (or less of a Type 1 error, at least). There are 13 early settlements where *caseload* is greater than 4.18, and these are the only early settlements in our sample notified between 18 November, 1999 and 31, March, 2000.[32] A potential avenue for future research to assess this conjecture is to conduct *ex post* analysis of the EC's decisions to see whether more of these early settlements were errors compared with early settlements notified in less busy periods. Coincidentally, Davies and Lyons [2007, ch.8] have reviewed two of these 13 early settlements (*Monsanto/Pharmacia & Upjohn* and *Glaxo-Wellcome/SmithKline Beecham*), and they found problems with both the designs of the remedies and *ex post* performance of the divested assets.

The signs of the estimated coefficients on *coordinated* and *vertical* are negative and consistently significant at 1% level. The marginal effects imply that assessment of coordinated effects or of vertical effects is associated with a reduction in the probability of early settlement in excess of 20% points. Although the sign of the coefficient estimate on *conglomerate* is also negative, it is much smaller and not statistically significant.

The sign of the coefficient estimate on *efficiency* is negative and is statistically significant at 5% or better. There is also a large quantitative effect: assessment of an efficiency defence is associated with around a 50% point reduction in the probability of early settlement. Recall also that *failing* is dropped from the model because early settlement is not observed in our sample in the presence of a failing firm defence. This is equivalent to finding a 0% probability of early settlement in the data. Although based on very few observations, these findings are consistent with such defences' being difficult to assess, so the outcome of the EC's investigation is less certain and the probability of early settlement is reduced.

Turning to the time-dependent variables, we find that the coefficient estimate on *experience* is positive but insignificant. Thus, there is no evidence that the number of previous merger investigations in the same industry provides experience that facilitates early settlement in future mergers. In contrast, the signs of the estimated coefficients on *time* and *timesq* are negative

[32] This period is associated with the dotcom boom. It was a historic peak for mergers notified to the European Commission at the time, though it was subsequently exceeded by the 2006–08 peak. However, there were more interventions in each of the three calendar years 1999–2001 than in any other years since merger regulation began.

and positive, respectively, with significance at or around the 5% level. This suggests there was a decreasing tendency for early settlement over time until a minimum point, followed by a changing trend of increasing early settlement. Regression D estimates that this minimum point was reached on 5 June, 2002, which is almost exactly the date of the first of three major reversals of EC merger decisions published by the European Court in 2002.[33] The earlier decline may have been due to a slow response of firms and their advisers to an increasing confidence of the EC of requiring more extensive remedies. The more recent increasing trend is consistent with the subsequent reforms making EC findings increasingly more predictable, which facilitates early settlements. In this context, it is interesting that the coefficient estimate on *newecmr* is positive but not even remotely significant in the presence of the quadratic time trend. This suggests that there has been a fairly steady evolution of early settlement over time, and not a sharp structural break at the time of the introduction of the new merger regulation and associated institutional reforms.

Finally, the only significant domicile variable is *eeaus*, which has a positive coefficient estimate. Its marginal effect implies that the probability of early settlement is over 25% points higher for mergers between European and U.S. domiciled firms than for mergers between only European firms. This is consistent with either transatlantic cooperation between the EC and American agencies being equivalent to greater resourcing for the merger investigation, or a political dimension of support for transatlantic mergers which gives them a greater benefit of the doubt. The coefficient estimate on *usonly* is negative but insignificant, so there is no evidence of a bias against mergers between only U.S. firms.

V(ii). *Intervention*

Although not the main focus of this paper, the intervention equation is of intrinsic interest because it provides insight into the implementation of European merger control.

V(ii)(a). *Descriptive Statistics on Interventions Versus Non-Interventions.* Table VII reports the descriptive statistics of the intervention and non-intervention samples. As expected, the mean of *maxshare* is much lower for the non-intervention sample than for the intervention sample, suggesting that a high maximum combined market share is strongly associated with intervention.[34] Mergers in the non-intervention sample also cover fewer markets than those in the intervention sample. This applies to both

[33] The first was *Airtours/First Choice* on 6 June, 2002, followed by *Tetra Laval/Sidel* and *Schneider/Legrand*, both in October, 2002.

[34] In contrast, there is almost no difference within the intervention sample between the mean of *maxshare* for early settlements (66%) and referrals (64%).

TABLE VII
DESCRIPTIVE STATISTICS FOR THE FULL SAMPLE

	non-interventions (*intervention* = 0, obs = 206)				interventions (*intervention* = 1, obs = 167)			
variable	mean	st dev	min	max	mean	st dev	min	max
maxshare	0.109	0.175	0.000	0.850	0.661	0.267	0.000	1.000
#safe	0.825	2.254	0.000	22.000	3.976	6.391	0.000	57.000
#unsafe	0.413	2.327	0.000	30.000	9.299	15.462	0.000	129.000
caseload	1.980	1.157	0.167	5.607	2.499	1.350	0.295	5.734
coordinated	0.044	0.205	0.000	1.000	0.222	0.417	0.000	1.000
vertical	0.223	0.417	0.000	1.000	0.431	0.497	0.000	1.000
conglomerate	0.005	0.070	0.000	1.000	0.180	0.385	0.000	1.000
efficiency	0.005	0.070	0.000	1.000	0.030	0.171	0.000	1.000
failing	0.000	0.000	0.000	0.000	0.024	0.153	0.000	1.000
experience	6.990	7.505	1.000	43.000	9.287	9.167	1.000	42.000
time	3.995	2.417	0.033	7.901	3.577	2.444	0.038	7.978
newecmr	0.345	0.476	0.000	1.000	0.287	0.454	0.000	1.000
eeaonly	0.665	0.473	0.000	1.000	0.653	0.478	0.000	1.000
eeaus	0.214	0.411	0.000	1.000	0.168	0.375	0.000	1.000
usonly	0.044	0.205	0.000	1.000	0.090	0.287	0.000	1.000
otherhome	0.078	0.268	0.000	1.000	0.090	0.287	0.000	1.000

Notes: The minimum value for *maxshare* is zero due to the requirement that the combined market share must not be below 15% (see Section IV(iii)(a)).

#safe and (more obviously) *#unsafe*. Also as expected, each alternative theory of harm (*coordinated*, *vertical* and *conglomerate*) was assessed in a much smaller proportion of decisions in the non-intervention sample, though 22% of non-intervention decisions did have an assessment of vertical effects. This is consistent with the EC's taking a relatively benign view of vertical mergers. There are no failing firm defences and just one efficiency defence in the non-intervention sample.

The mean of *time* and the proportion of mergers post Monti reforms (*newecmr* = 1) are lower for the intervention sample than the non-intervention sample, which is consistent with a small trend away from intervention over time. In contrast, the means of *experience* suggest that merger decisions in the intervention sample are usually in industries with more previous merger investigations than those in the non-intervention sample. Finally, there is a smaller proportion of mergers between European and U.S. domiciled firms in the intervention sample than the non-intervention sample, but the reverse is true for mergers between only U.S. firms.

V(ii)(b). *Probability of Intervention.* As with the estimates of the early settlement equation, we report three specifications. The first specification in Table VIII is the intervention equation for the first specification in Table V, and so on for the other specifications. The intervention equation is identified by *maxshare*, *maxsharesq* and *maxsharecub* which are excluded from the early settlement equation. The *maxshare* variables have the

TABLE VIII
ESTIMATIONS OF INTERVENTION EQUATION (1)

dependent variable *intervention*	A	B	C	D
constant	−2.305	−2.727	−3.694	−3.811
	(0.281)	(0.303)	(0.635)	(0.632)
maxshare	0.542	−1.495	−0.887	−0.610
	(2.257)	(2.505)	(2.419)	(2.414)
maxsharesq	13.696**	18.586***	17.812***	16.885**
	(6.523)	(7.065)	(6.792)	(6.754)
maxsharecub	−10.673**	−13.258***	−13.037***	−12.277***
	(4.526)	(4.843)	(4.720)	(4.729)
#safe	−0.002	−0.003	−0.014	−0.011
	(0.034)	(0.035)	(0.026)	(0.027)
#unsafe	0.007	−0.002	0.002	−0.001
	(0.020)	(0.023)	(0.023)	(0.024)
caseload	0.290***	0.286***	0.413***	0.416***
	(0.084)	(0.082)	(0.118)	(0.120)
coordinated	—	0.888***	0.857***	0.879***
		(0.302)	(0.309)	(0.315)
vertical	—	0.745***	0.789***	0.813***
		(0.221)	(0.224)	(0.227)
conglomerate	—	1.823***	1.938***	2.054***
		(0.636)	(0.605)	(0.669)
efficiency	—	0.553	0.161	0.013
		(0.904)	(0.948)	(0.896)
ln(*experience*)	—	—	0.151	0.176*
			(0.098)	(0.100)
time	—	—	0.124	0.144
			(0.095)	(0.098)
newecmr	—	—	−0.249	−0.349
			(0.418)	(0.435)
usonly	—	—	0.414	0.399
			(0.350)	(0.342)
eeaus	—	—	−0.266	−0.256
			(0.256)	(0.256)
otherhome	—	—	0.147	0.190
			(0.319)	(0.317)
# of obs	373	369	369	369
log pseudolikelihood	−208.7	−180.3	−168.3	−83.4
pseudo r-squared	—	—	—	0.671

Notes: *, **, *** signify significance at the 10%, 5% and 1%, respectively. A, B and C estimate (1) using the bivariate probit with sample selection, and D estimates (1) using the univariate probit. Robust standard errors are in brackets, beneath the estimated coefficients.

expected sign pattern, which is robust across all specifications, with the quadratic and cubic variables significant at better than the 5% level. The cubic specification reveals that the maximum combined market share has a particularly strong marginal effect on the probability of intervention when this market share is between 40% and 60%. We return to this below after discussing the other variables.

As to the other control variables, the coefficient estimates of *coordinated*, *vertical* and *conglomerate* are consistently positive and significant at the 1% level. The marginal effects (see Table VI) imply that the probability of intervention is raised by just over 5% points if there is an assessment of coordinated, vertical or conglomerate effects. The only other variable with a

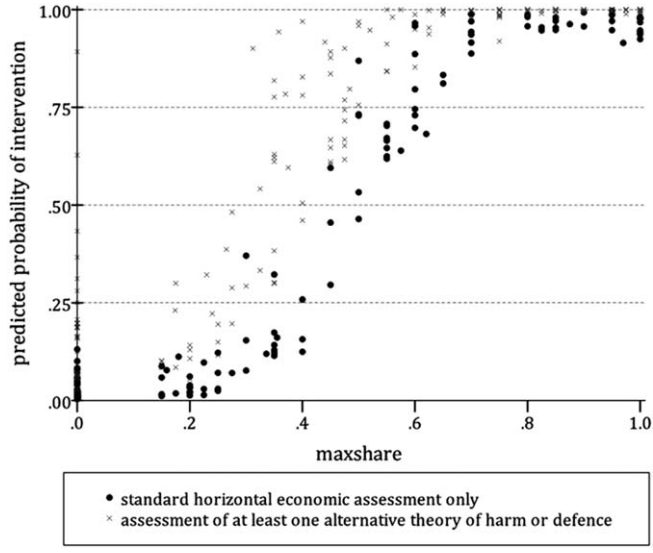

Figure 1
Relationship between Predicted Probability of Intervention and *maxshare*

estimated coefficient that is consistently significant is *caseload*. Its marginal effect suggests that the probability of intervention is raised by around 5% points when the number of merger investigations per 100 case officers per day is raised by 1. This may be because it is more difficult for the EC to rule out anticompetitive effects when the its resources are more stretched.

Returning to our results on *maxshare*, we illustrate the non-linear marginal effects in Figure 1, which is based on regression B to facilitate a cleaner presentation. The dots represent the predicted probabilities of intervention for each merger decision in which only standard horizontal economic assessment is required (e.g., unilateral effects), so each prediction depends on *maxshare*, *#safe*, *#unsafe* and *caseload*. The dots show that an increase in the maximum combined market share has little effect on the probability of intervention when *maxshare* is less than 40%, but the probability of intervention rises substantially for marginal increases in *maxshare* between 40% and 60%. The probability of intervention is close to one when *maxshare* exceeds 70%, so higher values have little additional effect. The point of inflection (i.e., the point of greatest sensitivity to *maxshare*) is at 47% for regression B, and Figure 1 shows that it is at approximately this level that the probability of intervention is 0.5. The crosses illustrate the extent to which combinations of alternative theories increase the probability of intervention. The vertical dispersion between the dots and the crosses is particularly large for *maxshare* between 35% and 45%. In this range, mergers with only standard horizontal economic assessment are cleared

more likely than not, whereas an intervention is more likely if additional theories of harm or defence are assessed.

V(iii). *Robustness Checks*

Our reported results are robust to reasonable changes to the construction of some of our key variables. We explain below what checks we have tried.[35] We focus initially on the definition of contentious versus uncontentious markets which are used to construct *#safe* and *#unsafe*. We then consider the measurement of *caseload* and *experience*, and discuss some alternative specifications.

V(iii)(a). *Alternative Measurement of #safe and #unsafe.* Our reported results use a simple 35% threshold for determining contentious markets. We justify this in Section IV(ii)(b) and our results from Section V(ii)(b) provide further support for a threshold around this level. Nevertheless, we developed a number of alternative definitions to confirm that our results are not overly sensitive to our definition.

The first adjustment was to move markets from *#safe* to *#unsafe* if they are investigated for coordinated effects. The reason for this is that low combined market shares can lead to coordinated effects when there are only a few firms of a similar size in the market. Ideally, we would identify such markets objectively using the pattern of shares across the market.[36] However, although it is possible to construct the merging parties' combined market shares for each market in our sample, it is not possible to do the same with rivals' market shares. This is because the data is not consistently reported, especially for mergers where coordinated effects is not an issue. Instead, we identify such markets by noting whether there was an assessment of coordinated effects in relation to each market. In the full sample, there are 112 markets assessed for coordinated effects, accounting for 1.6% of the total. Only 40 of these are moved from *#safe* to *#unsafe* due to combined market share less than 35%. Running our regressions after this adjustment shows virtually no change in the magnitude or the significance of our results.

Our next adjustment was to move markets from *#safe* to *#unsafe* when they were deemed to require a remedy for unilateral effects, despite low combined market shares. These unilateral effects cases seem to arise either because the geographical market is defined as worldwide, in which case 30% of the market may be considered a large share, and/or because the rest of the market is extremely fragmented. Although we prefer our objective market share threshold, this adjustment using the EC's opinion takes into

[35] Detailed results are available on request from the authors.

[36] See Davies *et al*. [2011] for an analysis of EC practice in relating market shares to coordinated effects in merger decisions.

account such aggravating factors. In the full sample, there were 50 such markets, which account for only 4.7% of the number of markets that raise concerns of anticompetitive effects. Whether such an adjustment is made on its own or with the first adjustment, there is no substantial change to the results presented in this paper.

Finally, we moved markets from #*unsafe* to #*safe* when the merger adds less than 5% to the combined market share. This might be treated by the EC as a *de minimis* increment that does not substantially add to market power, so the market may not raise concerns even for a large combined market share. The individual market shares for each firm are not always reported. In such cases, we assume that the increment is 5% or over. This seems an adequate assumption because a low increment is the second most frequently cited reason, after low combined market share, to dismiss unilateral effects. Consequently, when it is not reported, it is unlikely to be low enough to dismiss such effects. For the full sample, this adjustment moves 163 markets from #*unsafe* to #*safe*. Whether this adjustment is considered in isolation or with any combination of the above adjustments, we find that our main results are robust.

V(iii)(b). *Other Robustness Checks.* We also considered alternative measures of *caseload* and *experience*. In our reported results, both non-interventions and withdrawals were excluded in the construction of these variables. The former were excluded on the basis that such mergers will not require the resource levels of interventions and that the EC is unlikely to learn much from them. Withdrawals are more problematic for our measurement because some mergers that are withdrawn by the merging parties, especially following a substantial period of investigation, may have taken up significant resources and also provided the EC with valuable experience. As an alternative, we included in the measurement of *caseload* and *experience* all mergers that were withdrawn after 30 or more days. This made no substantial difference to our main results.

We also experimented by including broad industry dummy variables (1-digit NACE) which might capture certain unmeasured characteristics. These were not reported in the results above because the inclusion of industry dummies in a cross-section eliminates some of the interesting sources of variation in the data. We found that only the coefficient estimate on the energy industry dummy was significant in the intervention equation (at the 1% level) and the settlement equation (at the 10% level). This suggests that there is more likely to be an intervention, and the merger is less likely to be settled early, if the merger is in the energy sector rather than in manufacturing. This may be because of the political sensitivity of firms in gas and electricity, many of which are relatively recently privatised or retain state ownership. The magnitude and significance of our main variables were virtually unchanged.

VI. RELATED EMPIRICAL LITERATURE

Our paper complements a number of empirical studies that test the theory of early settlement.[37] While the results of this literature on the importance of the costs of delay are commonly consistent with ours, it has generally found weaker results on the importance of uncertainty. For example, Fournier and Zuehlke [1996] find that the probability of early settlement in U.S. civil lawsuits between 1979 and 1981 is increasing in litigation costs and decreasing in the expected value of the compensation, but the variance (or uncertainty) of the expected value of compensation is not statistically significant.[38] Fenn and Rickman [1999, 2001] find similar results for litigation costs and expected size of damages when analysing U.K. clinical negligence, health trust employment, and personal injury insurance claims, but they do not have a measure of uncertainty. Nevertheless, a small number of papers have found results consistent with our findings on uncertainty. Kessler [1996] finds that settlement for U.S. automobile insurance bodily injury claims are more likely to be delayed when the claims are complex, for example because the injury results in a disability or fatality. Fenn and Rickman [2014] find that medical malpractice claims are settled more quickly once an expert report is produced, which is consistent with the report reducing uncertainty.[39]

Our paper is also related to the empirical literature that uses data from competition agency decision reports to investigate certain aspects of merger control. That literature has not previously drawn on the theory of early settlement to clarify empirical specification, so proxies for delay costs, uncertainty and particularly resourcing of the agency have not been systematically constructed in the manner that we have done above. However, there are similarities and differences in the approaches and construction of variables that deserve further discussion.

[37] See Kessler and Rubinfeld [2007] for a wider review than the one offered here and see Cooter and Rubinfeld [1989] for a review of the early empirical literature on out-of-court settlements. Reviews of bargaining under uncertainty are provided by Kennan and Wilson [1993] and Ausubel *et al*. [2002]. In contrast to our approach, this literature typically uses a hazard model to estimate the probability of settlement over time, because private settlements can be agreed at any time, so they do not have the binary nature of our merger context (where either remedies are settled in Phase I or the merger is referred).

[38] In an earlier paper that estimates a similar specification using a discrete choice model and the same data, Fournier and Zuehlke [1989] do find this measure of variance to be a positive and significant determinant of settlement.

[39] Perloff *et al*. [1996] use private antitrust action data to estimate a probability of winning at court (p) and use this to construct a trial uncertainty variable ($= p[1 - p]$). In their estimated settlement equation, only the coefficient on this uncertainty term is statistically significant. Its sign is positive because, in contrast to all the above literature, this is equivalent to uncertainty over the outcome of the second phase. In the context of their model, they interpret this result as that risk aversion is important. They found no significant evidence that a divergence of expected gain/loss mattered.

For European mergers, Bergman *et al*. [2005] analyse a random sample of 78 mergers notified between 1990 and 2002.[40] In contrast to our approach, they pool non-intervention decisions with early settlements, and compare these against mergers referred to Phase II in a probit analysis. Furthermore, they use data from only one of the affected markets of each merger, so their measures provide a very limited characterisation of these mergers.[41] Broadly consistent with our results, they find that a Phase II investigation is more likely when there is a combination of high incremental and combined market shares, and when the merger would lead to coordinated effects or vertical effects.[42] They find that a Phase II investigation is less likely when a U.S. firm is involved, but do not make our distinction between *usonly* and *eeaus*, which we find to be important. They also report a significant estimated coefficient on a high entry barriers dummy variable but acknowledge that it is likely to be subject to a reporting bias.

Nearer to our paper but for U.S. mergers, Coate and Kleit [2004] analyse a sample of 172 mergers that the Federal Trade Commission (FTC) did not unconditionally clear between 1983 and 1999. Merger control in the U.S. differs from Europe because, if settlement is not reached with the agency (i.e., FTC or DOJ), the case can be litigated in court. Coate and Kleit [2004] investigate the factors that affect whether a merger is withdrawn, whether a remedy settlement is negotiated or whether the outcome is decided by litigation. Similar to our count of uncontentious markets, they measure the merging firms' cost of delay by the value of assets that do not raise competition concerns, and they find that as this value increases, the merging parties are more likely to negotiate a remedy settlement than withdraw, and are more likely to withdraw than to litigate. They further show that merging parties are more likely to litigate than withdraw when the merger generates greater efficiencies, which is broadly similar to our finding on the efficiency defence.[43]

Finally, related to our results on intervention, Coate *et al*. [1990] and Coate [1995, 2005] investigate the determinants of intervention for a sample of U.S. mergers. They show that intervention is positively associated with a Herfindahl index over 1800, an FTC belief that the industry is

[40] See also Lindsay *et al*. [2003], Fernández *et al*. [2008], and Bougette and Turolla [2008] for similar studies using EC data. Ormosi [2012] provides an econometric study of remedies and the efficiency defence.

[41] This is also the empirical strategy used in most of the literature mentioned below. Another strategy some papers use is to construct a sample that includes only mergers that have exactly one market (e.g., Coate [2005]). This would create a very unusual sample in the European context.

[42] Their coordinated and vertical effects dummy variables differ from ours. Their categories rely on the EC finding concerns, whereas we focus on there being an assessment of such a theory. Our approach is more appropriate as a measure of the complexity of the economic analysis (as this complexity exists whether or not the final decision finds concerns).

[43] See also Kouliavtsev [2007] for an empirical model that aims to explain the strength of the agreed remedy by using U.S. data on DOJ merger settlements.

conducive to collusion, and high barriers to entry. Our intervention variables differ in two ways. First, EC decision reports confirm a prime focus on the combined market share of the merging parties over market concentration measures. Second, we avoid the use of reported barriers to entry variables because they are subjectively assessed and we strongly suspect reporting bias in the EC's decision reports (see Section IV(iii)).

VII. CONCLUSION

We have drawn on the theory of early settlement to understand why some merger remedies are agreed quickly while others are not. The theory highlights the importance of delay costs, uncertainty and agency resourcing. Consistent with the theory, we have found evidence that the probability of early settlement in European merger control is increasing in delay costs of the merging parties, decreasing in the uncertainty associated with the complexity of the economic assessment that has to be completed within tight statutory time limits, and decreasing (increasing) in the case load of the EC when resources are plentiful (tight). This extends the range of contexts in which the theory of early settlement has previously been tested. Those other contexts have found the predictions relating to uncertainty particularly difficult to test and there has been no previous work on the effects of agency resourcing.

Some of our supplementary results predict when the European Commission is likely to intervene in a merger, either by requiring remedies in Phase I or by referring to Phase II. When only standard horizontal economic assessment is required, there is a rapidly rising probability of intervention as the maximum combined market share across all affected markets increases within the range 40% to 60%, and a less than 0.5 chance of intervention if this share is below 45%. However, intervention is likely to occur for a lower maximum combined market share if alternative theories of harm (such as coordinated, vertical and conglomerate effects) are assessed.

Finally, a number of our results have implications for merger policy. First, we find that the Monti reforms at the European Commission have been beneficial in reducing the uncertainty that had previously delayed settlement. We identify a trend decrease in early settlement up until the time when there was a sequence of successful appeals against three merger decisions. These reversals in court spurred an existing process of administrative and institutional reform. Afterwards, the trend in early settlement becomes positive, which is consistent with the reforms having helped to clarify expectations. Second, European merger decisions are sometimes accused of bias according to the domicile of firms. We find no evidence of any bias against U.S. mergers, but we do find that a merger between a U.S. and an EEA domiciled firm is more likely to be settled in Phase I. This is consistent with effective transatlantic cooperation between competition agencies.

Third, our results regarding the case load of the European Commission imply that firms will be in a strong bargaining position when resources are tight, so early settlement of complex mergers at times of tight resourcing may have resulted in inadequate remedies. This suggests that the agency should be provided with a flexible budget so it can buy in skilled support at times when there is a very high case load. This would reduce the temptation of the agency to agree potentially error-prone early settlements in complex mergers simply to reduce the case load.

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