Case law in European merger control*

Johan Callermo†

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Abstract

This paper studies references to case law in merger control decisions by the EC Directorate General for Competition (DG COMP) in 1990-2022. I use the full set of references to Court of Justice (CJEU) judgments in DG COMP decisions to examine implementation trends, industry dynamics and effects of the 2004 merger control reform. I show that new case law is immediately incorporated into the merger control practice without a learning period, that subsequent citations correlate with industry-specific M&A activity for a subset of industry-specific judgments and that the 2004 ECMR reform changed which judgments are frequently cited. I show that, when controlling for the above drivers of case law citations, the *ceteris paribus* time-dynamic of case law citation is flat for at least 25 years, implying that judgments do not intrinsically lose relevance to the merger control regime over that time.

Keywords: European Union, Antitrust Policy, Mergers, Jurisprudence, Case Law, Merger Waves, Text Analysis, Regular Expressions

JEL Classification: F55, G34, K21, L40

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†Johan Callermo is a PhD student at the Stockholm School of Economics' department of Economics. Email: johan.callermo@phdstudent.hhs.se.

1 Introduction

The establishment of new case law has the power to significantly change a merger control regime without action by the legislator. In the European context, the DG COMP suffered three high-profile defeats at the CJEU in the early 2000's: Schneider Electric, Alfa Laval and Airtours[1]. To my knowledge, there are no previous quantitative studies of how such case law influences subsequent DG COMP decisions. Airtours v. Commission, for instance, has been cited in 64 separate merger control decisions since it was decided in 2002. A recent CJEU ruling, CK Telecoms v. Commission[2] of 2020 may leave a similar trail of citations in European merger control over the coming years if it is upheld at the European Court of Justice.

Reference to case law is a reliable signal in decision text that a particular line of legal and/or economic reasoning has been applied, and as such it should be of use in the study of merger control regimes and their decision-making. I identify each explicit reference to CJEU judgments in DG COMP decisions from 1990 to 2022, distilled to a set of 861 unique decision-judgment pairs with extensive metadata available. I follow 87 CJEU judgments that are cited more than once and examine their citation frequency across time and industry as well as before and after the 2004 European merger control reform.

It is well-established that the outcomes and legal/economic motivations of DG COMP's merger control decisions changed after the 2004 merger control reform, see for instance Affeldt, Duso, and Szücs (2021), Bernhardt and Dewenter (2022), Fernández, Hashi, and Jegers (2008), Mai (2016) and Mini (2018). In this paper, I show that a similar shift occurred in the set of CJEU case law cited as legal precedent by the DG COMP.

Given changes over time in technology, consumer demand and competitive environment, one might further hypothesize that merger-specific case law would lose relevance to the merger control regime over time, inducing a sort of *expiry date* on some judgments. I show that such a hypothesis is not borne out in the data.

It is generally understood that mergers tend to occur in waves. Cho and Chung (2022) provide a review of the relevant literature. Specifically, Gort (1969) and Harford (2005) show that industry-specific merger waves are driven by economic disturbances such as technological innovation or regulatory shocks. Netter, Stegemoller, and Wintoki (2011)

[1]Schneider Electric vs. Commission (T-310/01), Tetra Laval v Commission (T-5/02 and C-12/03) and Airtours v Commission (T-342/99). See Witt (2012) for a legal discussion of the aftermath of the three defeats and the 2004 European merger control reform.

[2]CK Telecoms UK Investments v Commission, (T-399/16), under appeal as (C-376/20). CK Telecoms was cited in 5 DG COMP decisions between its adjudication in 2020 and the end of my dataset on December 31, 2022. See Deutscher (2021) for a discussion of the legal implications of the judgment.

show that such rational explanations of merger waves tend to be borne out mainly on selective samples of mostly large public bidders, a bias that corresponds well to the DG COMP sample which only includes deals large enough to trigger the common market thresholds.

A challenge with regard to merger control case law relevance over time (as proxied by citation frequency) is that if case law is closely related to specific industries, then variation in M&A activity across industry and time may disguise the real legal relevance of CJEU judgments. This is especially worrying if there is some endogeneity with regards to the appearance of a case at the CJEU in the first place. For example, consider an industry entering a consolidation phase. Previously, there has been some base level of M&A interest, but now there is a wave of merger notifications over several years. Such a wave of mergers may increase the likelihood that some industry-specific legal question is brought to the CJEU. Once the issue has been tried, the judgment is frequently cited throughout the rest of the merger wave. After the merger wave subsides, the legal precedent set by the industry-specific CJEU judgment may still be very much relevant jurisprudence, but because the merger wave has subsided the frequency of citations falls, giving the appearance that the judgment is no longer relevant. In this paper, I control for the effect of fluctuating M&A activity in the originating industries of CJEU case law on the time-dynamics of case law citations.

There is a growing econometric literature on the determinants of merger control decisions at the DG COMP as well as in U.S. courts and at national competition authorities. Most studies center on binary choice models with an approval/intervention outcome. Mats Bergman with various sets of coauthors have published several analyses of European and U.S. merger control determinants, beginning with Bergman, Jakobsson, and Razo (2005) on a sample of DG COMP decisions. The more recent Bergman, Coate, Mai, and Ulrick (2019) studies convergence between U.S. and EU merger control after the 2004 reform. Mini (2018) and Affeldt, Duso, and Szücs (2021) both study the full sample of DG COMP decisions to date and examine the effects of structural market parameters such as barriers to entry as well as changes associated with the 2004 EC Merger Regulation. Bradford, Jackson Jr, and Zytnick (2018) takes a political economic perspective, looking for signs of European protectionism.

The wider M&A literature is reviewed in Asker and Nocke (2021) from an antitrust perspective and in Renneboog and Vansteenkiste (2019) from a corporate finance perspective. At the more conceptual level, Shapiro (2018) discusses antitrust enforcement as a resurgent popular concern and its contemporary challenges as a policy project and

as a field of research. My paper ties into the M&A literature especially through the antitrust perspective by developing new data which should help economists pin down quantitatively which legal arguments have been used in merger control judgments.

Text analysis on the DG COMP corpus has gained traction during the last few years. In an unpublished masters' thesis, Schwartz (2019) uses dictionary-based methods (essentially counting the occurrences of researcher-defined lists of words) to study changing methods of economic analysis over time. Bernhardt and Dewenter (2022) analyzes changes to DG COMP's economic reasoning brought by the 2004 reform using dictionary-based methods, keyness analysis and regression analyses. Bernhardt (2021) specifically investigates prohibited and withdrawn mergers, a highly relevant project given the common assumption in the determinants literature that withdrawals in Phase 2 should be considered equivalent to prohibitions.

Arthur Dyevre has published several quantitative text analyses in European legal studies, including Dyevre, Glavina, and Ovádek (2021) which studies CJEU judgments and opinions as part of a larger corpus of European legal texts. Billows, Kohl, and Tarissan (2021) count total references to CJEU case law as well as total DG COMP self-citations in a discussion about what sources of precedent tend to be cited in DG COMP decisions. Alschner and Charlotin (2018) identify case law self-citations at the International Court of Justice and construct subject matter concentrations and other networks of citations that develop in the corpus over time. Sartor, Santin, Audrito, Sulis, and Di Caro (2022) perform a similar exercise on a CJEU corpus.

2 Institutional setting

Mergers in the European Union[3] are regulated in *Council Regulation (EC) No 139/2004 of 20 January 2004 on the control of concentrations between undertakings* (hereafter "the 2004 reform" or ECMR). The ECMR applies to mergers with a "community dimension", fulfilling at least one of several thresholds in terms of aggregate turnover in and outside the EU[4]. The ECMR is global in scope in the sense that no regard is given to where the merging parties are headquartered or where their activities take place, only to whether their merger would affect competition on the European common market. The ECMR

[3]This section is based on information retrieved in February 2023 from the websites of the DG Competition and the Court of Justice of the European Union.

[4]For instance, a combined worldwide turnover of the merging firms of more than €5 000 million with a community-wide turnover for each of at least two firms of €250 million, with less than 2/3 of the EU turnover occurring in a single member state.

succeeded *Council Regulation (EEC) No 4064/89 of 21 December 1989 on the control of concentrations between undertakings*. As hinted at in the previous paragraph, mergers notifiable to the European Commission are large and multinational - a very specific subset of the totality of M&A activity.

The ECMR is implemented by the European Commission in dialogue with national competition authorities. Primary responsibility for enforcement lies with the European Commission's Directorate-General for Competition (DG COMP). The DG COMP must be notified following an agreement, public bid or other manifestation of intent regarding a merger with a community dimension, and implementation must be postponed until the merger is declared compatible with the common market.

Three types of investigations can be applied to a notified merger at the DG COMP. Mergers that are clearly unproblematic are treated with *Simplified Procedure*. Mergers that merit an investigation are subjected to a 25-day *Phase 1 investigation*, which can either clear a merger with or without remedies (such as specific divestments) or instigate a phase 2 investigation. A *Phase 2 investigation* is an in-depth analysis of the projected effect on competition within the EU single market of the proposed merger, which should take 90 days and may lead to decisions of unconditional clearing, approval subject to remedies or prohibition.

EU case-law is made up of judgments from the Court of Justice of the European Union (CJEU), which interpret EU legislation. Merger decisions of the DG COMP are subject to review by the CJEU. Cases are tried by the General Court (EGC, previously the Court of First Instance) and can be appealed to the Court of Justice (ECJ) which is the highest instance. In appellate cases, the CJEU also publish Opinions of the Advocate General, which are not legally binding as case law but can provide a more detailed discussion of the legal points relevant to a judgment.

3 Data

My dataset is derived from the full set of DG COMP merger control decisions made between July 11th 1990 and December 31st 2022. This includes all notified merger cases (8 910), all decisions made in a merger case (9 881) and all decision texts[5] (9 662) available on DG COMP's Isef registry as of Feb 15th 2023[6], excluding decisions made on

[5]Most but not all decisions are available in a public-version pdf containing the full decision text except redacted business secrets.

[6]123 cases marked JV (Joint Venture) or ECSC (decided with reference to the Treaty establishing the European Coal and Steel Community), decided between 1982 and 2004, which are published separately

or after Jan 1st 2023. Documents in all available languages are used. In the tradition of text analysis I will use the term *corpus* to refer to a collection of texts and associated metadata.

References to case law in DG COMP decisions are identified using a regular expression which matches all case number formats of the CJEU[7]. Only one match per DG COMP decision is counted, so if there are several references to the same judgment within a document, or if there are references to the same judgment in several documents attached to the same decision, these are only counted as one citation. The latter matters because some decisions have many versions of the same document, addressed to different recipients and/or translated into different languages.

The InfoCuria database is used to manually add metadata (*name of the parties* and *date of decision*) to each CJEU judgment identified by the regular expressions algorithm. This step also functions as a review of the regular expression results, ensuring that each identified case number corresponds to an actual CJEU judgment[8]. Also, the DG COMP case or CJEU judgment under appeal (where applicable) has been manually identified in the first few paragraphs of each judgment.

Sets of CJEU cases that were decided jointly by the CJEU but are referenced separately in DG COMP decisions are counted as citations of one and the same judgment, marked as "joined" in tables and figures for clarity. In contrast, I count references to first instance judgments and appellate judgments on the same case separately. This because the legal principles outlined in such judgments and related opinions may not be exactly the same at the two courts. Making legal distinctions separately for each occurrence falls outside the scope of this economics paper.

Judgments that are only cited by one DG COMP decision are not retained for the analysis since they may represent a single decision the DG COMP was required to make as a result of a particular CJEU judgment, for instance due to the annulment of some

on the DG COMP website, are yet to be included in the database. This project will be completed during 2023.

[7]All case numbers begin with a letter followed by a dash, then a number of digits (theoretically unlimited but in practice 1-4 digits), then a slash, then two more digits. The leading letter is either T (for first-instance cases tried at the the general court), C (for appellate cases tried at the Court of Justice) or F (for the now defunct Civil Service Tribunal). The trailing two digits correspond to the year in which the case was brought before the court. None of the groups of digits have leading zeroes. The format is described in the documentation of CJEU's InfoCuria Database.

[8]There are 42 regular expressions matches that do not correspond to CJEU judgments. Some are references to an older DG COMP case number format. Most are misspelled case numbers (C- instead of C/) from the Spanish competition authority CNMC. Such erroneous matches were dropped before descriptive statistics and analysis are preformed. For example, the regular expression identifies "C-0042/08" which is a misspelling of CNMC case C/0042/08.

previous decision. Such citations does not correspond to my view of case law that has a legal impact on the Merger Control regime, and their lack of time-dynamics make them less interesting from a statistical standpoint.

3.1 Most cited CJEU judgments

A total of 230 CJEU judgments are cited in the DG COMP corpus after combining cases that were decided jointly by the court. Of the 230 judgments, 86 are cited more than once. Table 1 enumerates the top 20 judgments and Figure 1 illustrates the raw time series of the top 10 judgments.

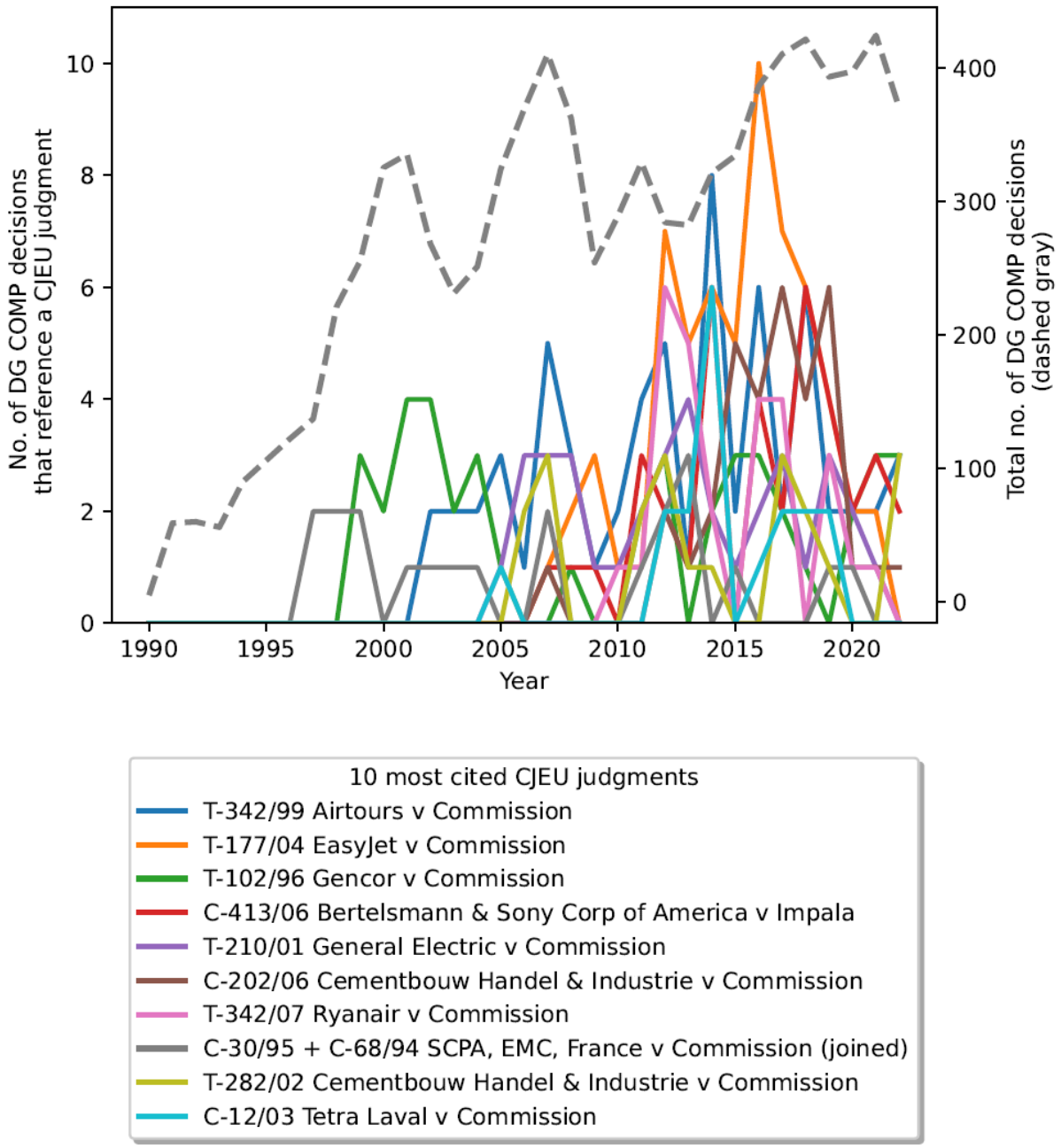
The frequency of citations is not a foolproof measure of DG COMP adherence to a particular piece of case law. It is entirely possible that case teams adhere to principles derived from a particular judgment without explicitly quoting it. Thus, citations constitute a lower bound of the adherence to the legal content of a particular judgment.

Furthermore, once DG COMP have interpreted and applied a court ruling once, it may be more appealing for later DG COMP decisions to refer to court judgments indirectly via a previous DG COMP decision, especially if the DG COMP decision in question is closer to the case at hand in terms of product markets or other characteristics. The latter dynamic will be addressed in a companion paper which adds DG COMP self-referencing their own decisions to the database of case law under investigation.

Table 1: Top 20 most influential CJEU judgments by number of DG COMP decisions that cite them.

Judgment	n	Title	Year
T-342/99	64	Airtours v Commission	2002
T-177/04	62	EasyJet v Commission	2006
T-102/96	44	Gencor v Commission	1999
C-413/06	38	Bertelsmann & Sony Corp of America v Impala	2008
T-210/01	36	General Electric v Commission	2005
C-202/06	34	Cementbouw Handel & Industrie v Commission	2007
T-342/07	28	Ryanair v Commission	2010
C-30/95	21	SCPA, EMC, France v Commission (joined)	1998
+ C-68/94			
T-282/02	21	Cementbouw Handel & Industrie v Commission	2006
C-12/03	18	Tetra Laval v Commission	2005
T-221/95	16	Endemol v Commission	1999
T-2/93	16	Air France v Commission	1994
T-346/02	16	Cableuropa, Aunacable v Commission (joined)	2003
+ T-347/02			
T-156/98	15	RJB Mining v Commission	2001
T-119/02	15	Royal Philips Electronics v Commission	2003
T-87/05	15	EDP v Commission	2005
T-464/04	14	Impala v Commission	2006
C-234/89	13	Delimitis v Henninger Bräu	1991
T-374/00	10	Verband der freien Rohrwerke & Others v Com	2003
T-79/12	8	Cisco Systems and Messagenet v Commission	2013

Figure 1: Raw time series of top 10 CJEU judgments



3.2 Associated decision categories

Decision types associated with the top-20 CJEU judgments are shown in Table 2. Note for instance the clear difference in permissive vs. prohibitive decisions between Airtours v. Commission (T-342/99) and EasyJet v. Commission (T-177/04).

Table 2: Decision categories of the top 20 CJEU judgments

Judgment	Parties	Compatible Phase 1	Compatible Phase 2	Commitments P. 1	Commitments P. 2	Prohibition	Referrals	Art. 14	Art. 21	Other
T-342/99	Airtours v Commission	22	9	13	18	1	1	0	0	0
T-177/04	EasyJet v Commission	1	2	36	15	7	1	0	0	0
T-102/96	Gencor v Commission	3	3	11	19	5	2	0	0	1
C-413/06	Bertelsmann & Sony...	6	8	8	15	1	0	0	0	0
T-210/01	General Electric v Comm.	3	1	7	17	6	1	1	0	0
C-202/06	Cementbouw Handel...	0	0	14	16	3	0	0	0	1
T-342/07	Ryanair v Commission	5	2	4	10	6	0	1	0	0
T-282/02	Cementbouw Handel...	4	0	2	6	3	0	3	0	3
C-30/95	SCPA, EMC, France	1	7	2	6	4	1	0	0	0
+ C-68/94	v Commission (joined)									
C-12/03	Tetra Laval v Commission	5	1	2	9	1	0	0	0	0
T-346/02	Cableuropa, Aunacable	0	1	0	0	0	15	0	0	0
+ T-347/02	v Commission (joined)									
T-221/95	Endemol v Commission	2	0	7	5	1	1	0	0	0
T-2/93	Air France v Commission	5	2	1	4	2	1	1	0	0
T-119/02	Royal Philips Electronics...	0	1	1	0	1	12	0	0	0
T-87/05	EDP v Commission	0	0	0	7	8	0	0	0	0
T-156/98	RJB Mining v Commission	10	1	1	2	1	0	0	0	0
T-464/04	Impala v Commission	2	3	3	6	0	0	0	0	0
C-234/89	Delimitis v Henninger Bräu	11	0	0	0	0	2	0	0	0
T-374/00	Verband freien Rohrwerke...	2	1	0	6	1	0	0	0	0
T-79/12	Cisco Systems and...	2	1	1	3	1	0	0	0	0

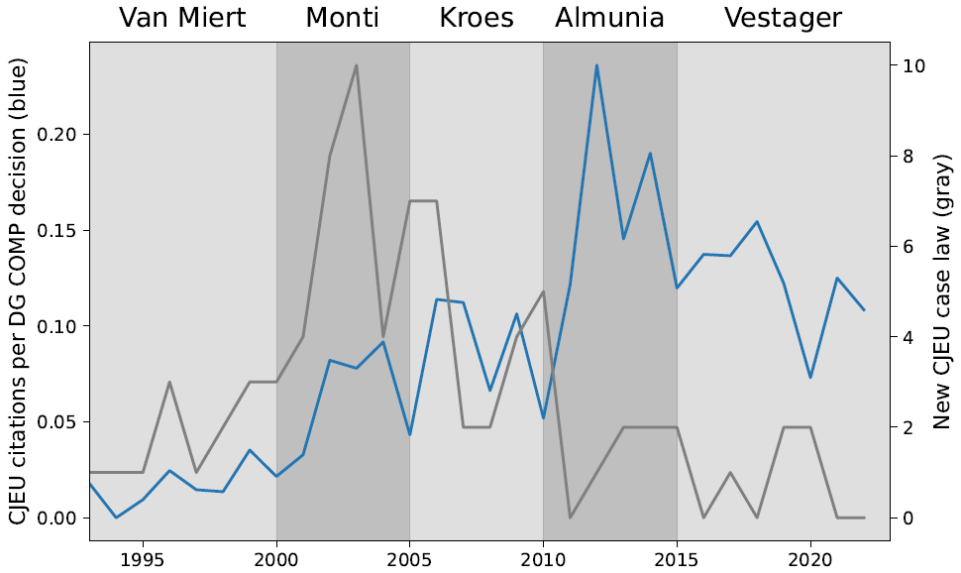
3.3 Citation density

There is significant variation over time in how frequently CJEU case law is cited in DG COMP decisions. We will call this variable the yearly *citation density* of CJEU case law in DG COMP decisions.

$$citation\ density_{year} = \frac{CJEU\ citations_{year}}{DG\ COMP\ decisions_{year}} \tag{1}$$

Figure 2 shows the time series of citation density overlaid by the tenure of five competition commissioners as well as the timeline of CJEU case law origination. Properly pinning down the sources of variation in citation density would be an interesting future project, the choice of correlating variables in Figure 2 is speculative. It is however worth noting that including a fixed effect for the serving competition commissioner probably has effects quite similar to controlling for yearly citation density.

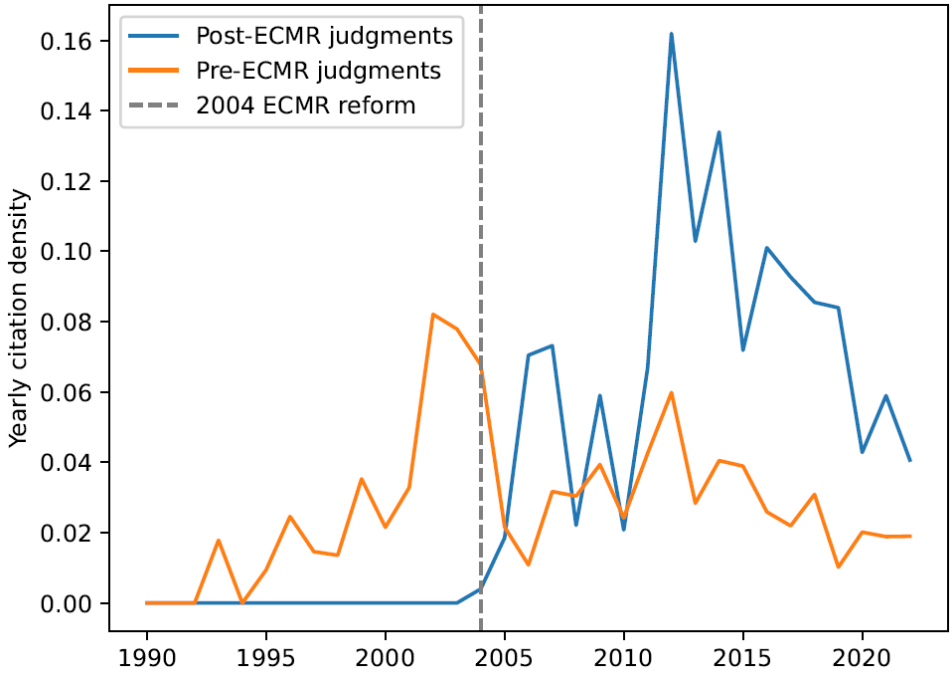
Figure 2: Density of CJEU case law citation over time (blue) and establishment of new CJEU case law (gray).



One might contend that increasing citation density over time could simply be driven by the accumulation of case law from a low starting point in the early nineties; that over time there are simply more judgments that clarify the legal facts and thus should be taken into consideration in DG COMP decisions. However, that explanation does not match the dramatic decline in citation frequency between 2012 and 2020. Furthermore, it does not match the timeline of case law origination shown on the right-side y-axis of figure 2 (gray line).

The citation density can be decomposed into judgments made before and after the 2004 ECMR reform to illustrate the shifting set of relevant case law (Figure 3) across the reform.

Figure 3: Citation densities for pre- and post-reform judgments



It is worth noting that the bump in CJEU citations just before the reform is not concentrated to one or even a few judgments but is evenly spread out across the whole set of pre-ECMR judgments.

3.4 Industry dynamics

Each DG COMP merger case is associated with one or several industries in the NACE[9] classification. NACE has four levels: 21 *sections* (e.g. C, Manufacture) subdivided into 88 *divisions* (e.g. C.17, Manufacture of paper and paper products), further subdivided into *groups* (e.g. "C.17.2, Manufacture of articles of paper and paperboard) and *classes* (e.g. C.17.2.4, Manufacture of wallpaper). NACE codes allow a comprehensive basis for the study of industry dynamics with a somewhat flexible level of aggregation, as opposed to the product markets identified in each case by the DG COMP which are much too

[9]Nomenclature statistique des Activités économiques dans la Communauté Européenne. My discussion of NACE is based on the general description published on Eurostat's metadata server.

disaggregated for the present project[10]. Table 3 enumerates the 20 most common division-level industries.

There is some variation in the NACE level provided by the DG COMP. Division- and group level statistics are most common. For a (nearly) complete industry classification we are limited to the *section* and *division* levels. At the division level, 84 unique NACE industries are present in the DG COMP corpus 1990-2022.

Table 3: Top 20 NACE divisions in the DG COMP corpus

NACE	n	Description
G.46	596	Wholesale trade, except of motor vehicles & motorcycles
D.35	594	Electricity, gas, steam & air conditioning supply
C.20	544	Manufacture of chemicals & chemical products
G.47	543	Retail trade, except of motor vehicles & motorcycles
K.64	480	Financial service activities, except insurance & pension funding
J.61	390	Telecommunications
H.52	383	Warehousing & support activities for transportation
K.65	381	Insurance, reinsurance & pension funding, except compulsory social security
C.29	338	Manufacture of motor vehicles, trailers & semi-trailers
L.68	337	Real estate activities
C.10	326	Manufacture of food products
J.62	309	Computer programming, consultancy & related activities
C.28	285	Manufacture of machinery & equipment n.e.c.
C.33	281	Repair & installation of machinery & equipment
K.66	235	Activities auxiliary to financial services & insurance activities
C.24	218	Manufacture of basic metals
C.27	209	Manufacture of electrical equipment
C.21	196	Manufacture of basic pharmaceutical products & pharmaceutical preparations
C.26	187	Manufacture of computer, electronic & optical products
C.30	181	Manufacture of other transport equipment

[10]It is likely that most DG COMP cases have a unique combination of exact markets in the sense used to motivate competition concerns in DG COMP decisions.

3.4.1 Are CJEU judgments industry-specific?

First some preliminary observations: There are examples of cases that appear clearly industry-specific at a glance. For example: Delimitis v Henninger Bräu[11] is cited 13 times, exclusively in C.10 (Manufacture of food products) and C.11 (Manufacture of beverages). Amministrazione Autonoma dei Monopoli di Stato (AAMS) v Commission[12] is only cited twice, but both citations are made in the C.12 (Manufacture of tobacco products) industry[13], and no other CJEU judgments are ever cited in that industry. Cases like AAMS v. Commission, rarely cited and only ever in one particular industry, are relatively common. How can we judge whether these seemingly industry-specific judgments are random outliers or truly only relevant to a particular (set of) industry(-ies)? One way is to give a legally informed answer, which this economist will leave to legal scholars. Another is to statistically test the hypothesis that citations do not cluster by industry.

The most common test for dependence between categorical variables is Pearson's chi-squared-test. In our case Pearson's would test the null hypothesis that the joint distribution of $judgment_j \times industry_i$[14] pairs is a simple product of the marginal distributions: how likely a particular judgment is to be cited and how often citations occur in a particular industry. Pearson's test statistic, which under standard assumptions is chi-squared-distributed, is:

$$\chi^2 = \sum_{i,j} \frac{(o_{i,j} - e_{i,j})^2}{e_{i,j}} \tag{2}$$

where o is the observed value and e is the expected value under the null hypothesis.

Kraus (2012) and Von Davier (1997) discuss the application of goodness-of-fit tests on sparse contingency tables. When some expected frequencies are close to zero (such as for the vast majority of $judgment \times industry$ pairs in our sample), an observed frequency of 1 or more in such a category causes an inflated contribution to the goodness-of-fit statistic and the null hypothesis is rejected too easily. To compensate I replace the standard

[11] C-234/89

[12] T-139/98

[13] A total of 8 cases were notified 1990-2022 in C.12

[14] Studying $judgment \times industry$ observations implies a data-generating process that stochastically produces such pairs in the DG COMP corpus. This is not as counterintuitive as it seems. There are probably "latent citations" present, where the DG COMP rely on some jurisprudence that they do not feel the need to cite. If one was to raise or lower the detail with which the DG COMP feel the need to cite legal sources for statements in merger control decisions, there is some marginal set of citations which would appear or disappear in the corpus.

asymptotic chi-squared distribution with a Monte Carlo simulation[15] to find the actual distribution of Pearson's test statistic under the null hypothesis. This exercise rejects independence between industry and judgments with a p-value of 0.019. That is: it is almost certain that there is some correlation between judgments and industries in our data. The 6 most industry-specific judgments contribute approximately 30% of the weight in this measure, which implies that there is a range of industry-specificity dominated by a few very industry-specific cases. The result is not significant at the *section* level industries (p = 0.166). None of the 15 most cited cases in Table 1 contribute heavily to industry-specificity by this measure, which is predictable: a very industry-specific judgment is unlikely to become the overall most cited case.

Table 4 lists judgments in order of their weight in the Pearson statistic (the row-sum in the contingency table of squared Pearson residuals over the total value of the test statistic). A greater weight means a judgment is further away from the citation pattern predicted by the null hypothesis of industry-independence.

Table 4: Most industry-specific judgments in the Pearson test

Judgment	Weight*	Names of the Parties	Industry
T-139/98	10.5%	AAMS v Commission	C.12
T-191/98	6.8%	Atlantic Container Line and Others v. Commission (joined)	H.50
C-234/89	6.8%	Delimitis v Henninger Bräu	C.11
T-66/89	3.8%	Publishers Association v Commission	J.58
T-374/94	3.4%	European Night Services and Others v. Commission (joined)	H.49
T-380/17	3.3%	HeidelbergCement & Schwenk Zement v. Commission	Q.86

$$* \quad \sum_j \frac{r_{i,j}^2}{\chi^2}$$

[15] In detail: All 1062 industry x judgment pairs are used as observations to populate a contingency table of 87 judgments (j) and 63 industries (i). 100 000 samples are simulated under the null hypothesis distribution $p_{j,i} = p_i p_j$, with marginal probabilities drawn from the real data. The p-value of Pearson's independence statistic in the real sample is calculated using its rank among the 100 000 simulated test statistics. For a discussion of the rank approach to p-value estimation, see North, Curtis, and Sham (2002).

3.4.2 Originating industries of CJEU judgments

Almost all of the top CJEU judgments originate with appeals of DG COMP merger control cases[16], for which we have industry information. This allows us a second way to examine the industry-specificity of CJEU judgments: examining the fraction of citations in and outside of the originating industry(-ies). Table 5 shows the fraction of citations that occur in the same industry or industries as the DG COMP case whose appeal was treated in the judgment. The fractions are given both at the *section* and *division* levels of NACE industry classification. "n" is the number of citations in the full DG COMP corpus. The E(X) columns give the expected fraction at that industry level under the null hypothesis that CJEU cases are not industry-specific (that is, the fraction of all merger control cases that are associated with this industry).

Table 5 particularly illustrates one crucial characteristic of the industry specificity of CJEU judgments. Clearly, many judgments are cited in their originating industry much more often than they would under the null hypothesis. Equally clearly, this is a marginal effect rather than a categorical distinction. That is, judgments are *more likely* to be cited in the industry that they originated in, but none of the highly cited judgments are *exclusive* to an industry, even at the *section* level.

We can formally test originating-industry specificity using Wilcoxon's signed-rank test. We posit the null hypothesis that the share of citations within the originating industry for a particular judgment equals the population share of citations in that industry (which should be the case approximately if there is no industry specificity). The null hypothesis is rejected with a p-value of 0.026 in the division case and 0.284 in the section case. That is: it is very unlikely that judgments are distributed across industries independently of their originating *division*, but at the *section* level the results are unclear.

[16]Not all CJEU judgments cited in the DG COMP corpus originate with appeals of DG COMP merger control decisions. Many are requests for preliminary rulings from national courts, some are appeals of European Commission decisions made in contexts other than Merger Control. Some originate in decisions made before the 1989 merger control regime was enacted. Among the non-merger control decisions, it would be possible to manually identify NACE codes for some but not all cases. This remains a future project.

Table 5: Fraction of citations that occur in the originating industry, top 20 judgments.

			Originating industry					
Judgment	n	Case	Section	%	E(S)	Division	%	E(D)
T-342/99	64	M.1524	N	0.09	0.04	N.79	0.09	0.01
T-177/04	62	M.3280	H	0.32	0.11	H.51	0.24	0.01
T-102/96	44	M.619	C	0.52	0.51	C.24	0.02	0.03
C-413/06	38	M.3333	R	0.03	0.01	R.93	0.03	0.01
T-210/01	36	M.2220	C	0.56	0.51	C.20	0.08	0.07
						C.28	0.06	0.04
						C.26	0.06	0.03
C-202/06	34	M.2650	C	0.79	0.51	C.23	0.03	0.03
T-342/07	28	M.4439	H	0.32	0.11	H.51	0.32	0.01
T-282/02	21	M.2650	C	0.48	0.51	C.23	0.05	0.03
C-30/95 +	21	M.308	B	0.1	0.03	B.08	0	0
C-68/94								
C-12/03	18	M.2416	C	0.39	0.51	C.28	0	0.04
T-2/93	16	M.259	H	0.38	0.11	H.51	0.38	0.01
T-221/95	16	M.553	J	0.25	0.15	J.60	0.06	0.02
T-346/02 +	16	M.2845	J	0.75	0.15	J.60	0.31	0.02
T-347/02								
T-156/98	15	-	-	-	-	-	-	-
T-87/05	15	M.3440	D	0.2	0.1	D.35	0.2	0.1
T-119/02	15	M.2621	C	0.13	0.51	C.33	0.07	0.03
T-464/04	14	M.3333	R	0.21	0.01	R.93	0.14	0.01
C-234/89	13	-	-	-	-	-	-	-

3.4.3 Are there merger waves in the DG COMP corpus?

For our purposes, industry-specific merger waves may be formalized as independent time-trends and strong autocorrelation in the M&A activity of division-level industries[17]. For the purposes of a specific study of merger waves this would be a coarse definition, but the purpose of defining merger waves in this paper is to show that there is independence and inertia in sector-specific M&A activity.

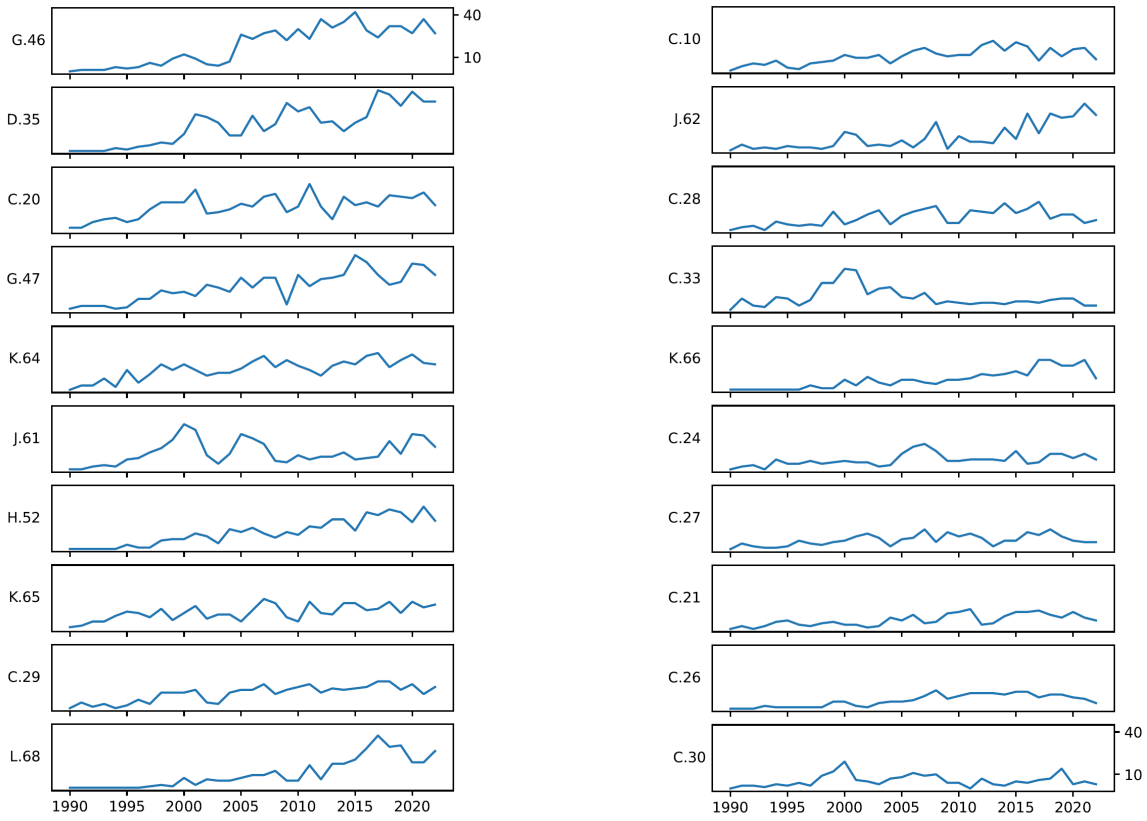
[17]Note that this definition of industry-specific merger waves does not necessarily match the well established concept of (aggregate) merger waves as reviewed by Cho and Chung (2022).

If there is inertia and autocorrelation in industry-specific M&A, then controlling for industry-specific variation in M&A activity entails a phenomena that has some level of predictability from one year to another, making it applicable in a real-world problem-solving context. The inertia also supports the view of industry-specific merger waves as driven by identifiable macro shocks such as technological and regulatory development. One might also hypothesize that M&A acticity is self-reinforcing in the sense that a merger in a particular industry tends to increase the likelihood that competing firms must reorganize in response to the changing competitive environment. This too would be consistent with industry-specific merger waves. A more random or more behaviorally rooted time-trend would imply a more fickle merger-wave dynamic, less readily identifiable in limited samples of real-world data.

The direction of causality between case law and merger notifications is not obvious. While increased merger activity in a previously unlitigated industry may increase the likelihood of new case law, new case law may also affect incentives with regards to merger activity in the industry. Thus, it would be inappropriate to treat industry-specific M&A activity as exogenous to case law citations.

A graphical examination of the top 20 industries over time (Figure 4) shows that there are clearly separate time trends in different industries. This suggests the presence of industry-specific merger waves, substantiating our concern about industry variation in M&A activity interfering with our measures of case law relevance.

Figure 4: Yearly frequency of top 20 industries in DG COMP decisions



Note: x- and y-axes are identical in all 20 plots and scales are linear. The y-axis shows the number of DG COMP decisions in a given industry and year. The highest frequencies are around 40 decisions in a particular industry in a particular year.

3.5 Superceded judments

Our dataset contains a few pairs of cases where a General Court judgment is appealed and decided by the Court of Justice, the CJEU's highest instance. Our algorithm will only pick up these pairs when both the appeal and the original case have been cited more than once in the DG COMP corpus. The "superceded" designation is not an assertation that the cases legally replace each other but a marker to be used in the coming regressions to investigate whether superceded cases are actually cited less after an appeal is decided.

Table 6: Superceded cases

Judgment	Year	Parties	Appeal	Year	Parties
t-282/02	2006	Cementbouw Handel...	c-202/06	2007	Cementbouw Handel...
t-464/04	2006	Impala v Commission	c-413/06	2008	Bertelsmann & Sony...
t-5/02	2002	Tetra Laval v Com.	c-12/03	2005	Tetra Laval v Com.
t-25/95	2000	Cimenteries CBR...	c-204/00	2004	Aalborg Portland...
+ several			+ several		

4 Empirical model and results

The aim of the main model is to combine several sources of variation in CJEU judgment citations and discuss their relative importance. The main object of interest is the time-dynamics captured in the *elapsed* fixed effects. This non-parametric time-effect allows us to answer questions such as: Is there a learning period after a CJEU judgment is decided before it is fully incorporated into DG COMP decision-making, or is new case law incorporated instantaneously into the merger control regime? Is there an expiry date attached to CJEU judgments after which their relevance to the merger control regime diminishes, or does CJEU case law stay relevant indefinitely?

Equation 3 pins down around half of the variation in citations of a given *judgment* × *year* in the DG COMP corpus.

$$\begin{aligned} citations_{t,j} =& \vec{\beta_1}\ \vec{elapsed}_{t,j} \\ &+\beta_2\ pre\text{–}ECMR\ judgment_j \times \frac{CJEU\ citations_{pre,t}}{DG\ COMP\ decisions_t} \\ &+\beta_3\ post\text{–}ECMR\ judgment_j \times \frac{CJEU\ citations_{post,t}}{DG\ COMP\ decisions_t} \\ &+\beta_4\ total\ citations_j \\ &+\vec{\beta_5}\ judgment_j \times originating\ industry\ M\&A_{t,j} \\ &+\beta_6\ superceded_{t,j} \\ &+u_{t,j} \end{aligned} \tag{3}$$

The outcome, $citations_{t,j}$, is the number of times a particular CJEU judgment is cited in a particular year. Subscripts t and j denote years and judgments respectively. Subscripts *pre* and *post* group judgments into those decided before and after the 2004 ECMR reform.

$elapsed_{t,j}$ is a fixed effect for the number of calendar years that have passed since a judgment was decided by the CJEU. There are fixed effects for each year from 0 to 25[18]. This set of coefficients is not meant to identify a causal driver of case law citations but should be seen as a decomposition informative of the time-dynamics of citations.

The fact that the model uses calendar year observations introduces an inexactness to the *elapsed* variable. Since a case can be decided on any of the 365 days in a calendar year, the value $elapsed = 0$ will represent a year in which the judgment was case law

[18]We could add fixed effects for judgments older than 25 years but at that point there are fewer than 10 observations, making for large confidence intervals. Instead, observations of $elapsed > 25$ are dropped. All conclusions are robust to including all 45 available elapsed-values as well as to including only the first 35.

for some but not all of the year. Thus we use $elapsed = -1$ as our baseline[19], which represents the year ending one to 365 days prior to the day of the CJEU judgment. Coefficients on the *elapsed* dummies should be interpreted with this 1-year fuzziness in mind.

Controls for *yearly citation density* and *judgment-specific relevance* (as proxied by $total\ citations_j$) are highly significant. These controls construct a baseline expectation of citations absent any *elapsed*-effects, based on citation patterns over time at the DG COMP and the base relevance of each judgment to the merger control regime (proxied by its total number of citations 1990-2022). Separating the citation densities for pre- and post-reform judgments presupposes that these two sets differ in their legal relevance over time, an assumption which is based on previous research[20] combined with our findings in Figure 3.

The set of $judgment_j \times originating\ industry\ M\&A_{t,j}$ pairs controls for the number of yearly merger notifications in the industry associated with the DG COMP case whose appeal gave rise to the judgment. $judgment_j$ is a dummy which is 1 only for judgment j, and $originating\ industry\ M\&A_{t,j}$ is the number of mergers notified in judgment j's originating industry in year t. Not all judgments have an associated originating industry, but as can be seen in Table 5, 18 of the 20 most cited judgments have the required information.

The dummy variable *superceded* is set to 1 in $judgment_j \times year_t$ pairs where an appeal of that judgment has been decided at the Court of Justice[21]. It is reasonable to expect fewer citations of a General Court (first instance) judgment once a decision has been made in the Court of Justice (final instance) in the same case, regardless of whether the original judgment was upheld or set aside.

[19]Naturally, we expect effects of 0 for all negative years. There should be no citations of a judgment before it was decided at the CJEU. In actuality there are four instances of the DG COMP citing a CJEU judgment on the year before it was decided by the court. These are retained in descriptive statistics but dropped for the regression in order to create a 0-citations baseline at $elapsed = -1$. In two instances (M.3333 on 3/10/2007 citing C-413/06) and M.4685 on 5/12/2007 citing C-196/07) the DG COMP are simply noting that an appeal has been lodged but not yet decided. In case M.890 on 26/6/1997 reference is made to an opinion of the Advocate-General in joined cases C-68/94 and C-30/95. In case M.7993 on 24/04/2018, the commission cites an order of the President of the General Court of 12 July 2017 regarding interim measures in case T-371/17.

[20]Most importantly Affeldt, Duso, and Szücs (2021), Bernhardt and Dewenter (2022), Fernández, Hashi, and Jegers (2008), Mai (2016) and Mini (2018).

[21]This will only be captured in our algorithm if the superceding judgment is cited more than once in the DG COMP corpus. If an appeal is decided, but that appeal is not cited by DG COMP, the original judgment is not considered superceded.

Table 7: Regression results: equation 3

R-squared:	0.505	**Adj. R-squared:**	0.480
Covariance Type:	HC3		

	coef	**std err**	**z**	**P> |z|**
Intercept	-0.5377	0.093	-5.764	0.000
***Elapsed* fixed effects**	——— See Figure 5 ———			
Yearly citation density: pre-reform judgments	5.6060	1.064	5.270	0.000
Yearly citation density: post-reform judgments	3.9330	0.714	5.508	0.000
Judgment total citations	0.0251	0.007	3.828	0.000
Industry controls	——— See Table 7 ———			
Superceded	-0.1499	0.114	-1.320	0.187

4.1 Equivalence with TWFE model

Controls for (aggregate) *yearly citation density* $\left(\frac{CJEU\ citations_t}{DG\ COMP\ decisions_t}\right)$ and *judgment-specific relevance* ($total\ citations_j$) are almost entirely equivalent to a more general but less transparent two-way fixed effect model. Equation 4 describes a benchmark model where fixed effect controls for *judgment* and *year* partial out changes over time in the practices of case law citation at DG COMP as well as varying relevance of specific judgments, replacing the controls for yearly citation density and total judgment citations. This base model might be considered more general than the main model and thus serve as a sort of robustness check with regards to researcher choices in the main model.

$$citations_{t,j} = elapsed_e + year_t + judgment_j + u_{t,j} \tag{4}$$

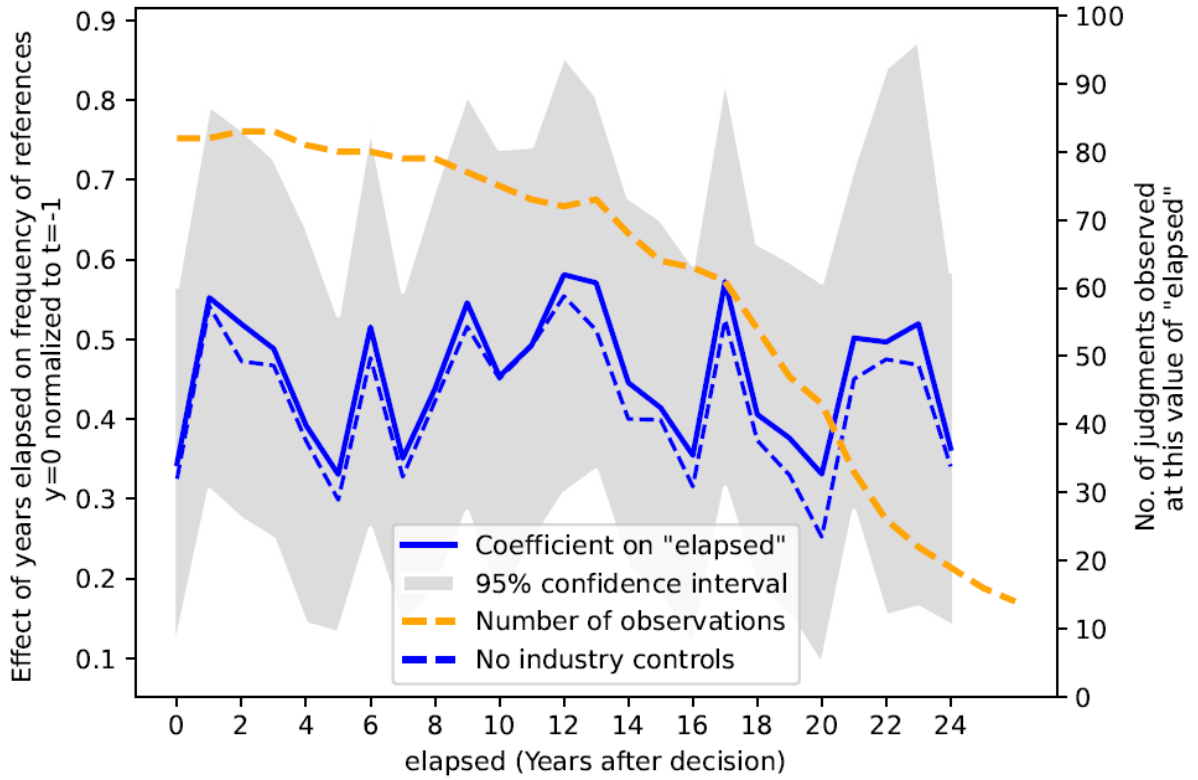
The yearly fixed effects in Model 4 are almost entirely explained by the *yearly density of case law citations* in the DG COMP corpus. The (Pearson) correlation between yearly *citation density* and the yearly fixed effect in the base model is 0.933.

Similarly, the judgment fixed effect in Model 4 is almost entirely (R=0.962) explained by the total number of citations of each judgment in the DG COMP corpus (see Table 1). This could be conceptualized as the base level of relevance of a particular judgment to the merger control regime.

4.2 Time dynamics

Figure 5 shows the *elapsed* fixed-effects over time in the main model. The interesting observation here is the relative difference in *elapsed*-effect over time[22]. On average, the effect is quite stable. This implies that after controlling for the 2004 reform, for superceded judgments, for intrinsic difference in relevance between judgments and for underlying industry-specific trends, the legal relevance of CJEU case law is very stable over time. All else equal, a judgment seems equally likely to be cited one or 20 years after it is decided by the CJEU. The average effect may hide some heterogeneity, but the statistical significance of the coefficients suggest that such heterogeneity (perhaps driven by some particular judgment losing relevance due to replacement or obsolescence) is at least relatively rare.

Figure 5: Effect of time elapsed, with or without industry controls



It is clear from Figure 5 that new case law is implemented immediately and fully

[22]Less relevant are the actual numeric effect sizes, but for clarity: An effect size of 1 for $elapsed = x$ in this model implies that, holding *judgment* and *year* constant, a judgment is expected to be cited by one more DG COMP decision x calendar years after the judgment than in the calendar year before the judgment was made

into the merger control regime as exercised by the DG COMP (keeping in mind the 1-year fuzziness of the observations noted above). This is consistent with a theoretical understanding of jurisprudence which leaves no room for a gradual implementation of law or precedent.

4.3 Industry dynamics

Table 8 lists all the controls for originating industry that are statistically significant at the 5% level. These judgments could reasonably be called industry-specific, although we should remember the observation in section 3.4.2 that industry-specificity is a marginal and not a binary property. The cutoff is arbitrary, the purpose is simply to give one more alternative definition of the most industry-specific judgments in the DG COMP corpus. There is a high degree of overlap with those judgment that seem industry-specific in Table 5, but not with Table 4. This is a result of the properties of the statistical tests. In short: the Pearson test emphasizes rarely cited judgments because deviations look larger when compared to fewer other citations, while the regression emphasizes frequently cited judgments because a larger sample ensures higher statistical significance.

The effect sizes are generally strong. Compare the effects with those in Figure 5 and Table 7. For the top judgment in Table 8, one additional notified merger in H.51 (Air Transport) is associated with 0.5 more citations of judgment T-177/04 (EasyJet v Commission) in that year.

Table 8: Most significant industry controls in the main specification.

Judgment x Industry	Coefficient	P-value
(T-177/04) x ('H.51')	0.50	0.0013
(T-342/99) x ('N.79')	0.44	0.0025
(T-282/06) x ('C.20')	-0.0085	0.0038
(T-209/01) x ('C.20')	-0.041	0.0052
(C-13/03) x ('C.28')	-0.017	0.0054
(T-210/01) x ('C.28')	0.11	0.0076
(T-145/06) x ('C.23')	-0.03	0.011
(T-48/04) x ('J.61')	-0.01	0.013
(T-282/02) x ('C.23')	0.12	0.024
(T-209/01) x ('C.26')	0.11	0.025

An early hypothesis motivating the inclusion of industry controls might be called

the *merger wave theory of case law relevance*. The author expected that inclusion of industry controls would change the regression results in such a way that a previously negative trend in the *elapsed* coefficients would be flattened. That is, exclusion of industry controls would give the impression of falling case law relevance over time while inclusion of industry controls would show that the falling citation trend was actually driven by subsiding merger waves in the industries relevant to particular pieces of case law. This hypothesis is not borne out in the regression results (neither is it disproven). It is clear that industry-specific variation in M&A activity drives citations of industry-specific case law, but for the merger wave theory to be borne out, the interactions would have had to follow a particular pattern: a clearly hump-shaped merger wave with a new CJEU judgment appearing in the rising part of the wave, which would have produced a merger wave-induced dip in citations within a few years after the appearance of the judgment. The fact is that industry-specific merger activity varies with a more random pattern than the stylized hump-shaped waves (as can be observed in Figure 4).

4.4 Impact of the 2004 ECMR

Does the coming into effect of *Council Regulation (EC) No 139/2004 of 20 January 2004 on the control of concentrations between undertakings* change the set of case law relevant to merger control decisions at DG COMP? It is well-established that DG COMP's reasoning with regards to structural market characteristics changed after the reform[23], but the effect on case-law relevance has not been examined quantitatively to my knowledge. Table 9 lists all pre-reform CJEU judgments that were cited more than once in the DG COMP corpus before the 2004 ECMR reform.

[23]See Affeldt, Duso, and Szücs (2021), Bernhardt and Dewenter (2022), Fernández, Hashi, and Jegers (2008), Mai (2016) and Mini (2018).

Table 9: Pre-2004 CJEU judgments

Judgment	n	Year	Parties
T-102/96	16	1999	Gencor v Commission
C-30/95 + c-68/94	9	1998	SCPA, EMC, France v Commission (joined)
C-234/89	8	1991	Delimitis v Henninger Bräu
T-342/99	4	2002	Airtours v Commission
C-60/00	4	2002	Carpenter
T-221/95	3	1999	Endemol v Commission
C-54/00	3	2001	Commission v Italy
C-41/90	2	1991	Höfner and Elser v Macrotron GmbH
T-66/89	2	1992	Publishers Association v Commission
T-310/01	2	2002	Schneider Electric v Commission
T-156/98	2	2001	RJB Mining v Commission
T-119/02	2	2003	Royal Philips Electronics v Commission
C-159/91 + c-160/91	2	1993	C. Poucet v Assur. Géné. &... (joined)
C-156/98	2	2000	Germany v Commission
T-5/02	2	2002	Tetra Laval v Commission

Figure 3 seems to show that the set of relevant case law changed immediately upon the reform. The inclusion in the main model of separate citation densities precludes testing for the impact of the reform because separate citation densities assumes *a priori* that the reform-effect exists. If instead we aggregate the citation densities into one population measure as in Equation 1 and Figure 2, and instead include a control for post-reform years on pre-reform judgments, we do not find a significant result (Equation 5).

$$\begin{aligned} citations_{t,j} =& \vec{\beta}_1 \, \vec{elapsed}_{t,j} \\ &+ \beta_2 \, \frac{CJEU \; citations_t}{DG \; COMP \; decisions_t} \\ &+ \beta_3 \, total \; citations_j \\ &+ \vec{\beta}_4 \, judgment_j \times originating \; industry \; M\&A_{t,j} \\ &+ \beta_5 \, pre\text{–}ECMR \; judgment_j \times post\text{–}ECMR \; year_t \\ &+ \beta_6 \, superceded_{t,j} \\ &+ u_{t,j} \end{aligned} \tag{5}$$

Table 10: Regression results: equation 5

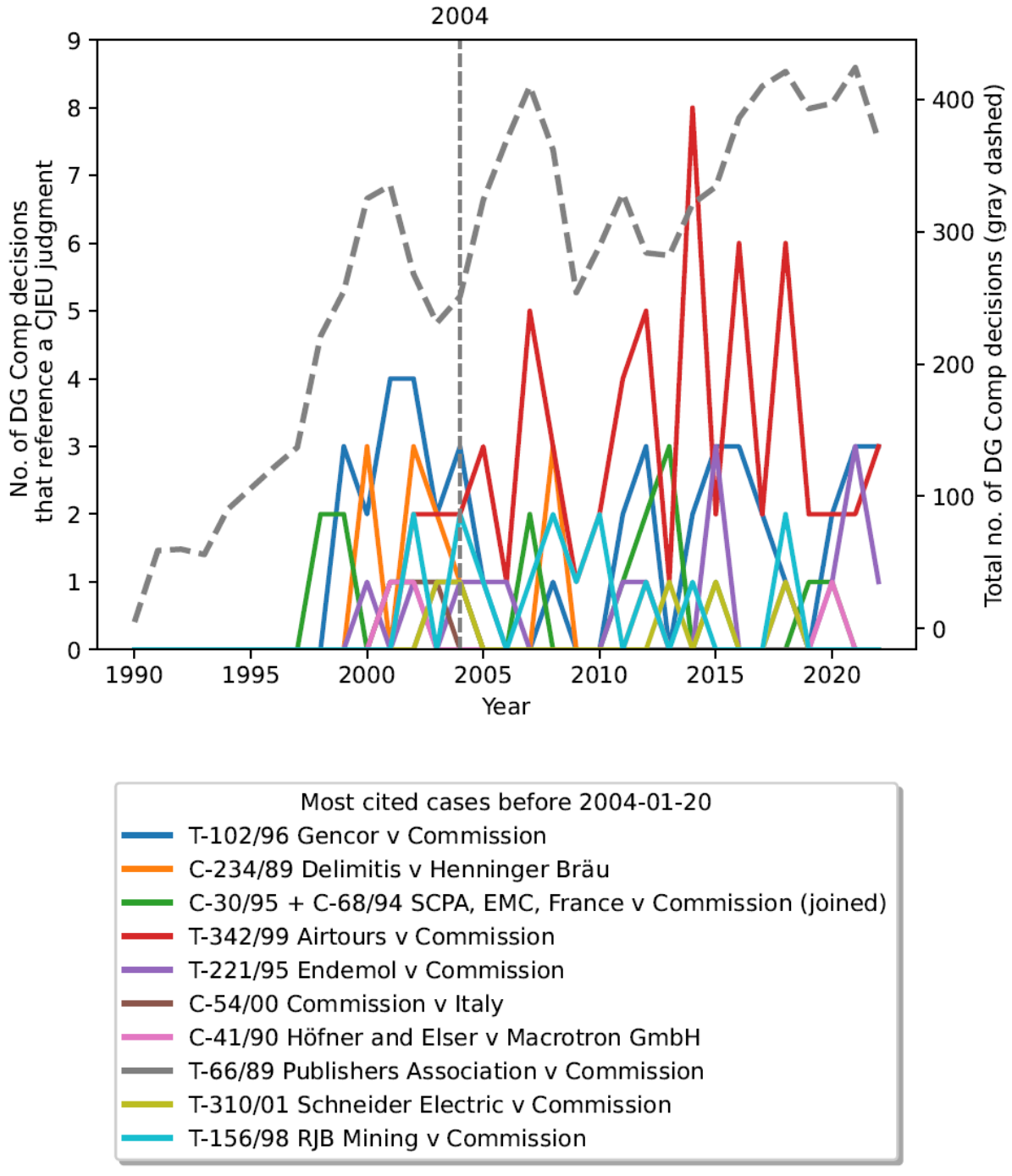
R-squared:	0.504	**Adj. R-squared:**	0.478
Covariance Type:	HC3		

	coef	**std err**	**z**	**P> |z|**
Intercept	-0.5270	0.094	-5.619	0.000
***Elapsed* fixed effects**	——— yes ———			
Yearly citation density	2.6038	0.440	5.919	0.000
Total citations of judgment	0.0252	0.007	3.812	0.000
Industry controls	——— yes ———			
Post ECMR year for pre-ECMR judgment	-0.0623	0.049	-1.266	0.206
Superceded	-0.1623	0.115	-1.412	0.158

The result in Table 10 is robust to running Equation 5 on only the pre-ECMR judgments (with a coefficient on the reform-control of -0.0737 and p-value of 0.251). How can we make sense of these conflicting results? The main mechanic is this: as citation density increases after the reform, pre-ECMR judgments do not keep up with the new post-ECMR case law. While it is not abandoned entirely, it does not see the same quick rise in citations as the new case law does. Instead, it sees a relative decrease. This is less readily captured by the dummy in model 5 than by the visual evidence in figure 3.

Figure 6 shows the raw time series of references to the 10 most important pre-2004 CJEU judgments. Airtours v Commission stands out from the others and has clearly left a mark long after the new regulation, likely dominated by its 62nd paragraph which outlines three conditions necessary for a finding of collective dominance. However, it is not clear that other judgments lose relevance either. There is a noticeable dip in the number of decisions made right around the reform (gray dashed line). This is driven by a corresponding drop in merger notifications, possibly because many firms chose to postpone M&A over the regulatory transition. Like with industry specificity, it seems that the reform-effect on pre-reform case law is marginal rather than binary. The pre-reform case law is cited less frequently after the reform, but it is far from abandoned.

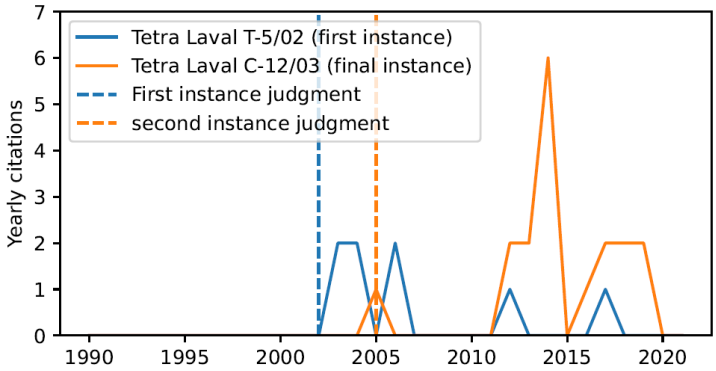
Figure 6: Raw time series for the top pre-2004 cases



4.5 Superceded judgments

The coefficient on *superceded* is not statistically significant. It applies to a very small subset of the data, so the test in the main model is probably under-powered to examine this effect. For a sense of the raw data, figure 7 shows the citations of the two Tetra Laval judgments. See Table 6 for context.

Figure 7: Tetra Laval citations



5 Concluding remarks

In this paper I describe the dynamics of an under-explored source of quantitative data in the study of European merger control. New CJEU case law seems to be implemented by DG COMP immediately rather than gradually and its legal relevance is not subject to a short expiry date, although its practical relevance may fluctuate with the M&A activity in certain industries. It seems clear that the 2004 ECMR reform did change the set of relevant merger control precedent, evidenced by falling citation frequencies for most pre-reform judgments after 2004, an observation which matches previous research on DG COMP's decision-making.

While the paper's main model is not specified to support causal inference, it does show a set variables that clearly correlate with citation frequencies and should be taken into account when considering case law citations in other applications. Table 2 shows the potential in terms of identifying legal reasoning on a range from generally permissive legal fact s to more prohibitive ones. Further work on the dataset should explore the explanatory power of case law with regard to DG COMP decision outcomes. Also, yearly citation frequencies should be investigated: is this a causal driver of case law citations or simply a statistical outcome driven by other underlying causes? If yearly citation

frequencies are important in a causal sense, then what drives the variation over time in citation frequencies? Variation in tactics and focus at the DG COMP as well as variation in the composition of proposed mergers would be good places to start looking.

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