

**FORTHCOMING IN  
THE JOURNAL OF COMPETITION LAW AND ECONOMICS**

**EVALUATING ANTITRUST LENIENCY PROGRAMS<sup>1</sup>**

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**ABSTRACT**

This paper identifies and then quantifies econometrically the impact of leniency programs on the perception of the effectiveness of antitrust policies in the business community using panel data for as much as 59 countries and 14-year span. We use the dynamics of the gradual diffusion of leniency programs across countries and over time to evaluate the impact of the program, taking care of the bias caused by self-selection into the program. We find that leniency programs increase the perception of effectiveness by an order of magnitude ranging from 10% to 21%. Leniency programs have become weapons of mass dissuasion in the hands of antitrust enforcers against the more damaging forms of explicit collusion among rival firms in the market place.

**JEL Codes:** D7; K2; L4; O4.

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<sup>1</sup> The authors thank the International Institute for Management Development (IMD), in particular Suzanne Rosselet and Madeleine Hediger, for supplying the data used in this study. They also thank the comments and suggestions from Carles Esteva-Mosso, participants at EARIE (2012), especially from Massimo Motta, and participants at the Competition and Regulation Summer School (CRESSE) 2012 Workshop on Enforcement in Chania (Crete), particularly from Joe Harrington. Joan-Ramon Borrell acknowledges the unconditional research grants from the Spanish Ministry of Science and innovation (ECO2009-06946), from the Catalan Government (SGR2009-1066), and from RecerCaixa (Obra Social La Caixa, the private charitable arm of La Caixa Savings Bank). Carmen García thanks Fundación La Caixa grant. Usual disclaimer applies.

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## I. INTRODUCTION

Leniency or amnesty programs have been gradually introduced over the last two decades for fighting more effectively against cartels. These antitrust enforcement programs secure lenient treatment for early confessors and conspirators who supply information that it is helpful to the antitrust authorities.

Under the terms governing a leniency program, a firm or individual that first confesses involvement in a cartel -- supplying details of meeting dates and the timing of the price agreements -- may avoid criminal conviction, fines, or a custodial sentence. In some programs these exemptions might also be extended to other cartel members who provide additional information.

The first antitrust leniency program was created in the United States in 1973. However, it was largely ineffective until reformed in 1993. Its apparent success in obtaining evidence to prosecute cartel members, in destabilizing existing cartels, and in deterring cartel formation was quickly noted by antitrust authorities elsewhere. Leniency programs were then gradually adopted as part of antitrust enforcement reform across developed and developing economies.

In Europe, the European Commission passed the first leniency program as early as in 1996 (a program that was overhauled in 2002) and Belgium in 1999. The Czech Republic, France, Ireland, the Slovak Republic, Netherlands, Sweden and United Kingdom in 2002 are all among the early adopters (i.e. within the first tercile of the distribution of adopters we study). Korea was also an early adopter of the program in 1997.<sup>2</sup>

By contrast, Austria, Germany, Greece and Portugal in 2006, Denmark, Italy and Spain in 2007, Lithuania, Slovenia and Cyprus in 2008, and Estonia in 2010 were all relative late adopters (i.e. within the last tercile of the distribution of adopters we study). By 2011, all 27 EU Member States had introduced leniency programs in their antitrust legislation except Malta. In the rest of Europe, Norway and Switzerland (2004), and Iceland (2005) are middle adopters, and Russia (2007) and Croatia (2010) are among the late adopters.

A leniency program was to be found on all five continents by 2004 when South Africa passed a leniency program. The already mentioned 1997 program in Korea was the first to be adopted in Asia. Brazil, Canada and New Zealand are also among the early adopters in 2000. India adopted in 2003 and Singapore in 2004. Finally, Mexico in 2006, Turkey in 2009, Colombia in 2010 and China in 2011 are among the late adopters.

Figure 1 and Table 3 show the S shaped diffusion curve among the 59 countries that we study. This global diffusion of leniency programs ran parallel to the increase in the number of developing countries adopting antitrust laws (OECD)<sup>3</sup>. Thus, in 2007, of 151 developing countries 77 had enacted antitrust legislation and appointed antitrust authorities, while in 1990 this figure stood at just 10 (Waked).<sup>4</sup>

Yet, Zhou claims that despite the evident popularity enjoyed by leniency programs, the empirical literature is ambiguous as to the deterrent effect of such programs.<sup>5</sup> While Miller has shown

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<sup>2</sup> In the United Kingdom, the basic OFT's leniency programme was established broadly at the same time as the Competition Act of 1998 came into force on 1 March 2000. However, the Enterprise Act of 2002 brought in the criminal cartel offence provisions and the leniency programme was in some sense 'expanded' to provide immunity from criminal prosecution from the cartel offence and the provision of comfort letters. The first Competition Act 1998 infringement decision involving a leniency applicant was the Leeds buses case which was issued in 2002.

<sup>3</sup> OCDE. *Fighting Hard Core Cartels: Harm, Effective Sanctions and Leniency Programs*. Paris. 2002.

OCDE. *Hard Core Cartels. Recent Progress and Challenges Ahead*, Paris. 2003.

<sup>4</sup> Waked, D.I., *Antitrust enforcement in developing countries: reasons for enforcement & non-enforcement using resource-based evidence*, 5TH ANNUAL CONFERENCE ON EMPIRICAL LEGAL STUDIES PAPER 2010. Available at: <http://ssrn.com/abstract=1638874>

<sup>5</sup> Zhou, J., *New Evidence on the Efficacy of Leniency*, 2011. Available at SSRN: <http://ssrn.com/abstract=1932473>

that the aura of efficacy of the US program is matched by the evidence, the efficacy of the EC leniency program is less clear (Brenner and De).<sup>6 7 8</sup>

Additionally, as stated by Harrington, the general conclusion of the theoretical literature is supporting leniency programs.<sup>9</sup> This literature shows that leniency can reduce cartel stability. He claims that 'it is well-documented that many firms have used the amnesty program and it has provided valuable evidence in support of the prosecution's case. However, he also states that 'it is unknown how influential leniency programs have been in inducing cartels to collapse or in deterring them from forming.'

And, these mixed findings raise questions regarding the impact of such programs in developing countries. Waked claims that legal provisions in such countries are often replicas of models employed by their developed counterparts and any evidence of the post-adoption efficacy of antitrust laws is scarce. As these so-called copy-and-paste laws are not tailored to meet local needs, their enforcement is often quite ineffective.

Waked show that many developing countries adopted competition law not out of any great domestic conviction but rather because it comprised an obligation of regional trade deals.

Indeed, an antitrust regime has often been a prerequisite for engagement in bilateral trade agreements, for securing admission into regional trade blocs, and for even participating in structural programs that open up developing economies.<sup>10</sup>

For example, many Euro-Mediterranean Association Agreements between the EU and countries such as Egypt, Jordan, Turkey and Tunisia include provisions linking financial co-operation to the implementation of antitrust legislation as highlighted by Waked.

Map 1 and Map 2 shows the geography of such diffusion in Europe, and in the world. The maps do not show a clear geographic pattern, as diffusion seems to be driven more by underlying economic, social and political developments rather than by contiguity.

This paper seeks to exploit this diffusion of leniency programs across the globe and the availability of country-year data on antitrust effectiveness perceptions by business executives to learn about leniency programs' efficacy.

The paper draws on program evaluation techniques to measure the impact of leniency programs on a broad measure of country-level perception of antitrust effectiveness. Exogenous drivers of program adoption such as regional or bilateral agreements that included antitrust reforms help in identifying econometrically the program's impact.

We find that leniency programs have had a significant impact increasing the perception of average country antitrust policy among business people by an order of magnitude of 10% to 21%. Leniency programs have become weapons of mass dissuasion in the hands of antitrust enforcers against the more damaging forms of explicit collusion among rival firms in the market place.

We also find that countries self-select: countries are more likely to adopt the program according to observables such as per capita income and regional policy commitments. Additionally, at each level of

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<sup>6</sup> Miller, N. H., *Strategic Leniency and Cartel Enforcement*, 99, AMERICAN ECONOMIC REVIEW, 750, 751-768, 2009.

<sup>7</sup> S. Brenner, *An Empirical Study of The European Corporate Leniency Program*, 27(6), INTERNATIONAL JOURNAL OF INDUSTRIAL ORGANIZATION, 639, 640-45, 2009.

<sup>8</sup> De, O., *Analysis of Cartel Duration: Evidence from EC Prosecuted Cartels*, 17, INTERNATIONAL JOURNAL OF THE ECONOMICS OF BUSINESS, 33, 34-65, 2010.

<sup>9</sup> Harrington Jr, J.E., *Optimal corporate leniency programs*, LVI (2), THE JOURNAL OF INDUSTRIAL ECONOMICS, 215, 216-246, 2008.

<sup>10</sup> See Marcos, F., 'Do Developing Countries Need Competition Law and Policy?', 2006. Available at SSRN: <http://ssrn.com/abstract=930562>

observed likelihood of adoption, those that exhibit lower unobserved antitrust effectiveness are also more likely to adopt the program as they benefit most from it.

The paper is organized as follows: Section 2 offers a brief literature review; Section 3 provides details about the data and the methods used in the program evaluation; Section 4 presents the results; and finally, Section 5 concludes and discusses the paper's findings.

## II. LITERATURE REVIEW

We review briefly the literature on measuring antitrust effectiveness, and then the literature on the effectiveness of leniency programs.

Comparing effectiveness of antitrust policy across countries and years has been a challenge during the last decade, as antitrust laws were passed in an increasing number of countries. Nicholson offers a summary of existing quantitative and qualitative antitrust effectiveness measures.<sup>11</sup>

When restricting to studies that have information for a large number countries, there have been mostly two types of efforts: some papers rely on aggregating a set of indications of antitrust inputs and outputs such as legal provisions, authority resources and decisions in a given year; others rely on data from surveys, conducted usually in a yearly basis.

Evenett compares countries only regarding merger notifications regimes.<sup>12</sup> Nicholson in the working paper version of the study, and then Nicholson in its journal version, provided an Antitrust Law Index (ALI) that offers comparative evidence for what it is on the books across 52 countries. Kee and Hoekman compare them regarding the date of introduction of antitrust laws.<sup>13 14</sup>

Voigt compiled four indicators that reflect the basis and contents of competition legislation, the degree to which these laws are based on economic reasoning, the formal degree of independence of the antitrust authorities and their factual independence.<sup>15</sup>

Borrell and Jiménez compiled qualitative information on the legal and institutional setting up the antitrust law and authorities, cartel policy enforcement, the handling of monopolization and dominant positions, and the features of merger policy for 47 countries. Ma compiled and used information on *de facto* and *de iure* authority independence.<sup>16 17</sup>

None of these studies, however, provide enough information to compare antitrust effectiveness cross country and year consistently. By contrast, three surveys offer panel data on effectiveness.

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<sup>11</sup> Nicholson, M.W., *An antitrust law index for empirical analysis of international competition policy*, 4(4), JOURNAL OF COMPETITION LAW & ECONOMICS, 1009, 1010-1029, 2008.

<sup>12</sup> Evenett, S., *Do all networks facilitate international commerce? US law firms and the international market for corporate control*, 17(4), JOURNAL OF JAPANESE AND INTERNATIONAL ECONOMICS, 520, 521-537, 2003.

<sup>13</sup> Nicholson, M. W., *Quantifying Antitrust Regimes*. FTC Working Paper 2004; Washington DC.

<sup>14</sup> Kee, H.L., Hoekman, B., *Competition Law as market discipline?*, 51(4), EUROPEAN ECONOMIC REVIEW, 831, 832-858, 2007.

<sup>15</sup> Voigt S., *The effect of competition policy on development: Cross-country evidence using four new indicators*, 45(8), JOURNAL OF DEVELOPMENT STUDIES, 1225, 1226-1248, 2009.

<sup>16</sup> Borrell, J.R., Jiménez J.L., *The drivers of antitrust effectiveness*, 185(2), HACIENDA PÚBLICA ESPAÑOLA – REVISTA DE ECONOMÍA PÚBLICA, 69, 70-88, 2008.

<sup>17</sup> Ma, T.-C., *Competition authority independence, antitrust effectiveness, and institutions*, 30, INTERNATIONAL REVIEW OF LAW ECONOMICS, 226, 227-235, 2010.

Ma, T.-C., *The effect of Competition Law enforcement on economic growth*, 7, JOURNAL OF COMPETITION LAW AND ECONOMICS, 301, 302-334, 2011a.

Ma, T.-C., *Legal tradition and antitrust effectiveness*, EMPIRICAL ECONOMICS 2011b; forthcoming.

Since 2000, the Global Competition Review publishes an annual survey titled ‘Rating the Regulators’ that try to assess the effectiveness of antitrust laws and authorities for a limited but increasing number of countries based on the opinions of professionals related to antitrust enforcement (34 countries and the EU in 2012). And, Global Competition Review offers comparative data on antitrust agency budget and staff. Regrettably, the survey does not contain enough cross country observations, particularly for the initial years.

The executive surveys of the International Institute for Management Development (IMD) and the World Economic Forum (WEF) have more and increasing number of countries (as much as 59 in the case of IMD since 2011), and more years than any other (as much as 14 in the case of IMD).

Both surveys are directed to business executives that offer their perceptions on a set of competitiveness questions that include antitrust effectiveness and local competition conditions. This is why IMD and WEF data have been extensively used in comparing antitrust effectiveness across country and time (for example, in Dutz and Hayri, Borrell and Tolosa, Nicholson, Voigt, Waked and Ma).<sup>18 19</sup>

The literature studying the efficacy of leniency programs has two strands: one is theoretical, and the other is empirical.

The former has a very general conclusion, and just some caveats. The seminal papers by Motta and Polo, and Spagnolo, and the following by Feess and Walzl, Motchenkova, Aubert, Kovacic and Rey, Chen and Harrington, Harrington and Hinloopen and Soetevent show that leniency deters cartels by destabilizing them: leniency reduces incentives to collude, and it enhances the incentive to cheat.<sup>20 21 22 23 24 25 26</sup>

Only Chen and Harrington and Harrington offer some caveats to this general conclusion. They show that when leniency is sufficiently mild, in such part of the policy space, it can have a perverse effect on antitrust enforcement as it can either raise or lower cartel stability. And Chang and Harrington show that the cartel rate may be higher when there is a leniency program if the antitrust authority prosecute a smaller fraction of cartel cases identified outside the program<sup>27</sup>.

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<sup>18</sup> Dutz, M.A., Hayri A., *Does more intense competition lead to higher growth?* WORLD BANK POLICY RESEARCH Working Paper, 2320 and CEPR Discussion Paper No. 2249. 2000.

<sup>19</sup> Borrell, J.R., Tolosa M., *Endogenous antitrust: Cross-country evidence on the impact of competition-enhancing policies on productivity*, 15(2), APPLIED ECONOMICS LETTERS, 827, 828-831, 2008a.

Borrell, J.R., Tolosa M., *Política de competencia y productividad: más allá de la calidad institucional y la apertura*, 76, CUADERNOS ECONÓMICOS DE ICE, 57, 58-72, 2008b.

<sup>20</sup> Motta, M., Polo, M., *Leniency programs and cartel prosecution*, 21, INTERNATIONAL JOURNAL OF INDUSTRIAL ORGANIZATION, 347, 348-379, 2003.

<sup>21</sup> Spagnolo, G., *Divide et Impera: Optimal Deterrence Mechanisms Against Cartels and Organized Crime*, University of Mannheim, 2003.

<sup>22</sup> Feess, E. and Walzl, M., *An Analysis of Corporate Leniency Programs and Lessons to Learn for U.S. and E.U. Policies*, University of Maastricht, 2004.

<sup>23</sup> Motchenkova, E., *The Effects of Leniency Programs on the Behavior of the Firms Participating in Cartel Agreements*, Tilburg University, 2004.

<sup>24</sup> Aubert, C., W. Kovacic, and P. Rey, *The Impact of Leniency and Whistle-Blowing Programs on Cartels*, 24, INTERNATIONAL JOURNAL OF INDUSTRIAL ORGANIZATION, 1241, 1242-1266, 2006.

<sup>25</sup> Chen, J. and Harrington Jr, J.e., *The Impact of the Corporate Leniency Program on Cartel Formation and the Cartel Price Path*, in THE POLITICAL ECONOMY OF ANTITRUST, V. Ghosal and J. Stennek, eds., Elsevier, 2007

<sup>26</sup> Hinloopen, J., Soetevent, A.R., *Laboratory evidence on the effectiveness of corporate leniency programs*, 39(2), RAND JOURNAL OF ECONOMICS. 607, 608-616, 2008.

<sup>27</sup> Chang, M.-H., Harrington, J. E., *The Impact of a Corporate Leniency Program on Antitrust Enforcement and Cartelization*. Mimeo 2010; <http://www.econ.jhu.edu/pdf/papers/WP548.pdf>.

By contrast, the empirical literature, offers mixed evidence on the efficacy of leniency to deter cartels from forming and acting in the shadows. Miller offers clear evidence of the effectiveness of the US revamped leniency program. On the contrary, Brenner and De show that EC leniency program is not so effective in deterring and destabilizing cartels.<sup>28 29 30</sup>

This study tries to offer new empirical evidence regarding this unsettled question on the impact of leniency programs on the perception of country-level competition policy effectiveness.

### III. DATA AND METHODS

#### A. Data

The International Institute for Management Development (IMD) kindly provided us with information for one of the criteria reported at the country level in its World Competitiveness Yearbook. Specifically, IMD supplied us with criterion number 2.4.11: “Competition legislation” (Factor: Government Efficiency; Sub-factor: Competition and Regulations).

The IMD’s World Competitiveness Center conducts an Executive Opinion Survey to complement hard statistical data drawn from international, national and regional sources. The aim of the survey is to measure competitiveness as it is perceived. The surveys are sent out to senior business leaders, representing a cross-section of the business community in each country. The questions are targeted to top and middle management, who are nationals or expatriates employed in local or foreign firms with an international dimension.

The sample size and its distribution are proportional to the GDP breakdown of the economic sectors in each country’s economy. Executives are asked to evaluate the present and expected competitiveness conditions for the country in which they have lived and worked for the past year. IMD alumni are also contacted. In the survey conducted in 2011, IMD obtained 4,935 responses from 59 countries worldwide.

Many papers have used this information or similar data provided by the World Economic Forum (WEF) as their perceived measure of antitrust effectiveness. Here, under criterion 2.4.11: “Competition legislation”, the survey asks a group of executives to rank on a scale from 1 to 6 whether “Competition legislation is efficient in preventing unfair competition.” The data are subsequently converted to a 0 to 10 scale.

Although the wording of this question is a bit confusing, as it is not clear whether it refers to the effectiveness of competition policy against cartels and monopolization, or other practices such as unfair behaviour, this indicator is very highly correlated to the one provided by WEF which explicitly asks whether “Antimonopoly policy is 1=lax and ineffective at promoting competition, 7=effectively promotes competition.” Both fairly rank countries with respect to competition policy effectiveness (see correlations in Voigt).

IMD provided us with an unbalanced panel dataset containing information about the average country-level result for the competition legislation criterion for the 46 countries included in its 1998 Yearbook and for the next 14 years. Each year the yearbook has increased the number of countries included so that in 2011 it provided information for 59. In our database, this measure of antitrust

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<sup>28</sup> Miller, N. H., *Strategic Leniency and Cartel Enforcement*, 99, AMERICAN ECONOMIC REVIEW, 750, 751-768, 2009.

<sup>29</sup> Brenner, S., *An Empirical Study of The European Corporate Leniency Program*, 27(6), INTERNATIONAL JOURNAL OF INDUSTRIAL ORGANIZATION, 639, 640-45, 2009.

<sup>30</sup> De, O., *Analysis of Cartel Duration: Evidence from EC Prosecuted Cartels*, 17, INTERNATIONAL JOURNAL OF THE ECONOMICS OF BUSINESS, 33, 34-65, 2010.

effectiveness ranges from 2.10 to 8.59. As Table 1 shows, the mean value for this variable is just 5.53, and its standard deviation is 1.25.

Figures 2 and 3 show the trend of this measure of antitrust effectiveness as perceived by business executives for the US and for the average EU Member States. Perceived antitrust effectiveness reached a peak in 2002 in the US and it gradually declined until 2009 during George W. Bush Administration, when it rebounded sharply until 2011 and then it levelled off in 2012. In the EU, perceived antitrust effectiveness also peaked in 2002 and it gradually declined until 2005 when it rebounded smoothly until 2007, when it peaked again, and it started to decline until 2010 when it rebounded sharply in 2011 and 2012.

As the main purpose of our paper is to study the impact of leniency programs on antitrust efficiency, we gathered data concerning antitrust policy reforms in all the countries for which IMD antitrust effectiveness data were available.

We were specifically concerned with identifying if and when leniency programs had been introduced. We obtained this information at the country level from various sources including the websites of individual antitrust authorities and the International Competition Network (ICN). Table 1 also shows the descriptive statistics of these data.

Our information includes antitrust effectiveness perceptions for three jurisdictions (Hong Kong, Philippines and United Arab Emirates), from a total of 59, that have yet to enact conventional antitrust legislation (at least before 2012), but whose governments forbid and prosecute certain restrictions on competition.

The remaining countries all introduced competition legislation either before or after 1998. Here, we distinguish between those (a total of 12 countries) that had enacted such legislation after 1998, from the rest, as our goal is to assess the impact of leniency programs during the 14 years commencing 1998. We will take this into account in the empirical analysis, as it is difficult to identify the stand-alone impact of leniency when such programs were created at the same time, or at a date very close to, the enactment of the first antitrust law.

Our dataset is characterised by the wealth of cross-country information it provides on EU Member States (37% of observations), new EU Member States that have entered the Union during the last two enlargements (9% of sample), NAFTA countries (6% of sample), countries from the Asian-Pacific (6% of sample), Mercosur countries (4% of observations), and the Andean Community (4% of observations).

Overall, 56% of our observations are of country-year pairs in which regional agreements were binding. This is of relevance as some of these regional agreements contain binding commitments as regards competition law, and some even provide for the adoption of leniency programs (the case of the EU). Table 2 lists the countries which were members of one of these regional agreements during all or some years of the dataset.

Table 1 shows that a leniency program is enforced for as many as 49% of the country-year pairs. Table 3 shows the adoption dynamics of leniency programs in the countries included in the IMD's Executive Survey between 1998 and 2011. So, while just 3% of the sample operated such a program in 1998, by 2011 three out of four countries had adopted one.

The "leniency" variable takes a value of 1 from the year a leniency program was implemented in any country. In addition to these two key variables, we collected a set of other covariates for which we wish to control in our econometric estimations.

Thus, we record whether the country had been implementing a competition law prior to the onset of our study period. We also control for "first law" whenever the first antitrust legislation was enacted during the period 1998 to 2011. Additionally, the "law reform" variable records if a country reformed its competition legislation during the sample period. This being the case then the variable takes a value of 1 in any year following that policy reform.

We also control the estimates for the possibly effect that the revamped European Commission leniency program may have had on the perception of antitrust effectiveness in the European Economic

Area (EEA) member states, that is, the EU member states plus Norway and Iceland from 2002 on (or from the date of accession on).

A further factor related to competition policy is the “age of competition law” variable, which indicates how many years the law has been in force. It has a mean of approximately 23 years in the data. We collected these data from a wide range of national legislative sources.

Other characteristics at the country level are captured by two variables. The first, gross domestic product per capita (hereinafter, GDP), measures the income of the country in current US dollars (\$). Table 1 shows that the mean GDP per capita in our sample is about 15,889\$.

The “elections” variable takes a value of 1 in the year the country held general or presidential elections. Twenty-five per cent of country-year pairs in our sample held elections. This variable is deemed relevant as leniency programs typically result from broader competition legislation reforms, which in turn may correlate with government changes following elections.

## **B. Method**

The dynamics of the adoption of antitrust leniency programs are ideally suited to the settings of a program evaluation exercise. Typically, the main problem in an empirical exercise of this type is assessing the impact of exposing a set of units to a treatment on a given outcome (Imbens and Wooldridge).<sup>31</sup>

Our units are the countries of the world, our treatment is the adoption of an antitrust leniency program, and the outcome is the perceived efficiency of competition legislation or antitrust effectiveness.

The key methodological concern in program evaluation is that each unit (in this instance, country) is exposed or otherwise to the treatment, and that only one case or the other is observed: i.e., the outcome can only be measured in the case of treatment or in that of non-treatment. Citing Holland, Imbens and Wooldridge refer to this concern as “the fundamental problem of causal inference”.<sup>32</sup>

Thus, to assess the impact of leniency programs, we need to compare countries at different points in time, some of which have adopted leniency programs (treated group) and others that have not (control group).

In the case of binary treatments, Imbens and Wooldridge remind us that the traditional focus in the econometrics literature is that of endogeneity or self-selection: countries that adopt leniency programs differ from those that choose not to do so.

When these differences condition the response to the treatment, comparing the outcomes of the treated and the control groups does not offer causal inferences of the impact of the program under evaluation, even when we are able to control for observed covariates.

The literature on randomised experiments provides a dominant approach to the analysis of the causal effects of programs or policies in observational studies. Imbens and Wooldridge refer extensively to Rubin’s proposals for interpreting comparisons of potential outcomes as causal statements: pairs for outcomes defined for the same country both when it is and when it is not treated. Moreover, Imbens and Wooldridge highlight that the main attraction of this potential outcome set-up is that it allows for general heterogeneity in the effects of the treatment from the outset. In practice, the heterogeneity of the effect is important, often motivating economists’ concerns about endogeneity.

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<sup>31</sup> Imbens, G., Wooldridge, J.M., *Recent developments in the econometrics of program evaluation*, 47, JOURNAL OF ECONOMIC LITERATURE, 5, 6-86, 2009.

<sup>32</sup> Holland, P., *Statistics and Causal Inference*, (with discussion), 81, JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION, 945, 946-970, 1986.



$$Y_{it} = \mu_i + \rho_t + D_{it}\alpha + X'_{it}\beta + \omega_{it} + \varepsilon_{it}$$

be the difference-in-difference (hereafter, dif-in-dif) to be estimated in order to identify the causal effect of the treatment (leniency program,  $D$ ) on the outcome (antitrust effectiveness,  $Y$ ),  $\mathcal{A}$ . In this equation we assume that the error term has two components, an *iid* shock named  $\varepsilon_{it}$  that cannot be anticipated at the beginning of period  $t$ , and an unobserved component of the outcome named  $\omega_{it}$  that can be anticipated at the beginning of period  $t$ .

And selection into the treatment is driven by observables ( $Z$ ) and unobservables ( $v_{it}$ ):  $D_{it} = 1 \{Z_{it}\gamma + v_{it} \geq 0\}$ .

In this case, selection on observables is a problem when  $E[\varepsilon Z] \neq 0$ . The unanticipated component of the antitrust effectiveness shock is correlated with the observable drivers of the self-selection on the treatment. The problem is still there when the anticipated part of the antitrust effectiveness shock is uncorrelated with the unobserved driver of selection,  $E[\omega_{it}v_{it}] = 0$ .

In the literature, what has been referred to as unconfoundedness, exogeneity, ignorability, or selection on observables removes any self-selection bias in comparisons made between the treated and the control groups. Adjusting treatments and control groups for differences in covariates, or pretreatment variables, is the key to obtain causal inference of effects.

Matching analysis can reduce this bias<sup>33</sup>. Let  $Y_1$  represent the outcome (here, antitrust effectiveness) in the case of a unit (a country) exposed to treatment (implementation of a leniency program), which implies that  $D$ , the binary variable describing treatment status, is equal to one. By analogy,  $Y_0$  is the outcome if the unit is not exposed to treatment ( $D=0$ ). Our causal effect of interest is defined by the difference between  $Y_1$  and  $Y_0$ , so it yields a problem of inference with missing data.

The Average Treatment effect of interest to us is that on the Treated group (hereafter ATT) and it can be defined as:

$$E(Y_1 - Y_0 | D = 1)$$

The problem is that we do not observe the untreated ( $Y_0$ ) outcome in treated country-year pairs, i.e. when  $D=1$ . We then look for a set of observable characteristics ( $Z$ ) that affect both the treatment status. We assume that potential outcome in case of no treatment is independent of treatment assignment, which states that:

$$Y_0 \perp D | Z$$

$$0 < \Pr(D = 1 | Z) < 1$$

The first of these is the untestable conditional independence assumption (CIA); the second is a requirement for identification. Under these two conditions, the ATT can be identified as:

$$\begin{aligned} ATT &= E(Y_1 - Y_0 | D = 1) = E(E(Y_1 - Y_0 | D = 1, Z)) = \\ &= E(E(Y_1 | D = 1, Z) - E(Y_0 | D = 0, Z) | D = 1) \end{aligned}$$

In our case, the impact of the introduction of a leniency program may not be homogeneous across countries, and there might be selection on observables. According to Heckman et al., there are two

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<sup>33</sup> This methodology was first proposed by Rubin, D., *Estimating causal effects of treatments in randomised and non-randomised studies*, 66, JOURNAL OF EDUCATIONAL PSYCHOLOGY, 688, 689-701, 1974, in a paper in which he established the potential-outcome framework for causal inference. The seminal paper here is Rosenbaum, P., Rubin, D., *The central role of the propensity score in observational studies for causal effects*, 70, BIOMETRIKA, 41, 42-55, 1983.

sources of bias: the first arises when changes have occurred in some country-year pairs, but there are no comparable country-year pairs in which changes did not occur and vice versa. The second bias arises from different distributions of the vector of observable variables that affect our endogenous variable within the two groups of country-year pairs.<sup>34</sup>

The use of a matching estimator can eliminate these two potential biases by pairing treated country-years (adopters of leniency programs) with control groups (the non-adopters) that present similar observable attributes.<sup>35</sup>

The plausibility or otherwise of causal inference can only be verified by conducting a sensitivity analysis, i.e., evaluating the robustness of the inference to a set of ‘what-if’ assumptions regarding the process of selection on observables.

On the other hand, plain OLS estimates of dif-in-dif estimators may be biased when there is selection based on unobservables as follows.

Selection on unobservables is a problem when

$$D_{it} = 1 \{Z_{it}\gamma + v_{it} \geq 0\},$$

and

$$E[\omega_{it}v_{it}] \neq 0.$$

For instance, at the beginning of the period, the policy makers know  $\omega_{it}$ , and they may decide to self-select into the treatment whenever this unobserved (by the econometrician) component of antitrust effectiveness is low. In this case, the bias of the OLS estimate is as follows:

$$Bias = \alpha_{OLS} - \alpha_{IV} = \frac{Cov(\omega v)}{Var(D)}$$

where  $\alpha_{IV}$  is the estimate of the causal effect using instrumental variables that overcome the endogeneity or selection based on unobservables by seeking to identify exogenous drivers of the country groupings ( $Z$ ).

The endogeneity bias depends on the sign of  $Cov(\omega v)$ , the covariance between the unobserved part of the outcome and the unobserved component of the selection on the treatment. In our example, this is the correlation between what is anticipated by the policy makers but what goes unobserved in the econometrician component of antitrust effectiveness and the unobserved driver of adopting leniency.

In this paper, we use four inference techniques to identify and quantify the causal effect of antitrust leniency programs on the efficiency of competition legislation across countries. First, we test the equalities of the means and of the distribution of the treatment group with respect to those of the control group. Second, we estimate the effect of leniency on antitrust effectiveness non-parametrically using matching techniques, assuming that self-selection of the countries that adopt the program is conditioned only on observables.

We then estimate diff-in-diff regressions to determine the mean causal effect, drawing on information before and after a comparison of the effect across countries. Finally, using a selection model we check whether the estimates suffer from bias due to self-selection of adopters according to unobservables.

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<sup>34</sup> Heckman, J.J., Ichimura, H., Todd, P., *Matching as an econometric evaluation estimator: evidence from evaluating a job training programme*, 64, THE REVIEW OF ECONOMIC STUDIES, 605, 606-654, 1997.

<sup>35</sup> See Galiani, S., Gertler, P., Schargrodsky, E., *Water for life: the impact of the privatization of water services on child mortality*, 113, JOURNAL OF POLITICAL ECONOMY, 83, 84-120, 2005, for an application of this method.

#### IV. RESULTS

Table 4 and Figure 4 clearly show that the antitrust effectiveness outcomes of country-year pairs treated with a leniency program and those of non-treated country-year pairs differ, the average difference being 0.50 to 0.58 (i.e., 9.5% to 11.2%). This difference in the mean is statistically significant. The estimates and the distribution functions are for the full sample, for the sub-sample of country-year pairs with competition law in force, and for the sub-sample of those with a competition law enacted before 1998.

Figure 4 suggests that antitrust effectiveness for country-year pairs under leniency stochastically dominates the distribution of antitrust effectiveness of the non-leniency control country-year pairs. Table 4 confirms the stochastic dominance of country-year pairs under leniency for the three different sampling according to the Kolmogorov-Smirnov test.

Figure 4 also shows that the increase in antitrust effectiveness for country-year pairs under leniency shrinks for the upper tail of the distribution of antitrust effectiveness. For the whole sample, the increase in antitrust effectiveness under leniency turns to be much smaller for countries scoring 7 or more (i.e. Finland, Denmark, Australia, Germany, Austria or the Netherlands). For the sample of countries that have antitrust law passed before 1998, such difference disappears completely for countries scoring 7 or more such that the countries listed above.

However, this evidence is insufficient to indicate the unequivocal effect of the treatment since country-year pairs might self-select into the treatment.

Table 5 rank each country in our sample with respect its average antitrust effectiveness before treatment. It reports also antitrust effectiveness after treatment, and the dates on which the competition law was enacted, on which it was reformed, and on which the leniency program was adopted).

The data suggest that countries at the top of the effectiveness ranking are more likely to adopt leniency programs, but that there is a set of countries some way off the top that tend to have been early adopters such as Belgium, Korea, Brazil and the Czech and Slovak Republics. The data also show that adopters differ in terms of their observables, including the number of years since the enactment of their competition law, per capita GDP and NAFTA and EU membership.

Table 6 summarizes the results of the matching estimator. Average Treatment effect on the Treated (ATT) is obtained by using the kernel matching method.<sup>36</sup> Matching relies on the assumption that the selection is on observables rather than on unobservables, as outlined above. In all the estimations we include the following as the exogenous variables driving the selection of the adopters on observables: “new country in EU”, “age of competition law”, “elections (t-1)”, “GDP (t-1)”, “regional agreement”, “EU binary variable” and “year fixed effects”, as described in Table 1. The estimations were conducted using bootstrap techniques.

Matching shows an average effect of adopting leniency from 0.68 to 0.93 points (out of 10) of antitrust effectiveness. This is a permanent increase of 13% to 18% over the sample average, and between half and three quarters of one standard deviation. The effect is precisely estimated and it is statistically significant at the 1% level. It is slightly larger than the plain differences in the effectiveness means. Countries self-select on observables and this attenuates slightly downwards the mean differences between country-year treated and non-treated.<sup>37</sup>

As discussed above, the main weakness of using the matching estimator is that it relies on an assumption of conditional independence of potential outcomes and treatment assignment given

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<sup>36</sup> Four of the most widely used matching methods are nearest neighbor, radius, stratification and kernel. None of them is *a priori* superior to the others. See Becker, S., Ichino, A., *Estimation of average treatment effects based on propensity scores*, 2(4), THE STATA JOURNAL, 358, 359-377, 2002, for a further explanation.

<sup>37</sup> Very similar results in logs as shown in Table 6 ranging from 15% to 21%.

observables.<sup>38</sup> This implies that selection into treatment is driven solely by factors observable by the researcher.<sup>39</sup>

To check the robustness of these results to the selection solely on observables, we conducted a sensitivity analysis (Table 7) in line with Ichino et al. and as implemented by Nannicini. This analysis assesses whether (and to what extent) the estimated average treatment effect is robust to possible deviations from the conditional independence assumption<sup>40 41</sup>.

To perform this test, we simulate in the matching estimator a ‘killer’ *confounder* (in the authors’ terminology,  $U$ ), which is used as an additional covariate. This confounder uses a set of parameters  $p_{ij}$  (where  $i$  refers to being treated or otherwise and  $j$  to a binary outcome), so that if  $U$  were observed, the estimated ATT would be driven to zero or far from the baseline estimate<sup>42</sup>. Thus, we attributed some values to these four probabilities in accordance with the being treated ( $i$ ) parameter and the outcome given ( $j$ ) ( $p_{11}$ ,  $p_{10}$ ,  $p_{01}$ ,  $p_{00}$ ) and, if some of these configurations could be considered as being highly unlikely, then we had our support for the robustness of the matching estimations.

Nannicini denominates  $d$  as a measure of the effect of  $U$  on the untreated outcome ( $d=p_{01}-p_{00}$ ); and  $s$  as a measure of the effect of  $U$  on the selection into treatment ( $s=p_{11}-p_{01}$ ). Both measures have to be greater than zero since this implies a positive effect on the untreated outcome and on the selection into treatment, respectively. They are associated with the values of  $\Gamma$  and  $\Lambda$ , which are the estimated odds ratios of  $U$  reported as the “outcome effect” and “selection effect” of the simulated confounder, respectively.<sup>43</sup>

We construct a table, in line with proposals in the two seminal papers (Nannicini and Ichino et al.), in which we simulate ATTs so that  $d$  and  $s$  increase by 0.1, varying from 0.1 to 0.6. What we are seeking are data that are quite similar to the baseline results when we increase both measures ( $d$  and  $s$ ). The estimations in Table 7 show the robustness of our results because the ATT only tends to zero when  $s$  and  $d$  are greater than 0.6.

We then check the robustness of the results to selection due to unobservables.

We first estimate the causal effect of leniency programs on antitrust effectiveness using a plain difference-in-difference estimator. Table 8 reports the OLS estimates of regressing antitrust effectiveness on a set of country fixed effects, year fixed effects, enter into force in each country of the 2002 EC leniency program, and the leniency program binary treatment.

The estimates suffer from a strong attenuation bias. All estimates of the impact of leniency are very close to zero and are not statistically significant. This suggests strong self-selection due to unobservables.<sup>44</sup>

We next estimate the difference-in-difference regression using a selection model a la Heckman for binary treatments using the same IV variables for estimating the selection probit equation.<sup>45</sup>

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<sup>38</sup> See Nannicini, T., *A simulation-based sensitivity analysis for matching estimators*, 7(3), THE STATA JOURNAL, 334, 335-350, 2008.

<sup>39</sup> See Heckman et al. (1997) for an explanation of the bias associated with matching analyses.

<sup>40</sup> Ichino, A., Mealli, F., Nannicini, T., *From temporary help jobs to permanent employment: what can we learn from matching estimators and their sensitivity?*, 23(3), JOURNAL OF APPLIED ECONOMETRICS, 305, 306-327, 2008.

<sup>41</sup> Note that this is not a ‘test’ of the conditional independence assumption, as this identifying assumption is intrinsically non-testable since the data are uninformative about the distribution of potential outcomes in the case of no treatment for treated units (Ichino et al., 2008).

<sup>42</sup> For continuous outcome (the antitrust effectiveness index in our case), we adapt the methodology on the basis of a binary transformation:  $Y=1$  if the effectiveness index is higher than average effectiveness and  $Y=0$  otherwise.

<sup>43</sup> The program *sensatt* in the STATA estimate, at every iteration, a logit model of  $Pr(Y=1|T=0, U, W)$  reports the outcome effect. For the selection effect, the logit model estimated is  $Pr(T=1|U, W)$ . The other covariates are summarized at  $W$ .

<sup>44</sup> Again, estimates taking logs of antitrust effectiveness are very similar to those in levels (original IMD data).

Tables 9 and 10 report the results of the IV binary treatment selection model. The key instruments for identifying the causal effect are observables, including GDP per capita (t-1) and the integration of a country into a regional agreement. As the sample includes a large number of European countries, joining the EU during the sample period is considered a driver of the adoption of new antitrust rules in general, and of adopting leniency programs in particular. Additionally, we use the election covariate (t-1) as an instrument on the grounds that countries seem much more likely to make legislative reforms in the “honeymoon” period following a general election. Finally, instruments include the age of competition law and its square.

The estimates are very similar to the ones obtained using matching techniques and more precise. In this case all range from 16% to 21%, around two thirds of one standard deviation, and all estimates are statistically significant. As the bias is negative, the countries that adopt leniency programs are precisely the ones that have lower anticipated unobserved antitrust effectiveness ( $\omega_{it}$  in the set up above). The covariance between the unobserved drivers of program adoption ( $v_{it}$ ) and the error term in the antitrust effectiveness dif-in-dif regression is negative:  $Cov(\omega v) < 0$ .

This seems to be consistent with the pattern we described above. Thus, early adopters such Belgium that introduced leniency in 1999 respectively, ranks relatively high in terms of their observables (e.g., GDP per capita), but this country have an unobserved component of antitrust effectiveness that is lower than that of countries that occupy a similar ranking of antitrust effectiveness based on their observable covariates.

## V. CONCLUSIONS

The dynamics of the creation of antitrust leniency programs across the globe provides a good description of policy innovation adoption. At the same time, the IMD survey data defining the perception of antitrust enforcement have enabled us to identify and quantify the impact of leniency program adoption on competition policy effectiveness across countries.

The main problem we have had to overcome in this paper is that countries tend to self-select into the treatment in a way that is unknown, although it would appear that program adoption does drive perceptions of antitrust effectiveness.

We report here that countries self-select, being more likely to adopt the program according to observables that include per capita income and regional policy commitments. Nevertheless, at each level of observed likelihood of adoption, countries that exhibit lower unobserved antitrust effectiveness are also more likely to adopt the program as they benefit most from it.

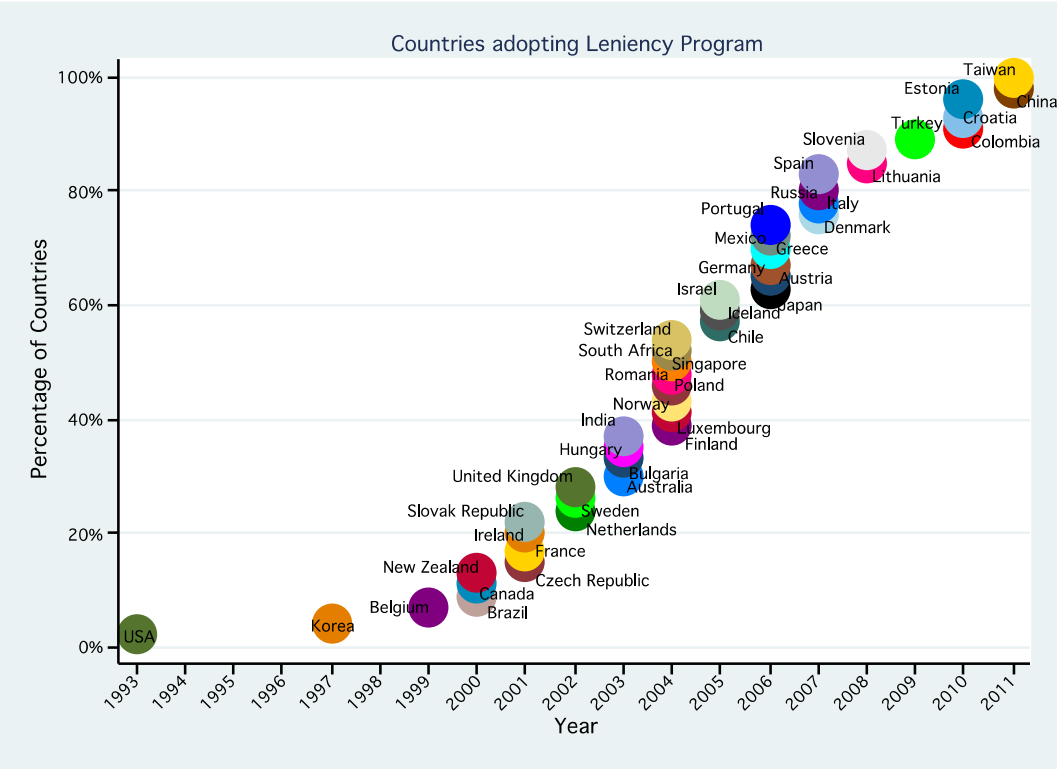
Leniency programs have become weapons of mass dissuasion in the hands of antitrust enforcers against the more damaging forms of explicit collusion among rival firms in the market place. We find that this impact is ranging from 10% to 21%.

By correcting for one or other type of self-selection, we show that leniency programs have had a significant positive impact on the perception of a country’s antitrust policy among the business community, especially in those countries whose antitrust enforcement is least credible.

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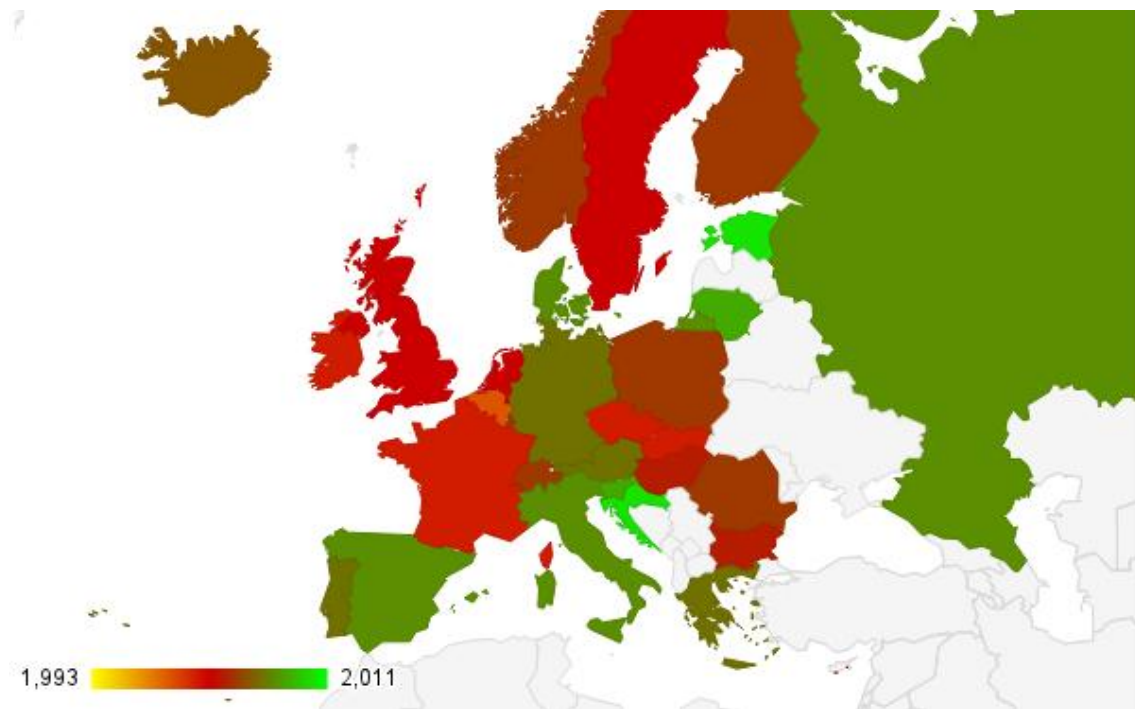
<sup>45</sup> Heckman, J., *The common structure of statistical models of truncation, sample selection, and limited dependent variables and a simple estimator for such models*, 5, ANNALS OF ECONOMIC AND SOCIAL MEASUREMENT, 475, 476-492, 1976.

Figure 1



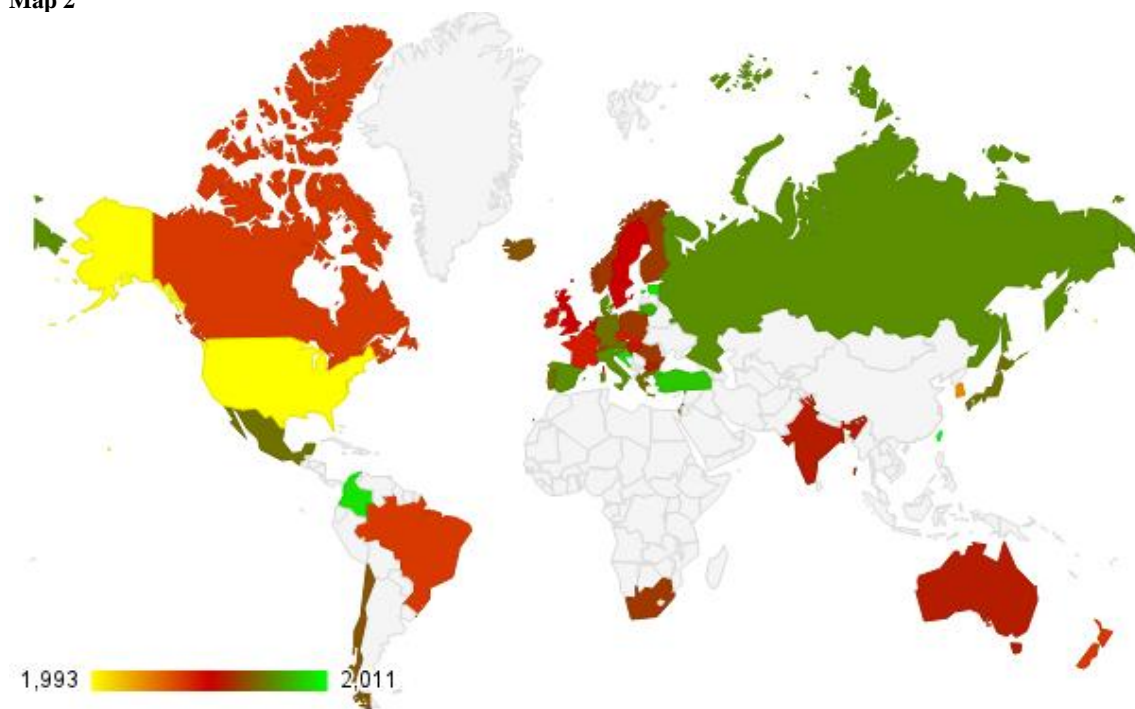
Source: Authors elaboration.

**Map 1**



Source: Authors elaboration.

**Map 2**



Source: Authors elaboration.

**Table 1. Descriptive Statistics**

<b>Variables</b>	<b>Mean</b>	<b>s.d.</b>	<b>Min.</b>	<b>Max.</b>	<b>Source and coding</b>
Antitrust effectiveness	5,53	1,25	2,10	8,59	IMD
Leniency	0,49	0,50	0,00	1,00	
Competition Law	0,94	0,24	0,00	1,00	
Age of Competition Law	22,91	25,40	0,00	122,00	
Per capita GDP*	15.889,14	12.889,07	419,40	56.389,21	Nominal prices. World Bank
Elections	0,25	0,44	0,00	1,00	1: Presidential elections
Countries that passed its first competition law between 1998-2007	0,15	0,36	0,00	1,00	
European Union (EU)	0,37	0,48	0,00	1,00	1: if the country is a EU member state
New country in EU	0,09	0,28	0,00	1,00	1: new EU member state 2004 enlargement
Nafta	0,06	0,23	0,00	1,00	1: if the country is a Nafta member
Asia-pacific	0,06	0,23	0,00	1,00	1: if the country is an ASEAN member
Andean Community	0,04	0,19	0,00	1,00	1: if the country is an Andean Community member
Mercosur	0,04	0,19	0,00	1,00	1: if the country is a Mercosur member
Regional agreement	0,56	0,50	0,00	1,00	1: Country included in any of the above regional agreements

Source: Author's compiled information unless stated. Unbalanced IMD sample of 59 countries during 14 years. 730 observations except for per capita GDP.

\*Taiwan for which data on per capita GDP is not available is excluded from the estimates using per capita GDP as control variable.



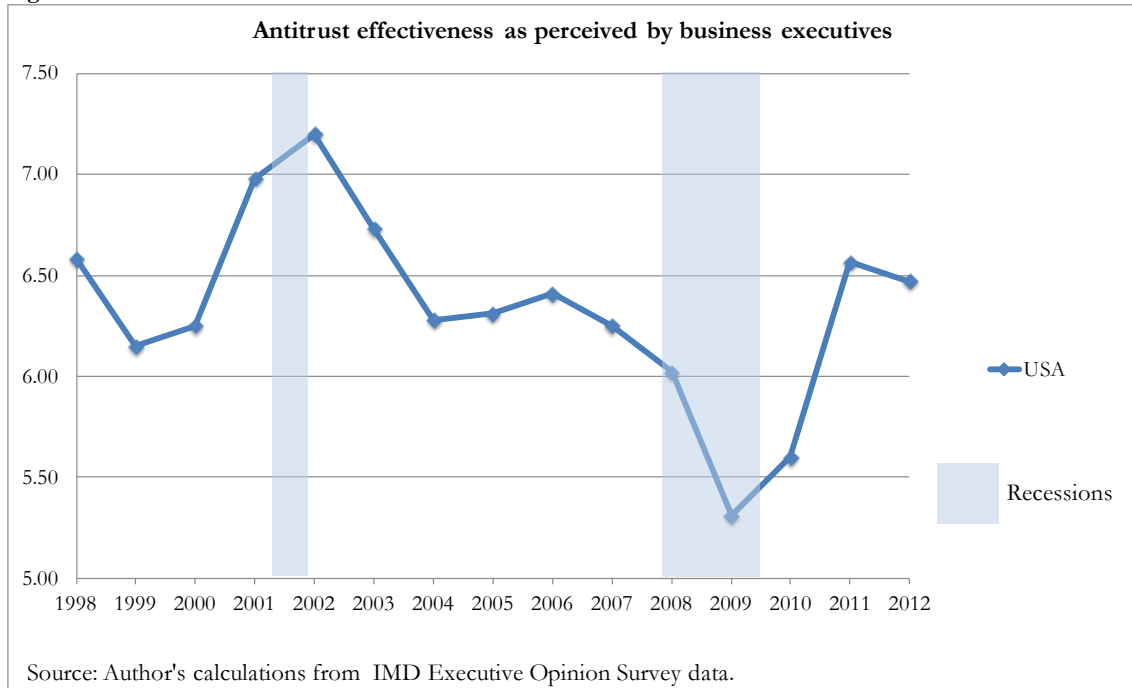
**Table 2.- Description of regional variables**

<b>Variables</b>	<b>Description</b>	<b>Countries for which the variable takes value 1 at least one year</b>	<b># countries</b>
European Union (EU)	1=The country is an EU member	Austria, Belgium, Bulgaria, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Lithuania, Luxembourg, Netherlands, Poland, Portugal, Romania, Slovak Republic, Slovenia, Spain, Sweden, United Kingdom	24
New country in EU	1= for new countries in EU since they entered into EU	Bulgaria, Czech Republic, Estonia, Hungary, Lithuania, Poland, Romania, Slovak Republic, Slovenia	9
Nafta	1=The country is a NAFTA member	Canada, Mexico, USA	3
Andean Community	1=The country is an Andean Community member	Colombia, Venezuela, Peru	3
Asia-pacific	1=The country is a member of the Asia-Pacific agreement	China Mainland, India, Korea	3
Mercosur	1=The country is a Mercosur member	Argentina, Brazil	2

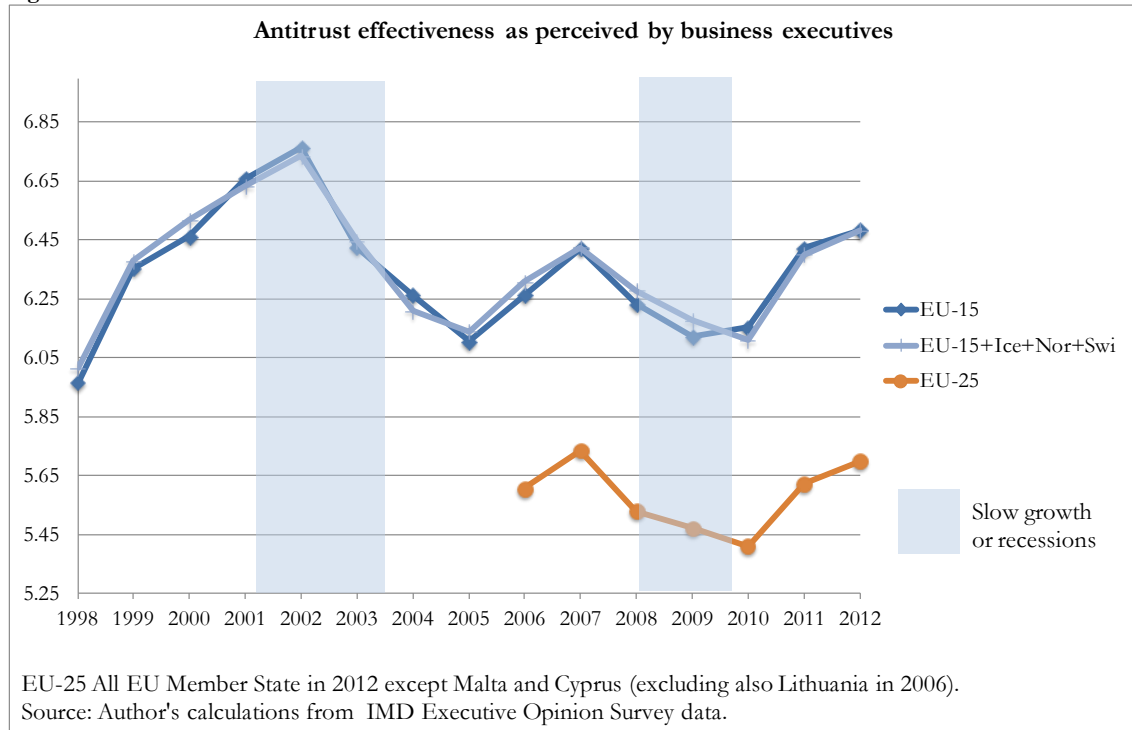
Note: 24 countries for which all those variables take value 0: Australia, Chile, Croatia, Hong Kong, Iceland, Indonesia, Israel, Japan, Jordan, Kazakhstan, Malaysia, New Zealand, Norway, Philippines, Qatar, Russia, Singapore, South Africa, Switzerland, Thailand, Turkey, United Arab Emirates, Ukraine.

Note: Venezuela leaves the Andean Community in 2006.

**Figure 2**



**Figure 3**



**Table 3.- Policy diffusion**  
**(59 countries in the IMD Survey in 2011)**

		<b>Countries with leniency program</b>	<b>% adopters</b>
1998		2	3%
1999		2	3%
2000	Early adopters	3	5%
2001		5	8%
2002		9	15%
2003		12	20%
2004	Middle adopters	16	27%
2005		25	42%
2006		28	47%
2007		34	58%
2008	Latecomers	38	64%
2009		40	68%
2010		41	69%
2011		44	75%

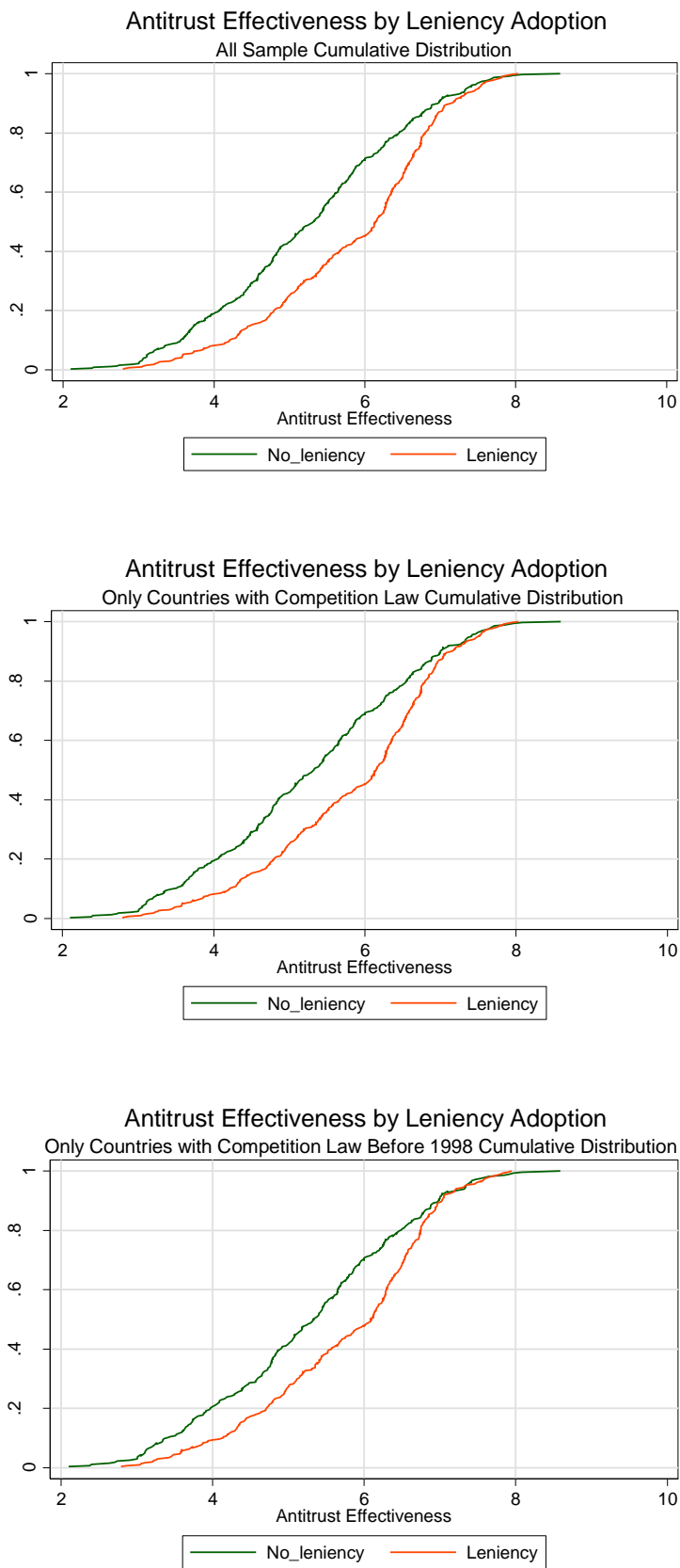
Source: Author's calculations based on Competition Authorities webpages and International Competition Network (ICN).

**Table 4. Mean antitrust effectiveness index**

No leniency	Leniency	Absolut e diff. T test of mean equality	Relative diff	two-sample Kolmogorov– Smirnov tests of the equality of distributions	Sample	# Obs
5.26 (1.28)	5.85 (1.15)	0.58 (0.09)***	11,22%	0.26***	All	730
5.29 (1.32)	5.85 (1.15)	0.56 (0.10)***	10,59%	0.24***	Only countries with competiton law	687
5.26 (1.31)	5.76 (1.15)	0.50 (0.10)***	9,51%	0.23***	Only countries with competiton law excluding those passing its first law between 1998-2011	577

Source: Own calculations. Note: \*\*\* 1%, \*\* 5%, \*10% significance test. Standard errors within brackets.

Figure 4



Source: Authors elaboration.

Table 5. Descriptive statistics by country (1998-2011)

Country	Antitrust Effectiveness Before Leniency (average)	Antitrust Effectiveness After Leniency (average)	Before and After Absolute Change	Before and After Relative Change	Year of First Competition Law	Year of Competition Law reform	Year of First Leniency Program
Finland	7,78	7,55	-0,23	-2,9%	1988	2004	2004
Germany	7,36	6,88	-0,48	-6,5%	1958	2005	2006
Denmark	7,33	7,68	0,35	4,8%	1998	No change	2007
Australia	7,27	7,25	-0,02	-0,2%	1974	No change	2003
Netherlands	7,17	7,08	-0,09	-1,3%	1998	2004;2007	2002
Austria	7,10	7,35	0,26	3,6%	1988	2006	2006
New Zealand	6,92	6,84	-0,08	-1,2%	1986	No change	2000
Norway	6,87	6,64	-0,23	-3,3%	1993	2004	2004
Canada	6,84	6,78	-0,06	-0,9%	1889	No change 2002;	2000
Ireland	6,57	6,41	-0,16	-2,4%	1991	2006	2001
Sweden	6,49	6,55	0,06	0,9%	1993	No change	2002
Iceland (*)	6,49	5,07	-1,42	21,9%	1993	2005	2005
Luxembourg	6,44	6,42	-0,02	-0,3%	1970	2004	2004
Chile	6,42	6,69	0,27	4,2%	1959	1999	2005
Singapore	6,40	6,71	0,31	4,9%	2004	2004	2004
USA		6,33			1890	No change	1993
France	6,22	6,28	0,06	1,0%	1953	2001	2001
Switzerland	6,21	6,26	0,05	0,8%	1995	No change	2004
South Africa	6,19	6,47	0,28	4,5%	1999	2001	2004
Israel	6,14	5,93	-0,21	-3,4%	1959	1988	2005
United Kingdom	6,07	6,12	0,05	0,8%	1948	2002	2002
Taiwan	5,81	5,89	0,08	1,4%	1992	2002	2011
Belgium	5,64	6,29	0,65	11,5%	1993	2006	1999
Spain	5,63	5,71	0,08	1,4%	1964	2007 2001;	2007
Estonia (*)	5,57	5,23	-0,34	-6,1%	1993	2006	2010
Malaysia	5,57				2012	No change	No Leniency
Hungary	5,51	5,41	-0,10	-1,9%	1996	No change	2003
Japan	5,44	6,35	0,91	16,7%	1947	2005	2006
Hong Kong	5,42				No law	No change	No Leniency

Country	Antitrust Effectiveness Before Leniency (average)	Antitrust Effectiveness After Leniency (average)	Before and After Absolute Change	Before and After Relative Change	Year of First Competition Law	Year of Competition Law reform	Year of First Leniency Program
United Arab Emirates (**)	5,40				No law	No change	No Leniency
Portugal	5,27	5,22	-0,04	-0,9%	1993	2003	2006
Korea	n.a.	5,27			1980	1997	1997
Jordan (*)	5,25				2004	2004	No Leniency
Greece	4,98	4,34	-0,64	-12,9%	1977	No change	2006
Brazil	4,97	5,14	0,18	3,5%	1988	2000	2000
Colombia	4,90	4,57	-0,33	-6,7%	1959	No change	2010
Mexico	4,89	4,09	-0,80	-16,4%	1992	2006	2006
India	4,87	4,73	-0,14	-2,8%	1969	2003	2003
Peru (*)	4,86				1991	2008	No Leniency
Italy	4,82	4,58	-0,24	-5,0%	1990	No change	2007
Turkey	4,81	5,83	1,02	21,2%	1994	2008	2009
China Mainland	4,80	4,36	-0,44	-9,2%	1993	2008	2011
Lithuania (*)	4,73	4,42	-0,31	-6,6%	1999	1999; 2004	2008
Slovak Republic (*)	n.a.	4,71			1994	2001	2001
Qatar (*)	4,64				2006	No change	No Leniency
Thailand	4,53				1999	1999	No Leniency
Kazakhstan (*)	4,48				2001	2008	No Leniency
Czech Republic	4,17	5,18	1,01	24,3%	2001	2001	2001
Philippines	4,06				No law	No change	No Leniency
Slovenia (*)	4,00	3,70	-0,30	-7,6%	1993	1999; 2004	2008
Venezuela	3,89				1992	1997	No Leniency
Indonesia	3,87				1999	1999	No Leniency
Romania (*)	3,73	4,13	0,40	10,7%	1997	2003	2004
Poland	3,55	4,46	0,91	25,8%	1990	2004	2004
Bulgaria (*)	n.a.	3,55			1991	2003	2003
Croatia (*)	3,53	3,76	0,23	6,5%	1995	2003	2010
Argentina	3,41				1923	1999	No Leniency
Russia	3,21	3,08	-0,13	-4,0%	1991	2006	2007
Ukraine (*)	2,85				1993	2001	No Leniency

Source: Own elaboration (from IMD World Competitiveness)

Note: Countries marked with (\*) did not have data available for all the period. Countries marked with (\*\*) only have data available for the last year.

European Commission introduced leniency in 1996, and reformed it in 2002 affecting not only EU Member States but also EEA Member States. Light green: Early adopters. Light red: Middle adopters.

**Table 6. Summary of results of the matching estimator (IMD original data)**

	Antitrust Effectiveness	Antitrust Effectiveness	Antitrust Effectiveness
in levels, original data			
Leniency	0.785***	0.681***	0.927***
Impact/Mean	14,92%	12,87%	17,62%
in logs			
Leniency	0.162***	0.140***	0.190***
Impact (%)	17,59%	15,03%	20,92%
Sample	All	Only countries with competition law	Only countries with competition law excluding those passing its first law between 1998-2011

Note: \*\*\* 1%, \*\* 5%, \*10% significance test. Standard errors within brackets.



**Table 7. Sensitivity analysis of the matching estimator (in levels, original data, only countries with competition law passed before 1998). Leniency. ‘Killer’ confounders**

	s=0.1		s=0.2		s=0.3		s=0.4		s=0.5		s=0.6	
	$\Gamma$	$\Lambda$	$\Gamma$	$\Lambda$	$\Gamma$	$\Lambda$	$\Gamma$	$\Lambda$	$\Gamma$	$\Lambda$	$\Gamma$	$\Lambda$
	<b>0,901</b>		<b>0,841</b>		<b>0,755</b>		<b>0,722</b>		<b>0,644</b>		<b>0,607</b>	
<b>d=0.1</b>	1,688	1,588	1,679	2,56	1,731	4,011	1,688	1,588	1,679	2,56	1,731	4,011
	<b>0,856</b>		<b>0,762</b>		<b>0,69</b>		<b>0,587</b>		<b>0,52</b>		<b>0,426</b>	
<b>d=0.2</b>	2,566	1,768	2,598	2,805	2,644	4,444	2,566	1,768	2,598	2,805	2,644	4,444
	<b>0,825</b>		<b>0,699</b>		<b>0,608</b>		<b>0,528</b>		<b>0,444</b>		<b>0,319</b>	
<b>d=0.3</b>	3,928	1,901	3,935	3,072	4,248	5,084	3,928	1,901	3,935	3,072	4,248	5,084
	<b>0,782</b>		<b>0,662</b>		<b>0,575</b>		<b>0,441</b>		<b>0,387</b>		<b>0,316</b>	
<b>d=0.4</b>	6,684	1,935	6,531	3,136	6,49	5,25	6,684	1,935	6,531	3,136	6,49	5,25
	<b>0,748</b>		<b>0,606</b>		<b>0,468</b>		<b>0,345</b>		<b>0,246</b>		<b>-0,027</b>	
<b>d=0.5</b>	11,986	2,157	10,931	3,66	11,931	6,742	11,986	2,157	10,931	3,66	11,931	6,742
	<b>0,699</b>		<b>0,538</b>		<b>0,362</b>		<b>0,244</b>		<b>0,146</b>		<b>-0,34</b>	
<b>d=0.6</b>	20,709	2,193	24,301	3,738	25,439	6,792	20,709	2,193	24,301	3,738	25,439	6,792

Note: Under the assumption that  $\Pr(U=1)=0.4$  and  $p_{11}-p_{10}=0$ , the differences  $d= p_{01}-p_{00}$  (which capture the outcome effect of U in the absence of treatment) and  $s= p_{1\cdot} - p_{0\cdot}$  (which captures the effect of U on the selection into treatment) uniquely define the parameters  $p_{ij}$ , with  $i,j=\{0,1\}$ . All ATTs are averaged over 100 iterations.  $\Gamma$  is the average estimated odds ratio of U in the logit model of  $\Pr(Y=1|T=0, U, W)$ ;  $\Lambda$  is the average estimated odds ratio of U in the logit model of  $\Pr(T=1|U,W)$ . The baseline estimate without confounder is equal to 0.927.

**Table 8. Difs-in-Difs Estimators. OLS. In levels, original data**

	<b>Antitrust Effectiveness</b>	<b>Antitrust Effectiveness</b>	<b>Antitrust Effectiveness</b>
Leniency	-0.009 (0.06)	0.0012 (0.07)	0.02 (0.07)
Country fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
EU leniency fixed effect	Yes	Yes	Yes
Observations	730	687	577
Sample	All	Only countries with competiton law	Only countries with competiton law excluding those passing its first law between 1998-2011

Note: \*\*\* 1%, \*\* 5%, \*10% significance test. Robust standard errors within brackets.

**Table 9. Difs-in-Difs Estimators. Binary IV Treatment. In levels, original data**

	<b>Antitrust Effectiveness (IV)</b>	<b>Antitrust Effectiveness (IV)</b>	<b>Antitrust Effectiveness (IV)</b>
Leniency	0.84 (0.13)***	0.87 (0.14)***	0.88 (0.16)***
Impact/Mean	15,97%	16,45%	16,73%
Country fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
EU leniency fixed effect	Yes	Yes	Yes
Observations	714	671	562
Instruments	New country in EU, Regional Agreements, GDP1, Elections1, Age competition law, Age competition law squared	New country in EU, Regional Agreements, GDP1, Elections1, Age competition law, Age competition law squared	New country in EU, Regional Agreements, GDP1, Elections1, Age competition law, Age competition law squared
Wald test of indep. Eqns.	68.47***	61.55***	44.12***
Sample	All	Only countries with competition law	Only countries with competition law excluding those passing its first law between 1998-2011

Note: \*\*\* 1%, \*\* 5%, \*10% significance test. Robust standard errors within brackets.

**Table 10. Difs-in-Difs Estimators. Binary IV Treatment. In logs**

	<b>Antitrust Effectiveness (IV)</b>	<b>Antitrust Effectiveness (IV)</b>	<b>Antitrust Effectiveness (IV)</b>
Leniency	0.18 (0.03)***	0.18 (0.03)***	0.19 (0.03)***
Impact/Mean	19,64%	20,12%	20,70%
Country fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
EU leniency fixed effect	Yes	Yes	Yes
Observations	714	671	562
Instruments	New country in EU, Regional Agreements, GDP1, Elections1, Age competition law, Age competition law squared	New country in EU, Regional Agreements, GDP1, Elections1, Age competition law, Age competition law squared	New country in EU, Regional Agreements, GDP1, Elections1, Age competition law, Age competition law squared
Wald test of indep. Eqns.	76.28***	67.32***	47.05***
Sample	All	Only countries with competition law	Only countries with competition law excluding those passing its first law between 1998-2011

Note: \*\*\* 1%, \*\* 5%, \*10% significance test. Robust standard errors within brackets.