Essays on Cartels and Competition Policy

Carmen García Galindo

Thesis submitted for assessment with a view to obtaining the degree of Doctor of Economics of the European University Institute

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European University Institute
Department of Economics

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Abstract

The aim of this thesis is to investigate cartels and the impact of competition policy from various angles.

Chapter 1, joint with Joan-Ramon Borrell, José Manuel Ordóñez-de-Haro and Juan Luis Jiménez, analyzes the relationship between cartel life cycles and business cycles. We analyze the relationship between cartel startups/breakups and economic cycles using a dataset of cartels sanctioned by the European Commission. Results show that cartels are more likely to be formed when the business has evolved positively in the previous months and managers expect prices to decline, but that cartels also tend to breakup when the business has evolved positively. Upturns in firm-specific business cycles appear to cause cartel turnovers: existing cartels die while new ones are set up.

Chapter 2 aims at obtaining a precise measure of how much firms benefit from collusion. I evaluate the causal effect of being a cartel member on the revenues and profits of cartelized firms, using comparable non-collusive firms as control group. A dataset of discovered cartel cases in Spain from 1990 to 2014 and an alternative dataset of firms' balance sheets are used. Results show that firms increase their revenues, on average, between 19% and 26% due to the collusive agreement, while no significant effect is found on profits. Estimations by cartel duration demonstrate that the members of long-lasting cartels not only increase their revenues ($29\% - 50\%$), but also their profits more than two times. Further analysis shows that cartels that are profitable from the beginning tend to last longer and do not apply for Leniency Programs.

Chapter 3, joint with Joan-Ramon Borrell, Juan Luis Jiménez and José Manuel Ordóñez-de-Haro, investigates how Leniency Programs destabilize cartels. We study the effect of the Leniency Program on cartel duration, cartel fines and on the years of investigation using a difference-in-differences program evaluation approach. Cartel cases discovered by the European Commission and the Spanish Competition Authority are analyzed. Results show a short-run effect of the Leniency Program: the detected cartels have longer duration than the ones in the control group. In the long-run, the program decreases cartel duration. On the other hand, no significant effect is found on fines, while the duration of the investigation decreases significantly around 0.8-1.3 years.

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Chapter 1

When are cartels more likely to be formed or broken? The role of business cycles

(joint with Borrell, J.R.[1], Ordóñez-de-Haro, J.M.[2] and Jiménez, J.L.[3])

Abstract

The literature presents mixed contributions about the economic conditions under which cartels form and collapse, and about how stable they are across firm-specific and industry-wide business cycles. The relationship between cartel life cycles and business cycles has not been sufficiently analyzed to date. In this paper, we study in depth whether collusion is pro-cyclical or counter-cyclical. We analyze the relationship between cartel startups/breakups and economic cycles using a dataset of sanctioned cartels by the European Commission that were active between 1997 and 2012, after the leniency program had already been introduced. We also double check whether this relationship has changed with respect to the pre-leniency period from 1991 to 1996. Our results show that cartels are more likely to be formed when the business has evolved positively in the previous months and managers expect prices to decline, but that cartels also tend to breakup also when the business has evolved positively. Upturns in firm-specific business cycles appear to cause cartel turnovers: existing cartels die while new ones are set up.

Keywords: Cartels; Business Cycles; Business Expectations, Antitrust.
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1.1 Introduction

In the last decades, the tools at the disposal of Antitrust Authorities in the fight against cartels have improved considerably, being the leniency program the cornerstone of the most important instruments. Although some improvements have been made, the fact that cartels are illegal makes them highly secretive, which complicates the task of proving their existence. Therefore, it would be useful to have some collusive markers or some screening instruments to monitor some markets with environments conductive to collusion.

In this paper we focus both on cartel formation and breakups[4], and how these two events relate to firm-specific business cycles, price expectations, and industry-wide cycles. We study how both the number of cartels formed or collapsing is determined not only by the demand or production faced by the firm recently and industry economic situation, but also by the expectations that firms' managers have.

There are two seminal papers that linked business cycles and cartel stability: Rotemberg and Saloner (1986) and Haltiwanger and Harrington (1991). However, the former shows that collusion is counter-cyclical while the latter that it is pro-cyclical. All in all, collusion may be one or the other depending on the modeling details that prevail on the data.

Several papers have also tried to offer insights on this question[5] (Bagwell and Staiger, 1997; Fabra, 2006; Levenstein and Suslow, 2011 & 2016; or Antonielli and Mariniello, 2014). Moreover, there are meta-analyses of case studies of cartel cases formed before the 1950's (Levenstein and Suslow, 2006); or other papers relate the percentage of months in the sample in which an economic downturn took place and the percentage of cartels formed in that period (Suslow, 2005). However, the question of whether collusion is pro-cyclical or contra-cyclical is however unsettled.

To address the question of interest we work with the number of cartels formed and with the number of cartels broken due to internal reasons in the European Union every month, mostly after the introduction of the leniency program (1997-2012), but also during the pre-leniency period, ranging from 1991 to 1996. By estimating a Poisson regression model, we analyze which is the effect of the sector real production (industry growth or decline), of the business evolution perceptions in the last months (firm perceived dynamics) and the managers' price expectations on the likelihood of setting-up or breaking-apart of any cartel.

The contributions of this paper are several. First, we study cartel formation itself, using a

[4]We are aware of the fact that working with discovered cartels is a limitation of the data. Several papers in the literature have looked at what may the discovered cartels tell us about the underlying cartel population (Harrington and Wei, 2017; Harrington and Chang, 2009). Our results are only applicable to discovered cartels.

[5]For a revision of this literature, see Levenstein and Suslow (2014).

EU database, which has not been broadly studied from an empirical point of view so far. Second, we make use of business surveys to analyze cartel formation and breakups, a link that has not been used in the literature for this purpose despite the information it provides regarding business managers' point of view. Finally, we do not only analyze the relationship between cartels and business cycles, but we do also analyze the effect of price expectations on cartel formation and breakup.

Our results show that cartels are more likely to be formed in firms' upturns, but also that cartels tend to breakup also in firms' upturns. Cartels are more prone to be created when managers consider that their firm production has evolved positively in the last three months (growing firms) and when they expect a decrease in prices in the near future (with expected declining prices). Cartels are more likely to collapse also when managers consider that their firm production has evolved positively in the last three months. Upturns in firm-specific business cycles and downturns in economic cycles appear to cause cartel turnovers: existing cartels die while new ones are set up. However, the effect of business evolution on cartel breakup is lower when the industry real production is high.

This paper is structured as follows. After this brief introduction, the second section contains a review of the literature. The data is described and discussed in section 3. Section 4 details the empirical strategy, before analyzing the results in section 5. Section 6 offers a wide set of robustness checks. Finally, the main conclusions of this work are discussed in section 7.

1.2 Literature Review

The literature that relates business cycle and cartels focuses mostly on the stability of cartels instead of cartel startups or breakups. It has been studied the effect of business cycle on cartel duration and collusive pricing from a theoretical and empirical point of view, although conclusions are contradictory.

The classic cartel dilemma was established by Stigler (1964) and developed by Tirole (1988). It analyzes if collusion can be sustained if the expected profit from colluding today outweighs the expected profit of defecting from a cooperative agreement (see Levenstein and Suslow, 2016, for further explanation). In this model, the impatience of any firm owners, so the firm-specific risk premium is a cornerstone of the cartel stability.

From the theoretical point of view, the model proposed by Rotemberg and Saloner (1986) is a pioneering work linking business cycles and cartel life cycles. Their model looks at collusion and business cycles, defined as a boom or recession of demand, but assumes that the level of

demand is determined each period from an independent and identically distributed process. So, in their setup, booms and busts occur from time to time with some known probability, but it cannot be predicted when they finally happen.

The authors investigate the effect of such business cycles of booms and busts on optimal collusive pricing. They conclude that for moderate values of the discount factor (mature industries, or at least not too risky industries or activities), collusion is countercyclical: cartels are more likely to break up in demand booms as deviation today from collusive price is less costly in terms of foregone profits in the future, and also the gain of deviating from a collusive agreement is greatest during booms. Additionally, collusive price is countercyclical: firms also tend to price competitively in demand downturns.

By contrast, Haltiwanger and Harrington (1991) present a model that allows for both the level of current demand and firms' expectations on future demand to change over time. Those authors conclude that while the gain of deviating from a collusive agreement is greatest during booms, firms find it even more difficult to collude during recessions, as the forgone profits from inducing a price war are relatively low. Therefore, collusion is pro-cyclical, more difficult in busts, easier in booms.

These two seminal contributions differ with respect the dynamics of the business cycle, obtaining completely contradictory results: when booms and bust come from i.i.d processes, collusion is countercyclical; but, when business expansions and downturns have some correlation over time, collusion is pro-cyclical.

Bagwell and Staiger (1997) extend the model of collusive pricing assuming that demand movements are stochastic and persistent. A Markov process determines the transition between slow-growth and fast-growth states. In this case, they show that collusive prices are weakly pro-cyclical when demand growth rates are positively correlated through time.

The authors note that the empirical evidence is mixed since there is both pro-cyclical and countercyclical pricing (see, among others, Bils, 1987a; Domowitz, Hubbard and Petersen, 1986a, 1986b, 1987; and Rotemberg and Saloner 1986).

Extending the Rotemberg and Saloner (1986) model by introducing capacity constraints, Fabra (2006) shows that when capacity constraints[6] are sufficiently tight, firms find it more difficult to collude during booms, whereas the contrary is true for larger capacity values.

[6]Athey and Bagwell (2001) study a model of collusion in which firms receive an iid cost shock, and Athey and Bagwell (2008) analyze a model of collusion in which the cost shock is persistent. However, business cycles are not considered.

From an empirical point of view, Hyytinen, Steen and Toivanen (2011) use a hidden Markov model, which consists of a hidden process (the industry cartel dynamics in this case, since there may be industries never investigated or convicted) and an observation process that reveals information on the state of the hidden process for some periods (what the researcher knows about the state of the industry in a given period). They find that the chance of forming a cartel is around 20%, increases over their sample period and responds to positive shocks to GDP being then pro-cyclical.

Suslow (2005) uses an empirical model to test for the importance of demand uncertainty and cartel organizational characteristics in determining cartel duration. She finds out that economic uncertainty, measured as the fluctuation of an industrial production index, accounts for most of the variance in the duration of the cartel agreements. Also Levenstein and Suslow (2011) analyze the impact of cartel organizational features, as well as macroeconomic fluctuations and industry structure, on cartel duration. They find that firm-specific measures of impatience are systematically related to cartel breakup. In a later work (Levenstein and Suslow, 2016), these authors found a positive relationship between market interest rates and probability of cartel breakup, outcome that they did not find for the international cartels analyzed in their previous work.

Table 1 summarizes the contributions of the literature on the relationship between collusion and business cycles.

As we have seen above, theory papers are offering different conclusions with respect to the relationship of cartel stability and the business cycle, and also the evidence provided by the empirical literature is mixed. There is a gap in the literature to study more in depth to what extend and in which circumstances collusion is pro-cyclical or counter-cyclical.

Instead of relying in country data, we are going use data coming from business surveys regarding firm-specific business dynamics and price expectations, and also industry specific production dynamics to analyze startups and breakups of cartel discovered and sanctioned by the European Commission.

Therefore, the objective of this paper is to study empirically whether the conditions or the variables related to the firm and industry business cycle affect the stability of collusive agreements, and to what extend such variables affect the formation and breakup of these illegal agreements symmetrically.

Table 1.1: *Empirical and theoretical literature regarding business cycles and cartel formation/breakups.*

Year	Authors	Methodology	Database	Results
1986	Rotemberg and Saloner	Theoretical	-	**Collusion is counter-cyclical.** Collusion is more difficult in booms, easier in downturns.
1991	Haltiwanger and Harrington	Theoretical	-	**Collusion is pro-cyclical.** Firms find easier to collude during booms, more difficult to collude during recessions.
1997	Bagwell and Staiger	Theoretical	-	**Collusion is pro-cyclical,** more likely in booms, when demand growth rates are positively correlated through time.
2005	Suslow	Proportional Hazard Model	71 international manufacturing and commodities cartels: 1920-1939	**Collusion is pro-cyclical:** economic uncertainty, measured as the fluctuation of an industrial production index, accounts for most of the variance in the duration of the cartel agreements.
2006	Fabra	Theoretical	-	**Collusion is counter-cyclical** when capacity constraints are sufficiently tight, firms find it more difficult to collude during booms. The contrary is true; **collusion is pro-cyclical,** for large capacity values.
2011	Hyytinen, Steen and Toivanen	Hidden Markov Model	109 legal Finnish manufacturing cartels: 1951-1990	**Collusion is pro-cyclical.** The chance of forming a cartel is around 20%, it increases over their sample period and responds to positive shocks to GDP.
2011	Levenstein and Suslow	Proportional Hazard Model	81 international cartels (US or EC): 1990-2007	Firm-specific measures of impatience (firm risk premiums) are systematically related to cartel breakup.
2016	Levenstein and Suslow	Proportional Hazard Model	247 US non-bid-rigging cartels: 1961-2013	Cartels are more likely to break up during periods of high real interest rates.

Source: Own elaboration.

1.3 Data

The dataset has been constructed from the European Commission's decisions in cartel cases between 1976 and 2012. The European Commission has sanctioned 121 cartels[7] over this period. Most of these cartels were discovered due to investigations initiated by the Commission's own initiative or following complaints by third parties (67 cartel cases), and the other 54 cartel cases have been detected under the EU Leniency Program since it was set up in 1996.

[7]Further information regarding the nature of these cartels can be found in Ordóñez-de-Haro, Borrell and Jiménez (2018).

From the published Commission's decisions we obtained information regarding the formation and breakup date of the cartels. The former corresponds to the first moment for which the authority has evidence of a collusive agreement, which usually is a date before the starting date of the investigation. The latter is the moment in which the cartel breaks up, which could be a date before or after the opening of the investigation procedures.

In this paper, we are going to analyze the likelihood of two different events across time (months): (1) the event of a cartel setup in the manufacturing industry in the EU in any month from January 1997 to December 2012 according to the European Commission files of sanctioned cartels; (2) the event of a cartel breakup in the manufacturing industry in the EU in any month from January 1997 to December 2012 according to the European Commission files of sanctioned cartels. We also double check whether the introduction of the leniency program in 1996 changed the impact of economic cycles on cartel set up and breakup using evidence of the previous pre-leniency period from January 1991 to December 1996[8].

When studying cartel breakups, we restrict the empirical analysis to the cartels for which the breakups are due to cartel internal reasons. We qualify a cartel breakup as one due to "internal reasons" whenever the cartel breakup date precedes the date at which the investigation started, and also all the cases that start by a leniency application.

Recall that cartel investigations may have been launched by four different means: (1) Commission own initiative (*ex-officio*), (2) third party complaint, (3) after a cartel member notification during the pre-2004 regime in which agreements among firms had to be notified and could be authorized by the Commission (when they were not authorized, a cartel investigation could also be launched), (4) after one or more cartel members notified their participation in a cartel and applied for a lenient sanctioning treatment under the leniency program available since 1996 onwards.

We do only focus on these cases in which the break-up is due to internal reasons because we are interested in analyzing firms' behavior regarding cartel activities taking into account their appraisal about the business cycle: firm perceptions of past business evolution, firm selling price expectations, and EU real production cycles. We leave for further investigation the question of what are the drivers of cartels breakups when there is an external action such as an European Commission investigation triggered by the EC own initiative (with or without having denied an agreement authorization), after a third partly complain, or when the break up occurs sometime

[8]The time series of business evolution and price expectations starts in January 1985, while the series of production index at European level starts in January 1991. This is the reason why our pre-leniency sample only goes back only until January 1991.

after one of the cartel members apply for the leniency program.

We have also computed the date of the final decision adopted by the European Commission to create a variable called *sanctioned cartels* (the number cartels sanctioned every month), which will control for the potential destabilizing effect of the European Commission cartel law enforcement on existing cartels, and its deterrent effect on cartels that would not have formed yet.

We restrict the empirical analysis to the manufacturing industry sector (sector C in NACE Rev.2 classification) because it is the only sector for which we have information for all the independent variables, i.e., business evolution, price expectations and the production index[9]. Although we are not able to exploit the fact that more and different industries different from manufacturing are cartelized due to the unavailability of data of either the business surveys or economic data, we do not consider this to be a major problem for two reasons.

First, 96 out of the 121 cartel cases sanctioned between 1976 and 2014 by the European Commission that we use to construct our dataset belong to the manufacturing sector (79%)[10]. As shown in Table 2, 71 out of 98 cartels sanctioned between 1991 and 2012, the whole time period pre- and post-leniency program used below in our estimates, belong to the manufacturing sector (72%). And also, 67 out of 85 cartels sanctioned in the post-leniency program period from 1997 to 2012 belong to the manufacturing industry (79%).

In addition, 48 out of the 96 cartel cases in the manufacturing sector have been discovered under the Leniency Program (50%)[11], which means that the firms cooperate with the European Commission and therefore, the date of formation reflects on average more closely the start of the collusive agreement[12].

On the other hand, if we look at the EU-27's non-financial business economy at the NACE section level, the manufacturing sector is summing up a wide variety of activities. Additionally, around 10% of all enterprises belong to the manufacturing industry. Moreover, within the EU-27's non-financial business economy, in 2012, the manufacturing was the second largest in terms of its contribution to employment (22.6%) and the largest contributor to value added (26.8%)[13]. Given the importance of the manufacturing sector in the EU, it is relevant to focus

[9]There are some sectors for which we have information, but not enough data to perform the full analysis. The baseline regressions including more sectors will be presented in the robustness check section.

[10]Sorting the sectors by number of cases discovered, the Manufacturing Sector is followed by the Transportation and Storage Sector (sector H), which has 12 cases.

[11]In 54 out of 121 cartel cases considered (44.63%), the European Commission's investigation was initiated following applications for leniency.

[12]Actually, 15 out of the 19 cases for which we analyze cartel formation and 45 out of the 50 cartels for which we study the breakup in the period 1997-2012 were discovered under the leniency program (78.95% of the formed ones and 90% of the broken ones).

[13]Source: Eurostat.

in this industry. Moreover, as Levenstein and Suslow (2014) state, there are some industries that seem particularly prone to collusion activity. Specifically they cited those characterized by high fixed costs as in manufacturing sector.

Table 1.2: *Summary of cartel cases sanctioned by the European Commission (1991-1996 & 1997-2012) (% of cartels belonging to the manufacturing sector).*

	All Sectors		**Manufacturing Sector**	
	1991-1996	**1997-2012**	**1991-1996**	**1997-2012**
Cartel sanctioned	13	85	4 (30.77%)	67 (78.82%)
Cartel set up	33	29	25 (75.76%)	19 (65.52%)
Cartel break up	21	80	12 (57.14%)	62 (77.5%)
Cartel break up (internal reasons): A+B, A+a+b	7	59	5 (71.43%)	50 (84.75%)
A. Breakup before opening investigation (no leniency)	4	8	2 (50%)	5 (83.33%)
B. Investigation started by leniency program	3	51	3 (100%)	45 (88.24%)
a. Breakup after leniency application	0	19	0	15 (78.95%)
b. Breakup before leniency application	3	32	3 (100%)	30 (93.75%)

Note: Own elaboration from EC publicly available decisions.

As Table 2 shows, 50 out of 67 cartels of the manufacturing industry sanctioned by the EC after 1997 broke up due to internal reasons, not as a result of a targeted investigation initiated by the Commission on its own initiative or following a complaint. 45 out of those 50 were initially investigated using the leniency program, so the leniency application of one cartel member enabled the Commission to initiate an investigation: 30 of them broke apart before the leniency application, while 15 of them broke apart after the leniency application. The other 5 out of 50 broke apart before the Commission started the investigation by its own initiative or third party complaint, not using the leniency program. Finally, only 19 (discovered) cartels were started up after January 1997. Table 2 also shows that most of the cartels sanctioned belong to the manufacturing industry, particularly in the 1997 to 2012 period.

The variables we are going to model are denoted as *formed cartels* and *broken cartels*. The former tells us the number of (discovered) cartels that were formed each month[14] of the database in the manufacturing sector. The latter is the number of (discovered) cartels in the manufacturing sector that were broken due to internal reasons each month of the period studied[15].

As summarized in Table 3, the variable regarding monthly cartel formation takes value from 0 to 3, while the one of monthly cartel breakup takes value from 0 to 4. The variable *sanctioned cartels*[16] takes value from 0 to 6, which means that up to 6 cartels were sanctioned the same

[14]Brenner (2009) also analyzes time series data on discovered cartels by the EC.

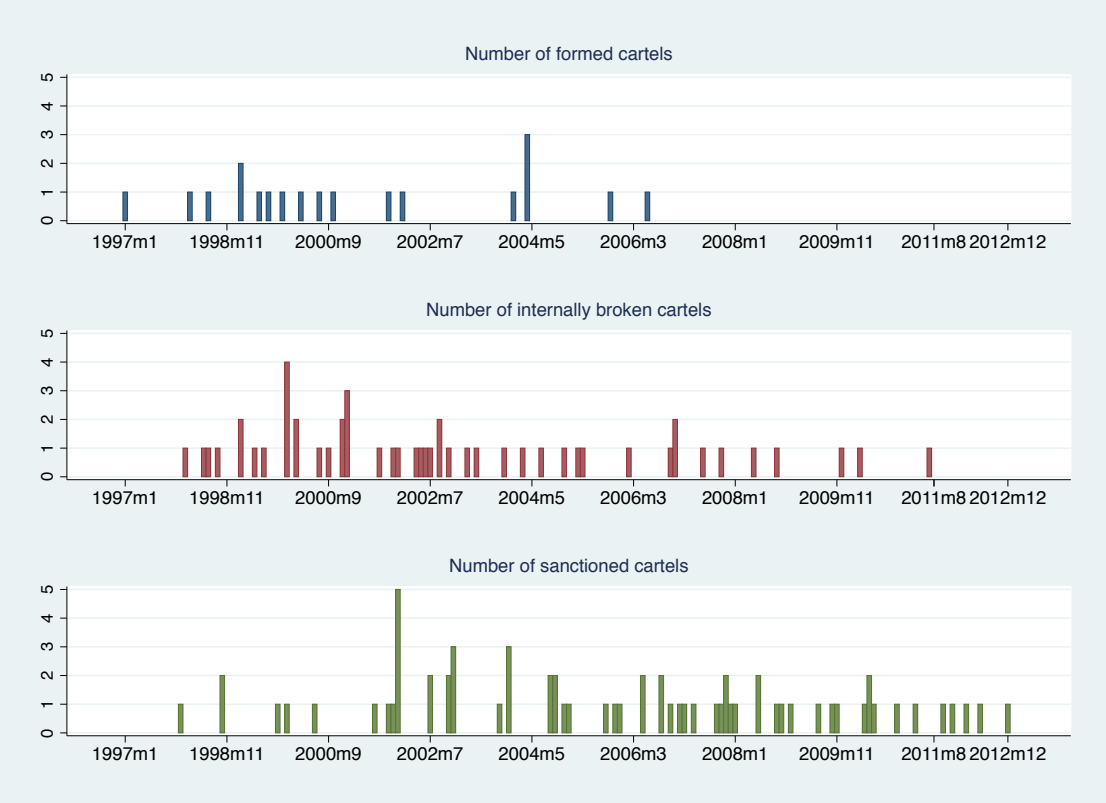
[15]As explained before, we include all the cartels that collapsed due to internal causes: they had broken up before they were discovered by the Commission or a third party, and also those that broke because one of the members applied for leniency. In the latter case, we use the date of the application as breakup date if application occurred before the collusive agreement completely collapsed as leniency applications are noticed and the data shows that it does not take long to completely collapse.

[16]This variable includes the cartels sanctioned by the European Commission every month in all sectors, since the discovery of a relevant cartel in another sector could increase the deterrent effect of the Commission in any unrelated sector. Results are robust to considering only the cartel cases sanctioned in the manufacturing sector.

month by the Commission.

Figure 1 depicts all three variables over time. As we focus in the cartels sanctioned between January 1997 and December 2012 that at the same time were formed in that time span, the figure shows that cartels sanctioned were mostly set up at the first half of the period under study. By contrast, the figure shows that sanctioned cartels break up all along the period under study, since many of them were formed before 1997. The cartels sanctioned during the period of study refer to cartels born before or after 1997 in any sector, although the cartels sanctioned in Figure 1 refer only to the manufacturing sector.

Figure 1.1: *Number of monthly formed, sanctioned and broken by internal reasons cartels in the manufacturing sector (1997-2012).*



Source: Own elaboration from EC publicly available decisions.

As noted above, we introduce a novel approach with regard to the related literature by taking into account business managers' expectations. The variables *business evolution* and *price expectations* were obtained from the Business and Consumer Surveys, which is a harmonized survey managed by the Directorate-General for Economic and Financial Affairs of the European Commission[17]. The data consists[18] of monthly time series according to the Classification of economic

[17]The Joint Harmonized EU Program of Business and Consumer Surveys, User Guide, 2007. Source: `http://ec.europa.eu/economy_finance/db_indicators/surveys/index_en.htm`

[18]Additionally, the Business Survey presents the data at a more aggregated level. In particular, they divide the NACE sectors in five big categories: industry, construction, retail trade, services sector and financial services.

activities in the European Community (NACE) at the two digits level, so it is disaggregated by sector and sub-sector.

According to Taylor and McNabb (2007), the business confidence indicator is pro-cyclical and it can generally predict movements in GDP over the business cycle and downturns.

Since the process of forming a cartel takes some time, we are glad to use the survey variable on *price expectations* instead of the current price as the decision of engaging in cartels today could be more affected by price expectations in the future than by current price levels.

Both variables are indices[19], and they correspond respectively to the questions "*how has your production developed over the past 3 months?*" and "*how do you expect your selling price to change over the next 3 months?*" The surveys are conducted at firm level, and then the data is classified and aggregated by stratum (i.e., by sector) and countries, using adequate weights[20]. The geographical coverage of the surveys includes all Member States as well as the candidate countries, although we do only use Member States results. The harmonized surveys are carried out at national level by national institutes and offices. The sample size for each survey varies across countries according to the heterogeneity of their economies and their population size. The sample of the industry survey includes more than 38,000 units that are surveyed every month, in the first two-three weeks of each month. The industry survey is largely qualitative.

The survey's questions of interest for our study admit three possible responses: increase, remain unchanged and decrease. Answers obtained from the surveys are aggregated in the form of balances, which are constructed as the difference in the percentage of positive and negative answers. The information provided allows the use of a range of variables to monitor cyclical dynamics.

We have used non-seasonally adjusted data for *business evolution, price expectations,* and *production EU*. The variable *production EU* has been obtained from Eurostat. The time series corresponds to monthly data of the volume index of production in industry (manufacturing sector), at European level (EU19) and where 2010=100. The variable *production change* measures the difference between *production EU* in a given month and the previous month, if it is denoted with *(-1)*; and the difference with respect to the same month in the previous year if it is denoted with *(-12)*. Similarly, the variable *production growth rate* accounts for the growth rate of the variable *production EU*. The descriptive statistics of these variables are shown in Table 3.

This classification is the one we use for our analysis.

[19]The data of the Business and Consumer Survey is at EU level.

[20]See footnote 12. Further information about the data treatment can be found in the Methodological User Guide.

Table 1.3: *Summary statistics (1997-2012).*

Variable	N	Mean	Std. Dev.	Min	Max
Formed Cartels	192	0.099	0.36	0	3
Broken Cartels due to Internal Reasons	192	0.260	0.58	0	4
Business Evolution	192	0.312	12.01	-49	19
Price Expectations	192	4.562	7.92	-14	23
Sanctioned Cartels	192	0.443	0.82	0	6
Production EU	192	101.23	10.19	68.8	122.8
Production Change (-1)	192	0.055	12.15	-28.1	30.5
Production Change (-12)	192	0.911	6.21	-25.4	9.2
Production Growth Rate (-1)	192	0.009	0.13	-0.29	0.43
Production Growth Rate (-12)	192	0.012	0.06	-0.22	0.10

Source: Author's computations from EC publicly available decisions and the Business Survey. Manufacturing sector only.

1.4 Empirical strategy

The purpose of this paper is to study the drivers of cartel formation and breakup. We analyze which is the role of managers' perception about the evolution of their business and expectations they have regarding the selling price, and also the effect of the industry production on the dependent variables of interest. Our empirical strategy uses Poisson model estimations, given that our dependent variable is a count variable. Even the value zero has positive probability of occurrence in both cases (formed and broken cartels).

The basic Poisson model assumes that y given x has a Poisson distribution, and the density of y given x is completely determined by the conditional mean. Another assumption imposed by Poisson distribution is that the conditional variance is equal to the conditional mean. However, since this assumption it not usually satisfied in the data, there are alternative characterizations of the model such as the Negative Binomial Regression, when there is over-dispersion.

We have tested whether this is the case in our data, but the test concludes that there is no over-dispersion, so we can estimate a Poisson regression. Nevertheless, even if it was the case, according to Cameron and Trivedi (2009) the Poisson panel estimators rely on weaker distributional assumptions than the negative binomial model, so that it would be more robust to use the Poisson panel estimators with cluster-robust standard errors to resolve the usual over dispersion.

We should also note that working with information regarding cartels means that we can only know characteristics of the uncovered cartels. Therefore, if the variables *formed* or *broken cartels* take the value of zero it could be because no cartel was formed or broken that specific month or because it was formed or broken but it hasn't been discovered. Unfortunately, we cannot distinguish between these two types of selection, as it is also the case in the Zero Inflated Poisson (ZIP) regression. However, this model tries to capture which are the relevant variables that cause the count to be zero. Therefore, we have also worked with this alternative model

specification, but results are not shown in the paper since the Voung (1989) test[21] of ZIP vs Poisson does not favor the former model.

Summing up, the equation of the full model estimated in order to estimate the relationship between formed cartels and expectations is the following one[22]:

$$\begin{aligned} log\left(\mathbb{E}\left(formed\_cartels_t|\mathbf{x}\right)\right) = \beta_0 &+ \beta_1 business\_evolution_t + \beta_2 price\_expectations_t \\ &+ \beta_3 sanctioned\_cartels_t + \beta_4 production\_EU_t \\ &+ \beta_5 production\_change_{t-1} + \beta_6 production\_change_{t-12} \\ &+ \beta_7 production\_growth\_rate_{t-1} + \beta_8 production_growth\_rate_{t-12} \end{aligned} \tag{1.1}$$

where x denotes the vector of independent variables. We estimate different specifications of the model by gradually introducing the explanatory variables.

Regarding the estimation of the equation to explain why cartels break internally, the empirical strategy is the same as in equation [1], but substituting the variable of formed cartels by broken cartels as dependent variable.

$$\begin{aligned} log\left(\mathbb{E}\left(broken\_cartels_t|\mathbf{x}\right)\right) = \beta_0 &+ \beta_1 business\_evolution_t + \beta_2 price\_expectations_t \\ &+ \beta_3 sanctioned\_cartels_t + \beta_4 production\_EU_t \\ &+ \beta_5 production\_change_{t-1} + \beta_6 production\_change_{t-12} \\ &+ \beta_7 production\_growth\_rate_{t-1} + \beta_8 production\_growth\_rate_{t-12} \end{aligned} \tag{1.2}$$

As previously mentioned, we will gradually introduce covariates in both estimations in order to control for different effects by groups of exogenous variables. Results are included in the following section.

1.5 Results

The results of the Poisson regression model for equation [1] are shown in Table 4. As stated above, we have considered alternative specifications of the model. Note that the standard errors have been corrected using the Newey-West estimator in order to overcome potential problems of autocorrelation and heteroskedasticity. The results shown in the table are the point estimates of the beta coefficients[23].

First of all, we can observe how regardless of the model specification the managers' perceptions of the evolution of the firm's business in the last 3 months affects positively and significantly

[21] It tests the null hypothesis that the two models fit the data equally well.

[22] The error term does not appear in equation [1] because the model is expressed in terms of the conditional expectation.

[23] They tell us that one unit increase in the independent variable will increase the average number of the dependent variable by β percent.

the number of cartels formed. Increases of the *business evolution* index in one unit will increase the average number of formed cartels by around $5.3 - 6.0\%$. This suggests that cartel formation is strongly pro-cyclical with respect to firm growth: the likelihood of cartel set up is related to firm growth in the near past.

Table 1.4: *Poisson estimation results. Formed cartels (1997-2012).*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	0.060**	0.053*	0.054*	0.067*	0.054**	0.080***
	(0.026)	(0.028)	(0.028)	(0.034)	(0.027)	(0.031)
Price Expectations	-0.072**	-0.064**	-0.071**	-0.062**	-0.075**	-0.060*
	(0.030)	(0.031)	(0.030)	(0.031)	(0.030)	(0.031)
Production EU	-0.017***	-0.008*	0.003	-0.009**	0.008	-0.012***
	(0.005)	(0.005)	(0.008)	(0.005)	(0.007)	(0.005)
Sanctioned Cartels		-1.065***	-1.106***	-1.067***	-1.127***	-1.064***
		(0.192)	(0.200)	(0.193)	(0.205)	(0.195)
Production Change (-1)			-0.012**			
			(0.006)			
Production Change (-12)				-0.033		
				(0.038)		
Production Growth Rate (-1)					-1.708***	
					(0.458)	
Production Growth Rate (-12)						-6.696*
						(3.542)
Constant	-0.398	-1.135**	-2.169***	-0.964**	-2.615***	-0.620
	(0.472)	(0.454)	(0.736)	(0.470)	(0.663)	(0.504)
Observations	192	192	192	192	192	192
Chi^2	4.610	6.607	6.717	6.739	7.033	7.082
p-value	0.203	0.158	0.243	0.241	0.218	0.215

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Newey-West standard errors within brackets.

Secondly, the higher the prices are expected to be at the beginning of the month for the next three months, the lower the number of cartels formed that month. The effect of an increase of one unit in price expectations is that the formation of cartels is reduced between 6% and 7.5%. On the other way round, collusion is more likely when firms expect a decline in prices in the near future: collusion is more likely with expected declining pricing.

It is also remarkable that the number of total sanctioned cartels (in the manufacturing and the other industries) in the same period influences negatively cartels formation. Since the decision of the European Commission is public, seeing that more cartels are being sanctioned seems to be an effective deterrent because it could increase the perceived probability of being caught.

Finally, results show that the effect of the industry real production index at the EU level negatively and significantly affects cartel formation in most of the specifications. This reflects weak evidence that the pro-cyclicality of cartel formation and firm-specific business cycle is less strong when the sector *production* is low. However, we cannot draw any conclusion regarding the effect of the variables considering production growth in the sector on cartel setting up.

In our results for the post-leniency period, the *business evolution* has a positive effect on cartel set up. Moreover, if firms' managers expect that prices will increase in the current and next two months, then fewer cartels are formed. Therefore, in this baseline model, what matters most for cartel formation is the perception that businessmen have about the evolution of their own production and the prices at which they expect to be selling their products in the market the following months, while the sector production also affects the decision of cartelizing or not.

Empirical results regarding cartel set up appear to show that collusion is pro-cyclical with respect to firm-specific business cycle, these results are robust to the ones obtained in the strand of the theoretical literature started by the seminal paper of Haltiwanger and Harrington (1991) who showed that it is easier to collude during booms and more difficult to collude during recessions, as also in the paper by Bagwell and Staiger (1997), and in the case of non-binding capacity constraints studied by Fabra (2006).

Table 5 presents the results of the estimations of equation [2] concerning the explanatory variables of the breakup of cartels.

Table 1.5: *Poisson estimation results. Internally broken cartels (1997-2012).*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	0.019	0.019	0.019	-0.013	0.019	-0.009
	(0.018)	(0.018)	(0.018)	(0.031)	(0.018)	(0.032)
Price Expectations	-0.013	-0.013	-0.014	-0.018	-0.014	-0.018
	(0.023)	(0.023)	(0.026)	(0.022)	(0.026)	(0.021)
Production EU	-0.002	-0.001	0.001	0.003	-0.000	0.004
	(0.007)	(0.008)	(0.014)	(0.007)	(0.013)	(0.007)
Sanctioned Cartels		-0.055	-0.061	-0.057	-0.058	-0.057
		(0.120)	(0.132)	(0.118)	(0.132)	(0.121)
Production Change (-1)			-0.003			
			(0.009)			
Production Change (-12)				0.077**		
				(0.039)		
Production Growth Rate (-1)					-0.140	
					(0.766)	
Production Growth Rate (-12)						7.025*
						(4.115)
Constant	-1.092	-1.150	-1.405	-1.606**	-1.266	-1.787***
	(0.777)	(0.847)	(1.440)	(0.711)	(1.341)	(0.671)
Observations	192	192	192	192	192	192
Chi^2	1.256	1.342	1.382	2.623	1.353	2.454
p-value	0.740	0.854	0.926	0.758	0.929	0.783

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Newey-West standard errors within brackets.

Contrary to equation [1], the Poisson estimations of the relationship between managers' appraisal of past production and selling price expectations and the breakup of cartels by internal reasons in the manufacturing sector yield non-conclusive results. The number of cartels broken does not appear to be related neither with the business evolution in the last three months, nor with the level of prices expected by firms' managers, nor with the number of sanctioned cartels

in the same or the previous months. However, we will see later on that we can extract some conclusions regarding cartel breakup when we include more sectors in the specification of our model[24].

Summing up, all these results combined tell us that there are asymmetries in the effect of economic cycles on cartel setup and breakup: if the firm is doing well in terms of their own production and the managers expect their selling price to decrease in the next periods, then firms will get involved in a cartel to get larger profits (pro-cyclical cartel setup). On the other hand, no conclusions can be drawn with respect to the collapse of cartels in the manufacturing sector, although we will see that including more sectors allow us to identify some effects of firm-specific and EU-wide business evolution, and price expectations, on cartel breakup.

1.6 Robustness Checks

In this section, we carefully deepen the analysis of our time series and check whether our results are robust to different model specifications. We first consider the inclusion of more sectors in the baseline regressions. The reason why we include this framework as robustness check is that due to data limitations we cannot perform all the desired analyses on the sample formed by several sectors.

Next, we follow the procedure explained below to check for endogeneity problems in both cartel formation and cartel breakup. We will also double check to what extend the relationship between economic cycles and collusion has remained stable in the post-leniency period (1997-2012) with respect to the pre-leniency period (1991-1996) in the EU. Finally, we analyze the relationship between sanctioned cartels and business cycles, in order to understand the economic conditions under which the Commission makes decisions, and to rule out any identification problem of the estimated relationship between cartel formation/breakup and business cycles.

1.6.1 Inclusion of additional sectors and endogeneity

The first step we follow, in order to obtain more robust and reliable results, is to include more sectors in our specifications to introduce more variability. We will see that results hold in the case of cartel formation, and we obtain some effects in the case of cartel breakup.

[24]Levenstein and Suslow (2011) study the determinants of cartel breakup. The authors find that fluctuations in firm-specific discount rates have a significant effect on cartel duration, whereas market interest rates do no. Also, other cartel organization features are relevant in explaining cartel duration.

Secondly, we consider a potential problem as robustness check. There are grounds for the suspicion that the variable *price expectations* may present endogeneity problems, since the expectations expressed by the firm's manager may reflect the fact that the firm has recently decided to join a cartel (decision which may or may not be observed by the econometrician later on). Therefore, we instrument *price expectations* using covariates that could explain these expectations but that are exogenous to the cartel formation[25]. By using a model of Instrumental Variables, we manage to capture only the exogenous part of this variable, this is to say, to capture the underlying variance in price expectations independently from being cartelized or not. As excluded instruments we use one lag of the dependent variable[26], the evolution of the price index in other sectors (Construction and Electricity, gas, steam and air conditioning supply) and time fixed effects. We conclude that results do not change when this model is used and according to the test of endogeneity, it is not necessary to treat the variable *price expectations* as endogenous.

1.6.1.1 Cartel Setup

As explained above, we start by showing the results of the inclusion of additional sectors in the main regressions of cartel formation. Results are shown in Table 6a and 6b. First note that the conclusions obtained from Table 4 still hold: *business evolution* affects cartel formation positively and significantly and *price expectations* affect cartel formation negatively and significantly. However, the variable *production* is not significant in this setting, while the effect of *turnover* is negative, and statistically significant. Similarly to the results regarding the manufacturing sector, this reflects weak evidence that the pro-cyclicality of cartel formation and firm-specific business cycle is less strong when the sector *turnover* is low.

[25]This idea has been obtained from Perdiguero (2010), although the procedure is slightly different in our case.
[26]Results do not change if the lag of the instrumented variable is not used as instrument.

Table 1.6: *Poisson estimation results. Formed cartels (1997-2012). Additional sectors.*

Table 1.6.A *Poisson estimation results. Formed cartels (1997-2012). Sectors C, F, H & K.*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	0.110***	0.109***	0.105***	0.132***	0.105***	0.138***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Price Expectations	-0.092***	-0.093***	-0.087***	-0.088***	-0.089***	-0.085***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Turnover EU	-0.080***	-0.070***	-0.077***	-0.055***	-0.075***	-0.056***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Sanctioned Cartels		-0.778***	-0.763***	-0.671***	-0.765***	-0.668***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Change Turnover (-1)			0.014***			
			(0.000)			
Change Turnover (-12)				-0.024***		
				(0.000)		
Growth Rate Turnover (-1)					0.703***	
					(0.000)	
Growth Rate Turnover (-12)						-3.329***
						(0.001)
Constant	5.064***	4.363***	4.996***	2.836***	4.811***	3.013***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Observations	516	516	515	504	515	504
Pseudo-R^2	0.355	0.368	0.373	0.337	0.372	0.338

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Cluster standard errors by sector within brackets.

Table 1.6.B *Poisson estimation results. Formed cartels (1997-2012). Sectors C, G, H & N.*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	0.110***	0.109***	0.105***	0.132***	0.105***	0.138***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Price Expectations	-0.092***	-0.093***	-0.087***	-0.088***	-0.089***	-0.085***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Turnover EU	-0.080***	-0.070***	-0.077***	-0.055***	-0.075***	-0.056***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Sanctioned Cartels		-0.778***	-0.763***	-0.671***	-0.765***	-0.668***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Change Turnover (-1)			0.014***			
			(0.000)			
Change Turnover (-12)				-0.024***		
				(0.000)		
Growth Rate Turnover (-1)					0.703***	
					(0.000)	
Growth Rate Turnover (-12)						-3.329***
						(0.001)
Constant	5.064***	4.363***	4.996***	2.836***	4.811***	3.013***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Observations	516	516	515	504	515	504
Pseudo-R^2	0.355	0.368	0.373	0.337	0.372	0.338

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Cluster standard errors by sector within brackets.

On the other hand, we analyze the results obtained from the IV estimation, which are summarized in Table 7. The partial R^2 is high, which tells us that the additional instruments are highly correlated with the potentially endogenous variable after partialling out the effect of the

other independent variables. Also the Shea's adjusted partial R^2 is high, which shows that the component of *price expectations* that is orthogonal to the other regressors can be explained by the component of the predicted value of *price expectations* that is orthogonal to the predicted values of the other regressors in the model.

As hinted above, we are interested in analyzing whether the variable *price expectations* can be treated as exogenous, in which case the OLS estimation would be more efficient than the IV estimation. We test the null hypothesis, that the variable can be treated as exogenous, with Wooldridge's score test (Robust Score) and the regression-based test (Robust Regression). The difference between these tests is that the former assumes that the variables being tested are exogenous when estimating the error term's variance, while the latter assumes that the variables being tested are endogenous. According to the results of Table 7, the null hypothesis cannot be rejected.

Table 1.7: *IV estimation results. Formed cartels (1997-2012).*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	0.005**	0.005**	0.005**	0.006	0.005**	0.008
	(0.002)	(0.002)	(0.002)	(0.005)	(0.002)	(0.005)
Price Expectations	-0.007**	-0.007**	-0.007**	-0.007*	-0.008**	-0.007*
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Production EU	-0.002	-0.001	-0.000	-0.001	0.000	-0.001
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Sanctioned Cartels		-0.045**	-0.046**	-0.044**	-0.047**	-0.045**
		(0.018)	(0.019)	(0.018)	(0.019)	(0.018)
Production Change (-1)			-0.001			
			(0.002)			
Production Change (-12)				-0.003		
				(0.009)		
Production Growth Rate (-1)					-0.143	
					(0.162)	
Production Growth Rate (-12)						-0.671
						(0.960)
Constant	0.290	0.245	0.168	0.263	0.126	0.299
	(0.239)	(0.235)	(0.326)	(0.237)	(0.300)	(0.248)
Observations	192	192	192	192	192	192
R^2	0.018	0.028	0.028	0.028	0.029	0.030
Robust Score Chi^2	0.642	0.555	0.591	0.592	0.569	0.590
Robust Score p-value	0.423	0.456	0.442	0.442	0.451	0.443
Robust Regression F	0.634	0.542	0.575	0.574	0.555	0.572
Robust Regression p-value	0.427	0.463	0.449	0.450	0.457	0.450
Overident. Score Chi^2	19.502	19.868	19.908	19.916	19.967	19.996
Overident. Score p-value	1.000	1.000	1.000	1.000	1.000	1.000
Partial R^2	0.940	0.941	0.937	0.940	0.938	0.939
Shea's Adjusted Partial R^2	0.916	0.915	0.910	0.913	0.910	0.913

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Robust standard errors within brackets

1.6.1.2 Cartel Breakup

We follow the same procedure used for the robustness check of cartel formation for the case of cartel breakup. We estimate the Poisson model including more sectors to check the robustness

of the results obtained in the baseline model.

The models presented in Table 8a and 8b are more informative than the baseline case considered in Table 5. The variable *business evolution* has a positive and significant effect on cartel breakup. In fact, broken cartels increase by around 2.7% $(1.8 - 2.3\%)$ if the *business evolution* index increases by one unit. This represents evidence of pro-cyclical collusion with respect to cartel breakups when economic cycles are measured using firm-specific business evolution. However, this pro-cyclicality is less strong when the sector production has increased with respect to the previous month or turnover is high.

This result is in line with the one obtained in the case of cartel formation: when the production faced by an individual firm has evolved positively in the last months it is more likely to form a cartel, and if the firms are already in a cartel, then it is also more difficult to sustain collusion since it is more likely that a cartel breaks up. Unlike in the case of cartel formation, the number of cartels broken is only related with the level of prices expected by firms' managers or with the number of sanctioned cartels when services sectors are considered together with the manufacturing sector.

Moreover, the breakup of the cartel is more likely to occur the lower the production growth is with respect to the previous month or the lower the turnover of the sector is. Thus, collusion seems to be pro-cyclical with respect to cartel breakups when economic cycles are measured using industry wide real EU production.

Table 1.8: *Poisson estimation results. Internally broken cartels (1997-2012). Additional sectors.*

Table 1.8.A *Poisson estimation results. Internally broken cartels (1997-2012). Sectors C, F, H & K.*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	0.027***	0.027**	0.027**	-0.001	0.027**	0.002
	(0.010)	(0.011)	(0.011)	(0.018)	(0.011)	(0.018)
Price Expectations	-0.028	-0.027	-0.028	-0.032	-0.028	-0.032
	(0.020)	(0.020)	(0.020)	(0.021)	(0.020)	(0.021)
Production EU	0.001	0.001	0.003	0.005	0.002	0.006
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Sanctioned Cartels		-0.014	-0.019	-0.017	-0.017	-0.017
		(0.070)	(0.072)	(0.069)	(0.070)	(0.069)
Production Change (-1)			-0.002***			
			(0.001)			
Production Change (-12)				0.073***		
				(0.027)		
Production Growth Rate (-1)					-0.149***	
					(0.035)	
Production Growth Rate (-12)						6.736**
						(2.716)
Constant	-1.362***	-1.377***	-1.573***	-1.770***	-1.494***	-1.947***
	(0.485)	(0.410)	(0.364)	(0.518)	(0.428)	(0.590)
Observations	636	636	633	600	633	600
Pseudo-R^2	0.286	0.286	0.286	0.280	0.285	0.279

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Robust standard errors within brackets.

Table 1.8.B *Poisson estimation results. Internally broken cartels (1997-2012). Sectors C, G, H & N.*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	0.023**	0.023**	0.018*	0.024	0.018*	0.025
	(0.010)	(0.010)	(0.010)	(0.019)	(0.010)	(0.018)
Price Expectations	0.017**	0.017**	0.029***	0.006***	0.029***	0.006***
	(0.007)	(0.007)	(0.007)	(0.002)	(0.007)	(0.001)
Turnover EU	-0.041***	-0.042***	-0.053***	-0.032***	-0.052***	-0.032***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Sanctioned Cartels		0.038***	0.039***	-0.054***	0.039***	-0.054***
		(0.007)	(0.010)	(0.003)	(0.010)	(0.004)
Change Turnover (-1)			0.026***			
			(0.001)			
Change Turnover (-12)				0.007		
				(0.020)		
Growth Rate Turnover (-1)					2.144***	
					(0.065)	
Growth Rate Turnover (-12)						0.321
						(1.946)
Constant	2.417***	2.455***	3.415***	1.537***	3.334***	1.524***
	(0.171)	(0.181)	(0.202)	(0.184)	(0.221)	(0.097)
Observations	516	516	515	504	515	504
Pseudo-R^2	0.308	0.308	0.317	0.298	0.317	0.298

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Robust standard errors within brackets.

Finally, we look at the results of the IV estimation. Also in this case both the partial R^2 and the Shea's adjusted partial R^2 are high. Still, our test of interest is the potential endogeneity of the variable *price expectations*. At the 5% level of significance we cannot reject the null hypothesis of the variable being exogenous according to both the statistic of the Robust Score and the

statistic of the Robust Regression.

Table 1.9: *IV estimation results. Internally broken cartels (1997-2012).*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	0.004	0.004	0.004	-0.003	0.004	-0.003
	(0.003)	(0.003)	(0.003)	(0.007)	(0.003)	(0.007)
Price Expectations	-0.001	-0.001	-0.001	-0.002	-0.001	-0.002
	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Production EU	-0.001	-0.001	-0.000	0.000	-0.001	0.000
	(0.004)	(0.004)	(0.006)	(0.004)	(0.005)	(0.004)
Sanctioned Cartels		-0.014	-0.015	-0.014	-0.014	-0.014
		(0.054)	(0.054)	(0.053)	(0.054)	(0.054)
Production Change (-1)			-0.000			
			(0.004)			
Production Change (-12)				0.015		
				(0.012)		
Production Growth Rate (-1)					-0.009	
					(0.364)	
Production Growth Rate (-12)						1.472
						(1.353)
Constant	0.346	0.333	0.294	0.245	0.326	0.214
	(0.421)	(0.434)	(0.559)	(0.413)	(0.529)	(0.403)
Observations	192	192	192	192	192	192
R^2	0.005	0.005	0.005	0.010	0.005	0.009
Robust Score Chi^2	1.191	1.376	1.391	1.340	1.417	1.381
Robust Score p-value	0.275	0.241	0.238	0.247	0.234	0.240
Robust Regression F	1.154	1.330	1.335	1.289	1.363	1.329
Robust Regression p-value	0.284	0.250	0.249	0.258	0.244	0.251
Overident. Score Chi^2	41.229	42.218	41.757	42.174	41.845	42.032
Overident. Score p-value	0.858	0.832	0.844	0.833	0.842	0.837
Partial R^2	0.940	0.941	0.937	0.940	0.938	0.939
Shea's Adjusted Partial R^2	0.916	0.915	0.910	0.913	0.910	0.913

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Robust standard errors within brackets.

1.6.2 Pre-leniency program period

So far, we have only analyzed the cartel cases formed or broken in the manufacturing sector during the period January 1997 to December 2012. However, both the formation and breakup of these cartels have been affected by the existence of the leniency program, which was introduced in July 1996 in the European Union.

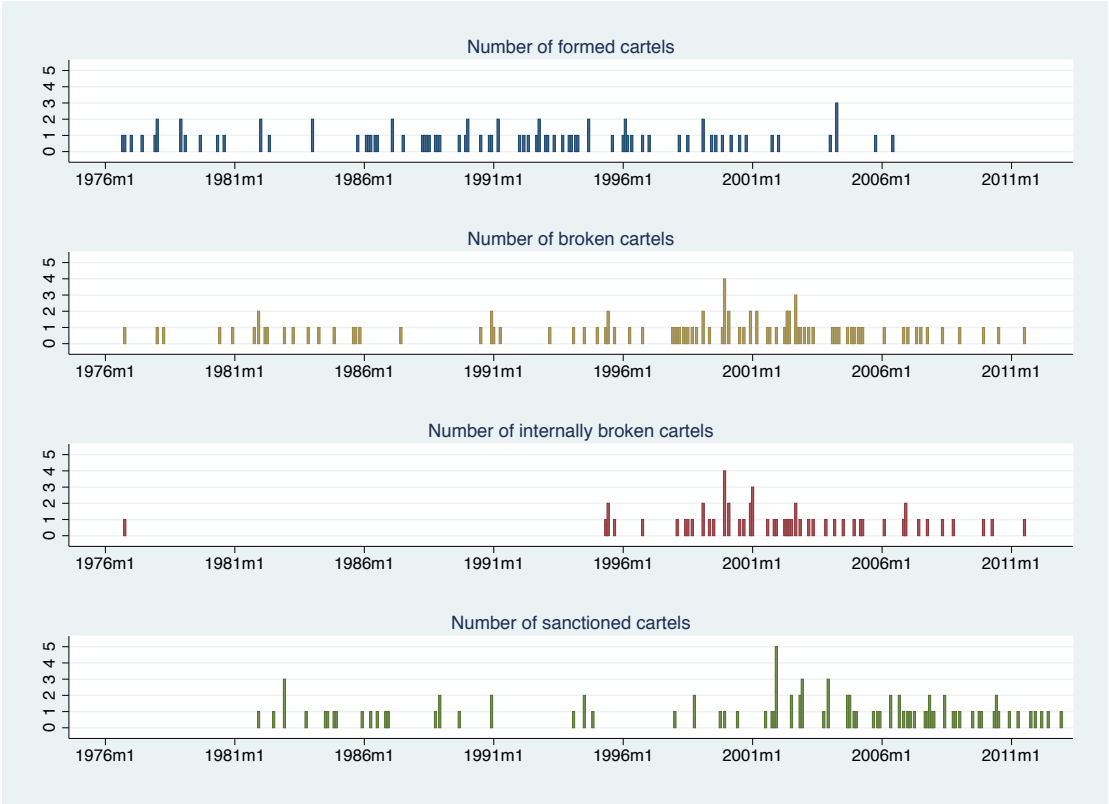
This program could produce two opposite effects in the dynamics of cartels birth and death. On the one hand, the members of a discovered cartel case could benefit from a fine reduction under certain circumstances. Therefore, the ex-ante profits from collusion are higher than in the case in which the sanctioned firm has to pay the full fine, for a given probability of detection, which means that the likelihood of cartel formation increases. On the other hand, the incentives to deviate and break up the cartel are higher under the leniency program, since the first member revealing the existence of the illegal collusive agreement could get a higher or even a full fine reduction. Thus, we should see that the probability of breakup is higher.

Summing up, the introduction of the leniency program may have affected the way in which the independent variables affect our dependent variables of interest. For this reason, we focus in this section in the period from January 1991 to December 1996. Following the methodology explained in Section 4, we will study how do the business evolution, the price expectations, the sanctioned cartels and the production at the European level affect the formation and breakup of cartels during this period.

At Table 2, we can also see how the number of cartels formed in the period 1991-1996 (33 cartels) is higher than the number of cartels formed after 1997 (29 cartels), while the number of broken and internally broken cartels is significantly lower in the period 1991-1996 (21 and 7 respectively) than in the period 1997-2012 (80 and 59 respectively).

In Figure 2 we can observe the formation, breakup and sanction distribution of the discovered cartels over time the whole time period 1976 to 2012. The number of internally broken cartels explodes since 1997, and the number of sanctioned cartels clearly increases since 1997 onwards.

Figure 1.2: *Number of monthly formed, sanctioned and broken by internal reasons cartels in the manufacturing sector (1976-2012).*



Source: Own elaboration from EC publicly available decisions.

We estimate the same equations than before, now for the cartels startups and breakups between 1991 and 1996, in the pre-leniency period using the monthly data described in Table 10.

Table 1.10: *Summary statistics (1991-1996).*

Variable	N	Mean	Std. Dev.	Min	Max
Formed Cartels	72	0.347	0.585	0	2
Broken Cartels due to Internal Reasons	72	0.069	0.306	0	2
Business Evolution	72	-4.194	10.954	-26	19
Price Expectations	72	8.389	8.187	-3	32
Sanctioned Cartels	72	0.181	0.422	0	2
Production EU	72	86.306	8.615	58.2	96.1
Production Change (-1)	71	-0.039	12.348	-26.3	30.4
Production Change (-12)	60	-0.158	3.693	-7.4	7.5
Production Growth Rate (-1)	71	0.013	0.170	-0.3	0.5
Production Growth Rate (-12)	60	-0.0007	0.043	-0.08	0.09

Source: Own elaboration from EC publicly available decisions and the Business Survey. Manufacturing sector only.

In Table 11 we can see the results of the Poisson model estimation of cartel formation, and in Table 12 the results of cartel breakup.

Again, for the period before the leniency program (1991-1996), we find that collusion seems to be weakly counter-cyclical when looking at cartel setup from the industry wide cycles perspective, while collusion also appears to be counter-cyclical when looking at cartel breakup from both the firm-specific business cycle and now the sector economic conditions too.

However, in the pre-leniency period, the drivers of cartel setup and break up are different with respect to the post-leniency period. The driver of cartel setup in the pre-leniency period is the firm-level business evolution with negative sign (Table 11), while the drivers of cartel setup in the post-leniency period are the firm-level business evolution, price expectations and the industry-wide production at the EU level (Table 4).

Table 1.11: *Poisson estimation results. Formed cartels (1991-1996).*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	-0.016***	-0.017**	-0.022***	-0.088**	-0.023***	-0.095**
	(0.006)	(0.007)	(0.006)	(0.041)	(0.006)	(0.039)
Price Expectations	-0.020**	-0.016*	0.006	-0.016	0.005	-0.020*
	(0.008)	(0.008)	(0.013)	(0.011)	(0.013)	(0.011)
Production EU	0.020	0.023	-0.025	0.032*	-0.011	0.032**
	(0.018)	(0.016)	(0.020)	(0.018)	(0.019)	(0.015)
Sanctioned Cartels		-0.835***	-0.621**	-1.360**	-0.665**	-1.404**
		(0.277)	(0.285)	(0.586)	(0.285)	(0.580)
Production Change (-1)			0.041***			
			(0.005)			
Production Change (-12)				0.209		
				(0.144)		
Production Growth Rate (-1)					2.310***	
					(0.368)	
Production Growth Rate (-12)						20.419*
						(11.975)
Constant	-0.016***	-0.017**	-0.022***	-0.088**	-0.023***	-0.095**
	(0.006)	(0.007)	(0.006)	(0.041)	(0.006)	(0.039)
Observations	72	72	71	60	71	60
Chi^2	1.779	3.075	8.371	5.756	7.347	6.208
p-value	0.619	0.545	0.137	0.331	0.196	0.287

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Newey-West standard errors within brackets.

The drivers of cartel breakup in the pre-leniency period[27] seem to be mostly the business evolution and price expectations at the firm level (Table 12) and also the EU industry real production, while in the post-leniency period price expectations do not affect breakup and the EU industry real production is not significant and the variable *turnover* has the opposite sign (Table 8a and Table 8b). Note that this last statement should be considered carefully, given that the number of cartels broken in the manufacturing sector due to internal reasons is very low during the period January 1991-December 1996.

[27]We double-checked as previously whether these results for the pre-leniency period were robust to the presence of endogeneity of price expectations, and we found that they are robust indeed. Results from these robustness checks are available upon request from the authors.

Table 1.12: *Poisson estimation results. Internally broken cartels (1991-1996).*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	0.419***	0.439***	0.498***	0.440***	0.488***	0.446***
	(0.031)	(0.045)	(0.046)	(0.034)	(0.046)	(0.032)
Price Expectations	-0.260***	-0.268***	-0.318***	-0.263***	-0.313***	-0.261***
	(0.018)	(0.020)	(0.023)	(0.037)	(0.022)	(0.035)
Production EU	0.353***	0.349***	0.403***	0.342***	0.394***	0.333***
	(0.059)	(0.057)	(0.052)	(0.061)	(0.051)	(0.061)
Sanctioned Cartels		0.392	-0.027	0.385	-0.002	0.388
		(0.347)	(0.319)	(0.396)	(0.318)	(0.384)
Production Change (-1)			-0.033***			
			(0.011)			
Production Change (-12)				-0.024		
				(0.173)		
Production Growth Rate (-1)					-1.974***	
					(0.682)	
Production Growth Rate (-12)						-4.815
						(15.360)
Constant	-34.947***	-34.791***	-39.449***	-34.143***	-38.631***	-33.250***
	(5.613)	(5.434)	(4.906)	(5.962)	(4.907)	(5.980)
Observations	72	72	71	60	71	60
Chi^2	3.903	3.989	4.067	3.940	4.124	4.020
p-value	0.272	0.407	0.540	0.558	0.532	0.546

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Newey-West standard errors within brackets.

1.6.3 Sanctioned cartels

In this section we analyze the relationship between sanctioned cartels and the economic indicators, in order to understand the economic conditions under which the Commission makes decisions. Previous results show that sanctioned cartels have a deterrent effect on cartel formation, but there is no relationship between the cartels sanctioned each month and the internal breakup of cartels. However, we have seen that the industry real production affects negatively cartel breakup. A possible explanation would be that the Commission devotes more resources to investigations[28] and sanctions in downturns, which would increase the likelihood of internal breakup of the cartel in order to apply for the leniency program before it is discovered by the EC. Therefore, we study the effect of industry real production on sanctioned cartels to rule out this explanation.

Results for the manufacturing sector are presented in Table 13[29]. It can be seen that production affects positively the number of sanctioned cartels. Therefore, this leads to the conclusion that managers' internal decision of breaking up the cartel comes from the firm-specific business cycle and the economic conditions, and not from the threat of being discovered by the EC.

[28]Regressions for the number of investigations and proceedings started each month were run, and no significant effect was found.

[29]The estimations for the sample containing sectors C, F, H & K, and the sample of sectors C, G, H & N, yield similar qualitative results.

Table 1.13: *Poisson Estimation Results. Sanctioned cartels (1997-2012).*

	(1)	(2)	(3)	(4)	(5)	(6)
Business Evolution	-0.011	-0.011	-0.011	-0.012	-0.011	-0.009
	(0.015)	(0.015)	(0.013)	(0.026)	(0.013)	(0.023)
Price Expectations	0.002	0.002	-0.009	0.002	-0.007	0.003
	(0.010)	(0.010)	(0.011)	(0.009)	(0.011)	(0.009)
ProductionEU19	0.038***	0.038***	0.061***	0.038***	0.057***	0.037***
	(0.008)	(0.008)	(0.015)	(0.008)	(0.014)	(0.008)
Production Change (-1)			-0.033**			
			(0.016)			
Production Change (-12)				0.003		
				(0.027)		
Production Growth Rate (-1)					-2.675*	
					(1.459)	
Production Growth Rate (-12)						-0.549
						(2.185)
Constant	-4.947***	-4.947***	-7.260***	-4.961***	-6.821***	-4.904***
	(0.891)	(0.891)	(1.574)	(0.942)	(1.491)	(0.944)
Observations	192	192	192	192	192	192
Chi^2	7.248	7.248	12.738	7.253	11.742	7.256
p-value	0.064	0.064	0.013	0.123	0.019	0.123

Note: * p <0.10, ** p <0.05, *** p <0.01 significance test. Newey-West standard errors within brackets.

1.7 Conclusions

In this paper we shed some light about the impact of economic cycles on cartel formation and cartel breaks up. Our results show that the average number of cartels formed increases when the firm-level perceived business has evolved positively in the last three months and managers expected in the previous period that their selling price would decrease in the next three months. Moreover, high production at sector level affects cartel formation negatively. We also find evidence that cartel breakups are also driven by past positive perceptions of firm-specific business evolution, and somehow low EU production growth rate.

In conclusion, our results show that cartels are more likely to be formed in upturns, but also that cartels tend to breakup also in booms. Cartels are more prone to be created when managers consider that their firm production has evolved positively in the last three months (growing firms) and when they expect a decrease in prices in the near future (with expected declining prices). Cartels are more likely to collapse when firms face upturns in real demand at the firm level, although this effect is less strong when the EU production growth rate is low. Upturns in firm-specific business cycles appear to cause cartel turnovers: existing cartels collapse while new ones are set up.

Collusion appears to be pro-cyclical with respect to cartel creation when cycles are measured using firm-specific perceived business evolution, while collusion seems to be counter-cyclical with regard to cartel demise when cycles are measured by firm-specific production (and also somehow with respect to cartel formation when cycles are measured by industry-wide EU production).

These results should be considered as a first approach to answer the question of interest as there is some scarcity of data and we are just relying on the time series of cartel startups, cartel breakups, cartels sanctioned and a set of business cycle variables. Ideally, the missing data problem should be overcome working with a panel data including more industries and territorial units.

New theoretical analysis should also be developed in line with the ones of Fabra (2006) as the results we have obtained are consistent with the result that collusion might be pro-cyclical when there is not capacity constraints, firms find it easier to collude during booms, while collusion is counter-cyclical when capacity constraints are sufficiently tight, firms find it more difficult to collude during booms. It might be the case that capacity constraints are on average not binding in industries still not colluding (before collusion), so demand booms are driving the startup of cartels. After having the cartels functioning, collusion might be the driver not only of price hikes but also of coordinated reductions in the colluding industries capacities. As capacity constraints get tight, collusion might become counter-cyclical, and cartels might finally breakup also during booms.

In addition, important further research would be the study, from a theoretical and empirical point of view, of the determinants and characteristics that make some cartels reach the screen of the Antitrust Authority while others don't. Finally, the impact of successive reforms of the Community leniency program in the dynamics merits some further research.

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Chapter 2

Cartelization: Is it worth it?

Abstract

Cartels are considered one of the most serious infringements in competition policy. Existing competition policies aim at deterring cartel formation and sanctioning detected cartel cases. However, a precise measure of how much firms benefit from collusion is needed in order to design effective policies. In this study I evaluate the causal effect of having been involved in a collusive agreement on the revenues and profits of cartelized firms. Using a dataset of discovered cartel cases in Spain from 1990 to 2014 and an alternative dataset of firms' balance sheets, I can distinguish between cartelized and non-cartelized firms (treatment and control group). After having constructed a matched sample containing comparable collusive and non-collusive firms (using the *Nearest Neighbor Matching Algorithm*), I can estimate the average treatment effect of cartelization on firm profitability using the difference-in-difference estimator. Results show that firms increase their revenues between 19% and 26% due to the collusive agreement on average, while no significant effect is found on profits. However, when results by cartel duration are considered, I find out that members belonging to a cartel that lasted long, not only do they increase their revenues by $29\% - 50\%$, but also increase their profits by around $82 - 91.5\%$ when compared to the average net income of the firms involved in these types of cartels. Further analysis shows that cartels that are profitable from the beginning tend to last longer and do not apply for Leniency Programs. These results have several policy implications.

Keywords: Cartels; Competition Policy; Business Economics
JEL Codes: L4; D7; K2; M2

2.1 Introduction

There is consensus in the economic literature that competition restrictions through collusion are undesirable (Werden, 2009). Companies participating in a cartel produce less and earn higher profits. Resources are misallocated and consumer welfare is reduced. In concrete, cartels are considered one of the most flagrant infringements of competition law. Fighting against cartels and preventing their formation is not only one of the main tasks of the European Commission but also of most of the National Competition Authorities. Thus, analyses that contribute to a better understanding of the determinants and consequences of the existence of cartels will be helpful for both detection and deterrence of collusive agreements.

The existing competition policies aim at deterring and detecting cartel formation. In particular, national and European laws establish a punishment for firms in case they are detected after having participated in a cartel. Fines depend on the gravity and the duration of the infringement. However, there exists a cap of 10% of a business' turnover in the year before the Authority's decision. As García-Verdugo(2016) points out, even though this percentage is considered the maximum effort that can be demanded to the firm, its origin is unclear. In any case, this mechanism limits the potential negative effects the sanctions could impose on the firms. Additionally, some lenient treatments and fines reductions are offered if the offender is the first one revealing the existence of the collusive agreement to the Antitrust Authority.

Nevertheless, we lack a precise measure of how much firms profit from collusion. The existing literature has studied the drivers of collusion and the determinants of cartel formation, stability and breakup (Levenstein and Suslow, 2006 and 2011; Herold and Paha, 2017). It has also been analyzed the overcharges applied by firms (Connor and Lande, 2008; Boyer and Kotchoni, 2015) and the deterrence attained due to the action of the Antitrust Authorities' activities or the existence of the competition policy (Smuda, 2014; Bos, Davies, Harrington and Ormosi, 2018).

On the other hand, there are some papers that study the impact of competition policy and Antitrust Authorities' actions on firms' profits and firms' valuation (Aguzzoni, Langus and Motta, 2013). However, to the best of my knowledge, the existing analyses regarding the effect of collusion on profitability either lack a (good) counterfactual or do not address the problem of causality properly.

In this project, I am interested in quantifying the impact of cartelization on firms' profits and revenues. Understanding how big are the incentives for firms to participate in a collusive agreement is relevant and essential for the design and effectiveness of competition policy. Therefore, in this study I use policy evaluation techniques to identify the causal effect of interest.

For this purpose, I use a panel data of cartelized and non-cartelized Spanish firms for the period 1992-2014 coming from two different sources. On the one hand, I use information provided by the reports of the cartels that have been sanctioned by the Spanish Antitrust Authority in the last two decades, which specify which firms have participated in a cartel case and when it took place. On the other hand, I have information of the balance sheet of around 21,000 Spanish firms. Therefore, I can identify which firms have participated in a discovered cartel case and designate them as *cartelized* firms (treatment group), and which of them have not been cartelized in principle (*non-cartelized* firms). After finding a good counterfactual or control group for the treated firms, I can estimate the effect of belonging to a cartel on firms' revenues and profits by using the difference-in-differences estimator.

Results show that firms' revenues increase around 19%-26% due to participation in a collusive agreement. However, profits measured as net income or profit margin do not show a significant impact on average. In the first three years of cartelization, which corresponds to the average cartel duration in the full sample, firms increase their revenues by 14%-17%. More importantly, only when the sample is split into *short-lived* and *long-lived cartels* a significant effect is found on net income on the latter. This is, firms that belong to a cartel that lasted between 8 and 13 years increase their revenues by 29%-50%. Also, these revenues are translated into profits since net income is on average 2.15-2.33 times higher than what these cartelized firms would have earned if they had not been involved in a cartel case. These longer-lasting cartels appear to be more profitable since initial periods. Finally, there exists some weak evidence that only members of non-profitable cartels apply for the Leniency Program.

It is well known in the industrial organization literature that data related to collusive agreements suffer from important limitations. Firstly, we can only work with discovered cartels since we know nothing or very little from the underlying population. In this particular analysis, this would mean that the control group could be cartelized but has not been discovered yet. In that case, I would not find a significant difference in the outcome variable for the two groups. Moreover, I would need the firm in the control group to be cartelized at the same exact time than the treated one, to completely invalidate the results. However, the placebo tests provide some evidence that this situation is unlikely to be happening in the data. Secondly, some firms could benefit from the existence of collusion in the market since there is less competition. Or alternatively, the colluding firms may try to expel these firms from the market. Thus, results must be interpreted as the effect of belonging to a cartel and not as the effect of the existence of a cartel.

The placebo tests presented below show there are no significant differences between the control group and the new artificial control group created to mimic the main analysis performed in this study. This robustness check provides some signal about the possibility that these two

potential issues may not be severe in this specific study. Despite the limitations of the data, the present paper contributes to the existing literature analyzing the incentives firms have (on average) to participate in a cartel by constructing and using for this purpose the best counterfactual possible given the limitations mentioned above.

The paper is structured as follows. In Section 2, I review the existing literature. After presenting in Section 3 the data and the methodology used for the analysis, Section 4 discusses the results. The last two sections present the robustness check of the analysis and summarize the main ideas of the paper.

2.2 Literature Review

The relationship between profitability and collusion has already been addressed in the literature.

Asch and Seneca (1976) examine empirically the role of collusion on the profitability of American manufacturing corporations during the period 1958-1967. The sample consists of 51 collusive firms that were found guilty in response to the Sherman Act and other 50 firms as controls that were randomly drawn from the pool of non-collusive firms. They analyze the effect of being a collusive firm (dummy variable) on profit rates. In order to isolate the effect of interest, they control for other variables that may affect profits such as firm size (total assets), concentration in the industry, advertising-sales ratio and growth of the firm during the period studied. They find that the presence of collusive behavior is negatively associated with profitability. This counterintuitive result makes them consider the problem of causality, which would have led to a biased estimate of the effect of interest. They wonder whether collusion is a determinant of firm profitability, with the resulting empirical conclusion that collusive behavior leads to lower profit rates, or whether it is the case that an unsatisfactory profit performance by the firm will provide an incentive to collude. Therefore, they study the effect of profits on the probability of colluding and they find that poor profit performance increases the probability of collusion.

The empirical evidence regarding the impact of cartelization on firm's profits is mixed. Levenstein and Suslow (2006) examine case studies of individual cartels and four types of cross-section samples of cartels in order to analyze what determines cartels success. They find that some cartels are able to increase prices and profits to varying degrees. Lübbers (2009) studies the case of the Rhenish Westphalian Coal Syndicate (RWCS), which took place between 1893 and 1913. Employing event study methodology and using a dynamic panel data analysis, they assess the effect of belonging to the Rhenish-Westphalian Coal Syndicate (RWCS) on the reaction of stock markets and on the companies' profitability. He concludes that the RWCS had no significant

effect on the profitability of its members.

Günster, Carree and van Dijk (2012) study how cartel formation and termination affects the performance and efficiency of their members. They analyze the profitability, productivity and innovation of cartel members using firm-specific data for a sample of 141 publicly listed firms involved in 49 cartels infringements in the European Union between 1983 and 2004. The authors compare the performance and efficiency of firms during the cartel period with those in the years before the formation and after the termination of the cartel. Results show an increase in firms' profitability (0.5%) during the cartel years and a decrease in efficiency. Another interesting conclusion is that the longer cartels are in place, the more profitable its members become and the weaker their incentives to produce efficiently.

On the other hand, the issue of self-selection into cartelization due to firms' financial conditions has been studied from different points of view. Bertrand, Lumineau and Fedorova (2014) use a sample of firms involved in cartels prosecuted by the European Commission between 2001 and 2011 to study which are the factors that explain the likelihood of a firm entering a cartel. They find that firms with relatively larger market share are more likely to participate in cartels while firms with high liquidity ratio are less likely to participate in cartels. They also show that relatively older firms tend to participate more in cartels and that the size of the firm is positively related to the likelihood of cartelizing, although this effect varies with industry concentration.

Gustafson, Ivanov and Ritter (2015) look at airfare hikes occurring between January 2005 and December 2005 and they conclude that firm-level financial conditions determine the extent to which firms collude. They find that in the context of low idle capacity, financially weak airlines appear to value the immediate cash flows of increased cooperation, but only liquidity-constraint firms seem willing to incur the cost of cooperative attempts. Thus, short-term liquidity and long-term financial concerns increase an airline's propensity to cooperate. Low levels of short-term liquidity predict airfares hike initiation, while the long-term financial health of the firm determines the hike success.

However, the existing approaches in the literature that study the relationship between collusion and profitability have not addressed the problem of causality carefully enough and lack a (good) counterfactual. To the best of my knowledge, this is the first study that aims at identifying and quantifying the causal effect of participating in a cartel on firms' profitability after having corrected for self-selection into treatment. Moreover, while the literature usually focuses on one important case or one sector, a variety of collusive sectors are represented in the data.

The contributions of this paper are several. First of all, I make use of a control group in order to be able to compare the evolution of revenues and benefits of cartelized firms with a

group of reference. The second relevant contribution is that the problem of self-selection into treatment has been considered. This is to say, there exists a set of factors that are conductive to increase the likelihood of participating in a collusive agreement, and which are related to the revenues and benefits a firm could earn. Therefore, the construction of the control group is based on having cartelized and non-cartelized firms similar in these factors and also in the likelihood of cartelization. The control group reflects as accurate as possible the potential evolution of the cartelized firms had they not belonged to a cartel. In addition, the difference-in-differences estimator allows for the existence of a difference in levels between the two groups, and controls for common factors affecting all cartelized firms, affecting both groups of firms during the cartelization period and for unobservable effects.

Table 2.1: *Summary of the literature closely related to the present study.*

Year	Authors	Methodology	Database	Results
1976	Asch & Seneca	Ordinary Least Squares	American firms (51 collusive and 50 random non-collusive); manufacturing sector (1958-1967)	Collusive behavior negatively associated with profitability. And poor profit performance increases the likelihood of colluding.
2006	Levenstein & Suslow	Case study	US and internacional cartels	Some cartels are able to increase prices and profits to varying degrees.
2009	Lübbers	Event study and dynamic panel data	*Rhenish Westphalian Coal Syndicate* (RWCS, 1893-1913)	Belonging to RWCS had not significant impact on the profitability of its members.
2012	Günster, Carree & van Dijk	Panel data with firm and year fixed effects	49 EU cartels (1983-2004); 141 listed firms	Increase in firms' profitability (0.5%) due to the cartel and decrease in efficiency.
2014	Bertrand, Lumineau & Fedorova	Rare events logistic regression	Firms from cartels prosecuted by EC (2001-2011)	Firms with larger market share, older firms and bigger firms are more likely to participate in a cartel; the higher the liquidity ratio, the lower the likelihood of participating.
2015	Gustafson, Ivanov & Ritter	Negative binomial regression	8 biggest American airlines (January 2005 - December 2005)	Firms' financial conditions determine the degree of collusion: low short-term liquidity predicts airfares hike initiation, while the long-term financial health of the firm determines the hike success.

Source: Own elaboration.

2.3 Data and Methodology

In order to study the question of interest, I have collected a panel dataset from the Iberian Balance sheet Analysis System (SABI), which is a tool developed by Bureau Van Dijk. It con-

tains information about the balance sheets of Spanish firms. In concrete, I had access to the information of the 21,514 biggest Spanish firms measured as the Operating Revenues in the year 2014. The biggest firm earned €22 billion in Operating Revenues in 2014 and the smallest earned €10m. The sample period ranges from 1992 to 2014.

On the other hand, I have information regarding the 68 cartel cases reports sanctioned by the Spanish Antitrust Authorities from 1990 to 2014[1]. A certain number of the cases were discovered due to investigations started by the Authority's own initiative or complaints and the rest have been detected under the Leniency Program. From the reports I can obtain information regarding the date of formation and breakup of the cartel. The date of formation refers to the first moment for which the Authority has evidence of the existence of the cartel. The breakup date is either the date in which the cartel died naturally, or the date in which they were caught and had to stop colluding. In addition, I have information about which were the firms that participated in the case and in which period they were a member of the cartel. There are around 748 cartelized firms that have been sanctioned in the period 1990-2014. Specifically, 253 out of these 748 cartelized firms have been matched in the dataset containing the balance sheets.

Therefore, I can distinguish which firms have participated in a collusive agreement (*cartelized*) from those that either have never participated in a cartel or have not been discovered yet (*non-cartelized*). The fact that there may exist cartelized firms that we cannot classify as such is one of the limitations of working with this data. However, I create a matched sample and use the difference-in-differences estimator in order to study the effect of interest. If the firms in the control group were a member of an undetected collusive agreement and I still find significant results, this would mean that the undetected cartels are less profitable (or their members are able to hide their illegal profits). Moreover, I only focus on firms that have been cartelized once during my period sample, so I exclude from my analysis the repeated offenders. If a firm is involved in many cases at the same time or over time, the intensity of treatment is different for these observations. In addition, the effect of interest may not be linear with respect to the number of cases in which the firm has been involved.

Since I am interested in the causal effect of cartelization on firms' profits and revenues, it is not enough to compare the outcome of cartelized firms pre- and post-treatment as in Günster, Carree and van Dijk (2012). The reason being that changes in either observable or unobservable characteristics, which are not related to cartelization, may have affected firms' profits. On the other hand, it is not appropriate to compare cartelized firms to any non-cartelized firm since they may differ both in observable and unobservable characteristics and therefore it would not be a good counterfactual for our treated observations.

[1]More information about the data can be found in Borrell, Jiménez and Ordóñez-de-Haro (2015), or in Ordóñez-de-Haro, Borrell and Jiménez (2018) for the European case.

In order to overcome these empirical difficulties, I combine two different strategies following Artés, Jiménez and Perdiguero (2015). Firstly, I construct a matched sample where the treatment and the control group are very similar in certain relevant observed characteristics. Secondly, I estimate the effect of interest using the difference-in-differences estimator in the matched sample in order to control for unobservable differences.

The different techniques employed in order to make sure that both cartelized and non-cartelized firms are comparable and that results are trustworthy are explained in the following subsections. It must be noted that this data processing, together with the imposed restrictions, come at the cost of losing observations not only from non-cartelized firms but also from cartelized firms. In the latter case, I start with the 253 cartelized firms that I can identify in the SABI dataset. After truncating the sample, 239 cartelized firms are left. As explained above, I work with those firms that have been involved in one cartel case only. With this restriction, 202 firms remain in the sample. Additionally, since the matching has to be performed on pre-treatment variables, I work with those firms that started being cartelized after 1995. With this, 38 firms more are lost and I keep on working with 164 of them. After performing the matching, I end up with 99 cartelized firms. Summary statistics of the characteristics of the cartel cases are presented in Table 2.

Table 2.2: *Summary Statistics.*

	Total			**Matched Sample**		
Cartel Cases	68			27		
Number of different firms	748			99		
Number of affected sectors	12			9		
	Mean	**Mode**	**Median**	**Mean**	**Mode**	**Median**
# of firms per cartel	12.24	6	7	3.67	1	2
Duration (in years)	3.27	2	4	4.03	1	2
Year of start	2001	2008	2004	2006	2008	2008
Year of end	2007	2009	2009	2010	2013	2011

Another important remark that needs to be made is that even if I am working with a dataset of Spanish firms and cartel cases that affected the Spanish market, I have no reason to think that there exist certain characteristics that would produce different results in comparison to other countries. In particular, after the matching I end up with a sample of 196 firms, 99 cartelized and 97 non-cartelized. From the 196 firms, I have been able to obtain information about the global ultimate owner of 115 of them. Around 30% of the firms have an international global owner, this figure being almost 34% in the case of cartelized firms. Therefore, this indicates that both Spanish and non-Spanish firms that operate in several countries may be colluding in these different locations too. Additionally, Spain is subject to common laws, as the other countries of the European Union, so the environment under which these illegal collusive agreements emerge is similar. Moreover, half of the sectors of the economy, classified by NACE Rev.2 sections

classification, are represented both in the full sample and in the matched sample. Figures are presented in Table A.1 in the Appendix.

2.3.1 Matched Sample

Before constructing the matched sample, I drop from the sample the outliers belonging to the upper and lower tails of the distribution. The reason for doing this is that I may not be able to find a good counterfactual for these observations. Since I am working with firms, an outlier in terms of profits could drive the results of the estimation, and having a good counterfactual for the cartelized firms is crucial for the validity of the results. Moreover, it is a common procedure in the literature when working with firm-level data[2].

In order to construct the matched sample, I have performed the matching five years before the treatment takes place[3]. For example, if a firm starts being a member of a cartel in 2000, I have looked for a control in the year 1995, using the characteristics of both firms in the year 1995. Then, I can track the evolution of the outcomes of *cartelized* and *non-cartelized* firms over time. An exemption is made with those firms that started participating in a collusive agreement in 1995 and 1996. Given that the sample starts in 1992, the matching for these firms was made three and four years before the start of the treatment, respectively[4]. Moreover, since the matching should be done on pre-treatment characteristics, I exclude from the analysis those cartel cases that started before 1995.

In particular, I apply the non-parametric nearest neighbor matching method. I follow Imbens and Wooldridge (2009) to define the algorithm. Let Y_i denote the outcome of interest, let X_i be the observable characteristics on which we are matching and let C_i be the treatment variable. Given a sample $\{Y_i, X_i, C_i\}_{i=1}^N$, let $\ell_1(i)$ be the nearest neighbor to i, that is:

$$\ell_1(i) = j, \;\; for \;\; j \in \{1, ..., N\}, \;\; if \;\; C_j \neq C_i, \;\; and \;\; ||X_j - X_i|| = \min_{k:C_k \neq C_i} ||X_k - X_i||$$

where the metric used is the Mahalanobis metric, which is based on the inverse of the full sample variance-covariance matrix and is the most common in the literature. In addition, I have used the option *exact* in Stata for one of the characteristics. Following Abadie et al. (2004), this option allows to specify exact, or as exact as possible, matching on one or more variables. In practice, it multiplies the corresponding elements in the weight matrix by 1,000 relative to the weights placed on the other variables; and in this case, the inverse sample standard errors are

[2]More information can be found in the Appendix A.2.

[3]I have also performed two other matchings: three years before the treatment and in the first year of the sample (1992). However, the match is especially not accurate in the second case.

[4]There are 2 cartel cases in 1995, which involve four firms; and 1 cartel case in 1996, which involves 3 firms.

used for the variables specified in *exact*.

The observable characteristics used for the matching are based on the firms' financial conditions that have been found to predict cartel participation in the literature. I follow Bertrand, Lumineau and Fedorova (2014), who find that firms with relatively larger market share are more likely to participate in cartels while firms with high liquidity ratio are less likely to participate in cartels. They do also find that relatively older firms tend to participate more in cartels and that firm's size is positively related to the likelihood of participating in a cartel, although this effect varies with industry concentration.

In this case, I match *cartelized* and *non-cartelized* firms on the following observable characteristics: age, indebtedness (or debt ratio), the ratio of long-term debt over total assets, leverage, sector (at two digits level), costs of employees, the ratio of costs of employee over operating revenues, the solvency ratio and total assets. This last variable was used in the *exact* option, which means that the size of the firm is the most relevant variable when looking for a good control.

While total assets account for firm's size and are related to profitability, the long-term debt over assets controls for the loans and financial obligations lasting over one year with respect to the firms' assets. In addition, I use two different measures of the company's leverage. On the one hand, the debt ratio (or indebtedness) compares a company's total debt to its total assets. On the other hand, leverage is the level of a company's debt related to is equity capital and is expressed in percentage form. It shows the extent to which its operations are funded by lenders versus shareholders. Finally, I use two measures of personnel expenses, which are costs of employees and the ratio of costs of employee over operating revenues, measured in levels and in percentage, respectively. Symeonidis (2008) finds no evidence of any effect of collusion on wages, while he shows that there exists a negative effect of collusion on labor productivity growth. For this reason, I look for a firm in the control group that has similar costs of employees than the *cartelized* firm before the treatment in order to control for firm's efficiency.

Table 3 shows the t-test of mean equality for the matching variables[5] in the whole pretreatment period. The equality of means shown in the table are never rejected if they are computed year by year in the pretreatment period. The variable *sector* was not included in Table 3 because the digits of the sector[6] have no economic interpretation.

With respect to the outcome variables of interest, I want to study the effect of cartelization on firms' profitability. Therefore, I focus the analysis on three main variables: Operating Revenues, Net Income and Profit Margin. Note that since the variable Operating Revenues only

[5]The summary statistics are presented in Table A.3. in the Appendix.

[6]It can be seen in Table A.1 in the Appendix how sectors are represented in the matched sample.

Table 2.3: *T-test of mean equality.*

Variable	Non-cartelized	Cartelized	Difference	p-value	N
Age	25.81 (0.83)	25.59 (0.85)	0.22	0.85	972
Total Assets	67978.92 (6705.87)	76733.87 (8984.51)	-8754.94	0.44	926
Indebtedness	67.08 (0.90)	67.21 (0.90)	-0.13	0.92	926
Long-term Debt/assets	0.12 (0.01)	0.10 (0.004)	0.02	0.05	884
Costs of Employees	7214.04 (585.42)	6685.40 (511.96)	528.64	0.50	926
Costs of Employees/Operating Revenues	13.88 (0.58)	15.70 (0.61)	-1.83	0.03	926
Solvency Ratio	1.37 (0.04)	1.28 (0.03)	0.09	0.05	926
Leverage	147.74 (35.64)	123.97 (7.15)	23.77	0.50	926

Note: Standard deviation in brackets.

takes on positive values, I can normalize this variable and work with the logarithm of it. Given that the distribution of firms' revenues is very skewed, this transformation will help making the relationship between the treatment variable and the outcome variable more linear. It also allows interpretin the coefficient of interest in percentage terms (as an elasticity). Table 4 presents the summary statistics of the treatment variable and the outcomes of interest.

Table 2.4: *Summary statistics.*

Period	Variable	Non-Cartelized					Cartelized				
		Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Pre-treatment	Cartelized	481	0	-	0	0	491	1	-	1	1
	Operating Revenues	453	61743.4	88091.37	215.02	545822	473	105570.1	415633.7	198.6	4707617
	Log of Operating Revenues	453	10.34	1.16	5.37	13.21	473	10.29	1.35	5.29	15.36
	Profit Margin	453	3.47	11.49	-159.92	34.45	473	6.29	49.58	-632.742	686.39
	Net income	453	1990.74	7523.92	-56906.64	47620	473	2127.31	7253.92	-32874	57282
Treatment	Cartelized	492	0	-	0	0	492	1	-	1	1
	Operating Revenues	421	81524.1	148819.4	4248.63	954321	467	128473.3	293007.4	4048.038	4699735
	Log of Operating Revenues	421	10.57	1.07	8.35	13.77	467	10.80	1.26	8.31	15.36
	Profit Margin	421	4.28	10.27	-50.69	115.43	467	5.13	58.84	-919.43	652.61
	Net income	421	1893.08	6864.07	-39713	47049	467	5729.42	20794.23	-81413	146205

Note: Profit Margin is expressed in percentage. Operating Revenues and Net Income are expressed in thousand euros.

The Operating Revenues are the revenues generated from a company's business activity, which means revenues posted from selling the company's products and services. It allows studying the effect of cartelization of firms' revenues. The second measure considered is a profitability ratio, the Profit Margin, which is calculated as net income divided by revenue or net profits divided by sales. This variable is expressed in percentage and it measures how much out of every dollar of sales a company actually keeps in earnings. The third variable of interest, Net Income, reflects the company's total earnings. It is calculated by taking revenues and subtracting the costs of doing business such as depreciation, interest, taxes and other expenses. This variable is considered to be an important measure of how profitable the company is over a period of time.

2.3.2 Difference-in-Differences Estimator

As pointed out above, I apply the difference-in-differences estimator in order to obtain the causal effect of interest. This method allows to control for unobserved differences between treated and control observations and for common shocks through the variables *cartelized* and *period*. In particular, I run the following regression on the constructed matched sample:

$$Y_{it} = \beta_0 + \beta_1 Cartelized_i + \beta_2 Period_t + \beta_3 Cartelized_i * Period_t + \alpha_i + \delta_t + u_{it} \quad (2.1)$$

where Y_{it} is the outcome of interest described in the previous section; $Cartelized_i$ takes value 1 if the firm has ever been cartelized in the sample period and 0 otherwise; $Period_t$ takes value 1 the years in which the treatment took place and 0 before; $Cartelized_i * Period_t$ is the interaction of the previous two dichotomous variables, so it takes value 1 for the cartelized firm during the period in which it was cartelized and 0 before; α_i represents individual fixed effects; δ_t represents time fixed effects; and u_{it} is the error term. Note that the variable $Period_t$ takes value 1 for the *non-cartelized* firm whenever it takes value 1 for its match in the treatment group[7].

The coefficient of interest is β_3, which tells how much more cartelized firms earned during the period of cartelization compared to the non-cartelized ones. Thus, it gives the average treatment effect of the treated. The estimation of β_3 by OLS from the matched panel data sample is going to be the baseline specification, which is equivalent to fixed effects (FE) estimator for panel data when individual and time fixed effects are included in the regression. I also estimate the random effects (RE) estimator and the random-effects linear model with an AR(1) disturbance (AR(1)). This last model uses the generalized least-squares method to estimate the parameters in a linear regression model in which the errors are serially correlated - the errors are assumed to follow a first-order autoregressive process.

Since firms start and stop participating in a cartel at different moments in time, we can consider the period 0 as the year in which they become a member of a cartel. Given the way I have constructed the matched sample, meaning five years before the treatment, in most of the cases I will have information for five periods before the treatment takes place (this would be up to distance -5 from period 0), except for those two first years in which the matching was made three and four years before the treatment. Similarly, each additional year in which the firm is still cartelized will show up, and the maximum distance from treatment that could be found in the sample is 13[8]. This distance from treatment determines the different divisions of the sample that I use in the estimations.

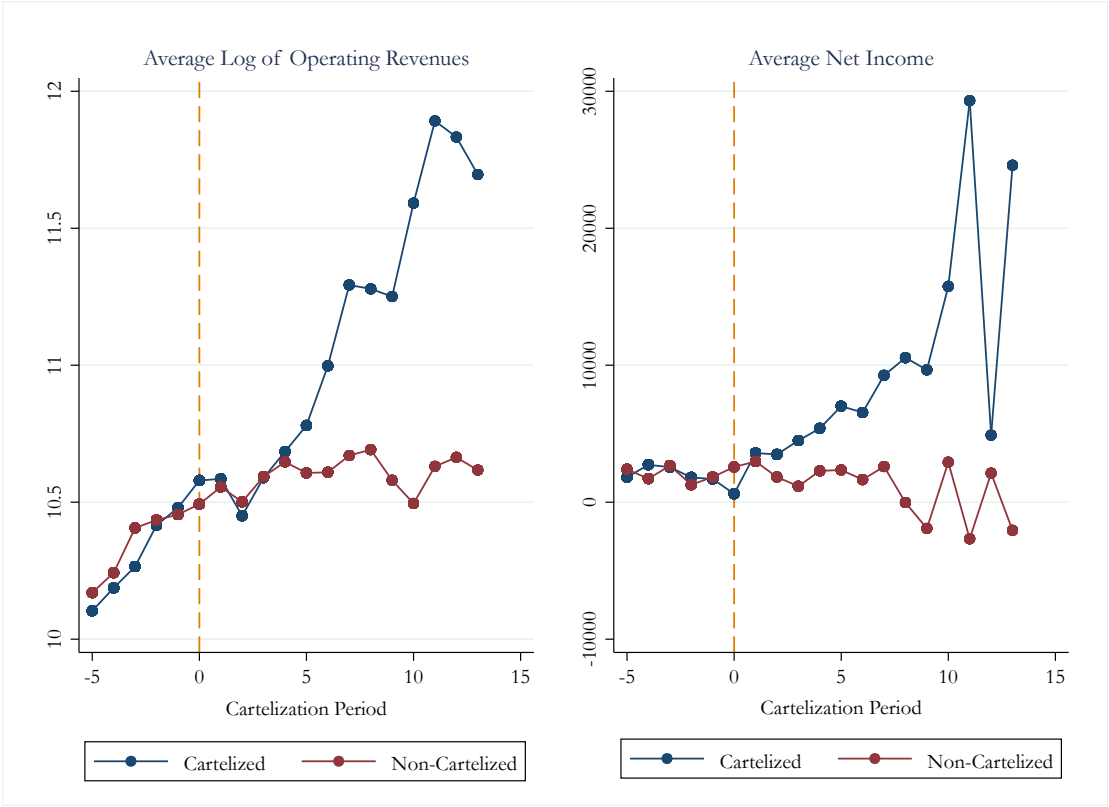
The basic identifying assumption of the difference-in-differences estimator is that the trends in the two groups are the same in the absence of intervention. As mentioned above, the matching has been performed five periods before the treatment starts. Despite having showed that the variables used in the matching are on average equal for the treatment and control group in each

[7]In order to cluster the standard errors by cartel, all the matches of the firms that belong to a given cartel are treated as if they would have formed another cartel themselves had they been cartelized. Therefore, apart from having pairs of treated and control firms, I have an artificial *control cartel* for each existing cartel when it comes to standard errors.

[8]There are only three firms that lasted up to 15 periods in a cartel, but they are outliers in terms of the outcome variable. Therefore, they are excluded from the estimation sample, so that outliers do not drive these results.

of the pre-treatment period, it still remains to test whether the outcome variables of interest follow parallel trends in the two groups. Figure 1 represents the average logarithm of operating revenues and the average net income for cartelized and non-cartelized firms. It can be seen that these outcomes follow similar patterns before the treatment starts, and then evolve differently over time.

Figure 2.1: *Average outcome by Cartelized and Non-Cartelized firms.*



Note: The dot line indicates the first period in which treatment takes place. This graph represents the average outcome variable for the treated and control group.

Additionally, I perform two different tests. First, I test for the equality of average changes in the treatment and control group before the treatment following Galiani, Gertler and Schargrosdky (2005). Secondly, I test if there exists any difference in time patterns before the treatment takes place.

For the test of parallel trends, I consider only the pre-treatment period for cartelized firms while the observations of the control group are considered for the whole period. I estimate the fully saturated model and test for equality of the relevant coefficients. Tests are performed for the different outcome variables of interest and for the different samples considered in the regressions. Results are shown in Table A.4 in the Appendix. In most of the cases we cannot reject the null hypothesis that the trends of the treatment and control group are the same in the pretreatment period at the 10% and 5% significance level. The same applies for the trends

of non-cartelized firms before and after the treatment, and equality of trend of cartelized before the treatment and non-cartelized before and after the treatment.

When the trends of the series can be better approximated by a non-linear function, there exists an alternative way to test the pattern in each of the periods before the cartelization takes place. Using again the sample of cartelized firms in the pretreatment period and the non-cartelized firms in the whole period, I estimate a regression that contains a dummy for each pre-treatment period for each treatment group. In this case, there exist differences in the levels of the outcome variables between cartelized and non-cartelized firms. However, what matters for the difference-in-differences models is that this difference in levels is constant over time in the pretreatment period. Therefore, the tests performed study whether the difference between cartelized and non-cartelized firms is the same in two or more periods. The null hypotheses of the tests and the corresponding results are presented in Table A.5 in the Appendix. Given that there exist many possible combinations, I tested the differences in consecutive periods from five years before the treatment up to one period before, and also all of them together. In most of the cases it cannot be rejected the hypothesis that the difference in the levels of the outcome variables of cartelized and non-cartelized firms is constant in the pre-treatment period.

2.4 Results

Given that I am interested in the overall profitability of cartelization, the main results are the ones coming from the full sample. Thus, the first sample (denoted as *Overall Effect*) contains all the observations, meaning the *cartelized* firms and their pair from the control group from distance -5 until distance 13. Secondly, since many of the cartel cases break up after a few years, after distance two I start observing an important decrease in the number of treated observations (and consequently in the control group counterpart). Table 2 shows that the average duration of cartels is around 3 years in the full sample. Therefore, the second sample of interest (*Short-term Effect*) contains all the observations from distance -5 until distance 2, which again means from five years before the treatment and up to three years of treatment. This will allow me to examine whether there exists a significant short-term impact of cartelization on profits. Additionally, I am interested in studying whether the effect of cartelization on firms' profits is different for those cartels that do not last long and for those that last for many years. Since the maximum distance from treatment is 13, I have considered two groups: 1) the first one is composed only by those firms that were in a cartel up to 7 periods or less (*Short-lived cartels*, from distance -5 until distance 7); 2) the second group considers only firms that were cartelized more than 7 periods (*Long-lived cartels*, from distance -5 until distance 13).

This section presents the results obtained from the different estimations. First, the main

results corresponding to the overall treatment effect and the short-term effect of cartelization on firms' profits will be analyzed. Then, the results by cartel duration are considered.

2.4.1 Main Results

Table 5 summarizes the results of the difference-in-differences estimator for the samples corresponding to the estimation of the *overall effect* and the *short-term effect*. Firstly, results show that being a member of a cartel has a positive and significant overall impact on revenues. The operating revenues of the cartelized firms are around $19\% - 26\%$ higher than the operating revenues of the non-cartelized firms during the period of cartelization. This effect on firms' revenues is also present in the short-run, although it is lower. In the first three years of cartel membership, firms manage to increase their revenues by $14\% - 17\%$ compared to what they would have obtained if they had not been cartelized.

Table 2.5: *Log of Operating Revenues. Diff-in-diff coefficient (β_3).*

Sample	**OLS**	**FE**	**RE**	**AR(1)**
Overall Effect	**0.26****	**0.26****	**0.26****	**0.19*****
	(0.11)	(0.10)	(0.11)	(0.05)
N	1814	1814	1814	1814
R^2	0.888	0.406	0.888	0.886
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Short-term Effect	**0.17****	**0.17****	**0.17****	**0.14*****
	(0.08)	(0.08)	(0.08)	(0.05)
N	1368	1368	1368	1368
R^2	0.905	0.268	0.905	0.904
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

Other measures of profitability such as Net Income and Profit Margin are not significantly affected by the cartelization. This is, neither the overall nor the short-term impacts are significantly different compared to the non-cartelized firms. Results are shown in Table A.6a and Table A.6b in the Appendix.

Thus, the first important result is that cartelization increases firms' revenues by $19\% - 26\%$ on average, but this is not translated into profits or earnings. This result may reflect the fact that managers are the ones making the decision of colluding or not. If their reputation or salary bonuses are based on the performance of the firm, which can be measured with firms' sales, then getting involved in a cartel may be beneficial for their own interests. However, these personal interests may not always be aligned with shareholders' interests.

It should be noted that one of the limitations of the work is that the firms I am considering as non-cartelized, could have been involved in a collusive agreement that was never discovered.

If it was the case, results may be biased downwards and the total effect of cartelization may be higher when compared to truly non-cartelized firms. Additionally, since results show a positive and significant effect on the treatment group compared to the control group, then they may be reflecting the fact that the discovered are cartels were more successful in terms of revenues than the existing and not discovered ones.

On the other hand, another possible limitation of the data is that the firms of the control group, even if they have never belonged to a collusive agreement, they could benefit or free-ride from the reduced competition in the market. Or they could be harmed if the collusive firms try to expel them from the market. If this was the case, then the treatment would not be being cartelized versus not being cartelized but participating in a collusive agreement versus not participating, given the existence of collusion in the market. Thus, in that setting results should be interpreted as how much more do firms benefit from directly participating in the collusive agreement compared to the rest. However, this scenario does not seem to be the most plausible in the matched sample. I have estimated a simple OLS regression on the control group to test the existence of a different trend before and during the period of treatment. The null hypothesis of equality of coefficients cannot be rejected at standard significance levels.

2.4.2 Short-lived and Long-lived cartels

A second interesting result is found when the sample is split in two, distinguishing between firms that belonged to cartels that lasted long enough and those that lasted less. From Table 6, it can be seen that firms that colluded for seven periods at most, manage to increase their revenues by $13\% - 15\%$ on average. However, no significant impact is found on Net Income. Similarly, Table 7 presents the results for the firms belonging to *long-lived* cartels. These firms had on average $29\% - 50\%$ higher revenues than what they should have got if they had not been cartelized. Additionally, cartelization also had a positive and significant impact on the Net Income of these firms. Due to cartelization, they gained on average 7939-8811 thousand euros more than they should have. This increased quantity is more than two times higher than the average profits they would have earned if they had not been cartelized. Again, Profit Margin is not significantly affected in any of the cases.

The second conclusion that can be extracted from the results is that cartelization always increases firms' revenues on average, but there exists a difference in the effects on profits. Two alternative scenarios are possible. On the one hand, it may be that both types of cartels are similar from the beginning in terms of profitability, but being cartelized long enough is the only way to turn the increase in revenues into profits gains (organizational costs are reduced, for instance). On the other hand, the causality could go in the opposite direction, meaning that some

Table 2.6: *Short-lived Cartels. Diff-in-diff coefficient (β_3).*

Sample	OLS	FE	RE	AR(1)
Log of Op. Revenues	**0.15***	**0.15***	**0.15***	**0.13****
	(0.09)	(0.08)	(0.09)	(0.06)
N	1201	1201	1201	1201
R^2	0.896	0.211	0.896	0.896
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Net Income	**-445.97**	**-445.97**	**-445.97**	**-535.74**
	(729.27)	(678.85)	(729.27)	(1000.42)
N	1201	1201	1201	1201
R^2	0.398	0.058	0.398	0.398
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

Table 2.7: *Long-lived Cartels. Diff-in-diff coefficient (β_3).*

Sample	OLS	FE	RE	AR(1)
Log of Op. Revenues	**0.50***	**0.50****	**0.50****	**0.29*****
	(0.23)	(0.22)	(0.23)	(0.11)
N	514	514	514	514
R^2	0.876	0.530	0.876	0.871
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Net Income	**8811.16***	**8811.16***	**8811.16****	**7939.08*****
	(4011.20)	(3883.04)	(4011.20)	(2632.21)
N	514	514	514	514
R^2	0.639	0.097	0.639	0.639
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

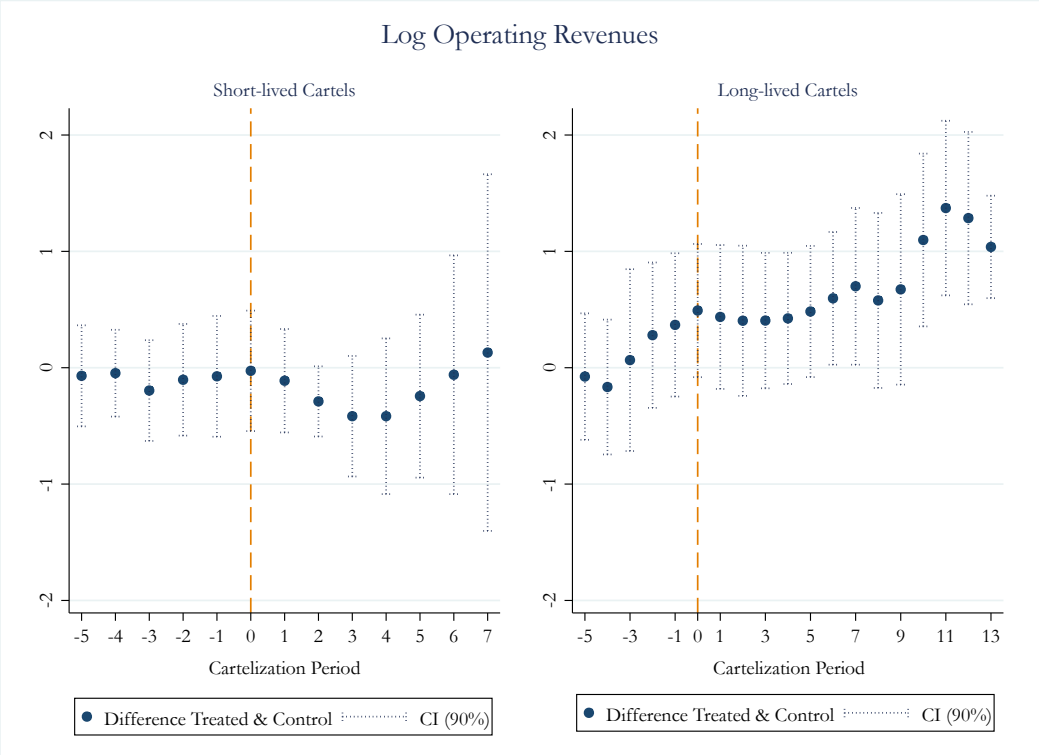
Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

cartels are more profitable than others from the very beginning and this is one of the reasons why they last longer. These two competing ideas are studied in the next section.

Evolution of Revenues and Profits

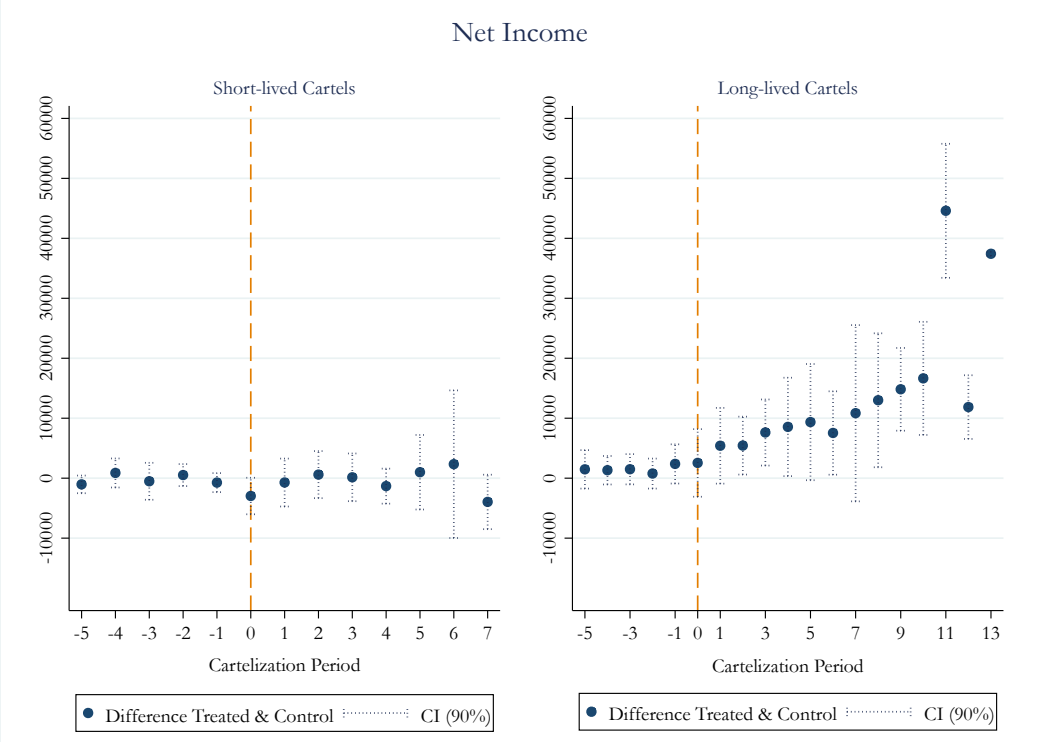
In this section, I analyze the evolution of revenues and profits period by period for short-lived and long-lived cartels separately. In Figure 2 and 3 we can observe some suggestive evidence that the amount of revenues and profits that cartelized firms earn, in comparison with non-cartelized firms, may be different since the first period of treatment for the two different cartel durations. However, these results should be interpreted carefully for two reasons. First of all, the sample contains 99 cartelized and 97 non-cartelized firms. Therefore, when the sample is split in two, according to cartel duration, the number of firms in each group is smaller. There are 80 cartelized firms that belonged to a short-lived cartel, and 16 cartelized firms that belonged to a long-lived cartel. The second reason is that firms drop out of the sample when they abandon the cartel or when the cartel dies - whatever happens first. Therefore, while in the short-lived cartels observations drop from period 1 onwards, the observations of long-lived cartels start dropping after period 7.

Figure 2.2: *Comparison of average Revenues between Treated and Control group.*



Note: The dot line indicates the first period for which treatment takes place. This graph represents the difference in equality of means between control and treated group.

Figure 2.3: *Comparison of average Profits between Treated and Control group.*



Note: The dot line indicates the first period for which treatment takes place. This graph represents the difference in equality of means between control and treated group.

In order to analyze the evolution of revenues and profits thoroughly, I perform two different tests. Pooling all the observations in the same regression, I test whether the overall impact of cartelization on firms' profitability is different for short-lived and long-lived cartels. In addition, I test whether there exists any difference in the revenues and profits earned by cartelized firms, compared to what the non-cartelized firms earned in the cartelization period, between the short-lived and long-lived cartels. For this last test, I have considered both a dummy variable for each period and another dummy variable that considers the cumulative effect up to the given period.

Table 2.8: *Test equality of diff-in-diff coefficient (β_3) across regression. Log of Operating Revenues.*

Diff-in-diff (β_3)	Distance	Short		Long		Short	Long	Chi^2	p-val
		Coeff	Std. Err.	Coeff	Std. Err.	N	N		
Cumulative	0-1	0.12	(0.07)	0.37*	(0.18)	1002	220	2.27	0.13
	0-2	0.11	(0.08)	0.36*	(0.18)	1074	250	2.13	0.14
	0-3	0.13	(0.08)	0.35*	(0.18)	1114	281	1.88	0.17
	0-4	0.14	(0.08)	0.35*	(0.19)	1148	312	1.68	0.20
	0-5	0.15*	(0.08)	0.36*	(0.19)	1177	343	1.69	0.19
	0-6	0.15*	(0.09)	0.38*	(0.19)	1195	373	1.66	0.20
	0-7	0.15*	(0.09)	0.40*	(0.20)	1201	401	1.81	0.18
	All	0.15*	(0.09)	0.50*	(0.23)	1201	514	2.96	**0.09**
Dummy	0	0.13	(0.08)	0.35*	(0.18)	1201	514	1.67	0.20
	1	0.17	(0.12)	0.37*	(0.18)	1201	514	1.32	0.25
	2	0.05	(0.10)	0.35*	(0.19)	1201	514	3.00	**0.08**
	3	0.24	(0.20)	0.35*	(0.18)	1201	514	0.28	0.59
	4	0.20	(0.19)	0.39*	(0.21)	1201	514	0.66	0.42
	5	0.27	(0.22)	0.43**	(0.19)	1201	514	0.41	0.52
	6	0.28	(0.29)	0.45*	(0.24)	1201	514	0.26	0.61
	7	-0.04	(0.30)	0.53*	(0.26)	1201	514	2.36	0.12

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets. Fixed effects for Firm and Year have been included in the regression

Results are presented in Table 8 and in Table 9. From these regressions and tests, it can be concluded that the overall impact of cartelization on profitability is different for short-lived and long-lived cartels, on average. With respect to results by period, there exists some evidence pointing at the existence of differences between the cartels duration since the first period. This evidence is stronger when the cumulative effect is considered.

Table 2.9: *Test equality of diff-in-diff coefficient (β_3) across regression. Net Income.*

Diff-in-diff (β_3)	Distance	Short		Long		Short	Long	Chi2	p-val
		Coeff	Std. Err.	Coeff	Std. Err.	N	N		
Cumulative	0-1	-1030.05	(853.76)	2328.28	(1852.53)	1002	220	3.59	**0.06**
	0-2	-724.53	(840.69)	2828.33	(1662.59)	1074	250	4.77	**0.03**
	0-3	-579.12	(768.77)	3607.94*	(1767.05)	1114	281	6.06	**0.01**
	0-4	-585.52	(751.18)	4266.67*	(2088.63)	1148	312	6.17	**0.01**
	0-5	-503.94	(735.13)	4848.70*	(2422.50)	1177	343	5.66	**0.02**
	0-6	-440.79	(736.40)	5001.67*	(2462.66)	1195	373	5.54	**0.02**
	0-7	-445.97	(729.27)	5458.09*	(2853.70)	1201	401	4.92	**0.03**
	All	-445.97	(729.27)	8811.16*	(4011.20)	1201	514	6.14	**0.01**
Dummy	0	-2912.03	(2367.11)	1331.12	(1536.26)	1201	514	3.06	**0.08**
	1	1035.56	(1834.12)	3751.56	(2452.01)	1201	514	0.87	0.35
	2	97.76	(1941.46)	3925.34*	(1855.86)	1201	514	2.39	0.12
	3	970.04	(1425.35)	5149.00*	(2501.66)	1201	514	2.52	0.11
	4	108.79	(1066.12)	6984.77*	(3259.35)	1201	514	5.19	**0.02**
	5	2264.28	(2364.01)	7558.13*	(4000.49)	1201	514	1.43	0.23
	6	2917.78	(5367.53)	5823.51*	(2819.80)	1201	514	0.25	0.62
	7	-449.93	(1145.31)	9241.60	(5703.33)	1201	514	3.09	**0.08**

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets. Fixed effects for Firm and Year have been included in the regression

Sensitivity Analysis

In order to analyze how qualitative and quantitative results depend on the cartel duration used to split the sample in two, a sensitivity analysis is performed. The maximum cartel duration determining how the sample is split in two parts, varies successively, starting by comparing cartels lasting up to one year or between 2 and 13 years (first row of Table 10), until the maximum duration is exhausted.

Table 2.10: *Log of Operating Revenues. Diff-in-diff coefficient β_3).*

Duration	**RE**		**AR(1)**		**N**	**Duration**	**RE**		**AR(1)**		**N**
(up to)	Coeff	Std. Err.	Coeff	Std. Err.		(from)	Coeff	Std. Err.	Coeff	Std. Err.	
1	0.17*	(0.10)	0.15	(0.11)	473	**2-13**	0.28**	(0.14)	0.17***	(0.06)	1242
2	0.12	(0.10)	0.11	(0.08)	726	**3-13**	0.33**	(0.16)	0.20***	(0.07)	989
3	0.16	(0.12)	0.14*	(0.08)	807	**4-13**	0.32**	(0.15)	0.18***	(0.07)	908
4	0.15	(0.11)	0.13*	(0.07)	847	**5-13**	0.35**	(0.16)	0.19***	(0.07)	868
5	0.12	(0.10)	0.11*	(0.06)	970	**6-13**	0.41**	(0.18)	0.23***	(0.09)	745
6	0.16*	(0.09)	0.14**	(0.06)	1123	**7-13**	0.43**	(0.21)	0.24**	(0.10)	592
7	0.15*	(0.09)	0.13**	(0.06)	1201	**8-13**	0.50**	(0.23)	0.29***	(0.11)	514
8	0.13*	(0.08)	0.11**	(0.05)	1317	**9-13**	0.69***	(0.17)	0.43***	(0.14)	398
9	0.12	(0.08)	0.10*	(0.05)	1347	**10-13**	0.79***	(0.16)	0.51***	(0.15)	368
10	0.12	(0.08)	0.10**	(0.05)	1371	**11-13**	0.82***	(0.16)	0.53***	(0.16)	344
11	0.11	(0.08)	0.10*	(0.05)	1404	**12-13**	0.96***	(0.13)	0.62***	(0.18)	311
12	0.14*	(0.08)	0.12**	(0.05)	1440	**13**	0.91***	(0.04)	0.52***	(0.15)	275
13	0.25**	(0.12)	0.18***	(0.05)	1715						

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets. Fixed effects for Firm and Year have been included in the regression.

Results are shown in Table 10 for the variable *Log of Operating Revenue*, and in Table 11 for the variable *Net Income*. These tables show that cartels lasting short do not manage to increase their revenues nor profits significantly, while the reverse happens for cartels lasting long. Therefore, qualitative results do not depend on the cartel duration chosen to split the sample.

Table 2.11: *Net Income. Diff-in-diff coefficient β_3).*

Duration	**RE**		**AR(1)**		**N**	**Duration**	**RE**		**AR(1)**		**N**
(up to)	Coeff	Std. Err.	Coeff	Std. Err.		(from)	Coeff	Std. Err.	Coeff	Std. Err.	
1	-932.8	(1088.24)	-885.97	(1538.71)	473	**2-13**	3277.46*	(1885.39)	3022.47**	(1250.08)	1242
2	-201.03	(1220.47)	-502.1	(1682.68)	726	**3-13**	3826.90*	(2273.50)	3070.53**	(1444.18)	989
3	-199.65	(1063.29)	-453.73	(1491.98)	807	**4-13**	4225.8	(2603.67)	3512.70**	(1578.52)	908
4	-315	(987.37)	-547	(1420.22)	847	**5-13**	4534*	(2696.29)	3716.38**	(1650.20)	868
5	-98.13	(846.96)	-316.2	(1226.97)	970	**6-13**	5166.89*	(3077.56)	4305.99**	(1936.08)	745
6	-115.95	(763.83)	-243.4	(1075.42)	1123	**7-13**	6838.74*	(3853.31)	6102.55***	(2300.29)	592
7	-445.97	(729.27)	-535.74	(1000.42)	1201	**8-13**	8811.16**	(4011.20)	7939.08***	(2632.21)	514
8	-431.68	(732.93)	-501.28	(908.56)	1317	**9-13**	11915.37***	(3121.50)	10943.78***	(3420.65)	398
9	-421.23	(710.28)	-489.26	(887.12)	1347	**10-13**	13068.67***	(2753.76)	12038.63***	(3701.22)	368
10	-457.03	(697.91)	-522.77	(871.45)	1371	**11-13**	14196.79***	(2305.03)	13090.95***	(3937.37)	344
11	-456.14	(674.81)	-519.55	(850.16)	1404	**12-13**	15908.83***	(1171.60)	14753.78***	(4327.47)	311
12	-206.45	(754.32)	-263.41	(832.33)	1440	**13**	16702.11***	(832.88)	15562.35***	(4855.59)	275
13	2350.26	(1685.43)	2065.55**	(1026.82)	1715						

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets. Fixed effects for Firm and Year have been included in the regression.

2.4.3 Leniency Program

One of the main policies at the disposal of Antitrust Authorities aiming at detecting and sanctioning cartels is the Leniency Program. This policy offers companies involved in a cartel either total immunity from fines or fines reduction if they self-report the existence of the cartel or if they cooperate with the Authority. The Leniency Program also aims at deterring cartel formation by destabilizing the trust among cartel members. The controversial effects of this policy have been studied in the literature. In Spain, the Leniency Program was passed in 2007 and implemented since 2008.

In this section, I analyze whether there exists any difference between the profitability of cartels that applied for Leniency Program and those that were discovered due to any other reason. This analysis is relevant because it helps us studying whether this program incentivizes the breakup and discovery of all types of cartels, or only of those that are not profitable and would have broken up anyway.

There are 18 cartelized firms that were members of a cartel that was discovered under the Leniency Program, while 81 cartelized firms belonged to a cartel in which no member applied for the Leniency Program. The last cartelization year of the firms belonging to a collusive agreement that was not discovered under the program was 2007 or afterwards. This means that by the time most of these firms stopped colluding they were aware of the existence of this law. Therefore, it is not the case that these cartels could not apply for the Leniency Program (due to time restrictions) but the case in which the members decided not to apply for it.

Table 2.12: *Log of Operating Revenues. Diff-in-diff coefficient (β_3).*

Sample	OLS	FE	RE	AR(1)
Leniency	**0.06**	**0.06**	**0.06**	**0.04**
	(0.11)	(0.11)	(0.11)	(0.10)
N	386	386	386	386
R^2	0.882	0.458	0.882	0.880
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
No Leniency	**0.33*** **	**0.33*** **	**0.33*** **	**0.24*** **
	(0.12)	(0.11)	(0.12)	(0.06)
N	1428	1428	1428	1428
R^2	0.891	0.405	0.891	0.889
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

Following the previous econometric strategy, I first analyze the effect of cartelization on firms' revenues and profits, distinguishing between these two types of detection. Results are shown in Table 12 and 13. The main conclusion that can be drawn is that cartels that did not apply for the Leniency Program benefited from an increase in revenues, compared to non-cartelized firms, while those cartels that applied for Leniency Program did not experience this increase. The evidence is weaker when the outcome variable Net Income is considered, as shown in Table 13. A question left for further research (and further data availability) is whether there exists a difference in cartel profitability in comparison to cartels finalized and discovered before the implementation of the Leniency Program. This is, whether the mere existence of the program (even if not all cartels apply for it) has some deterrence impact with respect to cartel profitability.

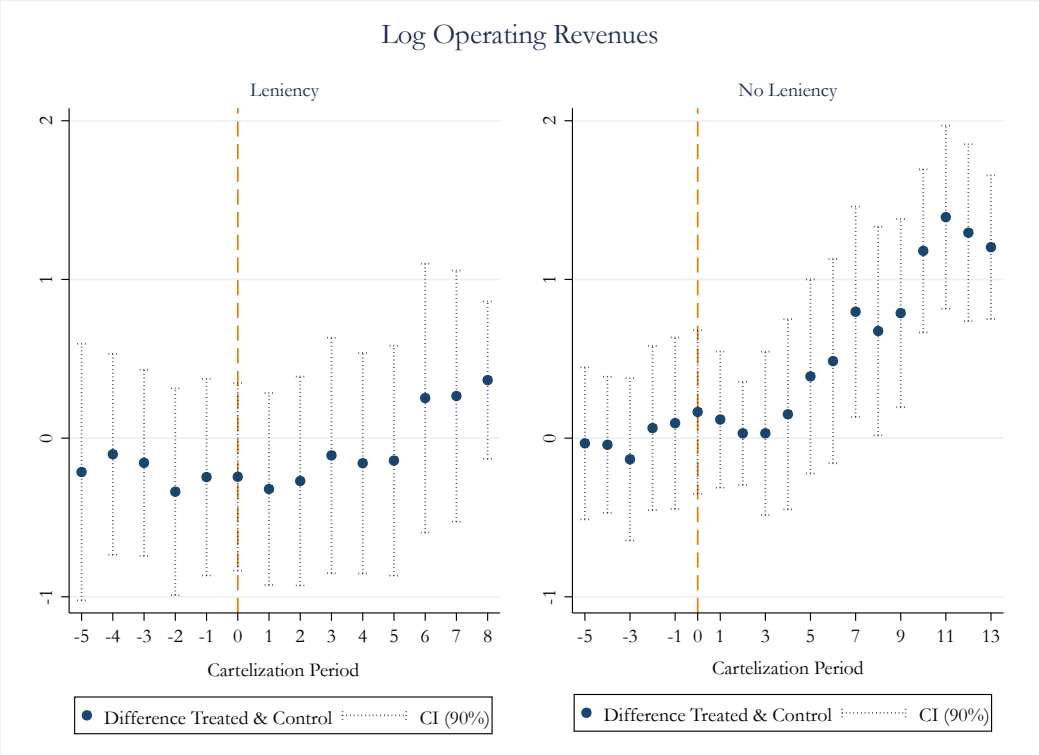
Table 2.13: *Net Income. Diff-in-diff coefficient (β_3).*

Sample	OLS	FE	RE	AR(1)
Leniency	**-1014.74**	**-1014.74**	**-1014.74**	**-1561.73**
	(1597.01)	(1520.59)	(1597.01)	(1455.09)
N	386	386	386	386
R^2	0.383	0.126	0.383	0.375
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
No Leniency	**2910.79**	**2910.79**	**2910.79**	**2600.10****
	(1971.78)	(1855.94)	(1971.78)	(1226.90)
N	1428	1428	1428	1428
R^2	0.537	0.024	0.537	0.537
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

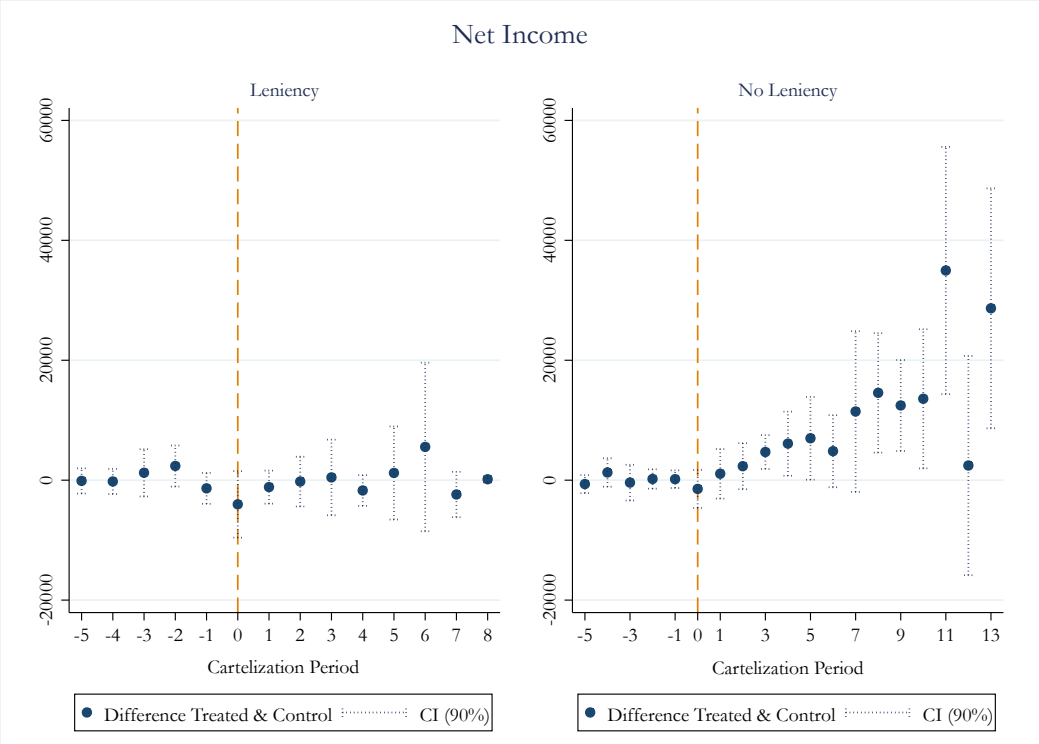
Apart from the average impact of the treatment, the effect can also be analyzed period-by-period. From Figure 4 and Figure 5, it cannot be concluded that cartelization had a different impact on firms' revenues and profits when they were or were not discovered under the Leniency Program even though they seem to follow different patterns.

Figure 2.4: *Comparison of average Revenues between Treated and Control group.*



Note: The dot line indicates the first period for which treatment takes place. This graph represents the difference in equality of means between control and treated group.

Figure 2.5: *Comparison of average Profits between Treated and Control group.*



Note: The dot line indicates the first period for which treatment takes place. This graph represents the difference in equality of means between control and treated group.

The weak evidence appearing in the figures should be tested across regressions. Again, it should also be taken into account the data limitations in terms of the number of observations. Results are summarized in Table A.7a and Table A.7b in the Appendix. There is clear evidence in Table A.7a of the existence of significant statistical differences in the revenues earned by the firms belonging to cartels discovered under these two types of detection. In particular, for the members of cartels applying for the Leniency Program, cartelization did not produce a significant impact on revenues, while the effect of cartelization on revenues is significant when cartels not applying for the program are considered. On the other hand the impact of cartelization on firms' profits is only significant in some late periods when the analysis is performed period by period. However, Table A.7b shows that there exists a statistical significant difference between the additional profits earned by the cartelized firms belonging to a cartel discovered under the Leniency Program and the ones belonging to cartels discovered due to other reasons (always in comparison with the control group).

2.5 Robustness Checks

In order to ensure that the effects found are due to the treatment, two different standard placebo tests from the literature are performed. The first placebo test is performed in the pre-treatment period to check whether some effect on firms' profits can be observed before the treatment takes place. The second placebo test replicates the estimations of interest in a sample formed by the control group and a new match for each of the firms belonging to it.

2.5.1 Placebo Test in Pre-Treatment Period

The test is performed in the sample of cartelized and non-cartelized firms in the pre-treatment period only. The following regression is estimated:

$$Y_{it} = \beta_0 + \beta_1 Cartelized_i + \beta_2 PeriodPla_t + \beta_3 Cartelized_i * PeriodPla_t + \alpha_i + \delta_t + u_{it} \quad (2.2)$$

where Y_{it} is the outcome of interest ; $Cartelized_i$ takes value 1 if the firm has ever been cartelized in the sample period and 0 otherwise; $PeriodPla_t$ takes value 0 for distances to treatment period -5 and -4, and takes value 1 for distances to treatment period -3 and -2; $Cartelized_i * PeriodPla_t$ is the interaction of the previous two dichotomous variables; α_i represents individual fixed effects; δ_t represents time fixed effects; and u_{it} is the error term. The period previous to the start of the treatment (distance -1) has not been included in this regression because this date could be measured with error given that cartels are an illegal activity. Given that the pre-treatment period is only composed by five periods, such a relevant period

could have an important weight and impact in the estimated coefficients[9].

Table 2.14: *Log of Operating Revenues. Diff-in-diff coefficient (β_3).*

Sample	**Placebo Pre-Treatment Period**			
	OLS	**FE**	**RE**	**AR(1)**
All Cartels	**0.08**	**0.08**	**0.08**	**0.08**
	(0.08)	(0.07)	(0.08)	(0.06)
N	750	750	750	750
R^2	0.92	0.186	0.92	0.92
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Long-lived Cartels	**0.27**	**0.27**	**0.27**	**0.27**
	(0.37)	(0.32)	(0.37)	(0.19)
N	127	127	127	127
R^2	0.889	0.254	0.889	0.889
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

The results of the first placebo test are presented in Table 14[10]. It can be concluded that cartelized firms do not behave differently from non-cartelized firms two and three periods before the treatment starts compared to four and five periods before cartelization. Results for the variable Net Income, presented in Table A.8a in the Appendix, are not significant either.

2.5.2 Placebo Test for Control Group

In this case, I work only with the non-cartelized firms. They are going to be the *artificial treatment group* in the placebo test. Therefore, I treat them as if they were the cartelized firms in the new sample I create. The period of treatment I use is the one that was artificially imputed to them when they were used as control, therefore, the one that corresponds to the cartelized firm for which they are the match. Thus, I can repeat the procedures explained in sections 3.2 and 3.3. For each of these non-cartelized firms, that are considered as *treated* in this case, I look for a pair or control in the full sample of non-cartelized firms. Again, I apply the non-parametric nearest neighbor matching method, and the observable characteristics used for the matching are the same as before. After constructing the matched sample, I apply the difference-in-differences estimator in order to check whether the coefficients of interest are significant in this case. Moreover, I test whether the outcomes of the *treated* and *non-treated* firms have parallel trends in the pre-treatment period as it was done in section 5.1. It is also tested whether the *non-treated* firms show different trends in their outcome variables for the pretreatment and treatment period.

[9]I have performed the placebo test also including also this period and the coefficients and its significance are basically not affected.

[10]Results for *short-lived cartels* can be found in Table A.8b in the Appendix.

Table 2.15: *Log of Operating Revenues. Diff-in-diff coefficient (β_3).*

Sample	OLS	FE	RE	AR(1)
Overall Effect	**-0.04**	**-0.04**	**-0.04**	**-0.04**
	(0.05)	(0.04)	(0.05)	(0.04)
N	1725	1725	1725	1725
R^2	0.921	0.396	0.921	0.921
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Short-term Effect	**-0.03**	**-0.03**	**-0.03**	**-0.03**
	(0.05)	(0.05)	(0.05)	(0.04)
N	1318	1318	1318	1318
R^2	0.938	0.266	0.938	0.938
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

The estimation results of the outcome variable logarithm of operating revenues for the overall and the short-term effects are presented in Table 15[11]. The placebo results of the analysis for short-lived and long-lived cartels are summarized in Table 16 and Table 17. No significant impact of *cartelization* on firms' revenues or profits can be found in these placebo analyses. This confirms that the main results obtained for cartelized and non-cartelized firms actually represent the causal effect of being a member of a cartel on firms' revenues or profits.

Table 2.16: *Short-lived cartels. Diff-in-diff coefficient (β_3).*

Sample	OLS	FE	RE	AR(1)
Log of Op. Revenues	**-0.08**	**-0.08**	**-0.08**	**-0.07**
	(0.06)	(0.06)	(0.06)	(0.05)
N	1132	1132	1132	1132
R^2	0.923	0.206	0.923	0.923
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Net Income	**707.18**	**707.18**	**707.18**	**920.81**
	(869.48)	(806.31)	(869.48)	(778.75)
N	1132	1132	1132	1132
R^2	0.71	0.083	0.71	0.709
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

[11]Results for the variable Net Income can be found in Table A.9 in the Appendix.

Table 2.17: *Long-lived cartels. Diff-in-diff coefficient (β_3).*

Sample	OLS	FE	RE	AR(1)
Log of Op. Revenues	**0.04**	**0.04**	**0.04**	**0.06**
	(0.10)	(0.10)	(0.10)	(0.09)
N	501	501	501	501
R^2	0.92	0.523	0.92	0.919
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Net Income	**-906.18**	**-906.18**	**-906.18**	**-899.64**
	(841.74)	(814.22)	(841.74)	(1060.59)
N	503	503	503	503
R^2	0.333	0.084	0.333	0.333
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

2.6 Conclusions

Cartels are considered one of the most serious infringements in competition policy. Fighting against cartels and preventing their formation is not only one of the main tasks of the European Commission but also of most of the National Competition Authorities. However, a precise measure of how much firms benefit from collusion is needed in order to design effective policies.

In this project, it has been analyzed and quantified the causal effect of having participated in a collusive agreement on the revenues and profits earned by its members. The main object is to understand how big are the incentives for firms to participate in a collusive agreement.

Despite the limitations originated from working with discovered cartels, this study contributes in two aspects. First of all, I make use of a control group in order to be able to compare the evolution of revenues and benefits of cartelized firms with a group of reference. The second relevant contribution is that the problem of self-selection into treatment has been considered. The construction of the control group is based on having cartelized and non-cartelized firms similar in factors conductive to increase the likelihood of participating in a cartel. The control group reflects as accurate as possible the potential evolution of the cartelized firms had they not belonged to a cartel. In addition, the difference-in-differences estimator controls for unobservable differences.

In order to study the impact of cartelization on firms' revenues and profits, I use a panel data of cartelized and non-cartelized Spanish firms for the period 1995-2014 coming from two different sources. On the one hand, I use information provided by the reports of the 68 cartels that have been sanctioned by the Spanish Antitrust Authority in the last two decades (1990-2014). On the other hand, I have information of the balance sheet of Spanish firms since 1992 to 2014. This data comes from the Iberian Balance sheet Analysis System (SABI).

Analyzing the two datasets together, I can distinguish which firms have participated in a

collusive agreement (*cartelized*) from those that either have never participated in a cartel or have not been discovered yet (*non-cartelized*). After creating a sample of interest that includes cartelized and non-cartelized firms similar and comparable in observable characteristics (using the Nearest Neighbor Matching Algorithm), I can compare the revenues and profits of these two groups. Using the difference-in-differences estimator, it can be estimated the effect of cartelization of firms' profitability.

Results show that firms' revenues increase around 19%-26% due to participation in a collusive agreement. However, profits measured as net income do not show a significant impact on average. In the first three years of cartelization, which corresponds to the average cartel duration in the full sample, firms increase their revenues by 14%-17%.

The most interesting results are found when the sample is divided into *short-lived* and *long-lived cartels*. While the first ones only experience an increase in revenues, not only a significant impact on revenues but also on profits is found on the latter. This is, firms that belong to a long-lasting cartel increase their revenues by 29%-50%. Also, these revenues are translated into profits since net income is, on average, 2.15-2.33 times higher than what these cartelized firms would have earned if they had not been involved in a cartel case. These *long-lived cartels* appear to be more profitable since initial periods. Finally, there exists some weak evidence that only members of non-profitable cartels apply for the Leniency Program.

Summing up, it can be concluded that colluding is profitable in Spain, especially when the collusive agreement lasts long enough. Additionally, the magnitude of this positive effect should be considered when designing competition policies.

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Appendix 2.A

2.A.1 Distribution of Cartelized Firms by Sector

Table A.1[12] contains the representation of sectors both in the raw data and in the matched sample.

Table 2.A.1: *Cartelized firms by sector.*

	Total Sample		**Matched Sample**	
Sector	**Firms**	**Freq.**	**Firms**	**Freq.**
A	3	1.19	1	1.01
B	1	0.4	0	0.00
C	74	29.25	21	21.21
D	3	1.19	0	0.00
E	17	6.72	9	9.09
F	33	13.04	22	22.22
G	70	27.67	28	28.28
H	24	9.49	10	10.1
J	4	1.58	0	0.00
K	5	1.98	1	1.01
M	2	0.79	1	1.01
N	17	6.72	6	6.06
Total	253	100.00	99	100.00

2.A.2 Truncation

Before constructing the matched sample, I drop from the sample the outliers belonging to the upper and lower tails of the distribution. The reason for doing this is that I may not be able to find a good counterfactual for these observations. Since I am working with firms, an outlier in terms of profits could drive the results of the estimation, and having a good counterfactual for the cartelized firms is crucial for the validity of the results. Moreover, it is a common procedure in the literature when working with firm-level data.

I proceed as follows. I look for the firms that have been, at least in one of the years (1992-2014), below the 0.1% or above the 99.9% of the distribution of the following dependent variables: Profit Margin, Net Income, Operating Revenues, Ebitda or Ebit. Once I have identified these firms, I drop them from the sample. From the initial 21,514 firms, I end up with 20,757 firms in the truncated sample. In Table 3, it can be seen the number of total different firms that have been

[12]Description of NACE Rev.2 Sector Classification by Section: A - Agriculture, forestry and fishing. B - Mining and quarrying. C - Manufacturing. D - Electricity, gas, steam and air conditioning supply. E - Water supply; sewerage, waste management and remediation activities. F - Construction. G - Wholesale and retail trade; repair of motor vehicles and motorcycles. H - Transportation and storage. J - Information and communication. K - Financial and insurance activities. M - Professional, scientific and technical activities. N - Administrative and support service activities.

excluded from each distribution and the total number of different firms dropped from the sample.

Table 2.A.2: *Truncation: Number of firms dropped.*

	Profit Margin		Net Income		Operating Revenues		Ebitda		Ebit		Total
	< 0.01	> 99.9	< 0.01	> 99.9	< 0.01	> 99.9	< 0.01	> 99.9	< 0.01	> 99.9	
#Firms	243	164	170	80	256	42	157	62	150	75	757

2.A.3 Matching Variables

The summary statistics of the observable characteristics used in the matching are presented in Table A.3.

Table 2.A.3: *Summary Statistics.*

Variable	Mean	Median	Std. Dev.	Min.	Max.	N
Age	30.43	26	19.39	2	114	2581
Total Assets	104266.9	28394.69	247482.2	1014.06	2257771	2258
Indebtedness	63.84	65.3	22.16	3.64	225.1	2258
Long-term Debt/Assets	0.11	0.07	0.15	0	1.88	2056
Costs of Employees	12224.39	4121.25	36219.61	45.22	533103	2258
Costs of Employees/Operating Revenues	15.98	12.81	13.13	0.26	106.1	2258
Solvency Ratio	1.50	1.24	1.27	0.06	26.33	2258
Leverage	119.91	61.73	479.10	–10803.45	9227.825	2258

Note: Age is expressed in years. Total Assets and Costs of Employees are expressed in thousand euros. The rest are expressed in percentage.

2.A.4 Parallel Trends

The first step is to test whether the cartelized and non-cartelized firms have parallel trends in their outcome variables before the treatment takes place. In this regression it is considered only the pretreatment period for each cartelized firm while the observations of the control firms are considered for the whole period. The following fully saturated model is estimated:

$$Y_{it} = \beta_1 CB_i + \beta_2 CB_i * Trend_t + \beta_3 NCB_i + \beta_4 NCB_i * Trend_t + \beta_5 NCA_i + \beta_6 NCA_i * Trend_t + \alpha_i + \delta_t + u_{it}$$

where CB_i is a dummy variable that takes value 1 for cartelized firms in the pretreatment period and 0 otherwise; NCB_i is a dummy variable that takes value 1 for non-cartelized firms in the pretreatment period and 0 otherwise; NCA_i is a dummy variable that takes value 1 for non-cartelized firms in the treatment period and 0 otherwise; $Trend_t$ is a trend variable and takes value 1 for distance -5 and increases in one unit for each distance until taking value 14 for distance 13; α_i represents individual fixed effects; δ_t represents time fixed effects; and u_{it} is the error term.

Table 2.A.4: *Test of Parallel Trends.*

Variable	**Sample**	H_0: β_2=β_4		H_0: β_4=β_6		H_0: β_2=β_4=β_6	
		F-stat	**p-value**	**F-stat**	**p-value**	**F-stat**	**p-value**
Log of Operating Revenues	Overall Effect	3.60	0.06	0.12	0.73	3.24	0.05
	Short-term Effect	4.08	0.05	0.02	0.90	2.27	0.11
	Short-lived Cartels	1.68	0.20	0.96	0.33	2.51	0.09
	Long-lived Cartels	1.51	0.24	1.21	0.30	1.50	0.27
Net Income	Overall Effect	0.66	0.42	0.30	0.58	0.33	0.72
	Short-term Effect	0.62	0.43	1.83	0.18	0.93	0.40
	Short-lived Cartels	0.53	0.47	1.88	0.18	0.94	0.40
	Long-lived Cartels	1.81	0.21	4.04	0.07	2.22	0.16

Note: Fixed effects for Firm and Year have been included in the regression.

2.A.5 Test Difference in Coefficients for each Distance

When the trends of the series can be better approximated by a non-linear function, there exists an alternative way to test the pattern in each of the periods before the cartelization takes place. Using again the sample of cartelized firms in the pretreatment period and the non-cartelized firms in the whole period, I estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 CD5 + \beta_2 NCD5 + \beta_3 CD4 + \beta_4 NCD4 + \beta_5 CD3 + \beta 6 NCD3 + \beta_7 CD2 + \beta_8 NCD2 + \beta_9 CD1 + \beta_{10} NCD1 + \alpha_i + \delta_t + u_{it}$$

where $CD5$ is a binary variable that takes value 1 for cartelized firms five periods before the treatment and 0 otherwise; $NCD5$ is a binary variable that takes value 1 for non-cartelized firms five periods before the treatment and 0 otherwise; the same reasoning applies for the rest of the binary variables where the number denotes the number of periods before the treatment; α_i represents individual fixed effects; δ_t represents time fixed effects; and u_{it} is the error term.

Table 2.A.5: *Test of Constant Difference by Distance to Treatment.*

Variable	Sample	H_0: β_2-β_1=β_4-β_3		H_0: β_4-β_3=β_6-β_5		H_0: β_6-β_5=β_8-β_7		H_0: β_8-β_7=$\beta_1$0-β_9		All Differences	
		F-stat	p-value	F-stat	p-value	F-stat	p-value	F-stat	p-value	F-stat	p-value
Log of Operating Revenues	Overall Effect	0.09	0.76	0.01	0.92	3.16	0.08	2.04	0.16	1.50	0.22
	Short-term Effect	0.09	0.76	0.01	0.90	3.19	0.08	2.11	0.15	1.67	0.17
	Short-lived Cartels	0.20	0.66	0.09	0.77	2.00	0.16	0.96	0.33	0.80	0.53
	Long-lived Cartels	2.21	0.16	0.45	0.52	1.07	0.32	6.60	0.03	2.51	0.10
Net Income	Overall Effect	1.47	0.23	1.64	0.21	0.20	0.65	0.01	0.92	0.79	0.54
	Short-term Effect	1.53	0.22	1.80	0.19	0.22	0.64	0.03	0.85	0.91	0.47
	Short-lived Cartels	1.53	0.22	1.96	0.17	0.41	0.52	0.41	0.53	1.51	0.22
	Long-lived Cartels	0.03	0.87	0.11	0.74	2.75	0.13	2.47	0.14	1.11	0.40

Note: Test of all differences: H_0: β_2-β_1=β_4-β_3=β_6-β_5=β_8-β_7=β_{10}-β_9. Fixed effects for Firm and Year have been included in the regression.

2.A.6 Estimation Results

The estimation results for the *overall effect* and the *short-term effect* of cartelization on Net Income and Profit Margin are presented in Table A.6a and A.6b, respectively.

Table 2.A.6: *Diff-in-diff coefficient (β_3).*

Table 2.A.6.A *Net Income. Diff-in-diff coefficient (β_3).*

Sample	**OLS**	**FE**	**RE**	**AR(1)**
Overall Effect	**1974.42**	**1974.42**	**1974.42**	**1657.29**
	(1700.99)	(1605.61)	(1700.99)	(1011.75)
N	1814	1814	1814	1814
R^2	0.524	0.025	0.524	0.523
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Short-term Effect	**-48.26**	**-48.26**	**-48.26**	**-156.98**
	(783.22)	(724.18)	(783.22)	(883.29)
N	1368	1368	1368	1368
R^2	0.453	0.016	0.453	0.453
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

Table 2.A.6.B *Profit Margin. Diff-in-diff coefficient (β_3).*

Sample	**OLS**	**FE**	**RE**	**AR(1)**
Overall Effect	**-2.04**	**-2.04**	**-2.04**	**-3.01**
	(1.66)	(1.56)	(1.66)	(4.84)
N	1814	1814	1814	1814
R^2	0.097	0.015	0.097	0.096
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Short-term Effect	**-1.64**	**-1.64**	**-1.64**	**-2.93**
	(2.49)	(2.30)	(2.49)	(6.33)
N	1368	1368	1368	1368
R^2	0.109	0.022	0.109	0.109
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

2.A.7 Evolution of Revenues and Profits by Leniency Program Application

The evolution period by period of the variables Log of Operating Revenues and Net Income distinguishing by Leniency Program application are presented in Table A.7a and Table A.7b respectively.

Table 2.A.7: *Test equality of diff-in-diff coefficient (β_3) across regression.*

Table 2.A.7.A *Test equality of diff-in-diff coefficient (β_3) across regression. Log of Operating Revenues.*

Diff-in-diff (β_3)	Distance	Leniency		No Leniency		Len	No Len	Chi2	p-val
		Coeff	Std. Err.	Coeff	Std. Err.	N	N		
Cumulative	0-1	0.05	(0.12)	0.21	(0.08)	230	1030	1.63	0.20
	0-2	0.01	(0.10)	0.22**	(0.09)	259	1109	2.93	**0.09**
	0-3	0.04	(0.09)	0.23**	(0.09)	285	1160	2.86	**0.09**
	0-4	0.04	(0.10)	0.25**	(0.10)	312	1204	2.86	**0.09**
	0-5	0.05	(0.11)	0.26**	(0.10)	338	1243	2.69	**0.10**
	0-6	0.07	(0.11)	0.27***	(0.10)	356	1278	2.14	0.14
	0-7	0.06	(0.12)	0.29***	(0.10)	369	1304	2.48	0.11
	0-8	0.06	(0.11)	0.30***	(0.10)	378	1328	2.80	**0.09**
	All	0.06	(0.11)	0.33***	(0.12)	386	1428	3.03	**0.08**
Dummy	0	0.02	(0.20)	0.21**	(0.08)	386	1428	0.97	0.33
	1	-0.04	(0.17)	0.29**	(0.11)	386	1428	3.27	**0.07**
	2	-0.08	(0.13)	0.24*	(0.12)	386	1428	3.94	**0.05**
	3	0.16	(0.24)	0.37**	(0.16)	386	1428	0.64	0.42
	4	0.16	(0.26)	0.39***	(0.14)	386	1428	0.75	0.39
	5	0.16	(0.27)	0.42***	(0.15)	386	1428	0.84	0.36
	6	0.23	(0.32)	0.39**	(0.15)	386	1428	0.24	0.62
	7	0.02	(0.24)	0.55***	(0.15)	386	1428	4.43	**0.04**
	8	0.11	(0.20)	0.53***	(0.17)	386	1428	2.92	**0.09**

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets. Fixed effects for Firm and Year have been included in the regression

Table 2.A.7.B *Test equality of diff-in-diff coefficient (β_3) across regression. Net Income.*

Diff-in-diff (β_3)	Distance	Leniency		No Leniency		Len	No Len	Chi^2	p-val
		Coeff	Std. Err.	Coeff	Std. Err.	N	N		
Cumulative	0-1	-2918.27	(2105.80)	281.72	(818.34)	230	1030	2.72	**0.10**
	0-2	-2276.99	(1641.20)	543.59	(848.54)	259	1109	3.05	**0.08**
	0-3	-1821.29	(1347.64)	927.40	(864.63)	285	1160	3.75	**0.05**
	0-4	-1846.41	(1301.47)	1246.96	(968.38)	312	1204	4.53	**0.03**
	0-5	-1531.68	(1425.02)	1489.79	(1090.13)	338	1243	3.51	**0.06**
	0-6	-1055.50	(1629.42)	1524.08	(1136.34)	356	1278	2.07	0.15
	0-7	-1141.47	(1612.88)	1722.32	(1259.62)	369	1304	2.40	0.12
	0-8	-1120.50	(1618.30)	1971.44	(1365.91)	378	1328	2.60	0.11
	All	-1014.74	(1597.01)	2910.79	(1971.78)	386	1428	2.85	**0.09**
Dummy	0	-5938.16	(4501.35)	-1405.78	(2352.76)	386	1428	1.00	0.32
	1	-2256.72	(1774.50)	2618.70	(1854.90)	386	1428	4.39	**0.04**
	2	-739.24	(1945.17)	1486.09	(1819.64)	386	1428	0.85	0.36
	3	530.67	(2946.15)	2672.41**	(1296.18)	386	1428	0.55	0.46
	4	-1681.08	(1581.55)	5373.86***	(1761.73)	386	1428	10.66	**0.00**
	5	2446.32	(3812.26)	4908.06*	(2575.98)	386	1428	0.35	0.55
	6	5326.49	(7449.84)	2987.70	(2302.75)	386	1428	0.11	0.74
	7	-838.04	(2333.42)	9299.08*	(4856.97)	386	1428	4.19	**0.04**
	8	-658.70	(1791.73)	10179.01**	(4064.66)	386	1428	7.01	**0.01**

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets. Fixed effects for Firm and Year have been included in the regression

2.A.8 Placebo Test in the Pre-Treatment Period

The results of the placebo test in the pre-treatment period for the variable Net Income are presented in Table A.8a. And the results for *short-lived cartels* are presented in Table A.8b.

Table 2.A.8: *Diff-in-diff coefficient (β_3).*

Table 2.A.8.A *Net Income. Diff-in-diff coefficient (β_3).*

Sample	**Placebo Pre-Treatment Period**			
	OLS	**FE**	**RE**	**AR(1)**
All Cartels	**-370.77**	**-370.77**	**-370.77**	**-507.12**
	(428.81)	(366.91)	(428.81)	-785.85
N	750	750	750	750
R^2	0.691	0.025	0.691	0.69
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Long-lived Cartels	**-325.09**	**-325.09**	**-325.09**	**-312.78**
	(408.51)	(351.30)	(408.51)	(941.31)
N	127	127	127	127
R^2	0.871	0.026	0.871	0.871
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

Table 2.A.8.B *Short-lived cartels. Diff-in-diff coefficient (β_3).*

Sample	**OLS**	**FE**	**RE**	**AR(1)**
Log of Op. Revenues	**0.04**	**0.04**	**0.04**	**0.04**
	(0.09)	(0.08)	(0.09)	(0.07)
N	603	603	603	603
R^2	0.924	0.171	0.924	0.924
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Net Income	**-332.92**	**-332.92**	**-332.92**	**-527.66**
	(526.13)	(449.60)	(526.13)	(965.66)
N	603	603	603	603
R^2	0.668	0.026	0.668	0.667
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

2.A.9 Placebo Test for Control Group (Net Income)

The results of the placebo test for the control group regarding the variable Net Income are summarized in Table A.9.

Table 2.A.9: *Net Income. Diff-in-diff coefficient (β_3).*

Sample	**OLS**	**FE**	**RE**	**AR(1)**
Overall Effect	**210.63**	**210.63**	**210.63**	**358.28**
	(662.64)	(623.94)	(662.24)	-612.09
N	1727	1727	1727	1727
R^2	0.635	0.055	0.635	0.635
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year
Short-term Effect	**302.68**	**302.68**	**302.68**	**439.82**
	(721.67)	(665.77)	(721.67)	(614.39)
N	1320	1320	1320	1320
R^2	0.729	0.052	0.729	0.729
Fixed Effects	Firm & Year	Year	Firm & Year	Firm & Year

Note: *p<0.10, **p<0.05, ***p<0.01 significance test. Cluster standard errors by cartel in brackets.

Chapter 3

Cartel destabilization effect of Leniency Programs

(joint with Borrell, J.R.[1], Jiménez, J.L.[2] and Ordóñez-de-Haro, J.M.[3])

Abstract

This paper investigates the effect of the Leniency Program on cartel duration, cartel fines and on the years of investigation. We use a difference-in-difference program evaluation approach to identify and quantify the impact of the policy of interest. Our dataset contains all the cartel cases discovered by the European Commission and the Spanish Competition Authority since 1980 and 1990, respectively, until 2015. Leniency programs were introduced in two different moments of time: in 1996 in the EU (it was modified later on in 2002 and 2006) and in 2008 in Spain. The exogeneity of the date of introduction and the fact that it was implemented in the two geographical areas at different moments of time allow us to identify the effect of interest. Results show a short-run effect of the Leniency Program: the detected cartels have longer duration than the ones in the control group. In the long run, the program decreases cartel duration. On the other hand, no significant effect is found on fines when we control for year fixed effects, while the duration of the investigation decreases significantly around 0.8-1.3 years.

Keywords: Antitrust; Leniency Programs.
JEL Codes: D7; K2; L4; O4.

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3.1 Introduction

Leniency programs have, in principle, destabilization and deterrence effects in the fight against cartels (Motta and Polo, 2003; and Spagnolo, 2004), although some have raised concerns that leniency programs may facilitate collusion if final expected fines turn smaller by applying into the program and obtaining amnesty (Spagnolo, 2004). The later effect will also be magnified if competition authorities reduce resources devoted to discovering cartels by their own initiative or substitute away from other potential more effective policies such as positive rewards to firms and individual informants (Aubert, Rey and Kovacic, 2006; and Harrington, 2008).

After the introduction of leniency programs, there is a surge in the number of discovered cartels in most jurisdictions and then the rate of cartel discovered gradually levels down.

There are not, however, many contributions that try to identify and quantify how leniency programs affect the amount of final fines and the duration and stability of cartels. There are even some concerns (Chen and Harrington, 2007) that leniency facilitates the discovery of the less stable cartels while it facilitates the coordination of the more stable cartels. Some suggest that collusive prices turn to be higher, conditional on a cartel forming, when there is a leniency program (Apesteguia et al., 2007; Hinloopen and Soetevent, 2008; Hamaguchia et al., 2009; Dijkstra et al., 2011; and Bigoni et al., 2012).

The results of the empirical literature are very inconclusive. Brenner (2009) examines the effectiveness and efficiency on the EU leniency program adopted in 1996. The critical issue is the potential sample selection bias from only observing detected cartels. To solve this, the author, following the existing literature, checks conditions on short and long-term changes of number and duration of cartels and also examines whether cartels differ in observable dimensions before and after the introduction of the leniency program.

Brener (2009) shows that the level of fine per firm is larger in the cases under leniency program, and that the duration of the investigation decreases by 1.5 years (for the 61 cases of the sample between 1990 and 2003). However, those effects are not properly identified using program evaluation techniques. And, the paper was not able to show how leniency programs affect cartel stability.

Klein (2010) uses the intensity of competition at the industry level of OECD countries. Estimating an instrumental variable approach, the results reveal a positive effect on industries' competition intensity of leniency programs indicating effectiveness in cartel destabilization and effective deterrence. And Zhou (2015) uses a dynamic model of cartel formation and dissolution

to illustrate how changes in antitrust policies and market and macroeconomic conditions might affect cartel duration.

Zhou (2015) shows that apparently cartels discovered just after the introduction of the leniency program have even larger durations than cartels discovered before the introduction of leniency (short-run stability effect), but that gradually cartel durations of the discovered cartels turn to be shorter than before the introduction of the leniency program (long-run destabilization effect). The latter conclusion is also found in De (2010).

These results are consistent with the concerns raised by Chen and Harrington (2007) that leniency in the long run facilitates the discovery of the less stable cartels. Note that De (2010) uses cartel cases discovered by the European Commission too, but she has no control group and she is not able to define the short-run properly. On the contrary, Zhou (2015) distinguishes between short-run and long-run impact of the policy on cartels the same way we do it in this paper: considering those cartels born before the implementation of the program and collapsed after (short-run); and those that were formed and broken under the existence of the Leniency Program. He uses data on cartel cases detected by the European Commission and the Division of Justice (US). However, he is only able to study the impact of the Leniency Program implemented in 1996 and modified in 2002, but he does not include the modification of 2006. In addition, he is not able to study the long run effect of the policy on the cartel cases born and dead after 2002 due to a lack, and he uses the American cases as a proxy of the European cases.

In this paper, we use a difference-in-differences program evaluation approach to identify and quantify the impact of leniency programs on cartel duration, cartels' final fines, and duration of the investigation. We analyze the impact of leniency on a set of cartel cases discovered by the European Commission and the Spanish Competition Authority since 1980 and 1990 respectively. Leniency programs were introduced in two different moments of time: in 1996 in the EU (it was modified later on in 2002 and 2006) and in 2008 in Spain. The key identifying assumption in this paper is that the exact moment at which leniency programs are introduced is largely exogenous as it depends on the political developments at the EU and Spanish level respectively.

These differences in the timing of the policy adoption allow us to separate out the changes in the mean of cartel duration, cartel fines and investigation duration across jurisdiction (EU versus Spain) and across time (before versus after), and once these effects are controlled for, we estimate the impact of the differences-in-differences effect of the introduction of Leniency Programs.

Therefore, the contributions of this paper are several. First of all, we study the Leniency Program implemented in EU in 1996, together with its modifications in 2002 and 2006; and the one implemented in Spain in 2008. We exploit the geographic and time differences of this exoge-

nous policy. Secondly, we allow for heterogeneous effects of the program in terms of short run and long run impact, distinguishing between the observations partially treated by the program (unexpected change in competition policy) and those fully treated (cartels born under the existence of the program). Finally, we carefully define our control group and our treatment group of interest with respect to those cases that applied for the Leniency Program, those that benefited from it by cooperating with the authority, and those that were affected by the policy even if they don't fall in any of the previous categories. In addition, we use a program evaluation technique (difference-in-differences estimator) since we always work with a treatment and a control group, where the latter is not only composed by the old cartel cases in that geographic area but also includes those cases of the second region considered.

We show that leniency program has a clear cut and sharp effect on cartel stability: cartel duration increases on the short run and decreases in the long run. We are able to separate out the impact of leniency program on the cartels born before leniency and died after leniency (short-run effect), and also the impact on the cartels born and died after leniency (long-run effect). According to the theory (Harrington and Chang, 2009), the positive short run impact on cartel duration and the negative impact in the long run show that the policy was effective in terms of more aggressive detection and conviction, and results into fewer cartels forming due to the program. Leniency has a clear and sharp destabilization short-run and long run effect, and also a sharp and clear deterrence effect.

We are not able, however, to tackle the pending question of whether leniency promotes the stability of hard-core cartels which both before and after leniency remain undiscovered.

With respect to the effect of the Leniency Program on cartels fines, we are not able to find a significant impact, and we explain the possible reasons that could lead to this result. We are able to find, on the contrary, a significant and negative impact of the program on the duration of the investigation, which is decreased by around 0.8-1.3 years.

The paper organizes as follows. After this introduction, section 2 details the review of the literature; section 3 shows the data collected for this paper on all cartels discovered since 1980 in the EU, and since 1989 in Spain. It also details the methods of the diff-in-diff program evaluation techniques used in the paper, and defines the groups of control cartels and the leniency treated cartels. Section 4 shows the results of the program evaluation exercise and offers the magnitudes of the impact of leniency programs on cartel duration, the amount of fines and investigation duration. Finally, section 5 offers some concluding remarks, some policy implication, and some discussion of the pending questions for further research.

3.2 Literature Review

The academic literature on leniency programs has been developed in three main fields of research: theoretical, experimental and empirical. The former includes the seminal papers by Motta and Polo (2003) or Spagnolo (2004), who support the advantages of these destabilizing instruments for cartels. Subsequent papers have focused on: the effect of leniency programs on the behavior of firms (Aubert, Rey and Kovacic, 2006), who propose the use of these programs complemented with bounties to individuals; to allow for the probability of discovery and successful prosecution to change over time (Harrington, 2008); and many other improvements to the general model by Fees and Walzl (2004), Motchenkova (2004), Chen and Harringon (2007), Hinloopen and Soeteven (2008), Harrington and Chang (2009) and Sauvagnat (2012).

All these papers yield to the same general conclusion: leniency programs hinder collusion. Experimental studies also find that leniency programs reduce cartel formation (Apesteguia et al., 2007; Hinloopen and Soetevent, 2008; Hamaguchia et al., 2009; Dijkstra et al., 2011; and Bigoni et al., 2012).

But our main related literature is that which studies empirically the effects of leniency programs. Miller (2009) and Brenner (2009) are the two seminal papers in this specific topic. The former uses 207 cartels discovered in USA between 1985-2005 to evaluate whether leniency entrance enhances detection and deterrence capabilities. His empirical strategy is based on a Poisson regression model in order to estimate cartel discoveries as function of some control variables as GDP, budget of Antitrust Authority (DOJ), fines, time and leniency program. He concludes that the number of cartels discoveries peaks after the introduction of leniency and it then falls to pre-entrance period.

Brenner (2009) studies how leniency programs affect (or not) three variables of interest: fines, duration of investigation and the duration of detected cartels. Using data from 61 European Union cases in the period 1990-2003, the author shows that the First EU Program Leniency allowed to obtain higher fines and to reduce by 1.5 years the duration of the investigation. However, the hazard model does not accept the hypothesis of deterrence effect of the program.

De (2010) focuses on cartel duration on 110 EC cartels convicted in the period 1990-2008. She employs a Cox-proportional hazard regression in order to test what factors affect cartel breakdown. Her results expose that the structure of the cartel and the external disturbances play an important role in cartels break up. However, she finds no significant result for the cartels detected under the leniency program. Zhou (2015) study the impact of the Leniency Program (1996 and 2002) on cartel duration using cartel cases discovered by the EC and the DoJ. He analyses separately the short run and long run effects of the policy, but he does not use a diff-

in-diff approach since he compares the EU treated cartels with the EU control group, and uses the DoJ data as a proxy for the effect that the Leniency Program EU 2002 would have had on the long run, due to data limitations.

In fact, Miller (2009) exposes some caveats about how cross-sectional variation could provide more robust identification, using data from introduction of leniency programs across the world.

As commented above, we study the Leniency Program, exploiting the geographic and time differences of this exogenous policy. We study the short run and long run impact of the policy using a diff-in-diff approach in which we compare the cartel cases partially treated by the program and those fully treated to the control group, respectively. We find a positive and significant impact on cartel duration in the short run, and negative in the long run, which goes in line with the previous literature (Harrington and Chang, 2009; Zhou, 2015). We do also find a negative and significant impact on the duration of the investigation, as in Brenner (2009). We cannot find, however, any significant impact on cartel fines that due to the program itself.

3.3 Data and Methods

We have collected the detailed information of all cartel decisions taken by the European Commission between 1980 and 2015, and by the Spanish Competition Authority between 1995 and 2015. In total there have been 196 cartel decisions (129 cases in EU and 67 cases in Spain), only 182 if we exclude 14 decisions involving only business associations but not actual firms (7 EU cases, and other 7 Spanish cases). There have been 89 decisions with leniency fine reductions since the introduction of the leniency program in the EU in 1996 (60 cases with leniency), and in Spain in 2008 (15 leniency cases).

Table 1 shows the basic descriptive statistics of the data collected by jurisdiction (EU/ Spain), and also by the no leniency/leniency split. The figures of the cases under the Leniency Program consider all cartels that benefited from it, had they applied for a lenient treatment or not. A description of the variables can be found in the appendix, and more in detail in Ordóñez-de-Haro, Borrell and Jiménez (2018).

Table 3.1: *Average Statistics by Leniency Program and Geographic Area.*

Variables	EU (1980-2015)				Spain (1989-2015)			
	No Leniency		Leniency Programs 1996, 2002 & 2006		No Leniency		Leniency Program 2008	
Number of cases (all)	40		89		49		18	
Number of cases (associations only excluded)	33		89		42		18	
Basic amount of fines	117.3	(291.9)	322.3	(352.5)**	22.9	(35.1)	25.5	(27.2)
Fines before leniency	14.8	(17.8)	339.2	(410.6)	20.0	(29.6)	23.2	(23.0)
Final fine	71.9	(201.6)	246.7	(309.1)***	17.9	(28.1)	17.9	(22.0)
Average percentage reduction by leniency	0.0	(0.0)	0.3	(0.2)***	0.0	(0.0)	0.5	(0.3)***
Final fine per firm	15.3	(64.5)	23.4	(34.6)	2.1	(3.4)	1.5	(1.7)
Final fine per consolidated firm	21.3	(96.9)	40.5	(53.8)	2.4	(3.9)	2.6	(3.4)
Max cartel duration (years)	8.0	(6.6)	7.8	(6.2)	6.8	(6.8)	11.0	(9.2)*
Duration of investigation	3.9	(1.9)	4.3	(1.6)	2.7	(1.0)	2.3	(0.4)
Average number of firms	11.1	(11.0)	12.4	(9.8)	14.2	(13.5)	11.1	(7.5)
Average number of consolidated firms	10.2	(10.1)	6.6	(3.7)***	12.7	(11.9)	7.6	(5.1)*
Average number of countries	5.0	(4.4)	5.1	(2.7)	1.2	(0.6)	2.5	(1.8)***
Average number of countries (parent firms)	5.1	(4.5)	4.1	(2.1)	1.2	(0.6)	2.4	(1.7)***
Begins by Leniency	0.0	(0.0)	0.7	(0.5)***	0.0	(0.0)	0.8	(0.4)***
Begins Commission Initiative	0.7	(0.5)	0.2	(0.4)***	0.5	(0.5)	0.2	(0.4)**
Begins Notification	0.1	(0.2)	0.0	(0.0)**	0.0	(0.0)	0.0	(0.0)
Cartel Stability	0.7	(0.5)	0.2	(0.4)***	0.4	(0.5)	0.1	(0.3)**

Note 1: Numbers expressed in million €deflated on the basis of year 2010 (WB Prices database). Standard deviation within brackets. Cases with sanctions only to firm associations (not individual firm sanctions) excluded: 7 cases out of 129 excluded in the EU, and 7 cases out of 67 excluded in Spain.
Note 2: *, ** and *** indicates that mean t-tests between categories shows statistical significance at 10%, 5%, 1% respectively.
Source: Authors' elaboration from the publicly available case files.

Table 1 shows that the average reduction in fines[4] is 30% in the EC cases, while it is as large as 50% in the Spanish cases. The number of cartels cases that begin with a leniency application is as large as 70% since the introduction of the leniency program in the EU, while it is as large as 80% since the introduction of the Spanish leniency program.

Table 1 also shows that there are some significant mean differences across the no leniency/leniency split in each jurisdiction. In the EC leniency cases, basic amount of fines and final fines are much larger. However, there is not such significant difference if we look at the final fines per firm. Additionally, there has been a significant reduction in the number of consolidated firms.

In the Spanish leniency cases, there are not significant differences in the amount of fines. There has been as in the EU, a significant reduction in the number of consolidated firm fined, but a significant increase in the number of different countries in which cartel participants are registered.

These changes might stem from changes in the type of cartels discovered and fined since the leniency program is approved and used, but the leniency program may not have caused those changes. We need an identification strategy that allows us to separate and quantify the causal effect of the leniency program.

Additionally, the EU leniency program has suffered a couple of reforms in 2002 and 2006

[4]Monetary values are deflated on the basis of the year 2010 (World Bank prices database).

since it was created in 1996. We can then look at the mean differences in cartel profiles using the split of the different EC leniency programs.

Table 2 shows that there has been since 2002 a significant increase in the amount of fines reaching 52,5 million euros per consolidated firm. The number of firms and the number of countries also increase since 2002. And the number of cases that begin with leniency applications is always increasing reaching a 90% in 2006.

Table 3.2: *Average statistics by EU Leniency Programs.*

Variables	**No Leniency**		**Leniency Program 1996**		**Leniency Program 2002**		**Leniency Program 2006**	
Number of cases (all)	40		39		29		21	
Number of cases (associations only excluded)	33		39		29		21	
Basic amount of fines	117.3	-291.9	242.2	-287.2	440.3	(428.1)**	341.8	-318
Fines before leniency	14.8	-17.8	249	-351.9	503.4	(492.8)**	245.5	-197.3
Final fine	71.9	-201.6	157.4	(190.1)*	344.2	(350.4)***	277.7	-386.6
Average percentage reduction by leniency	0	0	0.3	(0.2)***	0.3	-0.1	0.4	(0.2)***
Final fine per firm	15.3	-64.5	20	-25.5	21.3	-21.6	32.7	-57
Final fine per consolidated firm	21.3	-96.9	25.2	-27.2	52.5	(63.8)**	52.2	-69.5
Max cartel duration (years)	8	-6.6	8.8	-6	8.2	-7.2	5.5	-4.7
Duration of investigation	3.9	-1.9	4.3	-1.7	4.7	-1.5	3.7	(1.1)**
Average number of firms	11.1	-11	9.1	-6	17	(12.2)***	12.4	-9.6
Average number of consolidated firms	10.2	-10.1	6.6	(3.6)**	7.2	-3.9	5.7	-3.9
Average number of countries	5	-4.4	4.2	-2.5	6.4	(2.5)***	5	(2.9)*
Average number of countries (parent firms)	5.1	-4.5	3.8	-2	4.8	(2.3)*	3.6	(1.6)**
Begins by Leniency	0	0	0.5	(0.5)***	0.7	(0.5)*	0.9	-0.3
Begins Commission Initiative	0.7	-0.5	0.3	(0.5)***	0.2	-0.4	0.1	-0.3
Begins Notification	0.1	-0.2	0	0	0	0	0	0
Cartel Stability	0.7	-0.5	0.3	(0.4)***	0.2	-0.4	0.3	-0.5

Note 1: Numbers expressed in million €deflated on the basis of year 2010 (WB Prices database). Standard deviation within brackets. Cases with sanctions only to firm associations (not individual firm sanctions) excluded: 7 cases out of 129 excluded in the EU, and 7 cases out of 67 excluded in Spain.
Note 2: *, ** and *** indicates that mean t-tests between categories shows statistical significance at 10%, 5%, 1% respectively.
Note 3: Mean t-tests compare leniency 1996 cartels with no leniency cartels, leniency 2002 cartels with respect leniency 1996 cartels, and leniency 2006 cartels with respect leniency 2002 cartels.
Source: Authors' elaboration from the publicly available case files.

3.3.1 The Identification of the Leniency Program Effect

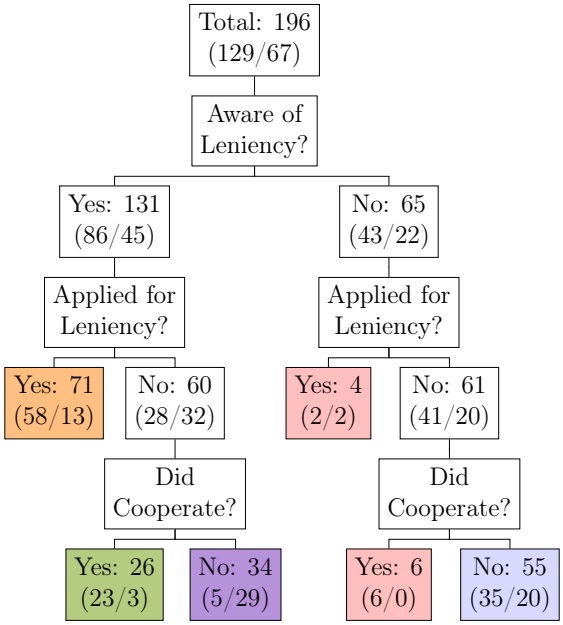
We are interested in studying the effect of the Leniency Program on cartel duration, fines and years of investigation. For this purpose, we are going to compare the cartel cases affected by this regulation with the cartel cases not affected by the Leniency Program. Our main identification source comes from the fact that the date of implementation of the program is exogenous, and that it has been implemented in the different geographic areas at distinct periods of time. We use a difference-in-difference approach in which we compare the cartel cases in the treatment group to those in the control group, both groups being composed of European and Spanish cartel cases.

However, we need to be more specific about our sample of interest given that it is not as simple as having a treated and a control group. There are three particular issues. First of all, cartel members can apply for Leniency Program, but they could also benefit from a lenient treatment if they cooperate with the authority even if the discovery is not due to the Leniency Program.

Secondly, the previous situation can take place even if the legislation was passed after the death of the cartel. Finally, we need to make an additional distinction apart from being treated by the program or not: some cartels were alive before the implementation of the Leniency Program and died after (partial treatment) and some others were born and broken under the existence of the program (full treatment).

The figure below summarizes all the possible cases[5]. Note that by *aware of leniency* we mean whether the cartel died before the Leniency Program was implemented (not aware) or died after (aware).

Figure 3.1: *Sample split EU/Spain. Definition of treated and control group.*



Source: Authors elaboration from the publicly available case files.

Those cartel cases that were not aware of leniency, did not apply for leniency and did not cooperate are our control group (in light blue). These cartels were born and died before 1996 for the European cases, and before 2008 for the Spanish cases. The control group is not affected by the treatment in any sense: they did not know abut the existence of the Leniency Program while they were alive, and they did not benefit from it afterwards either.

Our treatment group will be different, depending on the outcome of interest. When studying the effect of the Leniency Program on cartel duration, our first treatment group will be those cartel cases that were discovered due to application to the program (highlighted in green). The second treatment group will consider all the cases aware of the leniency program (those marked

[5]The division of European and Spanish cases is specified in brackets. The first figure corresponds to EU and the second one to Spain.

in orange, green and purple).

The reason for this distinction is the following: when studying cartel duration we are firstly interested in those cases that broke up explicitly because of the program, meaning, those that applied for leniency. This will give us the comparison between those cartel cases that made direct use of the program and the control group (highlighted in blue). Additionally, we are also interested in studying the effect of the existence of the program on the duration of all cartel cases discovered. Even if the members of the cartel did not apply for a lenient treatment, the implementation of the program could have some deterrence effect (maybe new cartels formed are shorter), and in that case the treatment group of interest are all the cases affected by the existence of the program.

When analyzing the effect of the policy on fines and the duration of the investigation, our treatment group is composed by those cases that were aware of leniency and either applied for leniency or cooperated with the authority (highlighted in orange and green).

On the other hand, those cartel cases that died before the program was implemented but either applied for leniency or cooperated with the authority are excluded from our sample (they should be in the control group but benefited from a lenient treatment).

As mentioned above, there is a second distinction we make in our treatment group. These cartel cases died after the Leniency Program was implemented either in Europe or in Spain. The classification comes from the date of formation: if the cartel was born before the implementation date (and died after), we consider this observation had a *partial* treatment. On the other hand, if the cartel both was formed and died after the program had entered then it had *full* treatment.

Table 3.3: *Classification of Partial and Full Treatment.*

	Control		**Partial Treatment**		**Full Treatment**	
	Born	**Died**	**Born**	**Died**	**Born**	**Died**
	(Before)	(Before)	(Before)	(In/After)	(In/After)	(In/After)
Leniency EU 1996	1996	1996	1996	1996	1996	1996
Leniency EU 2002	1996	1996	2002	2002	2002	2002
Leniency EU 2006	1996	1996	2006	2006	2006	2006
Leniency Spain 2008	2008	2008	2008	2008	2008	2008

It should be clarified that these treatment variables corresponding to EU overlap. The reason is that since Leniency EU 1996 considers all those cartel cases born and dead after 1996 (full treatment), it also includes those cases affected by the programs implemented afterwards. With respect to partial treatment we could also have a similar case: for instance, two different cartel cases may have been formed in 1994 but one could have died in 1999 and the other one in 2004. Thus, the effects obtained refer to the total effect of the Leniency Program form that moment

onwards, and not to the effect of the program implemented in a given year.

3.3.2 Methodology

Our first effect of interest is the impact of the Leniency Program on cartel duration. We compare the duration of the cartels in the treatment group against those in the control group. A limitation of working with cartel cases is that we can only work with discovered cartels, and results may not be inferred to the whole population. Harrington and Wei (2017) give the conditions under which the duration of detected cartel would be an unbiased measure of the duration of the cartel population.

Moreover, Harrington and Chang (2009) develop a model of cartel creation and dissolution that allows inferring the impact of the competition policy on the population of cartels by measuring the impact on the duration of discovered cartels. According to their model, if the probability of discovering and convicting cartel members increases due to a change in the policy, then the least stable cartels collapse immediately. Thus, the surviving cartels have longer durations this turns into a rise in average duration of discovered cartels in the short run. In the long run, average duration of observed cartels could go up or down, since less stable cartels do not form in first place (rise in duration) but the formerly stable cartels break up earlier (decrease in duration).

We distinguish between the short run and long run impact of the Leniency Program on cartel duration in the sense of those cartels that were formed before the implementation of the policy and died after (partial treatment or short run effect) versus those cases that were formed under the existence of the Leniency Program (full treatment or long run effect).

For our purpose, we estimate the Cox proportional hazard model for survival analysis[6]. The purpose of the model is to examine how specified factors influence the rate of a particular event happening. In this case, the event is cartel death. The Cox proportional hazard model assumes that the effects of the predictor variables upon survival are constant over time and are additive. If the coefficient is positive, or equivalently the hazard ratio is greater than one (exponential of the coefficient), it indicates that as the value of the covariate increases, the event hazard increases and thus the length of survival decreases. In other words, a hazard ratio above one indicates that it is positively associated with the event probability, and thus, negatively associated with the length of survival.

[6]Brenner (2009), De (2010) and Zhou (2015) also use this methodology, with the main difference that we study the cases of EU and Spain, which allows for a diff-in-diff approach (the comparison group is not only the previous cases of the corresponding geographic area but also the ones in the other region, and the treated group receives the treatment in different periods of time).

The regression estimated is the following one:

$$h_i(t) = h_0(t)exp(\beta_1 treated_i + \beta_2 spain_i + \beta_3 lncountry_i + \beta_4 lnfirm_i + \beta_5 stability_i + \varepsilon_t + \eta_s + u_i) \quad (3.1)$$

where: $treatment_i$ is a binary variable that either denotes partial treatment or full treatment and takes value 1 in the cartel case was affected by the corresponding Leniency Program, depending on the regression; $spain_i$ is a binary variable that takes value 1 for Spanish cases; $lncountry_i$ is the logarithm of the number of countries to which belong the firms of the cartel case; $lnfirm_i$ is the logarithm of the number of firms involved in the cartel case; $stability_i$ is a binary variable that takes value 1 if all firms entered and exited the cartel at the same time; ε denotes time fixed effects; η denotes industry fixed effects.

The time fixed effects correspond to two dummy variables: one of them takes value 1 if the year of the decision is between 1996 and 2007 (both inclusive) and the other one takes value 1 if the year of the decision is after 2007. These variables control for any possible effects that happened in those periods either in Spain or in the European Union. A dummy variable for each year cannot be used because the number of observations is not big enough. The industries fixed effects are a dummy variable for each sector of the NACE Rev.2 classification.

We estimate two different specifications of this model. In the first one, shown in the results section, the *treatment* variable takes value one if the cartel case was either partially or fully affected by any of the corresponding Leniency Programs. This will give us the average overall effect of the Leniency Program in EU and Spain. Thus, when it refers to partial treatments, it takes value one for all the cases that were born before the implementation of any of the programs (EU96, EU02, EU06 or SP08) and that died after. Similarly, when we study the full treatment the variable takes value one for all the cases that were formed and died after having passed the each policy.

The second specification studies each of the European Leniency Programs in a separate regression, but the Spanish Leniency Program is included in all regressions. For instance, the variable *partial treatment* for the EU96 & SP08 programs will take value one for those European cases formed before 1996 and died after that year, and those Spanish cases formed before 2008 and broken later on. Note that, as explained above, there exist some cases that can be classified either as partial or full.

Additionally, we are interested in studying the effect of the Leniency Program on the basic and final fines imposed to the discovered cartels, and on the duration of the investigation. Following Brenner (2009), if more information is disclosed due to the cooperation with the authority, then the basic fine (fine prior to the application of the leniency scheme) should be larger

than the basic fines imposed before the existence of the Leniency Program. The effect on the final fines of the treated group could be either positive or negative, depending on the reduction. However, the author finds that the fine reductions do not fully compensate for the increase of basic fines. With respect to the duration of the investigation, it should decrease given that the costs of obtaining relevant information are lower. We should also consider that the information disclosed could make the analysis of the case more complex.

The regression estimated, by OLS, is the following one:

$$y_i = \beta_0 + \beta_1 treatment_i + \beta_2 spain_i + \beta_3 lncountry_i + \beta_4 lnfirm_i + \beta_5 duration_i + \varepsilon_t + \eta_s + u_i) \tag{3.2}$$

where: y is the logarithm of the outcome of interest (basic fine, final fine or years of investigation); $lduration_i$ is the logarithm of the maximum duration of the cartel; and the rest of the variables are defined as above. Also in this case, two different specifications of each regression are considered.

An important methodological issue arises at this point. In order to study cartel fines, we have to control for cartel duration, since it is relevant to determine the fine imposed by the authority. However, our first regression of interest measures whether the Leniency Program had any impact on cartel duration. Therefore, the variables duration and treatment will be collinear in this model. More importantly, the Leniency Program could have an impact on fines either directly, or indirectly through cartel duration, or both. For now, we work with the simplest model specification (standard OLS), assuming that the LP has a direct effect on fines and controlling for cartel duration (the coefficient of this variable may be biased), and will leave this problem for future versions of this paper. This possible distinction between the direct and indirect effect of the program on cartel fines has not been considered in the literature as far as we know.

3.4 Results

In this section we present the results of the Cox regression for cartel duration, and the results for basic fines, final fines and years of investigation. In particular, we show the specification in which all Leniency Programs are summarized in one variable. Results for each program separately can be found in the appendix.

Table 4 presents the results (expressed as hazard ratio) for the Cox regression estimation. Results show that those cartel cases that were partially treated by the Leniency Program and applied for the program, experiment a 69% decrease in the hazard of failure (model (2)). This means that the duration of these cartel cases is higher than those in the control group. When all cartels partially treated are considered, meaning all the cartel cases affected by the existence of

the program, had they applied for the program or not, the decrease in the probability of dying is 66% (model (6)). The cases considered in these two samples are those that the implementation of the policy took them by surprise. Thus, results show a short-run effect (partial treatment) of Leniency Program: the detected cartels have longer duration than the ones in the control group (hazard ratio lower than one). This result is consistent with the one of Harrington and Chang (2009) and Zhou (2015). These authors conclude that the average duration of discovered cartels rises in the short run in response to a more effective competition policy. The reason is that if the policy is efficacious, then its adoption will immediately cause the marginally stable cartels to collapse and they will exit the cartel population, which means that they cannot be discovered.

Table 3.4: *Cartel Duration (Cox regression) Hazard Ratio. All Leniency Programs (EU96, EU02, EU06 & SP08*

Variables	**Partial Treatment**		**Full Treatment**		**Partial Treatment**		**Full Treatment**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Treatment	0.87	**0.31*** **	**3.20*** **	10.07	0.87	**0.34** **	**4.03*** **	**10.58*** **
	(0.16)	(0.14)	(0.96)	(21.09)	(0.13)	(0.18)	(1.01)	(8.76)
Log N. Countries	0.68***	0.66***	0.87	0.82	0.75***	0.72***	0.97	0.98
	(0.09)	(0.08)	(0.08)	(0.15)	(0.08)	(0.08)	(0.08)	(0.16)
Log N. Firms	1.32**	1.17	1.08	1.01	1.22*	1.17	1.07	0.92
	(0.16)	(0.29)	(0.13)	(0.20)	(0.14)	(0.22)	(0.19)	(0.17)
Stability	1.74***	1.42	2.52***	2.33	2.05***	1.80***	2.15***	2.12*
	(0.31)	(0.34)	(0.83)	(1.32)	(0.33)	(0.35)	(0.58)	(0.86)
After fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Spain fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	106	106	77	77	143	143	100	100
Pseudo R^2	0.017	0.046	0.037	0.044	0.018	0.035	0.045	0.083
Treatment	Applies for LP	Applies for LP	Applies for LP	Applies for LP	All	All	All	All

Note: * p<0.10, ** p<0.05, ***p<0.01. Standard errors in parentheses.

The coefficient of interest (the one of the treatment variable) is not significant for the sample of cartel cases that apply for Leniency Program when we include year and sector fixed effects. However, the result is significant when we consider all the cartel cases born under the Leniency Program. The hazard ratio can be interpreted as follows: the probability of dying of those cartels born and dead under the Leniency Program is around ten times higher than the one of the cartels in the control group. Therefore, the duration of the treated cases is lower than the duration of the cartels born and dead before the implementation of the program. Harrington and Chang (2009) find that the effect of the Leniency Program on cartel duration in the long run is ambiguous, it could go either up or down. On the one hand, those cartels at the margin that are less stable will not form under this policy, which entails a rise in the observed durations. On the other hand, the formerly stable long-running cartels break up earlier, reducing observed cartels durations. Our results are consistent with the second explanation: the long run effect of the Leniency Program is a decrease in cartels duration.

A pending question would be whether the Leniency Program brings shorter or less stable

cartels to light or whether it does really deter collusion by means of the formation of shorter cartels or the formation of fewer cartels. Harrington and Chang (2009) claim that in response to a policy that alters the likelihood of detection and conviction, the effect of the rate of cartels can be inferred by observing the duration of discovered cartels in the short run. If average cartel duration goes up, then the policy has caused the probability that firms are discovered and convicted to rise and thus we can conclude that it will result in fewer cartels forming in the new steady state. Our results prove this last point.

On the other hand, we find no significant effect of the Leniency Program on basic fines nor on final fines when we control for time fixed effects. These results should be interpreted carefully, since we have to either improve our estimation specification or explain in detail why this specification would be the right one despite the issues commented above.

For now, two reasons could explain these provisional results. Firstly, given that the descriptive statistics show that fines are high for those cartel cases that benefited from the Leniency Program but the treatment variable of interest is not significant, it could be that the authority adopted a stronger position in the fight against collusion during the whole period, and not necessarily only through the program. An alternative explanation could be that there is no direct effect of the Leniency Program on fines, but it has an indirect through cartel duration given that the coefficient of the variable duration is positive and significant, and we have shown previously the effect on the policy on cartel duration.

Table 3.5: *Log of Basic Fines (Deflated). All Leniency Programs (EU96, EU02, EU06 & SP08).*

Variables	**Partial Treatment**		**Full Treatment**		**Both Treatments**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	**2.09*** **	0.68	**2.81*** **	1.32	**2.01*** **	0.79
	(0.56)	(0.83)	(0.43)	(0.85)	(0.59)	(0.96)
Log N. Countries	0.13	0.58**	0.11	0.38	0.18	0.51***
	(0.22)	(0.15)	(0.50)	(0.62)	(0.23)	(0.14)
Log N. Firms	0.37	0.33*	0.18	0.04	0.29	0.21
	(0.23)	(0.18)	(0.23)	(0.23)	(0.20)	(0.17)
Log Duration	0.52**	0.39***	0.83***	0.62***	0.57***	0.47***
	(0.23)	(0.10)	(0.17)	(0.17)	(0.17)	(0.09)
After fixed effects	No	Yes	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes	No	Yes
Spain fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Obs	99	99	55	55	109	109
R^2	0.632	0.708	0.649	0.718	0.620	0.684
Treatment	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates

Note: * p<0.10, ** p<0.05, ***p<0.01. Standard errors in parentheses.

Table 3.6: *Log of Final Fines (Deflated). All Leniency Programs (EU96, EU02, EU06 & SP08).*

Variables	**Partial Treatment**		**Full Treatment**		**Both Treatments**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	**2.12*** **	0.32	**2.79*** **	1.54	**2.11*** **	0.49
	(0.27)	(0.84)	(0.29)	(0.95)	(0.27)	(0.93)
Log N. Countries	0.43**	0.62***	0.57	0.72**	0.48**	0.66***
	(0.18)	(0.16)	(0.32)	(0.22)	(0.17)	(0.13)
Log N. Firms	0.44**	0.40***	0.22	0.24*	0.40**	0.36**
	(0.17)	(0.12)	(0.17)	(0.12)	(0.15)	(0.12)
Log Duration	0.32	0.20*	0.57***	0.43***	0.33*	0.28**
	(0.23)	(0.10)	(0.17)	(0.09)	(0.18)	(0.10)
After fixed effects	No	Yes	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes	No	Yes
Spain fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Obs	127	127	86	86	143	143
R^2	0.551	0.648	0.579	0.665	0.543	0.627
Treatment	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates

Note: * p<0.10, ** p<0.05, ***p<0.01. Standard errors in parentheses.

Finally, we find a significant and negative effect of the treatment on the duration of the investigation. The Leniency Program decreases the duration of investigation (measured in years), of those cartel cases that apply for Leniency Program or cooperate with the authority, around 23%-35%. Given that the average years of investigation is 3.66 years for the observations in the control group, this means a reduction about 0.8-1.3 years.

Table 3.7: *Log Years of Investigation. All Leniency Programs (EU96, EU02, EU06 & SP08).*

Variables	**Partial Treatment**		**Full Treatment**		**Both Treatments**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	-0.01	**-0.32****	0.05	**-0.23***	-0.03	**-0.35*****
	(0.07)	(0.14)	(0.09)	(0.11)	(0.07)	(0.11)
Log N. Countries	0.13	0.16*	0.11	0.18***	0.11	0.15*
	(0.08)	(0.08)	(0.08)	(0.04)	(0.08)	(0.08)
Log N. Firms	0.06**	0.04	0.07**	0.05	0.07***	0.05*
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)
Log Duration	-0.03	-0.02	-0.02	-0.02	-0.02	-0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
After fixed effects	No	Yes	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes	No	Yes
Spain fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Obs	127	127	86	86	143	143
R^2	0.189	0.313	0.123	0.278	0.188	0.297
Treatment	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates

Note: * p<0.10, ** p<0.05, ***p<0.01. Standard errors in parentheses.

3.5 Concluding Remarks

This paper aims at identifying and quantifying the effect on the Leniency Program on cartel duration, cartel fines and the duration of the investigation. In particular, we study the effect of the Leniency Program implemented in EU in 1996, and modified later on in 2002 and 2006; and the one implemented in Spain in 2008. The exogeneity of the date of introduction and the fact that it was implemented in the two geographical areas at different moments of time allow us to identify the effect of interest, using a difference-in-difference approach.

Our dataset contains all the cartel cases discovered by the European Commission and the Spanish Competition Authority since 1980 and 1990, respectively, until 2015. There are, in total, 196 cases, out of which 129 belong to the EU and 67 to Spain. We exclude from our analysis those cases that involve only business associations but not actual firms.

In order to study the impact on the program on cartel duration, we use the Cox proportional hazard model. The treatment groups of interest in this case are two: those cartel cases that applied for the Leniency Program on the one hand (the discovery is a direct consequence of the program); and all of the cases broken after the implementation of the program (we distinguish between those that were formed before the existence of the program, and those that were born after). To analyze the effect on fines and the duration of the investigation, we estimate an OLS model where the variable of interest is the diff-in-diff. For this second part, the sample of interest is those cartel cases that benefited from the lenient treatment or fine reduction: either applied for the program or cooperated with the authority.

Our results show a short-run effect of the Leniency Program: the detected cartels that were partially affected have a longer duration than the ones in the control group. In particular, they experiment a 66%-69% decrease in the hazard of failure with respect to the control group. In the long run, the program decreases cartel duration: the probability of dying of those cartels born and dead under the Leniency Program is around ten times higher than the one of the cartels in the control group. These results are consistent with the theoretical ones proposed by Harrington and Chang (2009). They find that in response to a policy that alters the likelihood of detection and conviction, the effect of the rate of cartels can be inferred by observing the duration of discovered cartels in the short run. If average cartel duration goes up, then the policy has caused the probability that firms are discovered and convicted to rise and thus we can conclude that it will result in fewer cartels forming in the new steady state.

On the other hand, no significant effect is found on fines. It could be that the fines are higher during that period because the authority took a stronger position in the fight against cartels but this position it not necessarily taken through the program. Or it may be because the Leniency

Program has an indirect effect on fines through cartel duration, and not a direct one. However, these results should be interpreted carefully for now. Finally, we find that the duration of the investigation decreases significantly around 0.8-1.3 years, as previous studies in the literature.

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Appendix 3.A Description of Variables

From the publicly available case files, we have computed the following information. Monetary values are deflated on the basis of the year 2010 (World Bank prices database):

i *Basic amount of fines*$_i$ (euro): it is the total basic amount of fines of the case i before leniency application. This information is not always available in the publicly available case files, which implies we have a smaller number of observations than number of cartel decisions.

ii *Final fine*$_i$: the sum of fines imposed on all the undertakings involved in the cartel case i. It differs from the basic amount of fine because in the final fine it is taken into account aggravating and/or attenuation circumstances that increase or reduce the final fine with respect to the basic amount of fine. The data is offered before and after leniency.

iii *Average percentage of leniency reduction*$_i$: average of the percentage reductions granted to leniency applicants per case in the final fine.

iv *Final fine per firm*$_i$: the ratio between the final official fine and the total number of firms participating in the cartel i.

v *Final fine per consolidated firm*$_i$: the ratio between the final official fine and the total number of firms participating in the cartel i. All the subsidiaries and the parent company belonging to the same consolidated group (*holdings*) are counted only once

vi *Maximum duration*$_i$: maximum number of years the cartel i was functioning according to the final decision.

vii *Duration of the investigation*$_i$: the number of years between the starting date of the Commission's investigation and the date of its final decision in each cartel case.

viii *Average number of firms*$_i$: it is the average number of firms that participate in the cartel during its existence.

ix *Average number of consolidated firms*$_i$: this is the number of cartel participants but all the subsidiaries and the parent company belonging to the same consolidated group (*holdings*) are counted only once.

x *Number of countries*$_i$: this is the number of different countries from which cartel participants belonged to. Each company is assigned to the country where it has its registered head office.

xi *Number of countries (parents)*$_i$: this variable is similar to the previous one but discounting the effect of parent and subsidiaries, where they exist. We account for only one country in which the parent firm has its head office.

xii *Stability*$_i$: binary variable that takes value 1 when there was no entry or exit of cartel's members throughout the life of the cartel.

xiii *Case stems from ...*$_i$: binary variables which take value 1 for each way a case i starts with: a leniency application from one cartelist (post-1996 leniency notice), a notification (in the pre-2004 authorization regime), a Commission's own-initiative investigation (*ex officio*), or a Commission's investigation following a third-party complaint.

Appendix 3.B Additional Results

3.B.1 Cartel Duration by Leniency Program

Table 3.B.1: *Cartel Duration (Cox regression) Hazard Ratio by Leniency Program.*

Variables	Partial Treatment		Full Treatment		Partial Treatment		Full Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LP 1996 (& 2008)	**0.66****	**0.28*****	**3.20*****	10.07	**0.70***	0.44	**4.03*****	**10.58*****
	(0.13)	(0.13)	(0.96)	(21.09)	(0.13)	(0.24)	(1.01)	(8.76)
Obs	86	86	77	77	118	118	100	100
Pseudo R^2	0.031	0.067	0.037	0.044	0.027	0.054	0.045	0.083
LP 2002 (& 2008)	0.89	**0.39****	**4.58*****	9.53	0.83	**0.39****	**6.13*****	**18.78****
	(0.19)	(0.18)	(1.45)	(17.27)	(0.15)	(0.17)	(1.52)	(26.34)
Obs	81	81	61	61	107	107	77	77
Pseudo R^2	0.024	0.060	0.047	0.086	0.027	0.051	0.069	0.108
LP 2006 (& 2008)	1.04	0.47	**12.36*****	11.65	0.95	0.51	**13.84*****	**20.48****
	(0.22)	(0.28)	(7.02)	(17.70)	(0.19)	(0.26)	(6.04)	(28.83)
Obs	73	73	52	52	90	90	65	65
Pseudo R^2	0.015	0.057	0.060	0.108	0.020	0.058	0.093	0.136
After fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Spain fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Applies for LP	Applies for LP	Applies for LP	Applies for LP	All	All	All	All

Note: * p<0.10, ** p<0.05, ***p<0.01. Standard errors in parentheses.

3.B.2 Basic Fines by Leniency Program

Table 3.B.2: *Log of Basic Fines (Deflated) by Leniency Program.*

Variables	Partial Treatment		Full Treatment		Both Treatments	
	(1)	(2)	(3)	(4)	(5)	(6)
LP 1996 (& 2008)	**1.83****	0.76	**2.81*****	1.32	**2.01*****	0.79
	(0.58)	(0.77)	(0.43)	(0.85)	(0.59)	(0.96)
Obs	83	83	55	55	109	109
R^2	0.657	0.736	0.649	0.718	0.620	0.684
LP 2002 (& 2008)	**2.18*****	1.08	**2.75*****	0.47	**2.17*****	1.03
	(0.53)	(0.93)	(0.44)	(0.75)	(0.55)	(1.00)
Obs	68	68	37	37	76	76
R^2	0.667	0.762	0.555	0.697	0.661	0.748
LP 2006 (& 2008)	**1.84****	0.26	1.23	-0.06	**1.73****	0.24
	(0.58)	(0.88)	(0.83)	(0.88)	(0.59)	(0.84)
Obs	50	50	32	32	53	53
R^2	0.527	0.661	0.426	0.604	0.518	0.633
After fixed effects	No	Yes	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes	No	Yes
Spain fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates

Note: * p<0.10, ** p<0.05, ***p<0.01. Standard errors in parentheses.

3.B.3 Final Fines by Leniency Program

Table 3.B.3: *Log of Final Fines (Deflated) by Leniency Program.*

Variables	Partial Treatment		Full Treatment		Both Treatments	
	(1)	(2)	(3)	(4)	(5)	(6)
LP 1996 (& 2008)	**1.79*** **	0.32	**2.79*** **	1.54	**2.11*** **	0.49
	(0.28)	(0.83)	(0.29)	(0.95)	(0.27)	(0.93)
Obs	103	103	86	86	143	143
R^2	0.540	0.630	0.579	0.665	0.543	0.627
LP 2002 (& 2008)	**2.23*** **	0.57	**2.63*** **	1.18	**2.252*** **	0.783
	(0.34)	(0.92)	(0.53)	(0.69)	(0.25)	(0.89)
Obs	91	91	65	65	110	110
R^2	0.596	0.712	0.475	0.607	0.589	0.699
LP 2006 (& 2008)	**2.00*** **	0.06	**2.21***	0.25	**2.02*** **	-0.02
	(0.37)	(0.58)	(1.02)	(0.60)	(0.37)	(0.62)
Obs	76	76	53	53	83	83
R^2	0.476	0.621	0.346	0.518	0.481	0.612
After fixed effects	No	Yes	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes	No	Yes
Spain fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates

Note: * p<0.10, ** p<0.05, ***p<0.01. Standard errors in parentheses.

3.B.4 Duration of Investigation by Leniency Program

Table 3.B.4: *Log Years of Investigation by Leniency Program.*

Variables	Partial Treatment		Full Treatment		Both Treatments	
	(1)	(2)	(3)	(4)	(5)	(6)
LP 1996 (& 2008)	-0.05	**-0.36***	0.05	**-0.23***	-0.03	**-0.35*** **
	(0.08)	(0.18)	(0.09)	(0.11)	(0.07)	(0.11)
Obs	103	103	86	86	143	143
R^2	0.201	0.326	0.123	0.278	0.188	0.188
LP 2002 (& 2008)	0.06	**-0.40*** **	**-0.14***	**-0.50*** **	-0.02	**-0.36*** **
	(0.09)	(0.09)	(0.07)	(0.010)	(0.08)	(0.04)
Obs	91	91	65	65	110	110
R^2	0.243	0.423	0.156	0.339	0.219	0.372
LP 2006 (& 2008)	**-0.13** **	**-0.53*** **	-0.24	**-0.41** **	**-0.15** **	**-0.46*** **
	(0.06)	(0.05)	(0.14)	(0.13)	(0.06)	(0.06)
Obs	76	76	53	53	83	83
R^2	0.197	0.435	0.199	0.371	0.221	0.409
After fixed effects	No	Yes	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes	No	Yes
Spain fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates	Applies for LP or Co-operates

Note: * p<0.10, ** p<0.05, ***p<0.01. Standard errors in parentheses.