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The Neglected Experimental Evidence

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Abstract

Both in the US and in Europe, antitrust authorities prohibit merger not only if the merged entity, in and of itself, is no longer sufficiently controlled by competition. The authorities also intervene if, post merger, the market structure has changed such that "tacit collusion" or "coordinated effects" become disturbingly more likely. It seems that antitrust neglects the fact that, for more than 50 years, economists have been doing experiments on this very question. Almost any conceivable determinant of higher or lower collusion has been tested. This paper standardises the evidence by way of a meta-study, and relates experimental findings as closely as possible to antitrust doctrine.

I. The Issue

Normally, hard scientific evidence on legal issues is a seriously scarce resource. More than one empirical publication on a precisely defined doctrinal question is a rare event. Against this backdrop, a problem in antitrust is salient. Both in the US and in Europe, antitrust authorities prohibit merger not only if the merged entity, in and of itself, is no longer sufficiently controlled by competition. The authorities also intervene if, post merger, the market structure has changed such that "tacit collusion" becomes disturbingly more likely. This can be seen as a precautionary measure. Collusion in general, and tacit collusion in particular, is hard to detect, and even harder to control by authorities bound by the rule of law. Therefore, the antitrust authorities do not wait until market participants actually team up against the opposite market side, and against society at large. Rather they preempt later anticompetitive behaviour by prohibiting the further concentration of a market.

Antitrust authorities mainly use the traditional doctrinal approach to assess whether a proposed merger would significantly increase the risk of tacit collusion. Based on their experiences in dealing with mergers in narrow markets, both in the US and in Europe, the authorities have published catalogues of potentially relevant factors (US Merger Guidelines;[1] EU Merger Guidelines[2]). On the initiative of the chief economist, the EU Commission also relies on game theory for the purpose (Ivaldi, Jullien et al. 2003). There would also be rich econometric evidence (Feuerstein 2005), and a host of case studies in the economics literature (Levenstein and Suslow 2006). Yet this does not exhaust the available evidence. On this prognostic question, economists have also, for more than 50 years, been doing experiments. Almost any conceivable determinant of higher or lower collusion has been tested. This solid body of evidence is almost untapped by the legal community. It seems that the antitrust authorities are not even taking notice of the fact that one of their thorniest empirical questions has already been thoroughly investigated by a neighbouring discipline.

This paper makes the evidence available. It standardises the findings by way of a meta-study, and it matches the experimental findings as closely as possible to doctrine. In preparation, the key concepts of the doctrine of tacit collusion in the US and in Europe are reported (II), and the methodology of the experiments, and of the meta-study summarizing this literature, is explained (III). Antitrust can capitalise on the experimental evidence at two levels. It is most helpful in evaluating and further developing the guidelines that authorities on both sides of the Atlantic have issued for assessing the "coordinated effects" of mergers. The individual criteria in these checklists can be related to the experimental evidence. More importantly even, the relative importance of these criteria can be determined (IV). Individual cases combine these criteria in idiosyncratic ways. Although many parameter constellations have been tested ex-

1 Department of Justice, Federal Trade Commission, Antitrust Division, 1992 Horizontal Merger Guidelines of September 10, 1992, 57 FR 41552, preface; Horizontal Merger Guidelines of August 19, 2010, available at http://www.justice.gov/atr/public/guidelines/hmg-2010.html.

2 Guidelines on the Assessment of Horizontal Mergers under the Council Regulation on the Control of Concentrations between Undertakings, OJ 2004 C 31/5.

perimentally, an experimental result on exactly the parameter combination of a given case is often not available. Based on the meta-study, it is sometimes possible to predict the importance of specific factor combinations for the risk of tacit collusion. But the data on multiple interactions is usually not significant. A statistically less demanding approach is multiple regression. It introduces the features of the case as controls and checks whether a reduction in the number of suppliers still increases the risk of collusion substantially. The power, and the limits, of the approach are demonstrated with respect to the European landmark case on tacit collusion, the *Airtours* ruling (V). Often, meta-analysis is also used to check whether the existing body of evidence suffers from discernible bias since studies that have found the desired effect were more likely to be published. Happily, with this data, there is not much reason for this concern. But since experimental data is, by design, only analogous to the issue in the field one wants to understand, external validity warrants discussion (VI).

II. Doctrine

Under section 7 of the Clayton Act, mergers are prohibited if their effect "may be substantially to lessen competition, or to tend to create a monopoly" (15 U.S.C. 18). Under section 1 of the Sherman Act, they are prohibited if they constitute a "contract, combination [...], or conspiracy in restraint of trade" (15 U.S.C. 1). Both the Antitrust Division of the Department of Justice and the Federal Trade Commission have jurisdiction to intervene. In order to increase predictability, in 1992 both authorities have issued joint guidelines on the treatment of horizontal mergers. While most of these guidelines have been a restatement of court decisions and administrative practice, the section on potential coordinated and unilateral effects of mergers was meant to innovate (US Merger Guidelines 1992, preface). In the light of both the experiences made by the authorities, and the evolution of the accompanying scholarly debates, in 2010 the Guidelines have been revised and extended (US Merger Guidelines 2010).

The guidelines stress that the anti-competitive effects of merger are not confined to increased market power for the new commercial unit (US Merger Guidelines 2010, #1). Besides "unilateral effects", a merger may also be challenged since it creates a risk of "coordinated effects" (US Merger Guidelines 2010, #1). For coordinated effects, there is no need to show that firms will explicitly team up. It suffices to make a case of "parallel accommodating conduct". This "includes situations in which each rival's response to competitive moves made by others is individually rational" (US Merger Guidelines 2010, #7). To that end, the Agencies expressly engage in prediction: "The Agencies examine whether a merger is likely to change the manner in which market participants interact, inducing substantially more coordinated interaction" (US Merger Guidelines 2010, #7.1).

In Europe, under Art. 3 II of the Merger Regulation, "a concentration which would significantly impede effective competition, in the common market or in a substantial part of it, in particular as a result of the creation or strengthening of a dominant position, shall be declared

incompatible with the common market" (OJ 2004 L 24/1). According to the case law of the European Court of Justice, the test includes situations of "collective dominance". In the *Airtours* case of 2002[3], coordinated effects have been centre stage, with a rich echo in academic writing (Motta 2000, Christensen and Rabassa 2001, Haupt 2002, Overd 2002, Stroux 2002, Guerrero 2003, Nikpay and Houwen 2003, Scott 2003, Spink and Ong 2003, Veljanovski 2004, Kokkoris 2005). But the doctrinal concept is as old as the *Nestlé/Perrier* case of 1992.[4] It has been approved by the European Court of Justice in the *Kali und Salz* case of 1998[5] (Perez 1998, Ysewyn and Caffarra 1998).

In *Airtours*, the European Court of First Instance has held that a merger may be prohibited if, in light of "the relationship of interdependence [...] the parties [...] are in a position to anticipate one another's behaviour and are therefore strongly encouraged to align their conduct in the market, in particular in such a way as to maximise their joint profits by restricting production with a view to increasing prices. In such a context, each trader is aware that highly competitive action on its part designed to increase its market share (for example a price cut) would provoke identical action by the others, so that it would derive no benefit from its initiative" (ECJ *Airtours*, #60).

"Three conditions are necessary for a finding of collective dominance [...]: first, each member of the dominant oligopoly must have the ability to know how the other members are behaving in order to monitor whether or not they are adopting the common policy [...]; second, the situation of tacit coordination must be sustainable over time [...]; third, to prove the existence of a collective dominant position to the requisite legal standard, the Commission must also establish that the foreseeable reaction of current and future competitors, as well as of consumers, would not jeopardise the results expected from the common policy" (ECJ *Airtours*, #62). These conditions have almost literally found their way into the 2004 Horizontal Merger Guidelines of the European Commission (EU Merger Guidelines, #22, 39, 41) (see also Etter 2000, Briones and Padilla 2001).

When contrasting them with the experimental evidence, the main factors will be reported that the US and the European guidelines consider relevant for assessing the risk of tacit collusion. From this it will become evident that there is little substantive disagreement between the US and the European law of tacit collusion.

3 Court of First Instance, 6 June 2002, *Airtours plc v Commission of the European Communities*, European Court Reports 2002 II 2585.

4 Commission Decision, 22 July 1992, OJ 1992 L 356/1; see also Commission Decision, 14 December 1993, OJ 1994 L 186/38, *Kali+Salz/MdK/Treuhand*.

5 European Court of Justice, 31 March 1998, European Court Reports 1998 I 1519, para. 221; see also European Court of First Instance, 25 March 1999, European Court Reports 1999 II 753, *Gencor* and on this case (Albors-Llorens 2000); Commission Decision, 26 October 2004, OJ 2005 L 218/6, *Oracle/People Soft* and on this case (Pflanz 2005).

III. Methodology

This paper wants to inform antitrust practice of experimental evidence that may help it better predict the risk of tacit collusion if it clears a merger. This section introduces the character of this evidence, and explains the methodology for synthesizing it.

A. The Character of Experimental Evidence on Collusion

To illustrate the methodology of the experimental evidence, consider the study by Fouraker and Siegel, which is not only one of the earliest and most cited contributions to this literature, but which also for a long time defined methodological standards (Fouraker and Siegel 1963). The authors invited 291 students to participate in a series of 10 different oligopoly experiments. Students were individually conducted to separate cubicles and randomly assigned to groups of either two or three. At no time they learned the identity of their interaction partners, nor had they any chance to communicate with each other, except through their behaviour in the experimental market. According to the instructions, subjects assumed the role of sellers, with demand represented by an assistant of the authors, committed to buy according to a predetermined demand function (later experiments typically used a computer for the purpose).

Fouraker and Siegel measure collusion by the respective strategic variable. To generate a benchmark, they introduce a mathematical model of oligopoly. From this model, they derive three predictions: the competitive (or Walrasian) equilibrium (hereafter WE), i.e. the price or the quantity at the intersection of the demand and the supply curves, which are taken from marginal utility for consumers, and marginal cost for producers; the collusive (or Pareto) equilibrium (hereafter CE), i.e. the price or quantity that maximises sellers' profit, at the expense of consumers and society at large; finally the prediction of standard economic theory, the so-called Nash equilibrium. This is the price or quantity that result if sellers mutually anticipate that other sellers maximise profit. In this literature, it is standard to define the benchmarks for one-shot interaction, even if, as in this study, experimental subjects interact repeatedly. This practice is mainly due to the fact that predictions for the repeated game are normally not unequivocal. This is in particular true if experimenters, as Fouraker and Siegel, have not told participants at the outset how often the game will be repeated. Then the so-called folk theorem shows that the repeated game has an infinite number of equilibria (Aumann and Shapley 1994).

To induce preferences, the authors endow their subjects with experimental money (to be converted into real money at the end of the experiment), and they give them tables from which they can read off their choices, and how their payoffs depend on the (simultaneous or sequential) choices of their competitors. Later experiments have often also given participants the possibility to simulate market outcomes on the computer before they choose, or they have represented market design graphically. Fouraker and Siegel have a so-called factorial design, i.e. they combine 2 strategic variables (price or quantity) with 2 market sizes (2 or 3 sellers) with 2 information conditions (are subjects informed about profit of individual competitors?).

On top, on one parameter combination they test the effect of an extra bonus for cooperation or for competitiveness. Hence their study generates 10 data points.

In later experimental practice, typically the mean result per treatment is reported. Fouraker and Siegel's study is unusual in that, instead, only the result for the respective penultimate round is reported. To illustrate, consider the treatment with a market of 3, competition in quantity, and information reduced to own and industry profit. Given the parameters set by the experimenters, the Walrasian prediction for the one shot game was an industry quantity of 60, the Pareto prediction was 30, and the Nash prediction was 45. The mean result of 11 experimental groups of 3 in the penultimate round was 48.1. This result thus provides most support for the Nash prediction. To make the experiment affordable, every round earnings are calculated in an artificial experimental currency. At the end of the experiment, this currency is translated into $-Cents at a rate of 100:1. Hence experimental results are typically based on low stakes. Eventually, one of course only can know by testing other participants on high stakes games. Yet when this has been done with related games, the qualitative results from the lab usually replicate (see, e.g., Fehr, Tougareva et al. 2014).

If experimental participants (tacitly) collude, there is no sanction. The experiment thus tests an environment without antitrust rules. Recent experiments have become interested in the behavioural effects of antitrust rules and antitrust interventions (see, e.g., Bigoni, Fridolfsson et al. 2012). But merger control is specifically interested in the risk of "coordinated effects", even if it may later be difficult or impossible for the authorities to stop them (US Guidelines 2010, #7). The experimental literature on collusion in the absence of antitrust intervention informs the authorities about the factors that make such coordination more or less likely.

Learning how well the Nash equilibrium predicts behaviour is useful for antitrust authorities as a diagnostic tool. Therefore, in an earlier paper I have reported relative deviations from the Nash equilibrium per feature of the case (Engel 2007).[6] However, normatively deviations from the Walrasian equilibrium are more important. For if the Nash and the Walrasian equilibrium do not coincide (the most important case where they do is price competition with homogeneous products), producers still appropriate some of the consumer rent, and there is still a deadweight loss if suppliers play Nash. Since this paper intends to directly relate the experimental evidence to the decisions of antitrust authorities on mergers in oligopolistic markets, it reports deviations from the Walrasian equilibrium.

B. Sample

Meta-study is an attempt at organizing the cumulative evidence. Rather than just reporting the results of a single study, meta-study informs about a whole literature. Since this paper does so with the intention to provide antitrust authorities with an additional source of evidence, it is particularly important to specify the attempts at covering the existing material. The paper re-

6 See also below 0 for the distribution of this variable in the new, enlarged sample.

lies on five different sources: the database EconLit covers papers published in economics journals. The database IDEAS covers working papers from economists. The database SSRN covers working papers from lawyers and economists. For all three databases, all hits for "collusion, experiment" and "oligopoly, experiment" have been checked. Additionally, the references from a recently published survey of this literature (Potters and Suetens 2013) have been checked, and the material collected for the earlier 2007 meta-study. All in all this results in 963 hits. In all databases, the same paper may show up multiple times. Taking this into account, the gross sample is composed of 471 publications, including working papers. Many papers feature in several of these sources. Specifically, only the papers listed in the diagonal of Table 1 are exclusively covered by one of the five sources. The remaining cells report the number of papers that are covered by the two sources in question.[7]

	Engel 2007	Potters Suetens	Ideas	SSRN	EconLit
Engel 2007	103 (47)	43 (32)	29 (20)	15 (12)	18 (14)
Potters Suetens		69 (18)	44 (21)	23 (14)	25 (14)
Ideas			78 (5)	52 (20)	78 (25)
SSRN				15 (3)	29 (11)
EconLit					38 (2)

Table 1
Sample Composition

number of papers reported in the respective source
(a) included in gross sample, (b) included in final sample (in brackets)

Upon closer scrutiny, a substantial fraction of this gross sample was not apt to be included in the meta-study. 100 papers do actually not report lab experiments. The word "experiment" features in the abstract or the body of the paper, since the paper in some other way relates to experimentation, mostly because some change in the market or regulatory intervention is interpreted as an experiment. 31 papers survey some aspect of the experimental literature, without offering fresh evidence. 2 publications reuse the same evidence as an earlier publication. 58 papers do not report the data or the experimental design such that the collusion index can be calculated. Most frequently this is due to the fact that the raw data is only reported in a graph, or as a regression coefficient from which the raw data cannot be reconstructed. A further series of experimental papers has a different research question. 10 papers investigate spatial competition. 46 papers test auctions; this has developed into a separate literature (see the surveys by Dechenaux, Kovenock et al. 2012, Kwasnica and Sherstyuk 2013). 16 papers do not test collusion per se, but the effect of anti-trust intervention. 27 papers test some form of oligopoly, but use a different dependent variable, for instance investment in process innovation. Finally 41 papers are excluded that may or may not be motivated by oligopoly, but test

7 Note that the same paper may be covered by more than two sources, and is then counted more than once.

participants on the payoffs of an unframed game. Without this exclusion restriction, it would have been necessary to include the vast experimental literature on prisoner dilemma games. In one of my own experiments, I have shown that the frame indeed matters. If participants learn that they compete in a market, they are significantly less likely to cooperate (Engel and Rand 2014). There is one exception to these exclusion rules: if one and the same experiment covers treatments with and without the exclusion criterion, the whole data from that experiment is covered. Applying all these criteria, the final sample is composed of 140 publications and working papers. The numbers in brackets in Table 1 inform in which ways these publications are documented.

C. Dependent Variable

Of course, individual experiments only manipulate one, or at most a small number of features. The study by Fouraker and Siegel is already quite rich in that it has 10 different treatments. Yet oligopoly is a fairly well defined problem. Even if experimenters had only been interested in a single feature, they could not but define many more parameters, like product characteristics, market size, the shape of supply and demand, the strategic variable, the duration of the game, communication protocols, the information environment, and trading institutions. That way they have generated a rich body of data that has gone untapped thus far. The meta-study presented in this paper makes this evidence available. To that end, all features of the market the experimenter had specified are recorded as independent variables.

All papers used the respective strategic variable, i.e. price or quantity, as the dependent variable. Usually they themselves relate their findings to the Walrasian equilibrium. In many other papers, the Walrasian and the collusive equilibrium can be calculated from the specifications of the supply and the demand curves. To make the findings comparable across papers, an index is generated as in Figure 1.

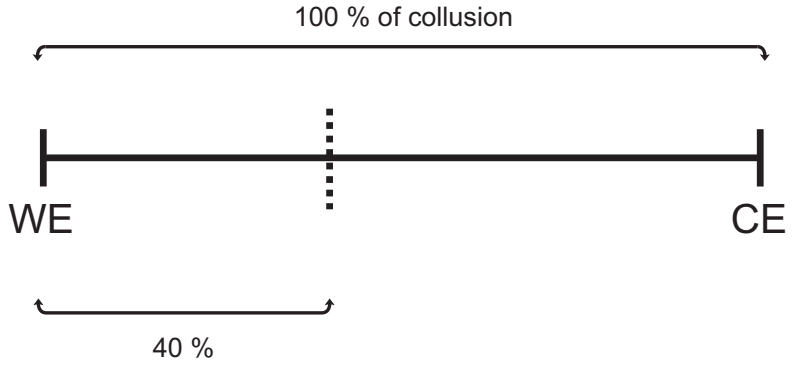

Figure 1
Normalization of Dependent Variable

WE: price or quantity at which the market clears ("Walrasian Equilibrium")
CE: price or quantity at which suppliers' profit is maximal ("Collusive Equilibrium")
dotted line: example finding

How this index works is best explained in an example. Go back to the one finding of Fouraker and Siegel reported above. If suppliers compete in quantity, a large number means a high degree of competition. If 60 items sell, the market clears. Collusion is at 0%. If only 30 items sell, sellers reach the joint maximum. This is defined as 100% collusion. By a simple arithmetic operation, one may express the respective experimental finding as a fraction of maximum collusion. Subtract the collusive from the competitive quantity, which gives 30. Subtract the experimental finding from the upper bound which, in the example, gives 60 - 48.1 = 11.9. Divide 11.9 by the distance between the competitive and the collusive quantity, i.e. by 30, and multiply the result by 100, to get 39.66% of collusion. In price competition, a high number means more collusion. Therefore, there the Walrasian price must be subtracted from the reported price, to get the normalised finding, to be divided by the distance between the collusive and the Walrasian price.

In experimental practice, subjects sometimes overshoot. They restrict quantity, or raise price for that matter, to a level that hurts consumers excessively, without giving sellers additional profit. In that case, a degree of collusion above 100% results. Occasionally, competition is so fierce in experimental markets that quantity is even higher, or price is even lower, than in the Walrasian equilibrium. In that case, the collusion index is negative. In the meta-data, (normalized) collusion ranges from -96% till 136%.

In the field, price fixing or giving each competitor a quota are not the only options for reducing competitive pressure. Firms may instead agree not to approach their competitors' customers, e.g. by giving each firm a monopoly region (US Guidelines 2010, #7.2). Or they may coordinate their investment choices, rather than supply decisions. Collusion in these dimensions has also been tested experimentally (see, e.g., Suetens 2005, Barreda-Tarrazona, García-Gallego et al. 2011). It is not covered by this meta-study, though, since the literature on alternative dependent variables is much less mature. Therefore meta-findings are still less reliable.

D. Weighted Estimation

In the meta-study, the normalized mean for every treatment is taken as one data point. There are a total of 657 such observations. These observations do not only differ with respect to the characteristics of the game. Arguably individual observations are also differently reliable. Ideally, one would want to weigh individual observations by the inverse of the normalised standard error. Yet unfortunately reporting standards in this literature are fairly diverse. Not so rarely, standard deviations or standard errors are not reported at all. By using the approach, one would lose a considerable amount of data. More importantly even, even if standard deviations are reported, it is often not clear whether this is a measure of the variance of the raw data, or whether dependence is taken into account. Dependence problems are pervasive in this literature since competition occurs by definition within groups. The larger the group (i.e. the market), the smaller the number of independent observations. It often is further reduced by the fact that one and the same group of participants is tested on more than one treatment. Obser-

vations from such a "within subjects" design are not independent from each other. Finally it is customary in this literature to repeat the game multiple times, and many papers have reshuffled groups from period to period. Then the larger group from which smaller groups are recomposed is the highest level of dependence. Not so rarely, experimenters have not even formed matching groups. Then a whole session is (at most) a single observation. This paper therefore uses a more conservative approach and weighs the data by the number of independent observations.

E. Dependence within Experiments?

One may hold different views about a final possible source of dependence. Treatment effects are the hallmark of experimental evidence. One achieves identification by randomly assigning participants to treatments. The very purpose of randomisation is guaranteeing independence. The treatment effect results from the manipulation, and from nothing else. Strictly speaking, the fact that one and the same experiment has had multiple treatments does therefore not create dependence. Yet meta data aims at comparing results across experiments. One may wonder whether there is some form of coherence within an experiment that is absent across experiments. It could in particular result from unobserved (or uncoded) features of the entire experiment that are relevant for the outcome. For instance the subject pool of one lab may be more prone to collusion than the subject pool of other labs.

If one wants to take this possibility into account, one needs a statistical procedure that allows for some form of correlation between the standard errors of data taken from one and the same experiment. Normally, such higher level dependence is taken into account by a fixed or random effects model. Yet a fixed or random effect would implicitly attach the same weight to each data point, and would therefore run counter the decision to weigh the data by the number of independent observations in the respective treatment.[8] The same concern is not present if one clusters standard errors at the level of experiments.

While clustering standard errors in this way is more cautious, it comes at a substantial price. One not only reduces statistical power (instead of 657, one only uses 140 independent observations). One interprets all within experiment variance as noise. One therefore loses what arguably is the best evidence for the effects of interest: when experimenters have explicitly manipulated the factor in question. With clustering, one only explains the variance in the dependent variable of such experiments that have fixed, but not manipulated this factor. Consequently, there is no right or wrong decision regarding clustering. Throughout the paper, both estimates are presented in parallel: with and without clustering standard errors.

8 This is why official Stata does not offer fixed or random effects estimation for weighted data (in Stata lingo: aweights are not available with the xtreg or mixed commands).

F. Univariate vs. Multivariate Analysis

In principle, there are two different ways of using this evidence. In a multiple regression, the impact of each independent variable on the dependent variable would be assessed, conditional on the effect of the remaining independent variables. This statistical approach will be used when exploiting the meta-study for making a contribution to the *Airtours* case. Yet the guidelines treat each factor separately. This should be mirrored by the analysis of the experimental evidence. Therefore the section of this paper that evaluates the merger guidelines uses univariate analysis.

IV. Evaluating the Merger Guidelines

Some factors listed in the US or EU merger guidelines do not have a direct analogue in the experimental literature. For instance, no experiment has directly tested the following test proposed in the US Merger Guidelines (#7.2): “if sales are small and frequent rather than via occasional large and long-term contracts” there is less reason to be concerned about coordinated effects. Other effects cannot meaningfully be entered into the meta-study because there is only a single experiment, as for firms being a "maverick" (US Merger Guidelines 2010, #2.1.5; EU Merger Guidelines, #42), i.e. being aggressive even if this results in a reduction of its profits (Engel and Ockenfels 2014). Yet other factors cannot be sufficiently standardised to be integrated into the meta-study. For instance, the antitrust authorities are more critical if there is evidence that market participants have already been engaged in collusion in the past (US Merger Guidelines 2010, #7.2; EU Merger Guidelines, #43). In a way, this is mirrored by time series information. Experimenters report how collusion develops over several rounds of repetition. In this meta-study, however, only the mean degree of collusion over all rounds of interaction is taken into account, chiefly because the raw data for mean collusion over time is normally not made available in numeric form. Many factors checked by the antitrust authorities are, however, also tested experimentally. The experimental evidence shows how much collusion is to be expected if one of the tested factors is present (1). And there is data on factors that are relevant for the degree of collusion in experiments, but that are not explicitly listed in the merger guidelines (2). Most importantly, by using marginal effects, the factors taken into account in predicting future collusion may be ranked (3). In all these ways, the merger guidelines may be checked back against an additional source of evidence, and they may be improved in light of this.

A. Factors Listed in the Merger Guidelines

1. Number of Suppliers

Merger control prevents the number of suppliers in a market from being reduced. All experiments had to specify this number. “The reduction in the number of firms in a market may, in itself, be a factor that facilitates coordination” (EU Merger Guidelines, #42). “The firm is

more likely to anticipate strong responses if there are few significant competitors" (US Merger Guidelines 2010, #7.2). Experimental findings support this view, but are not totally straightforward. As Figure 2 shows, collusion is highest in a duopoly. But collusion only continuously decreases from markets of 2 to markets of 3 to markets of 4. The more erratic mean values for larger markets likely result from the fact that testing large market is costly, which is why those means only represent small numbers of treatments.[9]

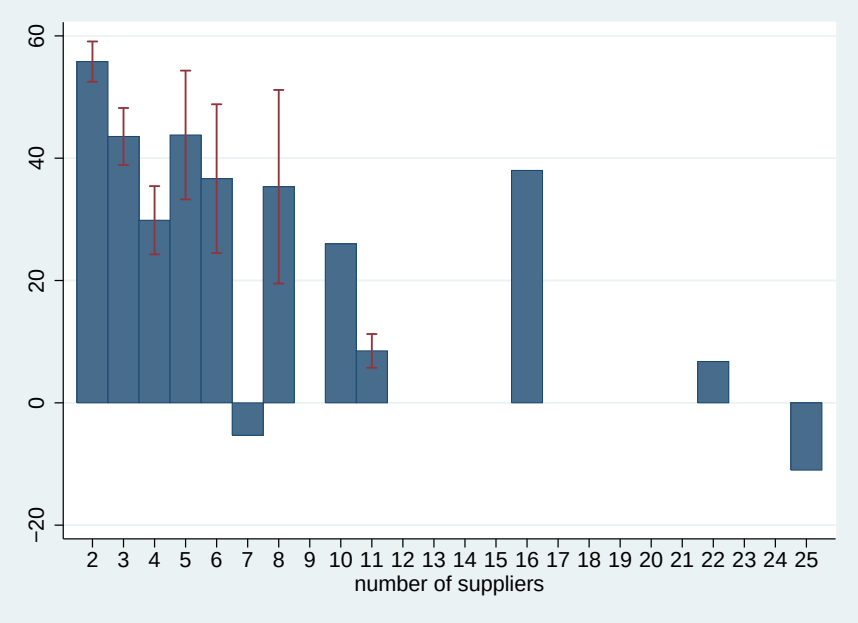

Figure 2
Effect of Number of Suppliers on Collusion

data weighted by number of independent observations
error bars: 95% confidence interval for two-sided test
remaining error bars are suppressed to keep the graph readable

If one treats the number of suppliers as a continuous variable, it is highly significant and has the expected negative effect (Table 2 Model 1). Significance is not affected if one uses the more conEervative statistical approach and clusters standard errors at the level of publications (Model 2). In a duopoly (the reference category), the model predicts 66.835% collusion. With every additional supplier, the model predicts a reduction in collusion by 6.614%. If, instead, one treats the number of suppliers as a categorical variable, results are less clear. One only finds a significant reduction in collusion when moving from a duopoly to a triopoly if one does not cluster standard errors. Collusion is predicted to decrease by 12.243%. The difference between duopoly and quadropoly is clearcut, but with a yet higher number of suppliers the picture becomes again less clear.

9 There are 5 datapoints from markets with 7 suppliers, 12 from markets with 8 suppliers, 1 from a market with 10 suppliers, 4 from markets with 11 suppliers, 2 from a market with 16 suppliers, 4 from a market with 22 suppliers, and 2 from a market with 25 suppliers.

	model 1 weighted	model 2 weighted and clustered	model 3 weighted	model 4 weighted and clustered
#suppliers	-6.614*** (.913)	-6.614** (2.050)		
#3			-12.243*** (3.218)	-12.243 (8.011)
#4			-25.936*** (3.428)	-25.936** (8.294)
#5			-12.001 (7.813)	-12.001 (8.844)
#6			-19.143^{+} (9.824)	-19.143^{+} (11.156)
#7			-61.093*** (16.901)	-61.093*** (8.083)
#8			-20.460 (17.809)	-20.460* (7.809)
#10			-29.793 (53.281)	-29.793*** (7.077)
#11			-47.293* (23.861)	-47.293*** (7.077)
#16			-17.793 (53.281)	-17.793 (17.846)
#22			-49.043 (75.339)	-49.043*** (7.077)
#25			-66.793 (53.281)	-66.793*** (7.077)
cons	66.835*** (2.663)	66.835*** (9.348)	55.793*** (1.393)	55.793*** (1.393)
N	657	657	657	657
N cluster		140		140
R^2	.0742	.0742	.1131	.1131

Table 2
Effect of Number of Suppliers on Collusion

OLS, weighted by the number of independent observations
models 2 and 4: standard errors clustered at the level of publications
models 1-2 treat number of suppliers as a continuous variable
models 3-4 treat number of suppliers as categorical variables
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

2. Product Homogeneity

"It is [...] easier to coordinate on a price for a single, homogeneous product" (EU Merger Guidelines, #45; cf. US Merger Guidelines 2010, #7.2). If collusion is measured as the relative deviation from the Walrasian equilibrium, theory would expect the opposite, i.e. a smaller deviation when products are homogeneous. Heterogeneity leads to monopolistic competition (Chamberlin 1933). This is also what one finds in experiment, as shown in Table 3. Note that experiments control homogeneity very precisely. Experimental suppliers offer an absolutely identical product, mathematically defined by the demand function. If products are heteroge-

neous, experimenters specify the substitution relationship by a parameter in either demand function indicating how demand for one product reacts to increases in the price of the substitute. If products are complements (so that more demand for product A increases demand for product B), in the lab collusion reaches a very high level.

	model 1 weighted	model 2 weighted and clustered
substitute	19.025*** (3.139)	19.025** (7.100)
complement	79.125*** (13.414)	79.125*** (4.469)
cons	45.875*** (1.247)	45.875*** (4.469)
N	657	657
N cluster		140
R^2	.0954	.0954

Table 3
Product Homogeneity

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, ** p < .01

3. Symmetry

"Firms may find it easier to reach a common understanding on the terms of coordination if they are relatively symmetric" (EU Merger Guidelines, #48; cf. US Merger Guidelines 2010, #7.2). Experiments do not provide support for this claim, Table 4.

	model 1 weighted	model 2 weighted and clustered
asymmetry	-1.541 (3.079)	-1.541 (7.527)
cons	49.720*** (1.328)	49.720*** (5.043)
N	657	657
N cluster		140
R^2	.0004	.0004

Table 4
Symmetry

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001

4. Transparency

"A market typically is more vulnerable to coordinated conduct if each competitively important firm's significant competitive initiatives can be promptly and confidently observed by that firm's rivals. This is more likely to be the case if the terms offered to customers are relatively transparent." (US Merger Guidelines 2010, #7.2). "If key information about specific transactions or individual price or output levels is available routinely to competitors, it may be difficult for a firm to deviate secretly" (US Merger Guidelines 1992, #2.12; cf. EU Merger Guidelines, #49). Experimenters have interpreted this in a more precise way. They have split this up, and separately checked the effect of ex ante information, and of feedback. Both only have a significant effect on collusion if suppliers know little about their competitor, and less than the precise shape of the demand function and their own past profit (which is coded as partial information), Table 5. If one clusters standard errors at the level of publications, there is no significant effect.

	model 1 weighted	model 2 weighted and clustered	model 3 weighted	model 4 weighted and clustered
partial ex ante information	-3.646 (4.169)	-3.646 (8.247)		
reduced ex ante information	-13.978*** (3.457)	-13.978 (10.301)		
partial feedback			-3.113 (2.936)	-3.113 (7.202)
reduced feedback			-10.170* (4.883)	-10.170 (15.908)
cons	51.699*** (1.348)	51.699*** (5.464)	50.767*** (1.408)	50.767*** (5.719)
N	657	657	657	657
N cluster		140		140
R^2	.0246	.0246	.0075	.0075

Table 5
Transparency

OLS, weighted by the number of independent observations
model 2 and 4: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, * p < .05

5. Gains from Collusion

"The Agencies regard coordinated interaction as more likely, the more the participants stand to gain from successful coordination. Coordination generally is more profitable, the lower is the market elasticity of demand." (US Merger Guidelines 2010, #7.2).[10] In the lab, potential gains from collusion can be precisely manipulated. Experimentalists have used two parame-

10 Motive to collude is also sometimes used a criterion for inferring a violation of the prohibition of conspiracy in restraint of trade, in the sense of 15 U.S.C. § 1, *Sherman Act*, e.g. *Interstate Circuit v. United States*, 306 U.S. 208 (1939) 222. From recent years, cf. *U.S. v. Apple*, 12 Civ. 2826 (DLC) 148.

ters for the purpose. They first have chosen between demand that is completely inelastic and demand that decreases in price, or increases in quantity. This echoes a concern of the US Merger Guidelines (#7.2): "collective market power is greater, the lower is the market elasticity of demand". As Models 1 and 2 of Table 6 demonstrates, in and of itself low elasticity of demand does not significantly explain collusion. The second parameter that has been manipulated is the elasticity of supply. In many experiments, production cost is constant, or it is even normalised to 0. In other experiments, production cost increases in quantity. If production cost is constant, collusion significantly and strongly increases (Models 3 and 4). In such a market, gains from collusion are substantial. Actually, without collusion suppliers make zero profit. As Model 3 demonstrates, in the lab the elasticity of demand only matters if supply is elastic as well. Yet the effect is only weakly significant, and it disappears if one clusters standard errors at the level of publications (Model 4).

	model 1 weighted	model 2 weighted and clustered	model 3 weighted	model 4 weighted and clustered
elastic demand	4.154 (3.399)	4.154 (6.767)	6.395^{+} (3.359)	6.395 (7.587)
inelastic supply			17.399*** (3.303)	17.399* (8.053)
cons	45.882*** (3.143)	45.882*** (4.594)	29.195*** (4.419)	29.195** (9.046)
N	657	657	657	657
N cluster		140		140
R^2	.0023	.0023	.0924	.0924

Table 6
Gains from Collusion

OLS, weighted by the number of independent observations
model 2 and 4: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

6. Innovation

"Firms are also less likely to be deterred [from making competitive initiatives] by whatever responses occur if competition in the relevant market is marked by leapfrogging technological innovation, so that responses by competitors leave the gains from successful innovation largely intact" (US Merger Guidelines 2010, #7.2). "In markets where innovation is important, coordination may be more difficult" (EU Merger Guidelines, #45).

Treatments that have given participants the option to invest in cost reductions or product differentiation have not been very frequent; 23 out of 657 treatments have made this possible. This unbalanced character of the sample may be the reason why none of the models in Table 7 finds a significant main effect of the opportunity to innovate. Yet if one controls for product

homogeneity and adds an interaction term,[11] one finds a significant negative interaction. From a policy perspective, the marginal effect of innovation conditional on products being substitutes is even more relevant. It is -20.719 and weakly significant if one weights the data by the number of observations (Model 3, p = .056); it is significant at conventional levels if one further clusters standard errors at the level of publications (Model 4, p = .008). Provided products are substitutes, and hence suppliers are partly protected from competition, the innovation opportunity increases competition.

	model 1 weighted	model 2 weighted and clustered	model 3 weighted	model 4 weighted and clustered
innovation	4.403 (6.924)	4.403 (9.702)	13.146 (8.362)	13.146 (12.188)
substitute			20.903*** (3.242)	20.903** (7.376)
complement			79.423*** (13.373)	79.423*** (4.530)
innovation* substitute			-33.864*** (13.670)	-33.864** (14.421)
cons	49.298*** (1.217)	49.298*** (4.364)	45.577*** (1.258)	45.577*** (4.530)
N	657	657	657	657
N cluster		140		140
R^2	.0006	.0006	.1038	.1038

Table 7
Innovation

OLS, weighted by the number of independent observations
model 2 and 4: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, ** p < .01

7. Stability

"It is easier to coordinate on a price when demand and supply conditions are relatively stable than when they are continuously changing" (EU Merger Guidelines, #45). The experiments have not tested stability directly. However there is a proxy in the number of rounds that experimental subjects have interacted. As Models 1 and 2 of Table 8 show, in isolation this proxy does not explain the data. This changes if one controls for the number of suppliers (Model 3). Specifically if one controls for the fact that there are more than 2 suppliers in the market, one finds a significant negative effect of the proxy for stability. The marginal effect of this proxy is insignificant for larger markets. Hence the only finding is in opposition to the expectations of the EU merger guidelines. In a duopoly market, there is less, not more collusion if circumstances are more stable. Yet this effect disappears if one clusters standard errors at the level of publications (Model 4).

11 The experiments that have tested complements have not given an innovation opportunity, which is why this interaction cannot be tested.

	model 1 weighted	model 2 weighted and clustered	model 3 weighted	model 4 weighted and clustered
#rounds	-.040 (.054)	-.040 (.108)	-.143* (.068)	-.143 (.172)
more than 2 suppliers			-25.396*** (4.040)	-25.396*** (6.622)
#round * more than 2 suppliers			.202^{+} (.105)	.202 (.187)
cons	50.682*** (2.076)	50.682*** (4.033)	60.355*** (2.587)	60.355*** (5.252)
N	657	657	657	657
N cluster		140		140
R^2	.0008	.0008	.0928	.0928

Table 8
Stability

OLS, weighted by the number of independent observations
model 2 and 4: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, * p < .05, $^{+}$ p < .1

8. Price Matching Guarantee

"A market typically is more vulnerable to coordinated conduct if a firm's prospective competitive reward from attracting customers away from its rivals will be significantly diminished by likely responses of those rivals. […] The firm is more likely to anticipate strong responses if […] suppliers use meeting-competition clauses" (US Merger Guidelines 2010, #7.2). This expectation is fully borne out by the experimental data, Table 9. In the presence of a price matching guarantee, collusion doubles.

	model 1 weighted	model 2 weighted and clustered
price matching guarantee	48.129*** (6.953)	48.129*** (6.185)
cons	48.063*** (1.174)	48.063*** (4.152)
N	657	657
N cluster		140
R^2	.0682	.0682

Table 9
Price Matching Guarantee

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001

B. Factors Not Explicitly Discussed in the Guidelines

Oligopoly experiments have manipulated further context factors that are at least not explicitly discussed in the US or European merger guidelines. Some of these variables may be of lesser importance outside the lab. For instance, merging firms will almost certainly have market experience (see below 0). Others, like communication opportunities (see below 0), are addressed in the case law on the prohibition of conspiracy in § 1 Sherman Act (see e.g. *U.S. v. Airline Traffic Publishing Co.*, 836 F.Supp. 9, 12). This section of the paper is therefore not meant as a critique of the guidelines, but as supplementary material that may help the authorities find and evaluate potential aggravating or attenuating factors.

1. Communication

A first variable that experimenters always have to define and often deliberately manipulate is communication. The standard protocol confines communication to feedback about other suppliers' choices in previous periods. If participants are additionally allowed to communicate price or quantity choices, collusion increases substantially and significantly, Table 10. In an indirect way, a price matching guarantee has the same effect, only that the reaction to information about price or quantity is automatic. This scheme increases collusion even more dramatically. The effect of communicating investment choices is less clear. It disappears if one clusters standard errors at the level of publications. Yet this effect is also sizeable. The same holds if the content of communication is not constrained. Surprisingly, the same effect cannot be found if face-to-face communication is allowed.

	model 1 weighted	model 2 weighted and clustered
communication about strategic variable	15.458** (5.457)	15.458** (5.575)
price matching guarantee	49.468*** (6.887)	49.468*** (6.343)
communication about investment decision	22.728* (10.581)	22.728 (14.938)
free form communication	24.550* (9.702)	24.550*** (4.459)
face-to-face communication	-2.358 (10.026)	-2.358 (11.397)
cons	46.724*** (1.216)	46.724*** (4.375)
N	657	657
N cluster		140
R^2	.0937	.0937

Table 10
Communication

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05

2. *Agreement*

Collusion also strongly and significantly increases if participants are allowed to agree on market behaviour, even if such agreements cannot be enforced, Table 11.

	model 1 weighted	model 2 weighted and clustered
agreement	23.072*** (6.057)	23.072** (8.677)
cons	48.514*** (1.210)	48.514*** (4.264)
N	657	657
N cluster		140
R^2	.0088	.0088

Table 11
Agreement

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, ** p < .01

3. *Enforcement*

Unsurprisingly, the effect is even stronger if this agreement can actually be enforced, Table 12.

	model 1 weighted	model 2 weighted and clustered
enforcement	26.529* (10.769)	26.529*** (4.680)
cons	49.104*** (1.200)	49.104*** (4.271)
N	657	657
N cluster		140
R^2	.0092	.0092

Table 12
Enforcement

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, * p < .05

4. *Strategic Variable*

In theory, and in the experimental lab, the strategic variable is fixed. Suppliers either use price as the strategic variable ("Bertrand competition"), or quantity ("Cournot competition"). In theory, this makes a huge difference. The difference is the larger, the smaller the market. If suppliers compete in price, and if marginal cost is constant, the Walrasian and the Nash equilibrium coincide (Bertrand 1883). Although collusion would be profitable, suppliers are una-

ble to bring it about since they are pitted against each other in a prisoner's dilemma. If they compete in quantity, however, the non-cooperative solution is bounded away from the point where the market clears. In duopoly, the difference between the Nash and the Walrasian equilibrium is largest (Cournot 1838).

Yet in the lab, if at all one finds the opposite result, Table 13 Model 1. When experimental participants compete in quantity, they succeed less to collude than if they compete in price. Yet this effect does not survive if one clusters standard errors at the level of publications.

	model 1 weighted	model 2 weighted and clustered
quantity competition	-8.011** (2.377)	-8.011 (7.788)
cons	53.357*** (1.663)	53.357*** (3.797)
N	657	657
N cluster		140
R^2	.0171	.0171

Table 13
Strategic Variable

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, ** p < .01

5. Capacity Constraint and Production

The EU Guidelines allude to the point: "Coordination may take various forms. In some markets, the most likely coordination may involve keeping prices above the competitive level. In other markets, coordination may aim at limiting production or the amount of new capacity brought to the market" (EU Merger Guidelines, #40), without making much out of the difference. This reticence is probably due to the difficulty of saying in practice which is the strategic variable.

In principle, there is a good proxy. If extending capacity is not feasible in the short run, or if this is prohibitively costly, aggressive pricing does not pay. Price cuts attract more demand, but the supplier is unable to exploit the opportunity to her advantage (Kreps and Scheinkman 1983). Consequently, in such markets, competition must focus on quantity. The antitrust authorities will often be in a position to determine whether the cost of extending capacity is pronounced. Yet the experimental evidence casts doubt on this prediction. With constrained capacity, experimental subjects stay much closer to the Walrasian equilibrium, Table 14 Models 1 and 2. This is the opposite of the expectation resulting from the Kreps/Scheinkman model. In principle, advance production could have a similar commitment effect. In the lab, one indeed finds a reduction in collusion, but it is only significant if one does not cluster standard errors at the level of publications, Table 14 Models 3 and 4.

	model 1 weighted	model 2 weighted and clustered	model 3 weighted	model 4 weighted and clustered
capacity constraint	-16.789*** (3.437)	-16.789* (7.829)		
advance production			-11.874* (4.922)	-11.874 (7.393)
cons	51.712*** (1.266)	51.712*** (5.062)	50.178*** (1.232)	50.178*** (4.570)
N	657	657	657	657
N cluster		140		140
R^2	.0337	.0337	.0088	.0088

Table 14
Capacity Constraint and Advance Production

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, * p < .05

6. Fixed Cost

In the short run, the amount of fixed cost required for serving the market should not matter for the intensity of competition; it is "sunk". Apparently this is not how experimental participants see this. They collude substantially more, and the effect is significant as long as one does not cluster standard errors at the level of publications, Table 15.

	model 1 weighted	model 2 weighted and clustered
fixed cost	10.683* (3.904)	10.683 (8.483)
cons	48.324*** (1.259)	48.324*** (4.529)
N	657	657
N cluster		140
R^2	.0113	.0113

Table 15
Fixed Cost

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, * p < .05

7. Sequential Interaction

In narrow markets, it happens that one seller has a chance to commit to quantity or price, in advance of her competitors. If products are homogeneous, theory then predicts a smaller deviation from the Walrasian equilibrium if firms compete in quantity (Stackelberg 1934). If they compete in price and if marginal cost is constant, there should be no deviation from the Walrasian equilibrium. Since, in this setting, sellers are playing a prisoner's dilemma, the switch to sequential interaction is irrelevant, at least in the stage game, which is the unit of reference for all experimental findings. This is due to the fact that defection (i.e. setting the market clearing price) is a dominant strategy for either of them.

Experiments do not support these theoretical expectations. If one only explains the data with the fact that the competitors do not choose their strategic variable simultaneously, one does not find a significant effect, Table 16 Models 1 and 2. This does not change if one controls for or interacts with the strategic variable.[12] One does, however, find a significant effect if one interacts sequentiality with the fact that products are substitutes. One then finds that, for homogeneous products, sequential interaction leads to more collusion. The effect reverses if products are substitutes.[13] Yet these effects are only significant if one does not cluster standard errors at the level of publications, Table 16 Models 3 and 4.

	model 1 weighted	model 2 weighted and clustered	model 3 weighted	model 4 weighted and clustered
sequential	5.785 (4.440)	5.785 (10.075)	12.161** (4.543)	12.161 (10.498)
substitute			21.863*** (3.236)	21.863** (7.062)
complement			80.107*** (13.312)	80.107*** (4.653)
sequential* substitute			-37.718** (11.908)	-37.718** (22.805)
cons	48.978*** (1.247)	48.978*** (4.493)	44.893*** (1.291)	44.893*** (4.653)
N	657	657	657	657
N cluster		140		140
R^2	.0026	.0026	.1125	.1125

Table 16
Sequential Interaction

OLS, weighted by the number of independent observations
model 2 and 4: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05

12 These additional regressions are available upon request.
13 The marginal effect of sequentiality is -25.557, p = .021 for substitutes.

8. *Power*

Symmetry addresses relative gains from collusion, whereas power says something about bargaining weights when it comes to (implicit) negotiations about how to split these gains. In experiments, power is typically implemented by giving some, but not all, sellers inframarginal units. These sellers then can exert influence on their competitors by withholding supply. As Models 1 and 2 of Table 17 show, in and of itself this does not explain collusion. Yet the picture clears if one controls for, and interacts with, the number of suppliers. In a duopoly market, collusion increases if one of the suppliers holds power. The opposite holds true in larger markets.[14]

	model 1 weighted	model 2 weighted and clustered	model 3 weighted	model 4 weighted and clustered
power	-9.579 (6.867)	-9.579 (15.020)	18.915^{+} (11.123)	18.915* (9.464)
more than two firms			-17.809*** (2.481)	-17.809* (7.659)
power* more than two firms			-34.228* (13.824)	-34.228^{+} (19.468)
cons	49.734*** (1.252)	49.734*** (4.397)	55.489*** (1.410)	55.489*** (7.096)
N	657	657	657	657
N cluster		140		140
R^2	.0030	.0030	.0947	.0947

Table 17
Power

OLS, weighted by the number of independent observations
model 2 and 4: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, * p < .05, $^{+}$ p < .1

9. *Trading Institution*

The majority of oligopoly experiments uses the posted offer institution. It mirrors consumer choice in a department store. Each seller is free to post a price. Buyers shop around, or efficient rationing does the shopping for them. With this trading institution, buyers are almost passive. The only choice they have is not to buy at all. Other experiments have made buyers more active, but introducing explicit negotiations, or even some form of auction. The regressions in Table 18 show that the choice of institution has a strong effect on the degree of collusion. It is robust to clustering standard errors at the level of publications.

14 The marginal effect of power is weakly significant in a duopoly market (model 3: coef = 18.915, p = .089), and significant at conventional levels if one clusters standard errors at the level of publications (p = .048). The marginal effect is negative in larger markets (model 3: coef = -15.312, p = .063), but only (weakly) significant if one does not cluster standard errors.

	model 1 weighted	model 2 weighted and clustered
posted offer	36.218*** (6.091)	36.218*** (7.431)
cons	14.599* (5.974)	14.599* (5.894)
N	657	657
N cluster		140
R^2	.0512	.0512

Table 18
Trading Institution

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, * p < .05

10. Seller Characteristics

Seller characteristics also play a role. In isolation, the fact that participants had experience with this specific or other market experiments does not explain the data, Table 19 Models 1 and 2. One finds a weakly significant negative effect of experience, though, if one controls for further seller characteristics. More important is, however, that sellers who have to decide jointly as a group collude substantially less. Collusion also goes down quite a bit if experimental participants face computer competitors who play the equilibrium. If one does not cluster standard errors at the level of publications, one also finds that managers of real firms collude more.

	model 1 weighted	model 2 weighted and clustered	model 3 weighted	model 4 weighted and clustered
experience	1.230 (5.502)	1.230 (10.443)	-11.153^{+} (6.321)	-11.153 (13.716)
developing country			1.569 (8.013)	1.569 (4.878)
manager			20.962* (10.445)	20.962 (13.909)
group			-21.039** (7.246)	-21.039** (7.964)
computer			-32.778*** (3.333)	-32.778*** (2.773)
cons	49.372*** (1.229)	49.372*** (4.426)	54.385*** (1.265)	54.385*** (2.759)
N	657	657	657	657
N cluster		140		140
R^2	.0001	.0001	.1407	.1407

Table 19
Seller Characteristics

OLS, weighted by the number of independent observations
model 2 and 4: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, * p < .05, $^{+}$ p < .1

11. Active Demand

Finally, if demand comes from other human participants, not from a computer program, collusion substantially and significantly decreases, Table 20.

	model 1 weighted	model 2 weighted and clustered
active demand	-22.229** (7.067)	-22.229^{+} (11.346)
cons	50.082*** (1.207)	50.082*** (4.407)
N	657	657
N cluster		140
R^2	.0149	.0149

Table 20
Active Demand

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, ** p < .01, $^{+}$ p < .1

C. Relative Weight

The experimental evidence does not only point antitrust practice to additional factors that make the risk of collusion more serious. Its most valuable contribution is quantitative. It is possible to estimate the relative weight of all factors that increase or decrease the risk of collusion. The higher the positive weight, the more antitrust authorities should be attentive to the presence of the respective factor. The higher the negative weight, the more antitrust authorities should be attentive to the absence of the respective factor. To that end, Figure 3 collects marginal effects from the previous estimations.

According to the experimental evidence, by far the most important source of collusion is the fact that the products of two firms are complements. This increases collusion by 79.125%. A price matching guarantee also has a big effect. Collusion increases by 49.468%. A further highly relevant factor is the constitution of the demand side of the market. If the only option for buyers who dislike the offer is not to buy the good at all (posted offer), collusion increases by 36.218%. By contrast collusion decreases by 22.229% if the demand side of the market is represented by real human subjects. Free form communication (24.55%), the possibility to conclude a non-binding agreement (23.072%) or even an enforceable contract (26.529%) highly increase collusion. Other forms of communication, regarding investment choices (22.728%) or the chosen price or quantity 15.458%), also increase collusion. Collusion is also more pronounced if students are replaced by real managers (20.962%). If products are substitutes, experimental participants also collude more (19.024%). In the lab, the most important element of the opportunity structure is the supply function. If marginal cost is constant, this increases collusion by 17.399%. The fact that one participant may move first increases collu-

sion by 12.161%. If there is a fixed cost of production, collusion increases by 10.683%. Finally if demand is elastic, somewhat surprisingly experimental participants collude a bit more (6.395%).

On the negative side, the most important factor is the market behaviour of competitors. If they are partly represented by computers who behave optimally, this strongly reduces collusion (-32.778%). This suggests that antitrust authorities are rightly concerned about the fact that a so-called maverick, i.e. a particularly aggressive competitor, is removed from the market (US Merger Guidelines 2010, #2.1.5). On real markets, price or quantity is rarely chosen by an isolated individual. In the lab, if firms are represented by groups, this substantially reduces collusion (by 21.039%). If capacity is constrained (-16.789%), goods are produced in advance (-11.874%), firms compete in quantity rather than price (-8.011%), all of this reduces collusion. The collusion rate also drops if experimental participants know little about each other, be that before they choose that competitive strategy (-13.937%) or afterwards (-10.170%). Finally participants who have experience with this kind of experimental situation collude less (-11.153%).

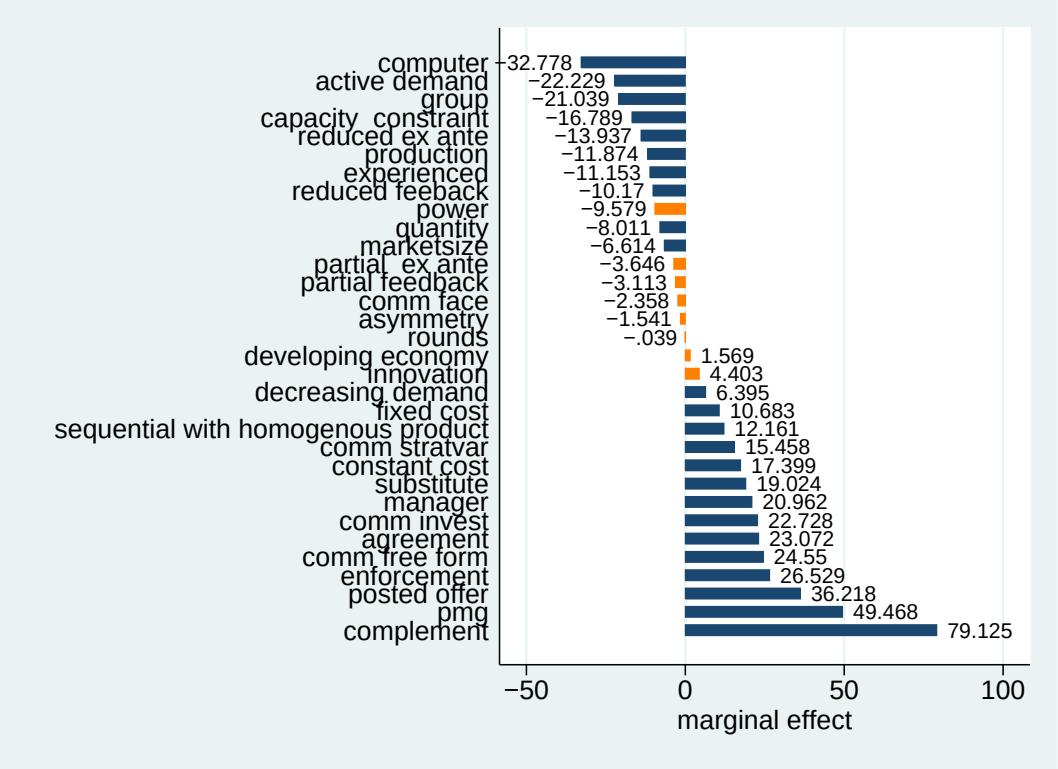

Figure 3
Relative Weight

marginal effects from Tables 2-20
blue bars: effect is (at least weakly) significant; orange bars: effect is insignificant

V. Deciding Individual Cases: *Airtours*

The experimental evidence is thus directly relevant for double checking and improving the merger guidelines on both sides of the Atlantic. It is less easy, but still useful, to rely on this evidence when deciding individual cases. The possibilities, and the limits, of this shall be illustrated with respect to the European landmark case, the *Airtours* case.[15] To that end, the facts of the case are summarised (1). There is no experiment with exactly these features. But in more indirect ways, the experimental evidence can nonetheless help the antitrust authorities in making better decisions (2).

A. Facts

The European Commission had defined the relevant market to be the English market for short-haul foreign package holiday (#4). Since no package holiday is exactly like another, in terms of the experimental evidence the product was (more or less) heterogeneous. At the time of the decision, there were four large providers. In the Commission's numbers, these four providers controlled 79% of sales (#66). The remaining market participants were not only small. They also lacked vertical integration with airlines and travel agents (#66). It is therefore defendable to consider this as a market with four (relevant) suppliers. In the short run, capacity was fixed. It had to be contracted one and a half years in advance (#80, #158), meaning that providers competed in quantity. Demand was relatively volatile (#140), but had no countervailing power (#121); in terms of the experimental evidence, this is a hint that demand was rather active than passive. Several issues were in dispute. How easy was it for providers to observe capacity decisions of their competitors (#180)? If not, in terms of the experimental evidence, feedback would have been partial, if not reduced. Was retaliation feasible, should one firm ignore tacit collusion (#183)? If so, those able to retaliate could come near to what experimenters mean by power; typically they implement it by giving some players inframarginal units. Finally, it was contested whether barriers to entry were high (#208).

Airtours merged with *First Choice*. The new firm controlled 32% of the market, with the remaining large providers controlling 27% and 20%, respectively. This makes for a Herfindahl-Hirschman Index (HHI) of 2153. In the 1992 Guidelines, an HHI >1800 was regarded to be very critical (US Merger Guidelines 1992, #1.5). The new guidelines regard markets with an HHI between 1500 and 2500 as “moderately concentrated” (US Merger Guidelines 2010, #5.3).

15 For the reference, see note 3 above.

B. Indirect Experimental Evidence

Although there is a total of 657 published experimental treatments on oligopoly, none of them has exactly the features of the *Airtours* case. The most important limitation, however, is statistical. For improving the guidelines, it was enough to show the correlation between a certain characteristic of an oligopolistic market and the degree of collusion. If this characteristic increases collusion in experimental markets, the antitrust authorities should be alerted. If it decreases collusion, the presence of this factor might induce them to become more lenient. Concrete cases, however, are characterised by specific combinations of factors. To fully test these combinations experimentally, one would have to check multiple interactions. In the *Airtours* case, a 7 x 7 interaction would be necessary, covering the number of suppliers, the strategic variable, computer versus human buyers (as a proxy for passive versus active demand), homogeneous vs. heterogeneous products, the presence versus the absence of power for some sellers, the degree of feedback, and market entry. To test this multiple interaction for significance, one would need a huge samples, much larger than the 657 observations available in the meta study.

A way out is multiple regression. If one is able to establish an interaction effect, one is in a position to predict the exact degree of collusion, given a specific set of features is combined. Multiple regression is less ambitious. It "partials out" the effect of other independent variables, and checks whether the effect of the variable in question remains significant. If so, one may be confident that the main effect in question is still present, once one has "controlled for" the other variables. One predicts the main effect, once the potentially disturbing other independent variables have been neutralised. Based on the existing experimental evidence, the antitrust authorities are thus not able to predict the concrete degree of collusion. But they are able to say, in relative terms, by how much collusion is likely to increase, were the merger to become effective. The regressions in Table 21 show that this effect is substantial: collusion can be expected to increase by more than 20%. This estimate is robust to clustering standard errors at the level of publications, which means that the finding is very reliable.

	model 1 weigthed	model 2 weighted and clustered
less than 4 suppliers	20.354*** (3.025)	20.354*** (5.090)
quantity competition	-5.545 (2.474)	-5.545 (7.891)
active demand	-10.142 (7.004)	-10.142 (9.686)
substitute	15.682*** (3.293)	15.682* (6.765)
complement	71.525*** (6.464)	71.525*** (5.114)
power	-1.976 (12.874)	-1.976 (12.874)
partial feedback	-1.162 (2.811)	-1.162 (5.515)
reduced feedback	-2.998 (4.786)	-2.998 (10.566)
entry	20.840^{+} (12.548)	20.840* (7.741)
cons	33.121*** (3.299)	33.121*** (5.108)
N	657	657
N cluster		140
R^2	.1796	

Table 21
Experimental Factors Relevant for the *Airtours* Case

OLS, weighted by the number of independent observations
model 2: standard errors clustered at the level of publications
standard errors in parenthesis
*** p < .001, * p < .05, $^{+}$ p < .1

Given the inevitable limitations of the approach, the antitrust authorities would certainly not want to decide merger cases exclusively based on the experimental evidence. But the experimental findings provide additional support for the claim that the Commission should not have lost the case in court.

VI. Discussion

A. Publication Bias?

A pervasive problem with empirical evidence is publication bias. Significant findings are easier to publish. Even if a theoretical claim is usually not supported by the data, all the non-results risk ending up in the file drawer. Ideally, meta-analysis makes it possible to detect, maybe even to quantify, publication bias. If almost all published results are the same, or if almost all of them show the effect, this suggests a selection effect. At first sight one might suspect the experimental evidence on oligopoly to suffer from this bias. On average, the mean degree of collusion is 49.43%,[16] and hence far away from 0.

Yet for a number of reasons, this conclusion may not be drawn. This meta-study uses meta-regression. It organises a rich literature that covers a host of manipulations. The distribution of the dependent variable is therefore not conclusive evidence for publication bias. It may simply result from the distribution of the independent variables. Moreover this paper uses the proportional deviation from the Walrasian outcome as the dependent variable because this measure is most interesting for the normative goal of antitrust intervention. Yet for many experimental designs covered by this paper, the Walrasian outcome is not the theoretical prediction. Figure 4 shows that the definition of the dependent variable is indeed critical. If one uses the proportional deviation from the Walrasian outcome, the bulk of findings is in the interval between 0 and 100%. The average outcome of a treatment is rarely so competitive that suppliers offer a quantity that exceeds demand, or sell at a price so low that they make a loss. The average outcome is also rarely so restrictive that participants miss a monopoly profit. But if one translates these choices into proportional deviations from the equilibrium, results peak at 0, and are almost symmetrically distributed to the left and the right side. The mean is even slightly below 0 (-13.67%), while the median is slightly above 0 (3,00%).[17] This does not suggest a relevant publication bias.

16 For this calculation, the data has again been weighted by the number of independent observations.

17 For this calculation, the data has also been weighted by the number of independent observations. For this index, only 600 observations are available. For the remaining 57 treatments, this index cannot be calculated.

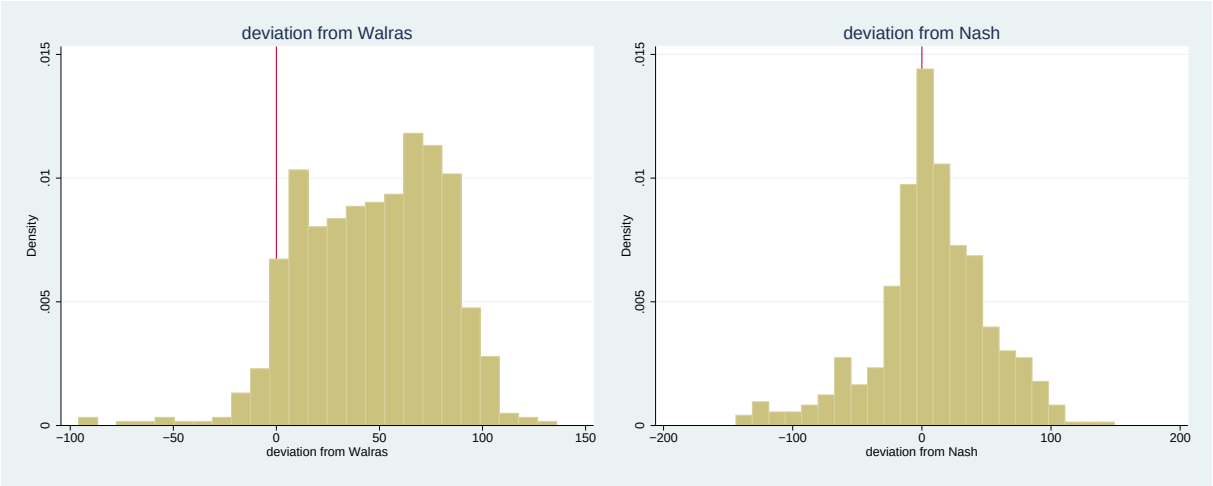

Figure 4
Deviation from Market Clearing and from the Nash Equilibrium

right panel: to increase readability, data is capped at -150 and +150

B. External Validity

Experiments are not meant to map reality. They are tools for identifying causal effects by randomly assigning participants to treatments. When they decide to challenge a merger, antitrust authorities have to face the reality of the merging firms, and the markets within which they compete. There is inevitably a gap between the much richer context to which anti-trust intervention reacts, and the clean conditions experimenters create in the interest of making sure that there is indeed only a single difference between their baseline and their treatment. Experimental evidence is never conclusive for the policy problems it aims to elucidate. If such evidence is available, it is always helpful to check for corroborating evidence from the field.

This evidence may be very different in kind. In recent years, field experiments on anti-trust matters have become more frequent (see, e.g., List and Price 2005). Field experiments randomly assign actual market participants to treatments. This is of course very informative, but not a panacea. One must study what one has a chance to investigate. And by going into the field, one inevitably gives up a certain degree of experimental control. Another supplementary source of evidence uses data from actual markets into which no scientist has artificially intervened (see, e.g. Levenstein and Suslow 2006). This is also very helpful, especially since it addresses the selection concern. Yet identifying causal effects with field data is challenging. Eventually one needs random variation, which introduces the selection problem through the backdoor: instead of studying reality untouched, one hunts for random shocks that have arguably not been anticipated by market participants. A related source of evidence is the ex post analysis of mergers that have or have not been challenged by the authorities (Kwoka and Greenfield 2013).

Ultimately, anti-trust authorities would be best served by not relying on any one of those sources in isolation, but to aim at finding converging evidence (for more discussion of the

complementary pros and cons of different empirical methods see Engel 2014). Evidence from lab experiments tends to be most remote from the phenomenon the authority wants to understand, but most reliable because identification is not an issue. This is why anti-trust authorities have increasingly become interested in this additional source of evidence (see in particular Hinloopen and Normann 2009). This paper is meant to give them structured access to one particularly rich body of experimental evidence.

This paper does not report the results from a single collusion experiment. It aims at organising the whole experimental literature on collusion by way of a meta-study. Hopefully, this is not only helpful for anti-trust authorities in that findings from different studies become comparable. In principle, one also learns which effects are more safely established than others, and which effects are more important than others. Yet for this additional service, there is a price to pay. One must be willing to compare all experiments from this literature on all dimensions. Other meta-studies have been criticised for overusing this approach (Chernev, Böckenholt et al. 2010). It is possible that, in one of the dimensions of interest, one does not find an effect just because the overall composition of the sample is such that positive and negative findings cancel out. Or the sample may be so unrepresentative in one dimension that the measured effect becomes spurious; this might explain the somewhat erratic results from markets with many suppliers (see above IV.A.1). Yet the findings should not suffer from this problem too severely. In many dimensions, the sample is relatively balanced. And additional analyses with more control variables could always be run.

VII. Concluding Remarks

Unbeknownst to each other, experimental economics and antitrust law have for decades been working on the same issue: under which conditions is tacit collusion likely in narrow oligopoly? This is a prognostic task. The antitrust authorities are bound to make mistakes. Currently, the authorities combine a bottom up with a top down approach. They use checklists that summarize earlier case experience. And they rely on game theory. As this paper shows, there is a solid, additional body of evidence. In cross-validating their estimations with the experimental evidence, the antitrust authorities are likely to reduce the error rate of merger control.

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