

# An Empirical Investigation of the U.S. Corporate Leniency Program\*

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**Very Preliminary - Comments are Welcome**

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## **Abstract**

This project measures the impact on U.S. public companies of the amendment to the U.S. Corporate Leniency Program in 1993. The current literature on this question deals with a population of discovered cartel and leaves aside other firms potentially colluding. This study doesn't suffer from this caveat by analyzing all potentially colluding firms and their reactions to the policy amendment. Results show that on the short run it is impossible to disentangle the effect of the policy on industries' intensity in competition (proxied by markup), while in the long run firms adjust their behavior. This raises questions about the effectiveness of the Corporate Leniency Program.

**JEL classification codes:** K21; K42; L41

**Keywords:** Collusion; Antitrust; Self-reporting

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# 1 Introduction

Measuring empirically the effectiveness of leniency policies on firms' behavior is a major challenge for antitrust agencies. Leniency programs are designed to encourage colluding firms to denunciate one another and eventually break down price fixation or market sharing agreements. The main advantage of a leniency policy compared to the status quo is the smaller cost assigned to finding and successfully prosecuting colluding firms. On paper (theoretically) and demonstrated by the recent increase in the number of successfully prosecuted cases, this policy ought to increase deterrence and desistance. But, does it really?

Antitrust agencies offer a degree of immunity and amnesty of prosecution in exchange of valuable incriminating evidence, where the treatment differs depending on the jurisdiction and on the timing of the leniency applicant. This may be the source of the success of the program as well as ... the source of its failure. Harrington (2008) documents the three main forces at stake fighting each other in dictating firms' binary choice of applying or not for leniency. An application for leniency may be triggered by a cartel member cheating on the cartel. Elaborated schemes elements for punishing deviator includes applying for leniency in delivering on a silver plate the cheating firm to the authority (Deviator Amnesty Effect). An application for leniency may also be prompted by a shift in leniency policies escalating firms' risk of being caught. In this scenario, a firm will rush to the antitrust agency diminishing the extend with which it will be punished (Race to the Courthouse Effect). So far, the two forces (Deviator Amnesty and Race to the Courthouse) reflect the effectiveness of the leniency policy. The third force (Cartel Amnesty Effect) lies in the fact that firms may simply use leniency to reduce the size of the penalty if caught colluding, hence favoring the counter sustainability of collusion. The present study accounts for the dominant force.

This paper broadens the understanding of leniency policies by examining the role of U.S. Department of Justice (DOJ) amendment of the Corporate Leniency Program in 1993 in destabilizing undetected (as well as detected) cartels. Using the data from the Compustat Database, I perform a difference in difference estimation to examine the effect of the Leniency Act on the price markup in numerous industries.

The Corporate Leniency Program ought to affect firms on many levels. Ultimately, the measure of success of such a policy should be accounted for in terms of the ratio of "the number of successfully prosecuted cartel using leniency" over "the number of active cartel", then and only then can a discussion regarding effectiveness of deterrence and desistance be accurate. Obviously, the denominator of this ratio can at best be estimated without any real idea of its value. To overcome this difficulty (also known as the "single observation"

dilemma), researchers<sup>1</sup> have instead narrowed their focus on the numerator by characterizing the conditions proper to firms applying for leniency (conditional on a firm being successfully prosecuted). This avenue has yield interesting conclusion which will be discussed in the next section. Another avenue inspired by experimental economics recreate an environment where subjects are playing a game mimicking firms blowing the whistle on each other. Finally, the current approach, similar to Klein (2010), considers a population of both colluding and not colluding firms, and investigate the impact of the amendment on firm-level data. Using firm-level data allows me to investigate the effect of leniency policy on the variable of interest to the policy maker, which is not directly the number of active cartels, but the market outcome. I investigate the most important component of the outcome measure: markup.

The difficulty of this outcome based measurement is that we don't know whether the change in markup after the implementation of the leniency policy is due to the effect of the policy or to other factors that influences markups at the same time as the leniency policy. To deal with this issue, I follow the standard industrial organization literature and use industry concentration as the proxy for the inherent competitiveness of an industry. I thus assume that the leniency policy should be potentially effective only in highly concentrated industries. The implicit assumption is that even though there may have been aggregate changes the year of the policy change, there has not been any changes that were only significant to the concentrated industries. Then, I can estimate the model in a similar spirit as the standard difference-in-differences (DID) approach, where one would use the low concentration industry as the control group and the high concentration industries as the treatment group.

My results show that the Corporate Leniency Program amendment in 1993 has no significant effect on price markups on the short run while a persistent effect on the long run. Hence, this suggests that the three forces even out on the short run and render impossible the support of the effectiveness thesis of the leniency policy.

The paper is organized as follows: the second section provides a brief review of the literature, while the third section introduces the data. The fourth section presents the estimation strategy and section five provides results of the analysis. Sections six and seven, respectively, discuss the results and possible extensions while the last section concludes.

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<sup>1</sup>With the exception of Klein (2010)

## 2 Corporate Leniency Program

The original version of the Department of Justice (DOJ) Corporate Leniency Program dates from 1978. The program aims at deterring and forcing desistance of colluding practices. On paper this first version had all the right elements in providing complete protection from criminal prosecution to trigger leniency applications. Unfortunately, the success of the program was mitigated as shown by the small number of leniency applications per year (an average of one case per year) and the absence of successfully prosecuted international cartel. Adjustments had to be made and resulted in the amended version in 1993.

The major criticism to the 1978 Leniency Program was its lack of transparency. At that time, a leniency applicant satisfying a set of pre-determined criteria before an investigation has started, was granted protection from criminal prosecution but amnesty was only awarded based on the DOJ judgment. This uncertainty seems to have prevented firms from applying.

In the amendment of the Corporate Leniency Program in 1993, adjustments have been made to account for transparency issues by means of guidelines easier to use as well as higher compensation from cooperation. This new version has three significant changes (Hammond, 2004). First, if there is no pre-existing investigation, the DOJ will grant leniency automatically to the applicant. This implies that only the first applicant will receive amnesty before an investigation has started independently of the prosecution judgment. Second, similarly to the period “before an investigation has started”, the first applicant applying during an ongoing investigation will receive amnesty. Said otherwise, a firm may now apply before as well as during an ongoing investigation. Finally, all cooperators within the corporation (directors, officers and employees) will receive amnesty whenever the corporation is granted amnesty.

On paper, the amendment is a success story: over \$5 billion in fine collected since 1996 from which 90% have been brought forward by the usage of leniency, and currently more than 50 international cartels are under investigation where, in half of them, firms have granted leniency (Hammond, 2009). Following this “success” other jurisdictions have adopted derivative of the Corporate Leniency Program<sup>2</sup>.

Still, this paper takes a step back and inquires about a real measure of success: firms’ behavior. The main motivation behind the present approach is the unobservable feature of the colluding firms pool and the incapacity of past approaches to answer questions such as “What is the impact on firms’ behavior of the amendment of the Corporate Leniency

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<sup>2</sup>Over 50 jurisdictions in the world currently have a Leniency Policy, for example, Australia, Brazil, Canada, the European Union, Japan, Korea, Poland and the United Kingdom.

Program in 1993 ?”

The next section discusses the relevant literature.

### 3 Literature Review

The empirical literature on the effectiveness of leniency policies is divided into two literature strands: experimental economics and empirical “single observation” studies. I briefly discuss the former, while discuss in details the latter, which is closer to the present framework.

The growing literature in experimental economics, such as Bigoni et al. (2009), Hamaguchi et al. (2009) and Hinloopen and Soetevent (2008), build upon the technical limitation that observables (e.g., number of cartel and cartel duration of successfully prosecuted cartels) or indirect methods (e.g., pricing algorithm<sup>3</sup>) lack the observability of the actual variables that matter such as the number of cartel alive or communication schemes, hence creating a justification for running experiences. For example, in lab, Hinloopen and Soetevent (2008) recreate the conditions for collusion by allowing subject to engage in discussions ultimately leading to price fixing. Results are sharp and show the added value of implementing leniency programs by means of higher deterrence, desistance and harsher climate for sustaining pricing agreements compared to the status quo. This is definitely an avenue worth pursuing. At the same time, it is important to point out that rigorous empirical studies are still necessary for understanding the effect of leniency policies. Parameters need to be estimated for the researchers conducting experimental studies. For the experiments to be relevant in understanding the actual effect of leniency programs, those parameters should mimic the actual environment of industries as close as possible, given resource limitations in conducting the experiments. But those parameters can only be recovered following empirical studies based on real world data. This briefly survey experimental economics applied to leniency policies. In what follows, I focus on empirical studies.

Miller (2009) overcomes the unobservability of undetected cartels by estimating his theoretical model and inferring the discovery rate using filed violations of Section 1 of the Sherman Act over the period 1985-2005. The theoretical model and its empirical analogue show that the Leniency Act amendment in 1993 succeeded in enhancing both DOJ capabilities of detection and deterrence. Even though Miller (2009) found a way to avoid dealing exclusively with detected cartels, in the end, he still limits the analysis to public cases of prosecuted firms, hence falling into the “single observation” dilemma.

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<sup>3</sup>See Harrington and Chang (2000) and Miller (2009).

Brenner (2009) advocates that the benefit of leniency programs is mainly on the cost of investigation and prosecution drastically going down, on the additional information revealed to the agency as well as on the shorter time between detection and verdict period. Results are obtained using 61 cartel cases investigated and prosecuted by the European Commission between 1990 and 2003. The author concludes that leniency policies failed to deter colluding agreement, which is similar to the results I obtained. This study suffers from the reliance on “prosecuted” population.

The closest work to mine is the one of Klein (2010). The author studies the impact of leniency policies over a 20 years period from a population of 23 European Union (EU) countries. Instead of focusing on cartel detection, the author measures the effect of leniency policies on markups. After building industry specific price cost margin separately for each EU country, the author then estimates whether country specific leniency policy has an impact on the price cost margin of its industries. Even though the approach doesn’t suffer from the typical identification problem of limiting the analysis to discover colluding agreements, it faces a potential endogeneity problem.

The change in price cost margin could be due to policies or other factors than the introduction of the said leniency policies. To deal with this endogeneity issue, Klein (2010) proposes several instruments, which he claims are correlated with a country’s leniency policy but uncorrelated with any other policy or shocks that affects markups. One of the instrument used is other countries’ leniency policy enactment. The EU is a very much integrated region where many large EU firms operate across many EU countries, other country’s leniency policy could be directly related to home country’s cartel, and thus may change home country’s profit margin. The second instrument is the policy position of political parties of the country. Obviously, the policy position should simultaneously influence many policies other than the leniency policy. So, the instruments used seems discussable and lead to biased estimates.

I complement Klein (2010) results on many regards. First, the present analysis has the U.S. for population. I expect different results coming from the particularity of the U.S. leniency policy<sup>4</sup>. More importantly, the present study uses firm level data from Compustat. This database only tracks publicly traded firms, which tend to be large firms and are more likely to have bigger market power, and thus are more likely to gain from collusion, whereas Klein (2010) calculates the measure at the industry level aggregate market power and includes all firms. Also, using firm level data, instead of industry level data, allows me to extract

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<sup>4</sup>For example, firms’ treatment when applying to leniency when an investigation is undergoing differs. In the U.S. only the first applicant may receive leniency while all applicants if they “add value” to the case may be granted leniency.

a maximum of information as well as an higher degree of flexibility in the estimation. The estimation make use of quasi difference-in-differences model. I essentially consider industries with low concentration ratio as control group and those with high concentration ratio as treatment group. Therefore, any changes that occur at the same time as the leniency policy would have no effect on the parameter estimates as long as it affects the markup of both low and high concentration industries equally. Since I have detailed information on each publicly traded firms, I could have used different measures for classifying firms into treatment group and control group, other than the concentration ratio. For example, I could have used the measures of past markup or the highest market share firms only to do the DID analysis, since it is the high market share firms that are more likely to benefit from collusion. On the other hand, with industry level data, there are limits to the analysis one can conduct.

The next section presents Compustat and derived variables.

## 4 Data

I rely on Compustat database to get annual plant level data of all publicly traded firms in the United States over the period 1991 to 1997. Compustat excludes privately held firms.

The database includes for each firm the following annual variables: current asset, book value per share, capital, dividends, earnings per share, number of employees, inventory, price close, gross profit, cost of goods sold, total revenues, operating expenses, geographic location, NAICS and SIC code as well as CUSIPs<sup>5</sup>.

I present summary statistics in Table 1. For example, the gross profits (loss) span from -\$179M to \$39.2B for the 2523 firms over the period 1991 to 1997. Note that negative values indicate losses. In Table 2, I show firms' demographic by NAICS<sup>6</sup>. In Compustat database, manufacturing (NAICS 31-33) have the highest intensity in terms of firms population (respectively 144+360+844=1348 firms) while warehousing and storage (NAICS 49) as well as educational services (NAICS 61) have the lowest (6 firms).

I construct the dependent variable, called markup, by subtracting the cost of goods sold by the total revenues and dividing by total revenue. I also construct industry level variable, such as market shares and HHI index using three digits SIC<sup>7</sup>. Within an industry, market

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<sup>5</sup>A definition of each variables can be found in the Appendix.[TO COME]

<sup>6</sup>For more information North American Industry Classification System, please refer to the U.S. Census Bureau website at: <http://www.census.gov/eos/www/naics/>

<sup>7</sup>The alternative would have been to use two digits SIC. Three digits NAICS was also an option, but the level of precision was too high. Different specifications have been tried and SIC three digits seem the most promising for recreating industries

shares and HHI index were calculated using annual sales. Market shares are ratio of a firm sale over industry total sales, while HHI index is the sum of squared market shares of every firm in an industry.

The policy variable is build as follows. From the Fact Finder of the US Census, I survey the concentration ratios in each two digits NAICS classification. In a typical difference-in-difference approach, one would assign industries with high concentration ratio to be the treatment group and those with low concentration ratio to be the control group. This uses the idea that industries with high concentration ratio cartels can be formed and sustained easier with an higher benefit than industries with low concentration ratio. Therefore, the probability of a cartel existing is higher in highly concentrated industries, and thus anti-cartel policies such as leniency policies should be more effective. However, the present study does not utilize the difference in concentration ratio within the treatment group and the control group for the identification of the policy effect. Instead, we conduct a quasi-DD approach where the concentration ratio itself is used in the estimation as the continuous policy variation<sup>8</sup>.

Table 1: Summary statistics

	Unit	N	Mean	SD	Min	Max	P25	P50	P75
Gross Profit	\$M	17661	402	1589	-179	39279	9	43	199
Book-Value/Share	\$	-	8	10	-85	268	2	6	12
Capital	\$M	-	95	383	-1	8309	1	6	39
Dividends	\$M	-	30	147	0	4032	0	0	6
Earnings/Share	\$	-	0.6	2.5	-63.5	144.2	0	0.5	1.4
#Employees	K	-	7.4	28.0	0	825.0	0.2	1.0	4.2
Inventory	\$M	-	129	507	0	16497	2	12	66
Price Close	\$	-	18	19	0	487	5	12	26
Current Asset	\$M	-	401	1512	0	41338	15	56	232
Cost Goods sold	\$M	-	857	3385	0	94127	19	91	425
Revenue	\$M	-	1259	4728	0	120279	31	143	645
Operating Expenses	\$M	-	1071	4111	0	111112	29	125	546

I describe the estimation technique in the next section.

<sup>8</sup>Another option will have been to focus on antitrust exemption corporations, such as insurance companies. This avenue was not retain since too few companies qualified as insurance companies.

Table 2: Population NAICS

NAICS	11	21	22	23	31	32	33	42	44	45	48	49	51	52	53	54	56	61	62	71	72	81	99	Tota
# Firms	9	104	122	18	144	360	844	113	100	53	63	6	187	45	30	108	55	6	44	7	68	13	24	252

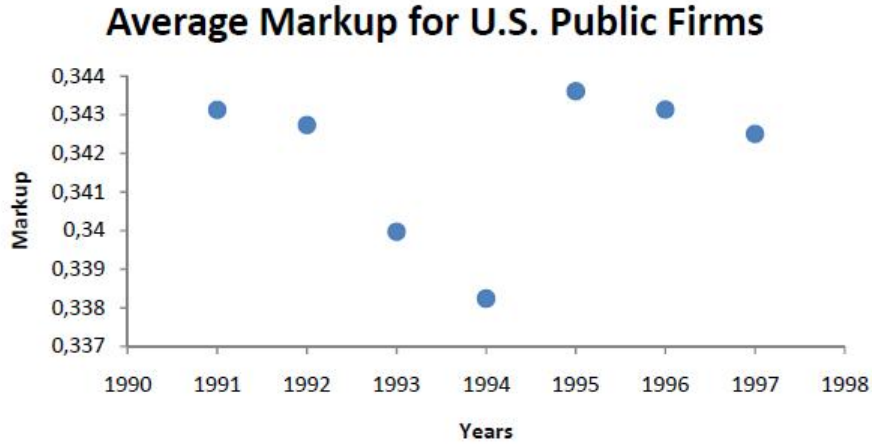


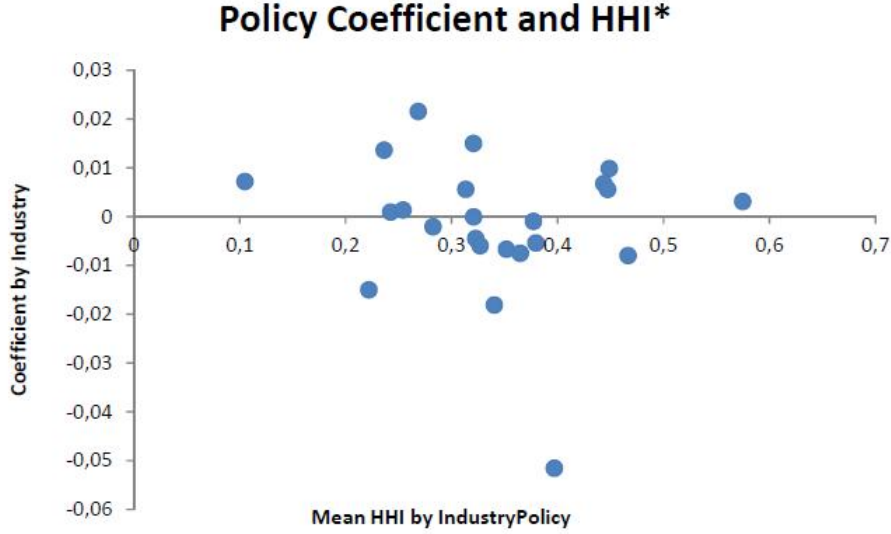
Figure 1: Structural Break 1993-1995

## 5 Model

In Figure 1, I plot the evolution of the average PCM for U.S. public firms over time. We can see that the average markup declined immediately in year 1993 from 0.343 in 1992 to 0.34, and declined further to 0.338 in 1994. But surprisingly, in 1995 the average markup went up dramatically to 0.344, the highest level since 1991, and has been declining slightly since then. It is important to note that the sharp decline in markup was temporary, even though the leniency policy was effective after that.

In Figure 2, I plot the change in industry level average before 1993 to after 1993. On the horizontal axis is the HHI concentration ratio and on the vertical axis the industry level change in markup. From the figure, we do not see any clear relationship between the HHI index and the change in markup. Even though Figures 1 and 2 do not show any patterns in favor of the leniency policy, we still need to make an econometric assessment, where we include time dummies and other variables that control for the firm profitability<sup>9</sup>.

<sup>9</sup>DEBATE ON DID APPROACH ... PAPERS



\* The policy coefficient is calculated regressing Markup over D.policy and other controls without a control group. As for the mean HHI by industry, I take the mean by industry over the time period 1991-1997.

Figure 2: DID Justification

The first model is a simple OLS estimation, while the second one is a fixed effect estimation controlling for firm level fixed effects. In both specifications, I cluster standard errors by firm. The first equation to be estimated is given by (1).

$$PCM_{it} = b_0 + b_X X_{it} + \sum_{y=1992}^{1997} b_y D_y(t) + b_h HHI_{it} + \delta [HHI_{it} \times D_y(t \geq 1994)] + \epsilon_{it} \quad (1)$$

The dependent variable is the markup  $PCM_{it}$ , whose derivation is explained above. The independent variables include  $X_{it}$ , Herfindahl index,  $HHI_{it}$ ; year dummies  $D_y(t)$  which equals 1 if  $t = y$  and 0 otherwise; and the interaction term of Herfindahl index and the dummy which equals 1 if the year is 1994 or later. The coefficient of the interaction term measures the policy effect.  $X_{it}$  is the vector of additional controls, which include market-share, the number of employees to control for the size of the firm, net income, gross profit, closing price, inventories, earnings per share and book value per share.

The interaction term,  $HHI_{it} \times D_{it}(t \geq 1994)$ , between the policy dummy and the Herfindahl-Hirshman Index is the DID variable of interest. Meyer (1995) points out that by including an interaction between the treatment indicator ( $D_{it}(t \geq 1994)$ ) and covariates

( $HHI_{it}$ ) it may help to control for heterogeneity in the dynamics induced by the different industries. Since the population of interest includes all public firms, there is a need to control for industry's specificities. Complementing this approach, I will also deal with this issue in the regression by adding a dummy variables for the industry.

A successful leniency policy implies that the coefficient should be estimated to be negative, that is,  $\hat{\delta} < 0$ , where firms are strongly reacting to the amendment of the Leniency Act by reverting their behavior from a high markup to a lower markup. If we define the treatment group to be the industries whose Herfindahl Index exceeds some threshold  $\bar{H}$  and the control group with the index below the threshold  $\bar{H}$ , and use  $I_{it}(HHI_{it} \geq \bar{H})$  instead of  $HHI_{it}$  in the OLS, then, we would have the standard DID framework, where the policy effect is identified as:

$$\hat{\delta} = \left\{ \mathbb{E}[PCM_{it}|treatment, t \geq 1994, X_{it} = X] - \mathbb{E}[PCM_{it}|treatment, t < 1994, X_{it} = X] \right\} - \left\{ \mathbb{E}[PCM_{it}|control, t \geq 1994, X_{it} = X] - \mathbb{E}[PCM_{it}|control, t < 1994, X_{it} = X] \right\}$$

In our case, instead of the treatment being either effective or not effective, the effectiveness of the treatment depends on the industry structure. That is, our hypothesis is that the more concentrated the industry, the higher is the likelihood that there is a cartel and also the higher the increase in the markup in a cartel. Hence, a leniency policy should have more effect in reducing the markup in highly concentrated industries, those with higher Herfindahl index.

I also estimate the specification that includes firm specific fixed effects ( $\eta_i$ ). The fixed effect estimation model is given by equation (2).

$$PCM_{it} = b_0 + b_X X_{it} + \sum_{y=1992}^{1997} b_y D_y(t) + b_h HHI_{it} + \delta [HHI_{it} \times D_y(t \geq 1994)] + \eta_i + \epsilon_{it} \quad (2)$$

Results are presented in the next section.

## 6 Results

Results are presented in Table 1. In Column 2 (OLS 1), we present the OLS coefficient estimates. Market share (estimate: -21.8 and standard error: 10.6) has a negative and

significant effect on markup. This is not consistent with the classical Cournot framework, the correlation should be positive as higher marketshare induces higher market power, hence higher markup. However, Berry et al. (1995), when estimating the markup in the automobile industry, also obtained similar negative relationship between markup and market share. They argue that the “high end” luxury automobiles, the ones with high markups, have smaller market shares, and the popular brands, the ones with higher market shares, tend to have lower markups. It is intriguing that the book value per share (est.: -4.00 and std.err.: 0.43) has a negative and significant effect on markup, whereas the coefficient on earnings per share (est.: 4.36 and std err.: 1.32) and gross profits (est.: 0.08 and std err.: 0.008) are significantly positive. It is reasonable that higher markup firms tend to be more profitable, hence higher earnings per share and higher gross profit. The coefficients on number of employees (est.: -0.71 and std err.: 0.32), current asset (est.: -0.04 and std err.: 0.006) and operating expenses (est.: -0.02 and std.err.: 0.003) are all negative and significant. That is, the coefficient estimates of variables related to cost are all negative and significant. We also see that the coefficient on the Herfindahl-Hirshman index is positive but insignificant, which is reasonable since it implies that industries that are highly concentrated have high markups. The coefficient on the interaction term of HHI index and the dummy for the policy period, which measures the policy effect, is negative but insignificant. Thus, we do not see any significant policy effect.

In Column 3 (OLS 2), we report the OLS results where we additionally include industry dummies as controls. Most coefficient estimates are very similar to those of the OLS without industry dummies, with some exceptions. The market share coefficient is negative but its value is only two thirds of that of the OLS without industry dummies. Similarly, the coefficient on the Herfindahl-Hirshman index is positive but half of that of the basic OLS specification. On the other hand, the policy effect is still insignificant but is estimated to be five times as large as that of the basic specification.

This implies the possibility of endogeneity bias, where the firm specific unobserved characteristics would be correlated with controls that are themselves correlated with the dependent variable.

It is important to notice that the endogeneity bias of the control variable coefficients also results in a bias in the estimation of the policy effect if the HHI in the interaction between HHI and the policy dummy, and other controls are correlated with the error term. To deal with the endogeneity issue of cross sectional correlation between the control variables and the error term, we also conducted the fixed effects estimation. Fixed effects estimation deals

with the above endogeneity issue to some extent, in particular, the bias due to the correlation between the firm specific unobserved component in the error term and the right hand side variables, by demeaning away the firm specific components of the error term and the RHS variables.

Next, we present the fixed effects estimation (FE(3)). There, the coefficient estimates have the same sign as the OLS ones, except for the fixed effect coefficient for HHI index (est.: -68.4 and std.err.: 40.7), which is negative and significant at a 10% level. However, except for the coefficients for the policy effects and the dividends, the coefficients have much smaller values than those of the basic OLS and the OLS with industry dummies. The dividend coefficient has larger value than the OLS estimates and is significant. The policy effect is similar to the one estimated from the OLS with industry dummy, and insignificant.

To further address the endogeneity issue, we also estimated the version of the model where we dropped all the control variables that we thought could be correlated with the error term: marketshare, gross profit, operating expenses and the number of employees. The results are presented in Table 4. Now, the market share coefficient is estimated to be negative, although insignificant. The policy effect is estimated to be slightly smaller for the basic OLS specification, and slightly larger for the OLS with industry dummies and the fixed effects, but they still are insignificant. We then conclude (for now) that the bias due to the endogeneity discussed above may be small.

Finally, similarly to Klein (2010), I control for a possible delay in firms' reaction to this new policy in Table 5. It is likely that firms take a few months, even a few years, to change their behavior and adapt to this new environment with new incentives for deterrence and desistence from colluding practices. It turns out that addressing the issue of two-way causality changes drastically the results. For the fixed model, the policy coefficient is now negative and significant implying that, in the aggregate, firms are affected by the amendment of 1993, when controlling for a reaction delay. Note that from Table 3, firms do not immediately react to the policy. The reaction takes 1 to 2 years to materialize. Surprisingly, after 3 years, the policy effect dies out. This may be true, but more likely a by product of data limitation as the sample spans form 1991 to 1997. Hence, since the policy variable takes the value "1" for 1994 and onward, this means that with a 3 years lag, I only have 1 year of post-policy effect which is rather small.

So far, the discussion should have convinced the reader that the benchmark is a good starting point for future extension, the model yield interesting prediction that are faithful to intuition. That being said, I can now investigate the role played by the policy parameter in

the next section.

## 7 Policy Analysis

All model specifications are pointing towards one direction: the amendment in 1993 of the U.S. Leniency Act hasn't affected firms' pricing behavior (similarly to Brenner (2009)) on a short run scale, but, ultimately, after 1995, firms have been adjusting for the policy (similarly to Klein (2010)). I can obviously not conclude on the Act effectiveness solely by this study as the population studied both include colluding and not colluding firms, but if assuming that high concentration industry are most likely to collude, we can still say that (colluding) firms have most likely been influenced by the revision in 1993.

## 8 Caveat and Extension

Even though this study do not suffer from the one observation dilemma, many considerations were silenced during the analysis. For example, it is very hard to assess the number of colluding firms and nothing can be said per se on this matter with the realized estimation. I will expect colluding firms to exhibit a different behavior than non-colluding ones. Now, are colluding firms reacting more or less to the policy compared to non-colluding ones? This is a hard question. One thing for sure, since in the first place firms are colluding it may be the case that colluding firms behave as if they were above the law in believing they are untouchable implying most likely no changes in markup. This suggests that colluding firms react to exterior pressures only when the heat is on, that is when feeling there is a sufficiently high probability that an investigation will open or that a rival will apply for leniency. This is purely hypothetical scenario and future work will address this issue.

Also, since recent prosecuted cases demonstrate the internationalization of cartelization as well as diversification of firms (air Cargo cartel, vitamin cartel, etc). The required coordination between countries' antitrust agencies complicate the conviction process as well as the interpretation of the present results. Capturing this reality is impossible with Compustat. The underlying difficulty here is that, in Compustat, potentially colluding firms are under represented limiting the effect on the variables.

Finally, Ali et al. (2009) investigate the cost of solely relying on U.S. public held companies data. It turns out that when building industry level aggregate variables such as HHI and marketshares, the weight of private held company in the entire population (both public

and private firms) is not negligible. Plus, if assuming that big public firms are less efficient than private ones, it means that when I constructed market shares with only public firms, firms have large market shares but very little markups. So, the bottom of the line is that by omitting the privately held companies, I am missing an important part of the U.S. economy. This is a fair critic of the the analysis and Compustat database.

## **9 Alternative Estimation Method**

For example: Instrumental variable and quantile regression.

## **10 Industry Level Estimation**

I study the impact of the amendment at the industry level instead of the national level.

## **11 Conclusion**

In this project, I investigate measures the impact on U.S. public companies of the amendment to the U.S. Corporate Leniency Act in 1993. At the aggregate level, I obtain that the amendment of the Leniency Act in 1993 didn't change the structure of industries, proxied by firms' markup, in the short run while the effect is present after 1 and 2 years. These are striking results and future work should focus on the persistence of the policy effect. In the sample, the policy effect is still there even when considering two lags, that is estimating the impact of the policy in 1996. Due to data limitation, the effect is absent for three lags as there is only one year for the post-amendment (the sample is from 1991 to 1997). These results are limited by the level of aggregation and the preliminary tools used for estimation (OLS and fixed estimation). Much work remains to be done, specially at the industry level where the future of this project lies.

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Table 3: Difference-in-Difference Regression Results

Variables	OLS(1)		OLS(2)		FE(3)	
Policy x HHI	-21.8	*	-13.2		-8.33	
	(10.6)		(10.4)		(9.73)	
HHI	37.8	*	14.6		-68.4	*
	(18.7)		(18.5)		(40.7)	
Marketshare	-95.0	***	-59.5	***	-26.0	
	(18.1)		(17.4)		(31.8)	
Gross Profit	0.08	***	0.08	***	0.05	***
	(0.008)		(0.007)		(0.007)	
Book-Value/Share	-4.00	***	-3.32	***	-0.17	
	(0.43)		(3.82)		(0.20)	
Capital	0.00		-0.00		0.01	
	(0.01)		(0.01)		(0.006)	
Dividends	-0.05		-0.02		-0.12	***
	(0.05)		(0.04)		(0.03)	
Earnings/Share	4.36	***	5.21	***	3.10	***
	(1.32)		(1.43)		(0.93)	
#Employees	-0.71	**	-0.64	**	-0.14	
	(0.32)		(0.29)		(0.14)	
Inventory	0.06	***	0.05	***	0.03	***
	(0.01)		(0.01)		(0.008)	
Price Close	2.65	***	2.42	***	0.75	***
	(0.11)		(0.25)		(0.11)	
Current Asset	-0.04	***	-0.04	***	-0.02	***
	(0.006)		(0.005)		(0.004)	
Operating Expenses	-0.02	***	-0.02	***	-0.01	***
	(0.003)		(0.00)		(0.002)	
Constant	yes		yes		yes	
Year-Dummy	yes		yes		yes	
Industry-Dummy	no		yes		no	
$R^2$	0.17		0.19		0.08	
#Observations	17661		17661		17661	

Table 4: Difference-in-Difference: Without Endogeneous Variables

Variables	OLS(1)		OLS(2)		FE(3)	
Policy x HHI	-28.8	***	-18.4	*	-12.0	
	(11.0)		(10.7)		(9.92)	
HHI	-10.3		-16.5		-70.0	*
	(15.9)		(16.1)		(40.5)	
Marketshare	-		-		-	
Gross Profit	-		-		-	
Book-Value/Share	-4.77	***	-4.01	***	-0.29	
	(0.56)		(0.49)		(0.20)	
Capital	0.04	***	-0.04	***	0.003	
	(0.01)		(0.01)		(0.004)	
Dividends	0.11	***	0.12	***	0.03	
	(0.04)		(0.03)		(0.03)	
Earnings/Share	5.02	***	5.89	***	3.37	***
	(1.69)		(1.72)		(1.01)	
#Employees	-		-		-	
Inventory	-0.04	***	-0.04	**	0.003	
	(0.02)		(0.01)		(0.007)	
Price Close	2.71	***	2.53	***	0.81	***
	(0.31)		(0.28)		(0.12)	
Current Asset	0.008	*	0.004		-0.004	*
	(0.004)		(0.005)		(0.002)	
Operating Expenses	-		-		-	
Constant	yes		yes		yes	
Year-Dummy	yes		yes		yes	
Industry-Dummy	no		yes		no	
$R^2$	0.06		0.14		0.02	
#Observations	17661		17661		17661	

Table 5: Difference-in-Difference Regression Results: Lags

Variables	FE(1)		FE(2)		FE(3)	
One lag Policy x HHI	-15.7	*	-		-	
	(9.08)					
Two lags Policy x HHI	-		-16.1	*	-	
			(9.34)			
Three lags Policy x HHI	-		-		-13.8	
					(9.29)	
HHI	-67.2	*	-68.5	*	-70.1	*
	(40.3)		(39.9)		(39.8)	
Marketshare	-26.2		-26.4		-26.1	
	(31.8)		(31.7)		(31.6)	
Gross Profit	0.06	***	0.06	***	0.06	***
	(0.007)		(0.007)		(0.007)	
Book-Value/Share	-0.17		-0.17		-0.18	
	(0.20)		(0.20)		(0.20)	
Capital	0.01		-0.01		0.01	
	(0.006)		(0.01)		(0.007)	
Dividends	-0.12	***	-0.12	***	-0.12	***
	(0.03)		(0.03)		(0.03)	
Earnings/Share	3.09	***	3.10	***	3.10	***
	(0.93)		(0.93)		(0.93)	
#Employees	-0.14		-0.14		-0.14	
	(0.14)		(0.14)		(0.14)	
Inventory	0.03	***	0.03	***	0.03	***
	(0.008)		(0.008)		(0.008)	
Price Close	0.75	***	0.76	***	0.75	***
	(0.11)		(0.11)		(0.11)	
Current Asset	-0.02	***	-0.02	***	-0.02	***
	(0.004)		(0.004)		(0.004)	
Operating Expenses	-0.01	***	-0.01	***	-0.01	***
	(0.00)		(0.002)		(0.002)	
Constant	yes		yes		yes	
Year-Dummy	yes		yes		yes	
Industry-Dummy	no		no		no	
$R^2$	0.08		0.08		0.08	
#Observations	17661		17661		17661	