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«ANGELO COSTA»
ECONOMICS UNDERGRADUATE
THESES AWARD
XVIth Edition

Preface
Gustavo Piga

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X-XII

CONFINDUSTRIA

«ANGELO COSTA»
ECONOMICS UNDERGRADUATE THESES AWARD
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RIVISTA DI POLITICA ECONOMICA
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Preface

In the year 2013, Rivista di Politica Economica (RPE) promoted the competition for the XVI[th] edition of the «Angelo Costa» Economics Undergraduate Theses Award consisting in the publication of the five most deserving papers taken from undergraduate theses in economics written by students who graduated in Italian universities between May 1[st], 2010 and October 30[th], 2012. This issue collects the 5 papers winning the competition.

The «Angelo Costa» Theses Award aims at drawing attention to the most promising graduates in Economics, awarding them with the publication of their paper in order to encourage studying and improve their post-graduate chances of admission to Master and/or Ph.D. programmes. We also wish this Award to bring the authors to the attention of a wider public, preventing that their works remain mere manuscript with a limited and random circulation as it often occurs.

The Award is named in memory of Angelo Costa, the first President of Confindustria (the Confederation of Italian Industry) in the immediate post-war period. He was elected President in 1945 and guided the organisation throughout the reconstruction period until 1955. Angelo Costa was again elected to chair the board of Confindustria from 1966 to 1970. A free-market advocate, on several occasions Angelo Costa firmly opposed the constraints imposed by statism and stressed the key role played by small and medium-sized enterprises in Italy's economic and industrial growth.

Ten graduates from seven Italian universities submitted papers for this XVI[th] edition. Two candidates graduated from "L. Bocconi" University, Milan and two from Siena University, one candidate gratuated respectively from "Cattolica del Sacro Cuore" University, Milan, LUISS "G. Carli" University, Rome, "Tor Vergata" University, Rome, Bologna University.

Each one of these papers was refereed in anonymous form by the Editorial Board of the journal and on the basis of their opinions the authors who qualified for the second stage of the contest were the following (listed in alphabetical order):

Alessandro Casini, Siena University, «Reconsidering Non-Keynesian Effects of Fiscal Consolidations over the Business Cycle»;

Ramona Dagostino, "L. Bocconi" University, Milan, «Are Short-Selling Bans effective? Evidence from the Summer 2011 European Bans on Net Short Sales»;

Paolo Giacomino, "Tor Vergata" University, Rome, «Are Sovereign Credit Ratings Pro-Cyclical? A Controversial Issue Revisited in Light of the Current Financial Crisis»;

Andrea Giovannetti, Siena University, «Financial Contagion in Industrial Clusters: A Dynamical Analysis and Network Simulation»;

Lavinia Piemontese, Bologna University, «The Spread of Mafia in Northern Italy: The Role of Public Infrastructure»;

Natasha Rovo, Luiss "G. Carli" University, Rome, «Housing Prices and Monetary Policy»;

Stefano Schiaffi, "L. Bocconi" University, Milan, «The Granularity of the Stock Market: Forecasting Aggregate Returns Using Firm-level Data»;

Giulia Tagliaferri, "L. Bocconi" University, Milan, «Information and Prevention of Sexually Transmittable Diseases: A Case Study on Non-Heterosexual Women».

These papers were then submitted – again in anonymous form – to the members of the International Scientific Committee who finally defined the winners of the 2013 competition. The Members of the International Scientific Committee for this edition were:

Prof. Kyle Bagwell (Stanford University);
Prof. Richard Blundell (University College London);
Prof. Michael Brennan (University of California in Los Angeles);
Prof. Heinz Kurz (University of Graz);
Prof. Axel Leijonhufvud (University of California, Los Angeles);
Prof. Charles F. Manski (Northwestern University);
Prof. Robert A. Mundell (Columbia University);
Prof. Lee E. Ohanian (University of California, Los Angeles);
Prof. Andrew Rose (University of California, Berkeley);
Prof. Bertram Schefold (J.W. Goëthe Universität Frankfurt am-Main);
Prof. Jean Tirole (Université des Sciences Sociales de Toulouse).

The five authors awarded with the 2013 "Angelo Costa" Undergraduate Theses Award are the following (listed in alphabetical order):
Alessandro Casini, Siena University, «Reconsidering Non-Keynesian Effects of Fiscal Consolidations over the Business Cycle»;

Ramona Dagostino, "L. Bocconi" University, Milan, «Are Short-Selling Bans Effective? Evidence from the Summer 2011 European Bans on Net Short Sales»;

Paolo Giacomino, "Tor Vergata" University, Rome, «Are Sovereign Credit Ratings Pro-Cyclical? A Controversial Issue Revisited in Light of the Current Financial Crisis»;

Lavinia Piemontese, Bologna University, «The Spread of Mafia in Northern Italy: The Role of Public Infrastructure»;

Stefano Schiaffi, "L. Bocconi" University, Milan, «The Granularity of the Stock Market: Forecasting Aggregate Returns Using Firm-Level Data».

Once again our initiative received appreciative comments in Italian and foreign academic circles and we would like to sincerely thank all those who gave their contribution to spread information on the Award. A special thanks for their personal direct and considerable commitment goes to the International Scientific Committee. The positive comments they expressed on the Award and the notable skill of the candidates encourage us and testify that the «Angelo Costa» Economics Undergraduate Theses Award is considered today among the important events capable of fostering and encouraging young Italian economists in their scientific studies by making them known to a broader public. The final choice of the winners, based on a criterion solely related to the quality of the manuscripts, is implemented by a double-blind refereeing procedure made by Italian and international economists who have given important contributions to the science of Economics and have acquired a rigorous capacity to evaluate scientific work over the years. Our guidelines for this Award can be summed up with two terms: merit and competition.

We believe these two characteristics have been assured by the rigour and transparency of the procedures adopted in the selection.

This issue of Rivista di Politica Economica also collects the profiles of the five winners of the XVI^th^ edition and a biographical update of the past-editions winners. We take this opportunity to congratulate our young colleagues and wish them great success in their future studies and professional activities.

THE MANAGING EDITOR
Prof. Gustavo Piga

WINNING PAPERS

Reconsidering Non-Keynesian Effects of Fiscal Consolidations over the Business Cycle

Alessandro Casini*
Siena University

This paper uses fiscal consolidation experiences of a sample of OECD economies over the period 1970 to 2008 to examine the interplay between fiscal adjustments and economic performance. The main purpose of this paper is to study whether the impact of fiscal consolidation on the real economy is not symmetric with respect to economic conditions. It follows Alesina and Ardagna (2010) and introduces a new approach based on business cycle. The results suggest time variation in the coefficients that describe the response of output to fiscal shocks. We find that fiscal austerity can be expansionary when it occurs in good times.
[JEL Classification: E60; H50; H60; H62].

Keywords: non-Keynesian effects; fiscal consolidations; business cycle; cyclical phases; asymmetric output responses; output-gap.

* <*acasini@bu.edu*>. I wish to express my gratitude to my supervisors Alberto Dalmazzo and Mario Tonveronachi. In particular, I am grateful to Alberto Dalmazzo for his constant monitoring and patience. I also owe a special acknowledgement to Riccardo Fiorito, for his suggestion to adopt an innovative approach introducing business cycle analysis on fiscal consolidations.
I wish to thank Alisdair McKay, Pierre Perron, Gustavo Piga and four anonymous referees for their precious comments. The usual disclaimer applies.

1. - Introduction

This paper is directly related to Alesina and Ardagna (2010) in studying the impact of fiscal austerity on short-run economic activity. As in Perotti (2013), and DeLong and Summers (2012), we argue that output responds asymmetrically over the business cycle. A new approach investigates on different effects of consolidation shocks when the impact of fiscal consolidation on real activity works through state-dependent variables referred to initial macroeconomic conditions. Hence, the main purpose is to distinguish the effects of the fiscal impulse according to the sign and movements of the output-gap.

Governments and Central Banks have implemented extensive support packages in response to the global crisis that started around September 2008. Discretionary fiscal measures, accompanied with automatic stabilizers (*i.e.* cyclical government revenue losses and public expenditure hikes), have generated acute rises in budget deficits and attendant accumulating public debts in many of the G20 countries. Despite the flimsy recovery seems to require fiscal stimulus, many governments are already devising, or in other cases, disputing exit strategies to assure fiscal sustainability in the coming decades[1].

However, fiscal adjustments have been required in the past for several reasons that not always coincide with the need of fiscal consolidation succeeding economic downturns.

This paper focuses on the short-term effects of fiscal consolidations. During the past the results of fiscal consolidation have been irregular, both along different time periods and across countries. Hence, there is no clear consensus regarding its short-run effects on output. According to traditional Keynesian theory, reductions in budget deficit can slow the pace of economic growth in the short-run and medium-run by reducing aggregate demand. Along these lines, a recent work by the IMF found that fiscal consolidation typically has a contractionary effect on output where a budget tightening equal to one percent of GDP generally reduces real GDP growth by about 0.5 percent within two years[2]. On the other hand, the literature on what has been termed *Expansionary Fiscal Contraction* (EFC) has provided both theoretical and empirical explanations for possible non-Keynesian effects of fiscal policy. The theoretical support comes from standard neoclassical models, modified to include features such as finite horizons and threshold effects (Bertola and Drazen, 1993; Blanchard, 1990; Perotti, 1999)[3].

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1 OECD, *Economics Department* (2010).

2 See IMF, *World Economic Outlook,* (October 2010, Chapter 3).

3 HJELM G. (2006).

The focus of this paper is on the empirical literature ultimately culminating in Alesina and Ardagna (2010) and Alesina *et* al. (2012), where the starting point was the case studies by Giavazzi and Pagano (1990) on the contractions in Denmark and Ireland in the 1980s. The authors found that during the years of fiscal retrenchment, the growth rate increased and unemployment decreased in both countries. The subsequent empirical *Expansionary Fiscal Contraction* (EFC) literature has achieved the conclusion that fiscal consolidations can be expansionary. This is most likely the case when deficit reduction is obtained through cuts in public spending rather than higher taxes.

There are several instances where an economic upturn coincided with fiscal contraction. The central question, however, is whether the upturn was due solely to the fiscal contraction or, at least partly, to other circumstances that had previously been ignored. In this paper, while studying the effects of fiscal consolidation on economic activity, we assume that output responses vary among different business cycle phases. This assumption finds clear validation in the recent empirical evidence provided by Perotti (2013); DeLong and Summers (2012) and Auerbach and Gorodnichenko (2012). Moreover, this seems to be consistent with the fundamental result in Favero, Giavazzi and Perego (2011). They argue that «There is no unconditional fiscal policy multiplier. The effect of fiscal policy on output is different according to the different debt dynamics, the different degree of openness and the different fiscal reaction functions in different countries».

Therefore, our goal is to verify if an equal fiscal impulse generates dissimilar effects on economic activity depending on the stage of the cycle at the time when the policy is implemented. For instance, we will evaluate whether it is the strength of the recovery – *i.e.* underlying cyclical growth forces – rather than the fiscal tightening itself, that is likely to affect positively output up to offset the standard Keynesian effects of fiscal policy. Empirical studies, such as Alesina *et* al. (2010, 2012) and IMF (WEO, 2010) provide contrasting evidences on the responses of short-term economic activity to fiscal consolidations. However, these studies do not account for the "state" of the economy at the time the fiscal adjustment is set up. On the one hand, Alesina and Ardagna (2010) investigate on the episodes of fiscal stabilizations selected on the basis of the conventional approach of "large" changes in the cyclically-adjusted primary balance (CAPB). On the other hand, the IMF identifies such episodes by following the "narrative approach" proposed by Romer and Romer (2007). As the IMF report states, both these two approaches suffer of some drawbacks which could unwillingly lead to a misguided selection of years with no relationship to actual change in fiscal policy and toward understating contractionary effects and overstating expansionary effects.

Our approach is thus to investigate on these asymmetries which imply time variation (depending on the phase of the cycle) in the coefficients measuring the impacts of fiscal consolidation shocks on aggregate activity. The distinctive feature of this approach is that it permits to evaluate the robustness of each effect of fiscal policy taking into account the independent cyclical development.

Finally, we investigate these issues using the Hodrick-Prescott decomposition (HP filter) of real output into trend and cyclical components. The structural representation in terms of trend and cyclical components allows for the introduction of fiscal policy variables such that policy actions have only short-run effects on the economy. This is a distinguishing feature from the majority of the literature, which generally proceeds by regressing output growth on measures of policy actions.

We find that a deficit cut of one percent of GDP reduces aggregate activity by 0.99 and 0.79 percent during contraction phases with, respectively, positive and negative output-gap. In contrast, when the economy is in expansion, the effect is not statistically significant. These results parallel the conclusions found in Auerbach and Gorodnichenko (2012).

Furthermore, as for government expenditure, a one percent cut in public spending raises output by around 0.30 percentage points when the economy is in a favorable business cycle position (*i.e.* positive output-gap). When the economy is instead growing at a rate below potential, a reduction in government spending does not lead to such non-Keynesian effects. Our results are consistent with the recent empirical evidence that fiscal policy has asymmetric effect on confidence and output growth during expansion and recessions (see Auerbach and Gorodniechenko, 2012; Bachmann and Sims, 2012; Barro and Redlick, 2011).

This paper is organized as follows. Section 2 contains a literature review and describes the data and methodology. Section 3 develops a business cycle analysis on fiscal consolidations by using the episodes identified by Alesina and Ardagna (2010). Section 4 concludes.

2. - Literature Review

There is no clear consensus on the main factors which are crucial in generating expansionary fiscal contraction. While Giavazzi and Pagano (1996) and Giavazzi, Pagano and Jappelli (2000) point out the importance of the size of the adjustments, other studies found that what matters most is instead the composition of the adjustment. From the latter point of view, fiscal consolidations based on spending cuts rather than tax revenue increases have a higher probability of showing expansionary effects, especially if expenditure cuts are concentrated on the public sector wage bill and on government transfers (Alesina, Perotti and Tavares, 1998; Alesina and Ardagna, 1998). In addition, as the initial state of public finance is concerned, some studies suggest the so called relevance of "emergency times": when debt-to-GDP *ratio* is high, fiscal consolidations are more likely to show non-Keynesian effects (Alesina and Ardagna, 1998; Perotti, 1999).

In summary, the results arising from such analyses need to be interpreted with caution for a number of reasons. First and foremost, there are problems in measuring and defining fiscal consolidation episodes. Evidence on the effects of fiscal policy on the economy is mostly based on three approaches and particularly these approaches differ in the way they select episodes of fiscal policy shocks. Firstly, the "narrative approach", proposed by Romer and Romer (2007) for the analysis of the impact of tax reductions on the US economy, is based on the idea of collecting single episodes of policy change and to record the timing and the magnitude of their (expected) effects, as reported by official documents (*i.e.* past budget laws, Economic Reports of the President, Congressional Records)[4]. This method relies on government official data and it is believed to effectively capture policy-makers' discretionary plans, or intentions. However, government's decisions as planned in the past may often fail to (fully) materialize[5]. A typical source of this problem arises when, for example, the government's projections at times of budgeting turn out to be considerably different from what is observed *ex-post*[6]. Despite this inconvenient, many studies have been conducted using this methodology in evaluating the effectiveness of fiscal policy[7]. Ultimately, the IMF staff (WEO,

4 ROMER C.D. and ROMER D. (2007) introduced the narrative approach to identify policy shocks on the tax side. They argue that other approaches are likely to find multipliers of tax changes which underestimate tax policy multipliers by treating as exogenous many policy changes that were actually responding to economic conditions or government purchases.

5 CIMADOMO J. (2008).

6 For instance, tax revenues depend crucially on the actual evolution of the tax base, which in turn follows the state of the economy. If the actual macroeconomic environment results substantially different from what the government expected, then the fiscal maneuvers, implemented on the basis of that forecast, could lead to unwanted impacts on the economy. Furthermore, when the government establishes interventions in terms of GDP *ratio* and the projections do not meet the real outcome, the ultimate effect could be far away from the original plan.

7 See, for instance, DEVRIES P. *et* AL. (2011); ALESINA A. *et* AL. (2012), and the IMF's WEO (October 2008).

2010) adopted the action-based approach in identifying episodes of fiscal activism taken to reduce the deficit in 15 advanced economies during 1980-2009. They identified 173 episodes in which there were budgetary measures aimed at fiscal consolidation. The average size of fiscal consolidation was about one percent of GDP per year, whereas fiscal contractions of more than 1.5 percent of GDP per year accounted for about one-fifth of all cases of consolidation. No dissimilarities were found on the estimated effects on output between large and small adjustments. For the overall sample, a key result was that fiscal consolidation was typically contractionary.

In contrast, Alesina *et* al. (2012), following the "narrative approach" and using the Devries *et* al. (2011) data, studies the effects of the adoption of multi-period fiscal consolidation plans – that is a combination of tax increases and spending cuts which can be either unanticipated or anticipated – and finds that adjustment based upon spending cuts are much more costly in terms of output losses than tax-based ones. Hence, concluding that what crucially matters is how the consolidation occurs. Fundamentally, the same result as in Alesina and Ardagna (2010) but using the "narrative approach".

The second approach has been pioneered by Blanchard and Perotti (2002). It involves identifying fiscal policy shocks using VARs and simulating the dynamic responses of the main macroeconomic variables to such shocks[8]. In those studies, a general result is a larger effect of government spending on GDP and eventually in some cases it suggests a crowding-in of consumption[9].

As mentioned early, the discretionary component of fiscal policy can be isolated by a third approach which relies on cyclical-adjustment method. Basically, the cyclically-adjusted primary balance is calculated by subtracting from the actual primary balance the estimated effect of business cycle fluctuations that is reflected on the fiscal account. This approach is also adopted by Alesina and Ardagna (1998, 2010).

A second problem in the empirical analysis of fiscal stabilizations is that these studies often do not properly take into account relevant factors, such as developments in monetary and exchange rate policies, which instead, have an important role in influencing the effectiveness of the ongoing adjustments. As a third, there is also a source of sample bias. In the past most of the actions of fiscal consolida-

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[8] However, these studies are not strictly directed to evaluate non-Keynesian effects of fiscal policy but rather to analyze the characteristics of fiscal multipliers. Difficulties in identifying policy shocks and the low frequency of fiscal data make the use of VAR for fiscal policy less frequent.

[9] See Blanchard O. and Perotti R. (2002) and Galì J. *et* al. (2007).

tion have been abandoned, once started, because of initial adverse output consequences. Therefore such cases are missed and in turn contractionary impacts are underestimated[10]. However, Alesina *et* al. (2012) make progress on this point by studying the effect of fiscal consolidation plans rather than individual shifts in fiscal variables.

Finally, a major problem arises from spurious relations and simultaneity issues. Interestingly, the output expansion following a fiscal tightening may be due to independent cyclical developments rather than to the other factors explained early, especially when fiscal consolidations are undertaken in weak phases of the cycle. In this sense, the relation between fiscal austerity and short-term economic activity may be uncertain. On the one hand, the expectations of a recovery (stronger after the trough of the cycle) may increase the likelihood of public finance consolidation[11]. But on the other hand, a persisting flimsy recovery would not allow premature budget tightening as it would likely hamper economic convalescence. Although this is a relevant point, the empirical research has not focused on it yet[12]. This paper will deal directly with this issue. The focus is on the asymmetric response of output, in relation to the phase of the cycle, to a certain action of fiscal austerity.

3. - Fiscal Adjustment Effects in Good and Bad Times

3.1. *Data and Methodology*

In this analysis, we investigate the episodes of large fiscal adjustments identified by Alesina and Ardagna (2010)[13]. We use a sample of OECD economies for the time period 1970-2008. The countries in the sample include: Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Portugal, Spain, Sweden, United Kingdom and United States[14]. In dating cyclical phases for each country, we use annual real GDP figures from OECD, Economic Outlook, no. 87.

10 Giudice G. *et* al.. (2003).

11 Giudice G. *et* al.. (2003).

12 Indeed, Alesina A. *et* al. (2012) check only for a potential endogeneity problem between the type of the adjustment and the cycle but not for the relationship between the cycle and fiscal adjustment. However, the results in Perotti R. (2013); Auerbach A. and Gorodniechenko Y. (2012); Bachmann B. and Sims E. (2012) and Barro R. and Redlick C. (2011) suggest that this argument is crucial.

13 For the episodes identified by Alesina A. and Ardagna S. (2010) see Alesina A. and Ardagna S. (2010) and Imf (WEO, page 115). Table 10 shows the episodes for a sample of 15 OECD economies.

14 For Germany the data are available from 1993.

Firstly, we date business cycles by identifying turning points, and phases of early/late economic upturn and downturn, in order to distribute the cycle in its four stages and calculate the relative frequencies. We use the "growth rate" cycle definition in referencing cycle chronologies, which tracks periods of cyclical upswings and downswings around an underlying trend. The identification of peaks and troughs relies on the growth rate of the time series. This is the difference between the "growth rate" and the "classical" cycle which, on the contrary, detects turning points by treating the time series levels. Different methods used for filtering a country's time series may outline different business cycle characteristics. In this paper we isolate fluctuations at business cycle frequencies using the HP filter with a smoothing parameter of 100[15]. We examine the business cycle properties of real GDP series for the panel of OECD economies using annual data at a constant prices over the period 1970-2010.

We focus on the estimates of cyclical component of GDP and on one dating rule. It defines a trough as a condition where one decline in the cyclical component of GDP is followed by an increase, *i.e.* at time t, $x_{t+1} > x_t < x_{t-1}$. Similarly, a peak is defined as a condition where one increase in the cyclical component of GDP is followed by a decline, *i.e.* at time t, $x_{t+1} < x_t > x_{t-1}$. This is a discrete version of the rule given for locating turning points in differentiable functions.

Discrete rule: peak at t if $\Delta x_t > 0$ and $\Delta x_{t-1} \leq 0$
Discrete rule: trough at t if $\Delta x_{t-1} \geq 0$ and $\Delta x_t < 0$

The dates of the peaks and troughs for all sample OECD real GDP series are reported in Appendix (Table 2)[16].

3.2 *Business Cycle Characteristics*

Once peaks and troughs are dated, a recession (contraction) is naturally defined as the period that lies between a peak and the following trough. The duration is simply given by the length (number of years). Graph 1 illustrates an example. In this picture, the series reaches a peak in period t and then it decreases

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[15] However, similar results arise when the smoothing parameter suggested by RAVN M. and UHLIG H. (2002) is applied. These, for brevity, are not presented.

[16] Table 2 displays peaks and troughs for a sample of twenty OECD economies. The data involve five additional countries in respect to the sample selected by ALESINA A. and ARDAGNA S. (2010).

by hitting its lowest point (trough) at time *t*+1. As it can be seen from the figure, the business cycle can be divided into four stages. The first stage "*expansion*" is between the points D-A. It represents the late stage of an economic upswing with positive output-gap and real GDP growth reaching its maximum deviation from trend after rising during a recovery. The second stage "*downturn*" is between A-B. It represents an early stage of a contraction with positive output-gap level and negative annul change from trend. The third phase "*protracted slowdown*" is B-C and it describes a late stage of a contraction with widening negative output-gap level. The fourth phase is called "*recovery*" and lies between C-D and shows an early stage of cyclical upswing with negative output-gap level and positive change in deviation from trend. According to these definitions, an upturn can be associated either with a positive or negative output-gap. Also a contraction can be associated either with a positive or negative output-gap. During the time period used for this dataset, a country business cycle passes through these phases cyclically according to the scheme A→B→C→D→A, etc.[17] A sample of twenty countries over the period 1970-2010 generates 797 annual observations which are distributed among the four phases as shown in Table 3. Additionally, Table 4 provides a summary of duration properties of the cycle. It presents descriptive statistics on the classical subdivision of the cycle into expansion and contraction. Obviously, an expansion (from trough to peak) is made up of both stages *recovery* and *expansion*. By contrast, a contraction (or recession) is constituted by both *downturn* and *protracted slowdown* stages.

3.3 *Estimating the Effects of Fiscal Consolidations*

The goal of this section is to study output responses to changes in fiscal policy. First of all, we must specify what indicator to use as a measure of government action. As mentioned early, discretionary fiscal policy refers to shifts in fiscal variables that can be unrelated to changes in economic activity. In short, fiscal policy consists of three components: *(i)* automatic stabilizers; *(ii)* discretionary fiscal policy that reacts to the state of the economy, and *(iii)* discretionary policy that is deliberately implemented for reasons other than current macroeconomic conditions[18]. It is fair to say that there is no overall agreement in the literature on which indicator is appropriate for measuring fiscal policy stance. In fact, a prob-

[17] See Table 1 for a summary on the properties of the four phases.
[18] FATÁS A. and MIHOV I. (2003).

lem arises from the simultaneity in the determination of output and the budget. To address this point, we focus on the government spending[19].

However, an alternative method is to construct a "cyclically-adjusted" fiscal balance related to a benchmark cyclical indicator and linking the budget balance to the state of the cycle relative to the benchmark. A large part of the literature, including Alesina and Ardagna (2010), adopts this approach. The idea is that changes in CAPB reflect merely policymakers' decisions since it is corrected for some cyclical behavior. On the other hand, the narrative approach may seem more reliable at first glance but it requires to examine carefully a voluminous documentation of political decision-making processes so as to pick up exogenous fiscal maneuvers implemented by the government. This means reviewing an overwhelming amount of reports, discussions and parliamentary records and, however, shortcomings may remain since it is actually difficult to distinguish external influences on the political decision-making process from what, instead, represents a deliberate government commitment.

Thus, in order to estimate the discretionary impulse of fiscal policy and, mostly important, for the sake of comparison with the work of Alesina and Ardagna (2010), we use the same variable definitions of the authors. Government spending and primary balance are defined as follows:

(1) *G=cyclically-adjusted current expenditure as a share of GDP = (Transfers + Government wage expenditures + Government non-wage expenditures+ Subsidies)/GDP;*

(2) *CAPB= cyclically-adjusted current expenditure as a share of GDP* (a negative sign indicates a deficit).

Now, with the episodes of fiscal adjustment and the cyclical phases identified, we develop the following two models by estimating the response of short-term economic activity to fiscal stabilization using panel data analysis.

[19] A standard approach in the literature is the use of government spending due to the empirical evidence that public expenditure does not vary much with the cycle.

MODEL 1:

$$\text{(3)} \quad y_{it} = \alpha_0 + \alpha_1 y_{it-1} + \alpha_2 CAPB_{it} + \alpha_3(D1_{it}CAPB_{it}) + \alpha_4(D2_{it}CAPB_{it}) + \alpha_5(D3_{it}CAPB_{it}) + e_{it}$$

where y is real GDP growth, *CAPB* is real first-differenced cyclically-adjusted primary balance as defined in *(2)*, $D1$, $D2$, $D3$ are dummy variables corresponding to the phase of the cycle as defined in the previous section, and e_{it}~$N(0,\sigma^2)$. For example, $D1$, equals to one when the economy is lying on the stage *downturn* (A-B). $D2$ and $D3$ are equal to one when the economy is on its *protracted slowdown* and *recovery* phase, respectively. Equation *(3)* controls for one lag of real GDP growth, to discriminate the impacts of fiscal stabilization policy from that of normal output dynamics. Furthermore, the cyclical component is captured by a collection of *dummies* ($D1$, $D2$, $D3$) where each one corresponds to the phase of the economy. The dummy variables included in equation *(3)* are set in accordance with earlier discussion on asymmetries. These dummy variables are meant to describe conditions prevailing at the time of policy action. With regards to fiscal policy shock, its effect is introduced with a collection of terms ($CAPB_{it}$, $D1_{it}$, $CAPB_{it}$, $D2_{it}CAPB_{it}$, $D3_{it}CABP_{it}$) which represent the interaction between consolidation shock and the phase of the cycle. State-variables are the parameters describing sign and size of the policy shock, and phase of the cycle, all at the time the policy is being performed. Since that these dummies represent the state of the economy during the adjustment year, other regressions are run by including one lag of each dummy and one lag in the *CAPB*. This is done in order to allow for a delayed impact of fiscal consolidation on growth[20].

Moving on to model 2, we follow the same approach but this time we substitute the cyclically-adjusted primary balance (*CAPB*) with government spending. In model 1, we evaluate the overall impact of fiscal adjustment on economic activity without distinguish between spending-based and tax-based adjustment. In model 2, government spending is used as a proxy for discretionary fiscal policy.

MODEL 2:

$$\text{(4)} \quad y_{it} = \alpha_0 + \alpha_1 y_{it-1} + \alpha_2 G_{it} + \alpha_3(D1_{it}G_{it}) + \alpha_4(D2_{it}G_{it}) + \alpha_5(D3_{it}G_{it}) + e_{it}$$

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20 This estimation is in column (4), Table 5.

where y is real GDP growth, G is real first-differenced government spending as defined in *(1)*, $D1$, $D2$, $D3$ are dummy variables corresponding to the phase of the cycle and e_{it}~$N(0,\sigma^2)$[21]. The same discussion for the model 1 applies here.

Indeed, we wish to explain not only the response of short-term output to consolidation shocks but also its changes with respect to the asymmetries introduced in the previous section. In other words, the final goal is to determine how response coefficients, measured by the terms of α's, are affected by the size of the fiscal policy shock and the state of the economy in the business cycle.

In pursuing this objective we investigate the same fiscal contraction episodes identified by Alesina and Ardagna (2010) where they found that fiscal adjustments associated with higher GDP growth are those in which a larger share of the reduction of the primary deficit-to-GDP *ratio* is due to cuts in current spending. We will test whether this holds in every business cycle phase. And whether a certain cut in government spending produces different effects on short-term output depending on which phase the economy is at that time. Discovering dissimilar impacts, as the economy is growing at a rate above/below its potential or when it is facing an expansion/contraction, is the principal aim of this section.

However, like Alesina and Ardagna (2010), here we do not deal with simultaneity issues. Also the other previous studies on fiscal consolidations do not claim to have solved them. Alesina and Ardagna (2010) argue that their findings confirm the evidence observed in their statistical analyses. Moreover, they outline that their goal is not to reach conclusions on the size of fiscal multipliers but, instead, they only aim at studying how the different compositions of the fiscal adjustments can generate different effects on output[22].

Tables 5 and 6 present the results from the estimations of models 1 and 2, respectively. Table 5 presents estimations of the overall effects of fiscal consolidations on GDP growth by studying changes in the cyclically-adjusted primary balance (*CAPB*). A positive change in *CAPB* is interpreted as an estimated size of the fiscal consolidation. Despite Alesina and Ardagna (2010) found no evidence in favor of the size of fiscal action, the IMF (WEO, 2010) points out that a decrease in the primary budget equal to one percent of GDP typically reduces real

[21] For more details on the dummy variables see discussion of model 1 or Table 1. A graphic illustration is given in Graph 1.

[22] For a further discussion on simultaneity issues see the end of this section and the sensitivity analysis conducted in Section 3.4.

GDP growth by about 0.5 percent within two years. A similar result is reproduced in column (1) and taken as a benchmark. In columns (2)-(4), fiscal consolidation appears to generate dissimilar effects. It shows typical Keynesian features during a contraction. In column (2), this is captured by the negative and highly significant coefficients of *CAPB* when interacted by the dummies *D*1 and *D*2 (-0.997 and -0.790, respectively). It implies that when the government reduces the budget deficit – *i.e. CAPB* increases through tax hikes or lower public spending – economic activity falls as the economy is facing a contraction. By contrast, despite the coefficients for the fiscal policy shocks are still negative when interacted with the other two upswing phases (*expansion* and *recovery)*, they are statistically insignificant.

Moving on to column (4), we focus on delayed impact of fiscal consolidation on growth. The objective is to measure the responses of output one year after the adjustment. The results are less strong and the direction of impacts is ambiguous. Indeed, when including lags of the interaction between fiscal shocks and cyclical phase the estimates might be biased. A positive (negative) coefficient for fiscal consolidation does not necessarily imply that the economy in t+1 benefits (gets worse) from fiscal consolidation in t but it may simply occur from following the independent business cycle path. For instance, when the adjustment occurs in the phase C-D, where the economy is growing toward the next peak after having hit a trough, the coefficient is unexpectedly positive (0.389). In phase C-D the economy is in an initial upturn, going toward the following peak even though the output-gap is still actually negative. This phase is certainly succeeded by years of growth either below or above the potential but, however, the economy is bound to grow up, at least, until the next peak. In other words, the positive coefficient for fiscal policy shock at time (when the economy is in C-D) on real GDP growth in may be distorted by cycle fluctuations. In fact, one can surely expect that, one year after the economy is in C-D, output will increase following its cyclical path[23].

[23] Such a bias may result only on regressions including lagged variables (*i.e.* column (4) in Table 5) but not when measuring the contemporaneous impact of the fiscal shock on output both at time t. The change in fiscal policy stance is likely to affect output predominantly in the year in which the fiscal adjustment is implemented. Therefore, the impact of the adjustment in may fade slowly and not influencing very much output in t and consequently making room for the independent business cycle development. On the other side, when studying the simultaneous impact of fiscal adjustment at time t on output at time t, what rather remains is the issue of causality. However, like Alesina A. and Ardagna S. (2010), we do not claim to have solved this problem. More discussions on causality issues at the end of this section. An attempt to deal with causality is provided in Section 3.4.

Moving on to the second model, column (1) in Table 6 reports the basic estimation of Alesina and Ardagna (2010). They outline that it is the composition of fiscal adjustment, more than its size, that matters for growth. Moreover, they argue that fiscal retrenchment based on spending cuts, rather than those based on tax hikes, are much more likely to boost economic activity in the short-run. Accordingly, as shown in column (1), government current expenditure has a negative coefficient of 0.24 which is statistically significant at all confidence levels. By contrast the effect of taxes is not significant. We reported this estimation for the sake of comparison and it actually serves to introduce the other regressions turned to assess what happens when asymmetries in the business cycle are considered.

First of all, the responses to government spending shocks show consistent differences among the four phases of the cycle. In columns (2) and (3) government spending has a negative and significant coefficient of 0.28 and 0.35 respectively, suggesting that in the phase D-A (which is the base case for the interaction between phase and fiscal policy shock – *i.e.* where each of the three dummies *D*1-*D*3 assumes value zero), where the economy is in *expansion* and it is growing at a rate above potential (positive output-gap), government spending cuts affect positively economic activity[24]. By contrast, a positive coefficient is shown when government expenditure reductions occur in *recovery* (C-D) – *i.e.* when the economy is again in an economic upturn but this time its actual rate of growth is below potential (negative output-gap). It means that in phase C-D, immediately after a trough has been hit, the assumption that spending cuts favor growth does not hold. In fact the coefficient is positive and highly significant so that it confirms the standard Keynesian view – in practice, output rises when public spending increases.

Along these lines, it should not surprise the non-significant effect when the economy contracts (phase from peak to trough [A-B and B-C]). Indeed, in this region the economic activity is falling and so a fiscal consolidation may not actually be appropriate even though in general spending-based adjustments may have positive impacts on growth. These two contrasting forces explain the non-significant reaction of output when the economy is in a cyclical downswing. It indicates that when the economy is in recession governments should increase spending so as to stimulate activity rather than trying to stabilize public finance.

These results are robust. The estimates also hold in columns (4) and (5) where only the interactions between the business cycle and fiscal policy shock are con-

[24] However, the effect is strictly contemporaneous and lagged spending shock is insignificant. For brevity, the estimations of delayed impacts of government spending reductions on GDP growth are not reported in Table 6.

sidered, while leaving out the state-dummy *D*1-*D*3. In column (4), after removing the state-dummies, we introduce *contraction,* which is a dummy equal to one when the economy is in contraction. Basically, it represents phases *downturn* and *protracted slowdown* (A-B and B-C) together without distinguish between positive or negative output-gap. Since contractions are less frequent and sporadic, and account for less than one third of cases of the total sample (31.36 percent as shown in the last row in Table 3), taking them into account together is an additional way to test their significance[25].

The specification in Table 7 is a reparametrization of models 1 and 2. It studies the asymmetric response of output to the interaction of fiscal shocks and cyclical conditions, depending on whether the latter are favorable or unfavorable according to the output-gap. This is done including a discrete variable equal to one when the output-gap is positive[26]. There is a growing evidence that fiscal variables react asymmetrically to positive and negative cyclical conditions. However, the main interest here is to evaluate whether the sign of the output-gap is associated with different effects on GDP growth for a particular fiscal impulse. In column (1), a one percent improvement in *CAPB* decreases output by 0.74 percent when the output-gap is negative. This coefficient is statistically significant at conventional confidence levels while when the output-gap is positive a similar shock raises GDP growth by 0.55 although not significant. Columns (3) and (4) suggest less evidence for spending shocks.

To sum up, Tables 5 and 6 draw the following picture: there are important divergences in output responses to fiscal policy shocks within episodes of consolidation. It is interesting to compare the results from this study with its counterpart conducted by Alesina and Ardagna (2010); Alesina *et* al. (2012), and also by the IMF staff. Although they find contrasting results, we provide evidence that there is no unique direction of output response to consolidation shocks. In Table 6, this has been shown by the negative coefficient for spending shock when the economy is growing fast compared with the positive coefficient in *recovery* (negative output-gap). In the former case, a fiscal consolidation based on spending cuts produces positive effects on short-term output. In *recovery,* instead, spending cuts are followed by a fall in economic activity. Therefore, a relevant result is that spending-based fiscal stabilization is not primarily associated by an expansion of

[25] However, the results do not change.

[26] The output-gap is measured as the difference between the logarithm of real GDP and the long-run trend (as given for instance by the Hodrick-Prescott filter) of the logarithm of real GDP.

the economy in the short-term but instead asymmetries are present over the business cycle. These first evidences are also found in Auerbach and Gorodnichenko (2012) and Perotti (2013).

In Table 5, the results indicate that the impact of the size of the adjustment as measured by an increase in *CAPB* changes direction whether it occurs during an expansion or contraction. In contraction, deficit cuts harm the economy as outlined by negative coefficients for fiscal shocks when interacted with *D*1 and *D*2. Finally, divergences also arise when the output-gap is concerned. Indeed, fiscal consolidations are less contractionary when growth is above potential.

As mentioned early, however, here concerns may be raised from the presence of simultaneity and endogeneity issues. Since real GDP and fiscal policy variables are likely determined simultaneously, this could possibly lead to reverse causality and measurement errors. The endogeneity of fiscal variables may arise for two reasons. Firstly, automatic stabilizers are directly related to output and income and thus fluctuate with the business cycle. Since changes in fiscal policy affect the business cycle and *vice versa,* neglecting such endogeinity may yield estimates of the coefficients of the fiscal equations that are biased towards zero, and consequently, the structural component of the budget may be overstated.

A second potential source of bias emerges when policy making process involves policy rules. In this sense, government decisions may be made on the basis of the output-gap through a feedback policy. It means that also fiscal rules on their part respond to the business cycle.

However, in their work, Alesina and Ardagna (2010) did not try to manage this problem but merely explained that their results were on the same line of that found in their statistical analysis. The authors first used the cyclically-adjusted primary balance (CAPB) in order to select episodes of consolidations that do not reflect the automatic reaction of the budget balance to economic fluctuations. Then they decided not to deal with endogeneity issues for the following reasons. Although the state of the economy affects discretionary policy choices of fiscal authorities, they noted that, for how the budgeting plans become effective, their assumption that the cyclically-adjusted primary balance is not related to GDP growth seems to be reliable. In other words, they observe that the budget for the current year is approved during the second half of the previous year and that the additional measures implemented in the ongoing year begin to produce effects, at least, with some delay, generally around the end of the fiscal year. Additionally, they assume that even though the implementation of fiscal adjustment is endogenous to macroeconomic conditions, the choice of what measures to take relies on political preferences and parliamentary bargains which are likely to be exogenous to the state of the economy. Therefore they state that their study does not involve the investigation of the size of fiscal multipliers but solely focuses on the effects of different compositions of fiscal consolidations.

In this sense, here, we follow Alesina and Ardagna (2010). We do not claim to have solved these issues and thus our analysis does not aim at obtaining precise estimates of the size of fiscal multipliers but it rather concentrates on the asymmetric impact of fiscal consolidation over the business cycle[27]. However, next section provides an attempt to address endogeneity and simultaneity biases. So far in models 1 and 2 GDP growth and business cycle phases have been considered to be exogenous – *i.e.* given with respect to fiscal consolidations shocks. Instead, good and bad times can be endogenous with respect to fiscal policy shocks. Section 3.4 will try to address this endogeneity issue by means of GMM estimation.

3.4 *Robustness*

To check on the robustness of the results we have experimented with alternative specifications of the basic model 1. These are presented in Table 8 in Appendix[28]. First, like Alesina and Ardagna (2010), in order to control for the influence of monetary policy and for exchange rate devaluations, we have included among the regressors the change in interest rate on deposit and the rate of change of the nominal exchange rate, respectively[29]. Second, we have calculated the change in the gross debt-to-GDP *ratio* and used the initial debt level to account for impact of existing conditions of public finances[30]. None of these rearrange-

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27 Indeed, such a choice of not dealing directly with causality issues is commonly made by almost all the empirical literature on fiscal consolidations. One exception is GIAVAZZI F. *et* AL.. (2005) where endogeneity of fiscal variables is treated by using the full employment government surplus net of interest payments as an instrument for net taxes.

28 For brevity, the sensitivity analysis conducted for model 2 is not reported since it does not change the main findings.

29 Indeed, the two most well-known episodes of expansionary fiscal contractions (Denmark and Ireland in the 1980s) were both preceded by exchange rate depreciations. On this point see ALESINA A. and ARDAGNA S. (1998) and ZAGHINI A. (1999). However, in a more recent work, PEROTTI R. (2013) recognizes that in his four case studies of "exchange rate stabilizations" (Denmark, Ireland, Finland and Sweden) fiscal consolidations were probably a necessary condition for output expansion given the respective economic situation of these countries at the time of policy action.

30 In the theoretical models by BLANCHARD O. (1990) and PEROTTI R. (1999) the initial level of the debt is crucial for private consumption response to fiscal tightening. ALESINA A. and ARDAGNA S. (1998) also control for the state of public finances.

ments fundamentally affect the main findings. For instance, the signs of $D1_{it}CAPB_{it}$ and $D2_{it}CAPB_{it}$ remain significantly negative. Among the controls introduced here, only the initial level of the debt seems to be statistically significant (although at ten percent level). However, it must be acknowledged that when the level of initial public debt and the change in debt-to-GDP *ratio* are included the estimates are slightly inferior. A problem with these variables is that for some countries no data on gross public debt are available.

A fourth more important change is that in this section we try to address endogeinity concerns by using instrumental variables and the generalized method of moments (GMM) estimation. Fiscal policy variable (*CAPB*) is instrumented using institutional variables as suggested by Fatás and Mihov (2003). The literature on political business cycles argues that the type and ideology of the government in office affect fiscal policy variables. And through fiscal policy variables, institutional variables can affect the state of the economy. For this reason, one can assume that political variables do not impact output growth directly. Therefore cyclically-adjusted primary balance (*CAPB*) is instrumented using the political characteristics of the government in office. The instruments are *Left* and *Centre*. These are dummy variables equal to one if the government in office is left or centre oriented and zero otherwise[31]. Following Agnello and Cimadomo (2009) other instruments may be the lagged public debt, the lagged change in debt-to-GDP *ratio*, lagged primary balance and the lagged cyclical phases[32]. The overidentifying restrictions of the system are not rejected at conventional confidence levels. Furthermore, the results are consistent with OLS estimation. The Hausman test indicates the feasibility of OLS estimation since these estimates are not significantly different from the IV estimates as shown in Table 8.

To sum up, the rearrangements conducted in this sensitivity analysis do not change the conclusions for the asymmetric response of short-term output to fiscal consolidation over the business cycle. Moreover, it provides evidence that the results are robust also when simultaneity bias is taken into account.

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31 See ALESINA A. and PEROTTI R. (1995) for an examination of political variables over episodes of consolidations.

32 For an empirical application on these issues see also GALÌ J. and PEROTTI R. (2003) and DEBRUN X. *et* AL. (2008).

3.5 *An Extension*[33]

In this subsection we ask whether the asymmetric responses of output growth to fiscal policies we found above can actually be decomposed into supply-side and demand-side components of output. Specifically, we use our models (1) and (2) to test whether fiscal consolidations have a different impact not only across business cycle phases but also within the major components of GDP. GDP is thus decomposed into a measure of labor productivity – output divided by total hours worked – and total hours worked. According to the Keynesian view, fiscal policy should have a larger impact on the demand-side rather than on the supply-side factor of aggregate output.

In order to investigate this point, we include in our models the dependent variables y^n and n, which represents real GDP per hour worked and total hours worked, respectively[34].

The results are shown in Table 9. It is interesting to evaluate these results taking into account those drawn from Section 3.3. In columns (1)-(3) the dependent variable is the growth rate in real GDP per hour worked while in columns (4)-(7) the dependent variable is the percentage change in total hours worked. A remarkable feature can be observed from comparing column (1) and (4). In column (1), the coefficients on *CAPB* (in either phase of the cycle) are statistically insignificant. In contrast, in column (4) the coefficients on *CAPB* when interacted by the dummies *D*1-*D*3 have negative and significant coefficients (-1.466, -0.659 and -0.279). This implies that a reduction in the budget deficit (either through tax hikes or public spending cuts) has no effect on the productivity component of real GDP but has a negative effect on total hours worked. Moreover, the magnitude of this effect is larger when the economy is facing a contraction. This fact holds true also in column (7) where we introduce some control variables.

On the other hand, spending-based fiscal adjustment seems not to display such evidence. In fact, the corresponding coefficient estimates are statistically insignificant as shown in column (2) and (5). Cuts in public spending have an asymmetric impact both on productivity and on hours across the four phases of the cycle but these effects are all not significant except for one coefficient in col-

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[33] The question addressed in this subsection is due to an anonymous referee.

[34] Nekarda C. and Ramey V.A. (2011) study the effects of government purchases at the industry level. The authors find that an increase in government spending raises output and hours worked but actually lowers labor productivity. They conclude that these findings are more consistent with the effects of government spending in the neoclassical model than the textbook New Keynesian model.

umn (2). In particular, we note a surprising negative and highly significant coefficient on government spending when interacted by the dummy *D*2 (-0.602). This suggests that during phase B-C when the economy is in the worst-case scenario, a reduction in government expenditure has a positive effect on productivity. This seems to support a “crowding-out” effect of public spending on (more productive) private investment when economic activity is falling and running below potential (*i.e.* negative output-gap). Non-Keynesian effects of fiscal consolidation would include such possibility. However, when the (negative) impact of cuts in public spending on hours worked adds to this (positive) impact on productivity during this phase of the cycle, the first effect would dominate according to our estimation in the previous section. The overall impact of government spending cuts on output would thus be contractionary when economic activity is falling.

From this extension we can draw the following conclusions: (*i*) fiscal adjustments based on overall reduction in the primary balance are likely to have a larger negative impact on hours worked than spending-based adjustments, which may arise because of the negative distortionary effect of taxes; (*ii*) according to the Keynesian argument, fiscal consolidations based upon either tax hikes or spending cuts have no significant effect on labor productivity, thus remarking the importance of the demand-side component of aggregate output[35]; (*iii*) more interestingly, we found some evidence for non-Keynesian effects of fiscal consolidation on labor productivity during a contraction but when combined with the (negative) impact on hours the latter seems to dominate[36].

3.6 *Final Remarks*

The primary goal of this section was to investigate the asymmetries that may arise in the real economy as a response to shocks to fiscal policy during episodes of consolidation in a sample of OECD economies. Our approach helped to reproduce phenomena such as time variability of the fiscal impulse and sensitivity to the regime observed when the initial shock is produced.

It has been shown that fiscal adjustments tend to be expansionary when they rely on spending cuts since it is the composition of the adjustment that matters for growth. Alesina and Ardagna (2010) found that fiscal consolidations based

[35] This result is consistent with Nekarda C. and Ramey V.A. (2011).

[36] The conclusions drawn from this subsection provide new meaningful insights that would need to be further explored in future research.

on cuts in current spending, government wage and non-wage components, and to subsidies are likely associated with higher real GDP growth.

On the other hand, the IMF staff finds little support for faster growth as a result of fiscal stabilization. They argue that stabilization has typically contractionary effects on economic activity leading to lower output and higher unemployment.

In this section, in order to provide comparable evidence to Alesina and Ardagna (2010), we investigated the response of cyclical component of output to government spending and fiscal policy shocks during the episodes of large fiscal adjustments identified by the authors. The results strongly suggest variation in the coefficients that describe the response of output to fiscal shocks according to cycle phases. Models 1 and 2 show evidence that this time variation can be well explained by a dummy variable indicating the phase of the cycle at the time the policy is implemented.

The results seem to be robust, for that the evidence contains a relatively large number of changes in the estimated regressions. It is worth noting that substituting the dummies *D*1, *D*2 and *D*3 with one dummy related to either expansion or contraction shows even more support to the findings in Section 3.3.

In conclusion, in contrast with the results found by Alesina *et* al. (2010, 2012), we present evidence that short-term output responds in different ways to consolidation shocks. A fiscal impulse has dissimilar impacts on GDP growth as it occurs in different phases of the cycle. This result is also found when this methodology applies to government spending. Our conclusions are consistent with Perotti (2013); Auerbach and Gorodniechenko (2012); Bachmann and Sims (2012) and Barro and Redlick (2011).

4. - Conclusions

Using a panel of OECD data from 1970 to 2008, this paper investigates the determinants of the different outcomes observed around episodes of fiscal adjustments in a sample of OECD economies. An extensive empirical and anecdotal evidence of episodes of non-Keynesian effects of fiscal consolidations still lacks convincing explanations. The literature on *Expansionary Fiscal Contraction* argues that a policy of tight fiscal retrenchment does not need to cause depressing economic effects. A sharp fiscal consolidation can reduce premiums on interest rates and boost investment spending, and expectations of lower future tax liabilities can encourage consumption and investment; both these forces can stimulate economic activity.

However, for reasons made clear in Section 3, this paper does not reach the same conclusion. The results in Section 3 call into question (*i*) the homogeneous reaction of output and (*ii*) the appropriate "timing" of fiscal consolidations, both over the cycle. We use a new approach which investigates on asymmetric responses of short-term output to fiscal consolidation shocks. As also suggested by Perotti (2013) and Favero, Giavazzi and Perego (2011), we argue that expansionary effects from fiscal consolidations depend on the existing business cycle position. We find that a one percent increase in *CAPB* reduces aggregate activity by around 0.90 percent during contractions. In contrast, when the economy is in upturn, the effect is not statistically significant. Furthermore, as for government expenditure, a one percent cut in public spending raises output by around 0.30 percentage points when the economy is in a favorable business cycle position (*i.e.* positive output-gap). When the economy is instead growing at a rate below potential, a reduction in government spending does not lead to such non-Keynesian effects. Recent empirical evidence as in Auerbach and Gorodniechenko (2012); Bachmann and Sims (2012) and Barro and Redlick (2011), supports these conclusions.

These evidences arise from the episodes identified by Alesina and Ardagna (2010) where, by contrast, they found that fiscal consolidations based on cuts in current spending, government wage and non-wage components, and to subsidies are likely associated with higher real GDP growth. However, such deviation in our results emerges because we consider the key influence played by the existing business cycle phase at the time the policy action is applied. And, in turn, we can detect such asymmetries.

Concerns with regards to (remaining) causality issues have been addressed with instrumental variables estimation using data on political variables of the government in office as instruments.

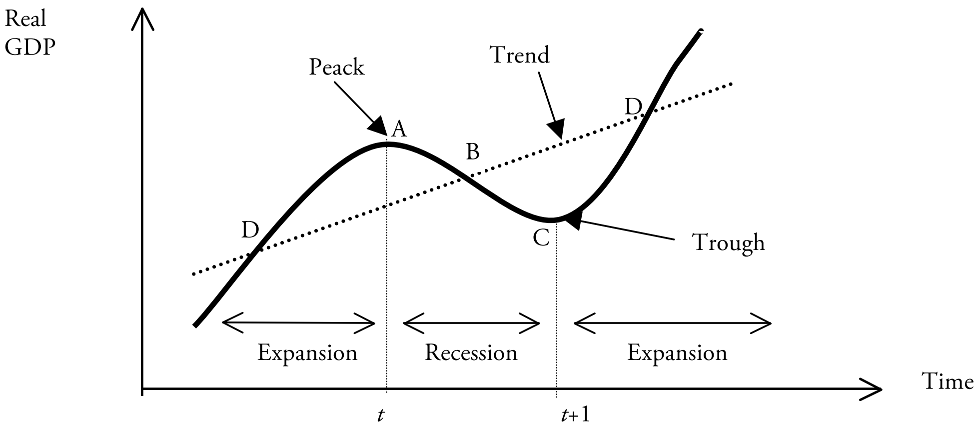
In sum, contrary to the previous empirical findings on non-Keynesian effects, this paper provides evidence that expansionary effects of fiscal austerity do not stem from the composition or the size of the fiscal tightening but what rather matters is the "timing" of fiscal austerity with respect to the business cycle phase related to a particular existing macroeconomic position. In our analysis, contractionary fiscal shocks lead asymmetric effects that exhibit a time dependence property. It implies that a certain fiscal impulse can produce either positive or negative effects on short-run output depending on the cyclical conditions and on the sign and magnitude of the output-gap. In particular favorable circumstances, a fiscal consolidation can result in line with the *Expansionary Fiscal Contraction* hypothesis. On the other hand, in adverse cyclical conditions, a standard Keynesian macroeconomic explanation is demanded. Therefore growth-boosting effects or, even better, non-Keynesian effects are to be searched in the interaction between fiscal policy shocks and the current phase of the business cycle.

In light of these findings, the policy implication that emerges could, in short, be said to be the following: if a country needs to undertake measures of fiscal austerity, policymakers should first focus on a close evaluation of the existing macroeconomic conditions and secondly, as many studies suggested, on the opportunity to effectively use exchange rate and monetary policies. Keeping in mind that fiscal austerity works differently in expansion and contraction, fiscal actions with the goal of consolidation should be implemented in good times. Specifically, when the economy is in an economic upswing and further, as it is being accompanied by a growth rate above potential. This recommendation calls for a counter-cyclical approach to fiscal policy. A large number of studies have indeed found fiscal austerity as potentially self-defeating during a recovery. Among others, DeLong and Summers (2012) find that fiscal austerity could eventually lead to private markets doubts about public finance sustainability as austerity measures have been proved to deteriorate the already weak recovery in some cases and enhanced further slowdown in economic activity in others.

APPENDIX

GRAPH 1

ACTUAL OUTPUT, TREND AND OUTPUT GAP



Note: the four phases are introduced in Section 3.2. The first stage "expansion" is between the points D-A. It represents the late stage of an expansion with positive output-gap and real GDP growth reaching its maximum deviation from trend after rising during a recovery. The second stage "downturn" is between A-B. It represents an early stage of a contraction with positive output-gap level and negative annul change. The third phase "protracted slowdown" is B-C and it describes a late stage of a contraction with widening negative output-gap level. The fourth phase, called "recovery", is C-D and shows an early stage of expansion with negative output-gap level and positive change in deviation from trend. During the time period used for this dataset, a country business cycle passed through these phases cyclically according to the scheme A→B→C→D→A, etc.

TABLE 1

THE FOUR BUSINESS CYCLE PHASES

Definition	Points	Expansion/Contraction	Output-gap
expansion	D-A	Expansion	Positive
downturn	A-B	Contraction	Positive
protracted slowdown	B-C	Contraction	Negative
recovery	C-D	Expansion	Negative

Note: see Graph 1 for the corresponding graphic illustration.

TABLE 2

DATES OF PEAKS AND TROUGHS

Australia		Austria		Belgium		Canada		Denmark		Finland	
Peak	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*
1971	1972	1974	1975	1974	1975	1973	1975	1973	1975	1973	1977
1973	1975	1977	1978	1980	1983	1979	1980	1979	1981	1980	1981
1976	1978	1979	1982	1984	1986	1981	1983	1986	1993	1989	1993
1981	1983	1983	1987	1990	1993	1989	1993	2000	2003	2000	2003
1985	1986	1991	1995	2000	2003	2000	2003	2007	2009	1973	1975
1989	1992	2000	2003	2007	2009	2007	2009			2007	2009
1994	1997	2008	2009								
1999	2001										
2007	2009										

France		Germany		Greece		Ireland		Italy		Japan	
Peak	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*
1974	1975	1996	1999	1973	1974	1975	1976	1974	1975	1973	1976
1979	1981	2000	2002	1979	1983	1978	1983	1976	1977	1979	1983
1990	1993	2007	2009	1989	1990	1984	1986	1977	1978	1977	1978
1976	1978			1991	1996	1979	1982	1980	1983	1991	1994
1994	1997			2001	2002	1983	1987	1983	1987	1997	1999
2000	2003			2008	2010	1990	1994	1989	1993	1991	1995
2007	2009					2000	2001	2001	2003	2000	2002
						2002	2004	2007	2009	2007	2009
						2007	2010				

Netherland		Norway		Portugal		Spain		Sweden		Switzerland	
Peak	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*	*Peak*	*Trough*
1974	1975	1972	1974	1973	1975	1974	1975	1975	1977	1974	1976
1978	1982	1980	1982	1980	1985	1977	1985	1980	1983	1981	1983
1986	1987	1986	1990	1989	1990	1991	1996	1989	1993	1990	1996
1990	1993	1992	1993	1991	1994	2001	2003	2000	2003	2000	2003
2000	2003	1997	2003	2000	2005	2007	2009	2007	2009	2007	2009
2008	2009	2008	2009	2007	2009						

UK		US	
Peak	*Trough*	*Peak*	*Trough*
1973	1975	1973	1975
1979	1981	1979	1982
1988	1993	1986	1987
2000	2003	1989	1991
2007	2009	1994	1995
		2000	2003
		2007	2009

Note: Peaks and Troughs are reported for 20 OECD economies and are detected with the "growth cycle" approach.

TABLE 3

DESCRIPTIVE STATISTICS FOR THE FOUR PHASES OF THE CYCLE

Country	No. years in phase (D-A) (1)	No. years in phase (A-B) (2)	No. years in phase (B-C) (3)	No. years in phase (C-D) (4)	*f*(D-A) (5)	*f*(A-B) (6)	*f*(B-C) (7)	*f*(C-D) (8)
Australia	17	5	4	15	41.46%	12.20%	9.76%	34.15%
Austria	15	3	10	13	36,59%	7.32%	24.39%	31.71%
Belgium	15	5	6	15	36.59%	12.20%	14.63%	36.59%
Canada	16	8	5	12	39.02%	19.51%	12.20%	29.27%
Denmark	17	5	8	11	41.46%	12.20%	19.51%	26.83%
Finland	16	5	6	14	39.02%	12.20%	14.63%	34.15%
France	15	8	6	12	36.59%	19.51%	14.63%	29.27%
Germany	5	1	4	8	27.78%	5.56%	22.22%	44.44%
Greece	13	5	6	17	31.70%	12.19%	14.64%	41.46%
Ireland	12	10	7	12	29.27%	24.39%	17.07%	29.27%
Italy	13	8	4	16	31.71%	19.51%	9.76%	39.02%
Japan	12	5	10	14	29.27%	12.20%	24.39%	34.15%
Netherland	13	7	8	13	31.71%	17.07%	19.51%	31.71%
Norway	15	5	7	14	36.59%	12.20%	17.07%	34.15%
Portugal	15	7	6	13	36.59%	17.07%	14.63%	31.71%
Spain	13	8	9	11	31.71%	19.51%	21.95%	26.83%
Sweden	12	8	8	12	31.71%	19.51%	19.51%	29.27%
Switzerland	13	4	9	15	31.71%	9.76%	21.95%	36.59%
UK	13	7	5	16	31.71%	17.07%	12.20%	39.02%
US	18	4	4	15	43.90%	9.76%	9.76%	36.59%
Total sample	279	118	132	268				
frequencies	35.00%	14.80%	16.56%	33.62%				

Note: D-A refers to the first stage "*expansion*". It represents the late stage of an expansion with a positive output-gap and real GDP reaching its maximum deviation from trend after rising during a recovery. The second stage "*downturn*" is between A-B. It represents an early stage of a contraction with positive output-gap level and negative annul change. B-C is the third phase "*protracted slowdown*" and it describes a late stage of a contraction with widening negative output-gap level. C-D is the fourth phase, called "*recovery*", and shows an early stage of expansion with negative output-gap level and positive deviation from trend. During the time period used for this dataset, a country business cycle passes through these phases cyclically according to the scheme A→B→C→D→A, etc. *f*(-) in columns (5)-(8) refers to the frequency of time each economy is in each region of the cycle, respectively.

TABLE 4

DESCRIPTIVE STATISTICS ON BC DURATION

Country	Number of years in expansions (1)	Number of years in contractions (2)	Average duration contraction (in years) (3)	Average duration expansion (in years) (4)	Average duration cycle (in years) (5)
Australia	32	9	2.20	6.25	8.5
Austria	28	13	2.50	3.40	6.20
Belgium	30	11	2.67	3.80	6.60
Canada	28	13	2.33	3.67	5.20
Denmark	28	13	2.83	4.33	6.80
Finland	30	11	2.17	4.20	7.00
France	27	14	2.83	5.60	6.60
Germany	13	5	2.33	3.50	6.00
Greece	30	11	2.80	7.75	8.50
Ireland	24	17	2.57	3.00	5.00
Italy	29	12	2.33	4.33	5.60
Japan	26	15	2.83	4.50	5.60
Netherland	26	15	2.67	3.80	5.60
Norway	29	12	3.00	5.50	6.80
Portugal	28	13	3.16	4.20	7.6
Spain	24	17	6.00	4.67	8.75
Sweden	25	16	4.20	4.31	10.67
Switzerland	28	13	3.40	5.00	6.30
UK	29	12	2.60	5.75	8.25
US	33	8	2.40	6.00	8.50
Total sample average	27.35	12.50	3.79	5.33	8.95

Note: columns (1) and (2) show the total number of expansions and contractions, respectively, for each country in the sample. Columns (3)-(5) present the yearly average duration of expansions, contractions and of the overall cycles, respectively. A cycle is considered as the period between two consecutive peaks.

TABLE 5

ESTIMATION OF MODEL 1; DEPENDENT VARIABLE y_{it}

	OLS – FE (fixed-effects model) with IV (1)	OLS with IV (2)	OLS – FE (fixed-effects model) with IV (3)	OLS – FE (fixed-effects model) with IV (4)
y_{it-1}	0.296***	0.316***	0.283***	0.559***
	(0.086)	(0.075)	(0.072)	(0.179)
$CAPB_{it}$	-0.424***	-0.184	-0.133	0.307**
	(0.151)	(0.136)	(0.141)	(0.139)
$D1_{it}\times CAPB_{it}$		-0.977***	-0.951***	
		(0.256)	(0.281)	
$D2_{it}\times CAPB_{it}$		-0.790***	-0.771.***	
		(0.150)	(0.167)	
$D3_{it}\times CAPB_{it}$		-0.141	-0.183	
		(0.147)	(0.141)	
$CAPB_{it-1}$				-0.308
				(0.200)
$D1_{it}\times CAPB_{it-1}$				-0.886**
				(0.429)
$D2_{it}\times CAPB_{it-1}$				0.059
				(0.269)
$D3_{it}\times CAPB_{it-1}$				0.389*
				(0.209)
Cost	0.031***	0.031***	0.032***	0.016**
	(0.004)	(0.004)	(0.0038)	(0.0084)
F-test	10.67	15.64	11.86	6.59
	(0.000)	(0.000)	(0.000)	(0.000)
F-test (*dummy* interactions)		12.04	9.36	3.76
		(0.000)	(0.000)	(0.000)
R-squared	0.27	0.51	0.54	0.55

Note: estimation of model 1. OLS regression with IV in column (2). Within group estimator for model with fixed effects and IV in columns (1) and (3)-(4). The dependent variable is real GDP growth; *CAPB* is cyclically-adjusted primary balance as defined in (2); *D*1, *D*2, *D*3 are dummy variables corresponding to the phase of the cycle (*downturn* (stage A-B), *protracted slowdown* (stage B-C), *recovery* (stage C-D). We use lagged levels and lagged first-differences as instruments for the variables on the right hand side. This is done in order to avoid the bias of dynamic panel but also possible endogeneity bias. Number of observations is 70. Numbers in parenthesis are standard errors. ***, **, * indicate significance levels below one percent, between one and five percent, and between five and under 10 percent, respectively. The Data Appendix for a detailed definitions of variables is available upon request. *F*-tests are conducted for all coefficients and for the dummies interaction alone.

TABLE 6

ESTIMATION OF MODEL 2; DEPENDENT VARIABLE y_{it}

	OLS with IV (1)	OLS – FE (fixed-effects model) with IV (2)	OLS with IV (3)	OLS – FE (fixed-effects model) with IV (4)	OLS – FE (fixed-effects model) with IV (5)
y_{it-1}	0.325***	0.277***	0.311***	0.274***	0.313***
	(0.094)	(0.088)	(0.084)	(0.083)	(0.099)
G_{it}	-0.238***	-0.283*	-0.355***	-0.257**	-0.244*
	(0.076)	(0.159)	(0.130)	(0.129)	(0.13)
$D1_{it}\times G_{it}$		0.224	0.281	0.237	0.088
		(0.367)	(0.308)	(0.356)	(0.421)
$D2_{it}\times G_{it}$		-0.164	-0.171	-0.181	-0.274
		(0.299)	(0.265)	(0.291)	(0.344)
$D3_{it}\times G_{it}$		0.402**	0.404***	0.362**	0.349***
		(0.180)	(0.149)	(0.161)	(0.191)
TAX	-0.168				
	(0.0881)				
$D1_{it}$		-0.015***	-0.016***		
		(0.0069)	(0.0057)		
$D2_{it}$		-0.018***	-0.018***		
		(0.005)	(0.0049)		
$D3_{it}$		0.002*	0.003*		
		(0.0013)	(0.0021)		
contraction				-0.0181***	
				(0.0039)	
cost	0.021***	0.0262***	0.024***	0.027***	0.021***
	(0.0041)	(0.0039)	(.0039)	(0.0027)	(0.0029)
F-test		5.85	10.81	2.83	3.83
		(0.000)	(0.000)	(0.047)	(0.005)
F-test (*dummy* interactions)		5.06	7.92	7.73	2.17
		(0.0004)	(0.000)	(0.0001)	(0.1032)
R-squared	0.27	0.57	0.58	0.57	0.34

Note: estimation of model 2. OLS regression with IV in columns (1) and (3). Within group estimator for model with fixed effects and IV in columns (2), (4) and (5). The dependent variable is real GDP growth; *G* is real first-differenced government spending as defined in (1); *D*1, *D*2, *D*3 are dummy variables corresponding to the phase of the cycle (*downturn* (stage A-B), *protracted slowdown* (stage B-C), *recovery* (stage C-D); *TAX* is cyclically-adjusted current revenue as a share of GDP ((*Income taxes + Business taxes + Indirect taxes + Social security contributions + Other taxes)/GDP*); *contraction* is a dummy that is equal to one when the economy is in a contraction phase (from Peak to Trough, or when the economy is on A-B or B-C stages). We use lagged levels and lagged first-differences as instruments for the variables on the right hand side. This is done in order to avoid the bias of dynamic panel but also possible endogeneity bias. Column (1) is the estimation that follows ALESINA A. and ARDAGNA S. (2010). Number of observations is 70. Numbers in parenthesis are standard errors. ***, **, * indicate significance levels below one percent, between one and five percent, and between five and under 10 percent, respectively. The Data Appendix for a detailed definitions of variables is available upon request. *F*-tests are conducted for all coefficients and for the dummies interaction alone.

TABLE 7

ESTIMATION WITH OUTPUT-GAP; DEPENDENT VARIABLE y_{it}

	OLS with IV (1)	OLS – FE (fixed-effects model) with IV (2)	OLS with IV (3)	OLS – FE (fixed-effects model) with IV (4)
y_{it-1}	0.358***	0.303***	0.280***	0.271***
	(0.096)	(0.091)	(0.094)	(0.095)
$CAPB_{it}$	-0.745***	-0.712***		
	(0.217)	(0.219)		
$GAP_{it} \times CAPB_{it}$	0.555*	0.585		
	(0.305)	(0.341)		
G_{it}			-0.109	0.0271
			(0.086)	(0.0924)
$GAP_{it} \times G_{it}$			-0.338**	-0.256
			(0.155)	(0.181)
GAP_{it}	-0.008	-0.008		
	(0.0079)	(0.0089)		
Cost	0.035***	0.035***	0.0209***	0.022***
	(0.0055)	(0.005)	(.0030)	(0.003)
F-test	7.73	6.92	10.06	4.69
	(0.000)	(0.0002)	(0.000)	(0.0056)
F-test (*dummy* interactions)	5.30	2.55		
	(0.07)	(0.087)		
R-squared	0.32	0.31	0.31	0.27

Note: estimation with output-gap. OLS regression in columns (1) and (3). Within group estimator for model with fixed effects and IV in columns (2) and (4). The dependent variable is real GDP growth; *CAPB* is cyclically-adjusted primary balance as defined in (2); *G* is real first-differenced government spending as defined in (1); *GAP* is a dummy variable corresponding to the phase with positive output-gap. We use lagged levels and lagged first-differences as instruments for the variables on the right hand side. This is done in order to avoid the bias of dynamic panel but also possible endogeneity bias. Number of observations is 70. Numbers in parenthesis are standard errors. ***, **, * indicate significance levels below one percent, between one and five percent, and between five and under 10 percent, respectively. The Data Appendix for a detailed definitions of variables is available upon request. *F*-tests are conducted for all coefficients and for the dummies interaction alone.

TABLE 8

ROBUSTNESS; DEPENDENT VARIABLE y_{it}

	OLS – FE (fixed-effects model) with IV (1)	OLS – FE (fixed-effects model) with IV (2)	OLS – FE (fixed-effects model) with IV (3)	OLS – FE (fixed-effects model) with IV (4)	IV – Instrumental variables estimation (5)	IV – (GMM) estimator (6)
y_{it-1}	0.28***	0.28***	0.24***	0.19**	0.35***	0.26**
	(0.07)	(0.07)	(0.09)	(0.08)	(0.09)	(0.12)
$CAPB_{it}$	-0.13	-0.13	-0.13	-0.09	-0.12	-0.06
	(0.14)	(0.14)	(0.13)	(0.13)	(0.32)	(0.26)
$D1_{it}\times CAPB_{it}$	-0.94***	-0.93***	-0.66**	-0.52	-1.15***	-1.13***
	(0.28)	(0.28)	(0.39)	(0.40)	(0.38)	(0.19)
$D2_{it}\times CAPB_{it}$	-0.74***	-0.76***	-0.70***	-0.74***	-0.87***	-0.83***
	(0.17)	(0.16)	(0.16)	(0.16)	(0.19)	(0.21)
$D3_{it}\times CAPB_{it}$	-0.17	-0.19	-0.18	-0.25*	-0.19	-0.18
	(0.14)	(0.14)	(0.14)	(0.14)	(0.17)	(0.13)
EXC	-0.001					
	(0.001)					
INT		-0.0003				
		(0.0007)				
DEBT			-0.009*			
			(0.005)			
ΔDEBT				0.0009		
				(0.003)		
Cost	0.03***	0.03***	0.03***	0.03***	0.03***	0.03***
	(0.003)	(0.0038)	(0.004)	(0.004)	(0.009)	(0.007)
F-test	9.75	9.76	5.63	5.47	16.07	
	(0.000)	(0.000)	(0.000)	(0.004)	(0.000)	
OID (*p*-value)						0.11
Hausman test *p*-value					0.53	
R-squared	0.54	0.55	0.57	0.44	0.57	0.56

Note: within group estimator for model with fixed effects and IV in columns (1)-(4). Instrumental variables estimation (IV) in column (5). Generalized method of moments (GMM) in column (6). The dependent variable is real GDP growth; *CAPB* is cyclically-adjusted primary balance as defined in (2); *D*1, *D*2, *D*3 are dummy variables corresponding to the phase of the cycle *downturn* (stage A-B), *protracted slowdown* (stage B-C), *recovery* (stage C-D); *EXC* represents the rate of change of nominal exchange rate; *INT* is the change in interest rate on deposit; *DEBT* is a dummy variable equal to one if the debt-to-GDP *ratio* is above the average value of the same variable in all episodes; *ΔDEBT* is a dummy variable equal to one if the growth of the debt-to-GDP *ratio* is above the average value of same variable in all episodes. We use lagged levels and lagged first-differences as instruments for the variables on the right hand side. This is done in order to avoid the bias of dynamic panel but also possible endogeneity bias. In column (5)-(6) institutional variables are used as instruments for fiscal policy variables. Number of observations is 70. Numbers in parenthesis are standard errors. ***, **, * indicate significance levels below one percent, between one and five percent, and between five and under 10 percent, respectively. The Data Appendix for a detailed definitions of variables is available upon request. *F*-tests are conducted for all coefficients. OID repots the *p*-value of test of over identifying restrictions in the instrumental variable regression. Hausman test compares IV and OLS estimates.

Table 9

ESTIMATION OF MODEL 1 AND 2; DEPENDENT VARIABLES y^n_{it}, n_{it}

	Dependent Variable y^n_{it}			Dependent Variable n_{it}			
	OLS – FE (fixed-effects model) (1)	OLS – FE (fixed-effects model) with IV (2)	OLS – FE (fixed-effects model) with IV (3)	OLS – FE (fixed-effects model) with IV (4)	OLS – FE (fixed-effects model) with IV (5)	OLS – FE (fixed-effects model) with IV (6)	OLS – FE (fixed-effects model) with IV (7)
y^n_{it-1}	0.292***	0.336***	0.309***				
	(0.096)	(0.083)	(0.089)				
n_{it-1}				0.000	0.000	0.000	0.002
				(0.000)	(0.000)	(0.000)	(0.000)
$CAPB_{it}$	-0.014		-0.074	-0.085		-0.305**	-0.018
	(0.161)		(0.162)	(0.180)		(0.167)	(0.155)
$D1_{it}\times CAPB_{it}$	0.189			-1.466***			-0.023
	(0.365)			(0.386)			(0.668)
$D2_{it}\times CAPB_{it}$	-0.171			-0.659**			-0.559**
	(0.196)			(0.271)			(0.260)
$D3_{it}\times CAPB_{it}$	0.377			-0.279*			-0.249*
	(0.159)			(0.181)			(0.166)
G_{it}		0.083			-0.159		
		(0.106)			(0.153)		
$D1_{it}\times G_{it}$		-0.442			0.388		
		(0.297)			(0.429)		
$D2_{it}\times G_{it}$		-0.602***			0.269		
		(0.216)			(0.320)		
$D3_{it}\times G_{it}$		-0.137			0.298		
		(0.119)			(0.171)		
$GAP_{it}\times CAPB_{it}$			0.053			0.110	
			(0.139)			(0.036)	
EXC							
							0.000
							(0.019)
INT							0.008
							(0.111)
ΔDEBT							-0.001
							(0.004)
Cost	0.016***	0.016***	0.017	0.017***	0.008***	0.011***	0.017***
	(0.004)	(0.002)	(0.040)	(0.004)	(0.002)	(0.004)	(0.004)
F-test	2.46	5.05	4.19	4.34	1.40	12.99	4.34
	(0.011)	(0.000)	(0.010)	(0.003)	(0.1952)	(0.000)	(0.003)
F-test (*dummy* interactions)	0.52	2.84	0.15	5.84	1.06	1.05	5.84
	(0.670)	(0.004)	(0.703)	(0.002)	(0.373)	(0.421)	(0.002)
R-squared	0.31	0.34	0.33	0.25	0.04	0.04	0.25

Note: within group estimator for model with fixed effects and IV in columns (2)-(7). The dependent variables are real GDP per hours worked and hours worked; *CAPB* is cyclically-adjusted primary balance as defined in (2); *D*1, *D*2, *D*3 are dummy variables corresponding to the phase of the cycle (*downturn* (stage A-B), *protracted slowdown* (stage B-C), *recovery* (stage C-D). We use lagged levels and lagged first-differences as instruments for the variables on the right hand side. This is done in order to avoid the bias of dynamic panel but also possible endogeneity bias. *GAP* is a dummy variable corresponding to the phase with positive output-gap. *EXC* represents the rate of change of nominal exchange rate; *INT* is the change in interest rate on deposit; *DEBT* is a dummy variable equal to one if the debt-to-GDP *ratio* is above the average value of the same variable in all episodes; *ΔDEBT* is a dummy variable equal to one if the growth of the debt-to-GDP *ratio* is above the average value of same variable in all episodes. Number of observations is 70. Numbers in parenthesis are standard errors. ***, **, * indicate significance levels below one percent, between one and five percent, and between five and under 10 percent, respectively. The Data Appendix for a detailed definitions of variables is available upon request. *F*-tests are conducted for all coefficients and for the dummies interaction alone.

TABLE 10

EPISODES OF LARGE FISCAL ADJUSTMENTS IDENTIFIED BY ALESINA AND ARDAGNA (2010)

Country	Episodes
Australia	87, 88
Belgium	82, 84, 87, 06
Canada	81, 86, 87, 95, 96, 97
Denmark	83, 84, 85, 86, 05
Finland	81, 84, 88, 94, 96, 98, 00
France	96
Germany	96, 00
Ireland	84, 87, 88, 89, 00
Italy	80, 82, 90, 91, 92, 97, 07
Japan	84, 99, 01, 06
Portugal	82, 83, 86, 88, 92, 95, 02, 06
Spain	86, 87, 94, 96
Sweden	81, 83, 84, 86, 87, 94, 96, 97, 04
United Kingdom	82, 88, 96, 97, 98, 00

Source: ALESINA A. and ARDAGNA S. (2010).

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Are Short-Selling Bans Effective? Evidence from the Summer 2011 European Bans on Net Short Sales

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During the Summer of 2011, securities regulators in France, Italy and Spain reintroduced a ban on short sales. This ban differed from previous restrictions: it was the first time regulators prohibited net short sales on selected financial stocks, and extended the ban widely to include synthetic short positions. The nature of the ban allows me to employ a unique identification strategy that overcomes endogeneity problems discussed in past literature. Results indicate that the ban is associated with a significant deterioration in market quality – particularly for high free-float stocks – in contrast, I do not find any effect on stock price performance.
[JEL Classification: G01; G12; G14; G18].

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«They wanted to test French resistance. This is our response – as always very determined – and it will be so for all those who want to put us to the test».
(Jean-Pierre Jouyet, head of the AMF, 11 August 2011, quoted from The Financial Times)

1. - Introduction

At the end of the summer 2011, following a drop in financial stocks' prices, a number of euro-zone securities regulators called for a reintroduction of a ban on short sales. Financial authorities claimed that the ban aimed at restricting the benefits that can be achieved by spreading false rumours. The European Securities and Markets Authority (ESMA) backed national regulators defining short selling as an abusive market practice when used in combination with the dissemination of false or misleading information.

This is not the first time regulators have reacted to a decline in share prices by imposing bans on short sales. The previous experience relates to the 2008-2009 financial crisis, when regulators around the world hurried to impose constraints on this trading strategy, on the basis that short sellers contributed to the deviation of stock prices from their true valuation. According to the regulators, prices went down because short sellers abusively spread false information to make a profit; therefore banning this trading strategy would lead to an increase in share prices. Theoretical models however cast doubts on the effectiveness of short selling bans in supporting share prices, and, most importantly, suggest that short selling bans may have a negative impact on market liquidity. A regulation that impairs market quality is even more detrimental when it comes at a time of hardship and liquidity restraints for market participants, as it has recently been the case.

In this paper I henceforth investigate whether short-selling bans are the right regulatory answer; in particular, I study the impact of the European short selling ban of the summer 2011, to shed light on its impact on market liquidity as well as on its effectiveness on stock price performance.

Short-selling restrictions were first imposed only on selected financial stocks; afterwards, starting from the beginning of December of the same year, some countries extended a naked ban to all stocks from any industry; moreover the European Parliament voted into law a regulation to ban certain CDS trades starting from November 2012.[1] This paper departs from the previous literature along several

[1] A net short position can also be achieved via Credit Default Swaps (CDS). For example, buying a naked CDS is equivalent to a bet on rising credit risk, since the investor is not exposed to the credit risk of the underlying bond issuer. From a regulatory perspective, this is hence analogous to a short sale of the underlying bond.

aspects: first of all, the characteristics of the 2011 European ban are remarkably different from short-selling restrictions previously imposed; it is indeed the first time regulators introduce a ban on *net* short sales on selected financial stocks only and extend the scope widely to take into account synthetic positions altogether. Studying the impact on the market of this different regulatory intervention is therefore crucial from a policymaking point of view to understand which type of regulation is the most effective. The second important contribution of this paper lies in the ability to address the *identification* problem. Indeed the existence of an endogeneity bias has been pointed out in the past literature, and has resulted in a main limitation in the interpretation of the estimated effect of the ban. In order to control for unobservable characteristics, some authors have matched each banned financial stock with one non-banned stock belonging to a non-financial sector (Boehmer, Jones, Zhang, 2008), while others have introduced stock-level fixed effects (Beber and Pagano, 2009). Although these specifications both try to capture some variability in the market that may indeed affect the estimated results, they still have several shortcomings. In fact, to have a causal interpretation of the estimated effect of the ban, these models make the rather stringent assumption that trends in market quality for stocks in the financial sector are comparable to trends in market quality for non-financial stocks, thus causing results to be driven by possible liquidity differentials between the two groups as well as differential time effects. Additionally, most studies on short-sale bans were conducted in the US amid of the 2008 crisis, concomitantly with TARP announcements; therefore any estimated effect of the ban on prices might have been clouded by rescue plan approvals. To address all these shortcomings I focus on the period from August 11 up to November 11, 2011 when the ban applied only to *selected* financial stocks. This is a particularly interesting time span because it allows me to specify the model under milder assumptions: I am able to construct counterfactuals from the financial sector and investigate whether there are significant differences in the time trends for the two groups, *i.e.* the banned financial stocks and the non-banned financial stocks, before the ban. The only assumption behind the causal interpretation of the ban effect in my model is the presence of common time effects across European financial stocks. The tests I conduct for the pre-ban period all give strong supportive evidence in favour of the goodness of the selected controls. Moreover, the single assumption I make is also mild, not only from an econometric point of view, but also from a financial perspective given the high degree of interconnectedness of stocks in the financial sector (see Acharya and Yorulmazer, 2007).

My results indicate that the ban on net short sales is associated with a statistically significant deterioration in market quality, *i.e.* an increase in percentage quoted bid-ask spreads. I also investigate whether the ban had heterogeneous impacts across different classes of stocks, specifically stocks with and without traded options, and stocks with low and high free float. In contrast with previous literature, I find that the ban did not affect disproportionately stocks without traded options; this is not surprising since the 2011 ban was extended also to synthetic net short positions. Stocks with high free float instead appear to have been affected more harshly by the ban compared to low free float stocks. This might be due to the fact that low free float stocks are mainly in the hands of controlling interest shareholders who are known for being reluctant (or sometimes even forbidden) to lend their shares; it follows that low free float stocks are already difficult for traders to locate and short, even in the absence of a ban. Instead high free float stocks are the ones for which short positions are easier to enter into, and are therefore the group that is more likely to have been hit by the ban. Finally, I inspect the impact of the ban on prices and find that the ban is not associated with a statistically significant improvement in price performance.

2. - Literature Review

Theory is ambiguous over the effects of short sale bans on market liquidity. Diamond and Verrecchia (1987) show that the impossibility to trade on negative news reduces the speed of price discovery, which in turn creates uncertainty about the fundamental value of the stocks and hence tends to increase the bid-ask spread. However substantial empirical evidence (Desai, Krishnamurthy, and Venkataraman, 2006; Cohen, Diether and Malloy, 2007; Boehmer, Jones and Zhang, 2008) shows that short sellers tend to trade on information about fundamentals; theory would hence predict that, by removing a consistent share of informed traders, the ban would reduce the bid-ask spread.

Another channel of transmission would then be the competition among liquidity providers. Khandani and Lo (2007) provide evidence that many quant funds do provide liquidity even if they are not registered market makers. Hence, even if an exemption from the short-sale ban is in place for market makers during the summer 2011, the ban would reduce competition by limiting *de facto* liquidity suppliers. As a consequence, exempted market makers would have the incentive to widen their spreads.

Theory is ambiguous as well over the impact of short sale restrictions on stock prices. Miller (1977) predicts that short sale constraints would lead to overpricing by taking pessimists out of the market. Diamond and Verrecchia (1987) however incorporate rational expectations and show that when investors are aware of the impact of the ban on the composition of the pool of investors, stocks are not overvalued on average. Yet, Marin and Olivier (2008) develop an extension of the Grossman and Stiglitz (1980) model, and show that when insiders face some realistic trading constraints (such as not being able to short-sell shares of their companies), asset prices exhibit crashes. The authors label this the "dog that did not bark" effect: uncertainty about the stock fundamentals is larger when insiders (who possess valuable information) are out of the market than when they are trading- indeed, uninformed traders see that insiders cannot short sell stocks but they also see that insiders are not buying back the stock either, therefore uninformed investors can infer that there is bad news but not how bad the news really is; it follows that such uncertainty along with a downward revision of beliefs about the stock fundamentals causes prices to go down even more rapidly than if there had been moderate selling by insiders.

Recent literature has focused on price manipulation; in this respect, Goldstein and Guembel (2007) introduce feedback effects from financial markets onto the real economy and show that speculators have the incentive to short sell and hence manipulate prices, even when uninformed. Shkilko, Van Ness and Van Ness (2011) document that short sales may increase downward pressure on prices even in the absence of negative information: they examine large negative intraday price reversals on no-news days and find that short sellers during these reversals are abnormally active and cause substantial price declines.

Empirical evidence was mainly gathered during the 2008-2009 short-selling restrictions. Boehmer, Jones and Zhang (2009) use a matched pairs panel regression to analyse the US ban and find that stocks subject to the ban experienced a severe degradation in market quality (larger spreads, higher price impacts, and increased intraday volatility). Beber and Pagano (2009) exploit cross-country variation in short-selling restrictions and study the effects of the bans on liquidity, price discovery and stock prices; they find that the ban imposed a serious disruption in the liquidity of the banned stocks – particularly the small-cap ones. They also document that information was incorporated into prices more slowly for banned stocks. Finally they provide evidence that the ban failed to support prices, with the exception of the US. It is important to note, however, that both Boehmer *et* al. (2009) and Beber *et* al. (2009) acknowledge in their papers that confounding effects may be driving the estimated results: in fact, the 2008 short-selling ban in the US was concomitant with TARP announcements, therefore the positive effect estimated on stock prices in the US might in fact be due to rescue plan approvals, and not to short-selling bans. It is also worth noting that the 2008 short-selling ban covered virtually all stocks in all major sectors; therefore, in order to identify the effect of the ban researchers had to either look for counterfactuals within non-financial industries or to resort to fixed-effect model estimation. The former specification implicitly assumes that market-wide variability in the financial sector is similar to, and can be captured by, the variability appreciated in other sectors. However, it may be quite stringent to assume that trends in financial stocks can be controlled for using counterfactuals belonging to very different industries, such as retail and manufacturing. Therefore, all of this left researchers with a possibly spurious effect estimated on the ban.

Additional empirical evidence was gathered by Battalio and Schultz (2009), who study the impact of the 2008-2009 short selling restrictions on the equity options markets and show that the ban is associated with dramatic increases in bid-ask spreads for options of banned stocks. Kolasinksi, Reed and Thornock (2010*a*,*b*) report that the negative relation between the percentage volume of short sales and stock returns became more marked during the 2008 US ban, suggesting banned stocks became more responsive to allowed or synthetic short sales. Marsh and Payne (2011) restrict their attention to the UK and find similar adverse effects of the ban on market efficiency. Diether, Werner and Lee (2009) instead study the temporary suspension of the 2004 US Regulation SHO and show that the suspension of price tests in the US pilot securities worsened spreads and intraday volatility. Saffi and Sigurdsson (2011) show that short selling constraints (*i.e.* low lending supply and high borrowing fees) are negatively related to stock price efficiency, and that less constrained firms experience shorter price discovery delays.

In sum, the consequences of short-selling bans are ambiguous from a theoretical perspective; moreover existing empirical studies on the impact of the bans on stock prices performance reach conflicting conclusions and suffer from confounding factors. It is therefore important to gather more evidence on short-selling restrictions in order to identify the causal effect of the ban on market quality, and I do it in the following sections.

3. - The Setting

3.1 *Notes on the Ban*

As shown in Table 1, the ban was introduced on the same day – 12th August 2011 – in France, Italy and Spain, with the French markets authority leading the calls and urging a coordinated pan-European response[2]. The ban was initially intended to last 15 days only, but it was then rolled over to September 30, and once again, up to November 11. It affected only selected stocks in the financial sector, regardless of the trading venue where transactions are executed (MTFs and OTC were henceforth included in the calculation of net short positions). Differently from previous bans, the 2011 short selling restrictions covered also ETFs, covered warrants and certificates, along with synthetic short positions entered through, for instance, the purchase of put options or reverse-ETFs strategies.

In all three countries, reporting obligations were in place since before the introduction of the ban for any player taking short positions in all stocks traded in regulated markets. More specifically, the CNMV, the Spanish regulator, adopted disclosure requirements starting from June 2010 in accordance with ESMA- proposed guidelines; the AMF, the French markets authority, introduced new requirements on reporting starting from February 2011; in Italy reporting obligations have been in place since July 2011 only.[3]

[2] To isolate the effect of this ban, I do not include in my analysis countries where short-sale restrictions have been in place since 2008 and never lifted, such as Germany.

[3] Moreover, while the CNMV and the AMF explicitly provide for the publication of aggregate short sales positions filed by market participants, under the Italian regulation the Consob did not disclose information regarding bearish positions taken on Italian shares. The different periods of adoption of disclosure requirements in the three countries, along with the impossibility to retrieve evidence for short sales for the Italian shares, make a possible visual inspection of the evolution of short sale positions before and during the ban, not much accurate to derive meaningful insights. Moreover, even in the presence of complete data, information would be subject to an overestimation bias: indeed, under the disclosure requirements, it is mandatory to report short positions taken irrespective of the trading venue; short positions taken in OTC markets are hence included in aggregate short volumes; however understanding the evolution of short positions as a percentage of total volume, would require detailed information also on the volume of OTC transactions, for which there is not a regulatory framework. Data Explorers is a data provider that allows the monitoring of short positions alongside the market data; it recently published a note, reported in the Financial Times, showing the level of shorting in the three European countries. The level of short sales reported, appeared already extremely low before the ban was introduced. It would be in fact useful (and I plan to do for future research) to request access to the tick-by-tick database to compute and check on the levels of shorting *pre* and during the ban, in line with Marin J. and Olivier J. (2008). At the moment, the database is not available free of charge for students at my University.

It is worth noting also, that these European bans did not prohibit short sales *tout court*; what is prohibited under the ban is taking net short positions. For the sake of clarity, I report an example of permitted strategy as quoted by the Consob: an investor who has a long position on a banned stock through a call with cash settlement, is allowed to short a delta-equivalent number of shares, so that the short position is perfectly counterbalanced by the long position on the traded option.[4] It is also worth noting that there is a difference between this ban and the 2008 naked ban; for instance, taking the above example, a cash settlement would not be enough under a naked ban because it does not entitle to, in legal terms, "the right to receive", *i.e.* to the underlying.

The introduction of the ban was followed by a harsh debate, with the UK regulator, the FSA, taking a firm stance against the ban.

3.2 *Short Selling- How it Works*

Short selling involves the sale of an asset that is not owned[5]. Typically, a short-seller would need to turn to a broker to *locate* the stock. The broker might already have the stock in its own inventory or in the accounts of those clients who allow the broker to access their accounts for lending. If the broker is not able to locate the stock within his accounts, he will need to refer to a custodian bank or institutional investors with buy-and-hold strategies, such as insurance companies and pension funds. It may take considerable time for the broker to locate the stock due to several features of the stock in question, among which the float, the ownership concentration, the presence of certain events such as IPOs and mergers. Once the broker has located the stock, the short seller can short the stock and at the same time must place collateral with the original lender in excess of the market value of the borrowed stock; this collateral is usually placed in cash and is higher in case the short seller located the stock through a broker-dealer. The borrower pays the lender a fee that is *rebated* by the interest that the lender pays to the borrower for the use of the cash posted as collateral. This rate is negotiable and is adjusted on a daily basis; as long as this rate is below the market rate for cash funds, the lender is gaining a cheaper access to sources of funding. In addition, most lenders retain the right to interrupt the loan of the stock at any moment: the lender gives a notice of recall to the short seller, who then has three days to *cover* his position by purchasing the

[4] See the Consob short-selling FAQs for reference: *http://www.consob.it/main/trasversale/operatori/intermediari/faq_short.html.*

[5] See Duffie D., Garleanu N. and Pedersen L.H. (2002) for an in-depth review of how short selling works.

stock or by borrowing it from another lender. Failing to do so, a *short squeeze* occurs and the lender has the right to *buy in* the stock with the cash collateral. The short seller gains if the price of the stock has declined at the moment he has to close his position. Before disclosure requirements came into place, from a buyer perspective a short sale would be equivalent to a sale, since it was undistinguishable whether the counterparty to the trade already had the stock in its inventory. It is worth mentioning that the term going short refers also to the broad set of activities that come into existence through the derivatives market: for instance a put is a synthetic short position in so far as it entitles the buyer to sell the stock at an agreed strike price, which is equivalent to a bet on declining stock's price; analogously, being short in futures implies that the trader has the obligation to sell the stock at a later date at a given price, therefore the trader will gain if the price falls below the agreed sale price, since he will be able to buy the stock at a lower price and gain from the difference. As previously mentioned, the 2011 short selling ban covered also these synthetic short positions.

3.3 *Short Selling: The Rationale Behind Looking at Impact on Liquidity*

Liquid markets are desirable because they are perceived as providing efficient allocation of resources and increased information. There is no one single definition of liquidity, although market participants tend to view as liquid those stocks where a position can easily be unwound. In particular, from market microstructure literature, we can think of liquidity as the cost of reversing an asset trade almost instantaneously after having executed it. In this light, liquidity is naturally defined via bid-ask spreads. The bid-ask spread is the difference between the price at which liquidity suppliers (call them the dealers) are willing to sell (ask price) and the price at which they are willing to buy (bid price). The main determinants of the spread are adverse selection costs arising with asymmetric information and inventory costs. The highest the spread, the costlier it is to reverse the trade, hence the lowest the liquidity.

Liquidity in the markets is tied in a mutual reinforcing behaviour with another type of liquidity, namely funding liquidity defined as the ability of an investor to raise funds either through collateralised loans or thanks to his own capital. Trading when a short position is taken does not free up capital, instead a margin is usually needed to take positions, as mentioned in the previous paragraph. It follows that when funding liquidity is low, trading activity is reduced; decreased trading in turn lowers market liquidity and broadens spreads, increasing losses on existing positions; market liquidity restraints in turn put pressure on risk management as firms try to minimize their exposure, and consequently reduce the ability of firms to raise capital, hence creating a downwards liquidity spiral[6].

Given these premises, it is important for a financial market regulator not to impose policy interventions that, by broadening bid-ask spreads, amplify the liquidity spiral and that hence make the firms' quest for capital harsher.

4. - Data and Methodology

My data consist of daily bid and ask prices, market capitalization, volumes, float, leverage, volatility and ban-specific characteristics (starting and lifting date, stocks under shorting constraints, restrictiveness of the ban), for 256 financial stocks for a window of 165 days from May 31st up to November 11th. Daily data on bid and ask prices are retrieved from Bloomberg; details on the nature of the ban as well as on dates of inception and lift are taken from the website of Spain, France and Italy regulatory authorities, as well as from the European Securities and Market Authority (former CESR). For a stock to be included in my sample, I require it to have positive bid-ask spread over the sample period (223 stocks). Finally, to mitigate the effect of extreme observations, I winsorize the bid-ask spread at the top 1% level for the whole sample. Affected stocks represent 21.5% of the total number of financial stocks traded in the three European countries.

As mentioned before, during this time window the ban affected only selected financial stocks. Therefore, I can draw counterfactuals from the same industry of the stocks subject to short-selling restrictions. The possibility to have this type of controls allows me to rule out any bias due to liquidity differential between financial and non-financial stocks, which may have otherwise driven the results if I used non-financial stocks as controls (as it was done in previous studies, such as Boehmer, Jones and Zhang, 2009; Beber and Pagano, 2009; Marsh and Payne, 2011).

All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. The choice of the sample period is justified by the need to balance the importance to have enough variability with the trade-off of increased heterogeneity. Hence, in line with existing literature, I consider a pre- and a during- ban time windows that are similar in size, so that my estimated effects are the less affected by confounding factors, at the clear cost of foregoing some information.

[6] See Garleanu N. and Pedersen L.H. (2007) for an in-depth analysis of the interaction between risk management and market liquidity.

5. - Market Liquidity

5.1 *Descriptive Evidence*

I consider the percentage quoted bid-ask spreads as a measure of (il)liquidity. Graph 1 shows the evolution of equally weighted average quoted bid-ask spreads for all 223 financial stocks in the three countries considered; the dashed lines correspond to the introduction of the bans and their extensions. This Graph confirms the period of decreasing liquidity experienced in Europe. The evolution of spreads following the ban seems to suggest that restrictions have had a very limited impact on the liquidity of the stocks, whose spread rises back shortly after the (re-)introduction of the ban. On this note, Graph 2 represents quoted bid-ask spreads separately for the banned and the unbanned financial stocks, so to visually identify the effect of the ban. The behaviour of the two groups is initially very similar, showing common trends in liquidity for banned and unbanned financial stocks alike. After August 12 2011, however, banned stocks experience an upward trend in bid-ask spreads and the paths of the two groups diverge, suggesting the ban contributed to the deterioration in market quality for the stocks subjects to restrictions. More precisely, *post*-ban, the spread of the banned stocks is trending upward (they become less liquid) while the spread of the unbanned assets is stable, as Graph 2 shows. Since the spread of the banned stock is lower in level, this implies a squeezing gap, or in other words a small differential: the spread of the banned stocks is "catching up" with the spread of the unaffected stocks, so that the gap between the two is smaller than it used to be. But this (a no-larger gap) exactly means that the liquidity of the banned stocks has been disrupted compared to the unbanned ones.

5.2 *Regression Analysis*

5.2.1 *Panel Regressions DD*

To identify the effect of the ban, it is important to answer to the question of how would the banned stocks have moved had they not been banned. In order to do so, one must identify a group of stocks that would mimic the behaviour of the banned stocks in the absence of the treatment.

In fact a stock-level fixed-effect regression that does not take into account unobserved market variability, particularly at times of turmoil, risks of producing biased results, in so far as estimates may be clouded by omitted correlated variables that vary at the market level. To cloud out the source of omitted variable bias, the key is to find a group of stocks that appear to have reacted to market variability in the same way as banned stocks have in the past; in other words, identification relies on having two groups of stocks (banned and unbanned) whose *trends* in liquidity have appeared to be consistently similar, up until the start of the treatment. The introduction of the ban indeed creates a deviation from this common trend. The impact of the ban is then given by the behaviour of banned stocks compared to how they would have behaved had the ban not been introduced – which is captured by the behaviour of counterfactuals. It follows that, differentiating across the two cohorts of stocks, it is possible to disentangle the impact of the ban from unobserved market variability: the latter is in fact captured by the change in liquidity of the control stocks, which is then subtracted from the change in liquidity of the banned stocks.

As explained in Section 5.1, Graph 2 shows that the financial stocks from the three countries of interest that have not been subject to the ban, appear to have behaved in the same way banned stocks have for the period before the ban, their trends in liquidity being consistently similar. It follows that these stocks are good candidates for the group of counterfactuals in the model.

As mentioned, what matters for identification are common *trends*, not common levels of liquidity. In fact banned and unbanned stocks can differ in levels of bid-ask spreads, and the difference is captured by the presence of stock-level fixed effects, *i.e.* individual determinants of liquidity. In formulas:

$$E\left[S_{ict} \mid c,t\right] = \gamma_{ic} + \lambda_t \tag{1}$$

This equation says that in the absence of the ban, the bid-ask spread of stock i is determined by the sum of constant stock-specific effects, γ_{ic}, with "c" indicating the cohort (banned vs. unbanned)- which allows the levels for the two cohorts to differ, and a time effect, λ_t, that is common across the two cohorts of stocks, capturing the market variability. Calling D_{ct} the dummy variable indicating the presence of the short-selling ban, I can therefore write in regression form:

$$S_{ict} = \gamma_{ic} + \lambda_t + \delta D_{ct} + \varepsilon_{ict} \tag{2}$$

More in detail, the variable γ_{ic} captures determinants of stocks liquidity which can be considered time-invariant in the event window under analysis, namely analyst coverage, number of market makers, but also the hard-to-measure regulation and enforcement of insider trading. The time effect, λ_t, captures the unobserved market variability that would bias the estimate on the ban if omitted. To identify δ, *i.e.* the impact of the ban, I carry out a difference in difference analysis, where the first difference is a time difference and the second one is carried across the two cohorts of stocks. To illustrate: calling t=0 the time before the ban and t=1 the time after the ban, I write

(a)
$$E\left[S_{0ict} \mid c = banned, t = 1\right] - E\left[S_{0ict} \mid c = banned, t = 0\right] = \\ = \left(\gamma_{iban} + \lambda_1 + \delta\right) - \left(\gamma_{iban} + \lambda_0\right) = \left(\lambda_1 - \lambda_0\right) + \delta$$

(b)
$$E\left[S_{0ict} \mid c = not\ banned, t = 1\right] - E\left[S_{0ict} \mid c = not\ banned, t = 0\right] = \\ = \left(\gamma_{inoban} + \lambda_1\right) - \left(\gamma_{inoban} + \lambda_0\right) = \lambda_1 - \lambda_0$$

which shows the changes in liquidity for the two cohorts of stocks separately. As the equations show, γ_c has disappeared in time difference: hence time-invariant unobserved confounders do not bias estimates[7].

However, as *(a)* shows, the impact of the ban is still clouded by the presence of the unobserved time trend λ_t. Differencing across cohorts (treated and controls) allows me to do just that, namely to filter out the day effect, λ_t from the estimated effect on the ban, δ, on the mild assumption that this time effect is common across the two groups. I write:

(c)
$$\left(E\left[S_{0ict} \mid c = banned, t = 1\right] - E\left[S_{0ict} \mid c = banned, t = 0\right]\right) - \\ \left(E\left[S_{0ict} \mid c = not\ banned, t = 1\right] - E\left[S_{0ict} \mid c = not\ banned, t = 0\right]\right) = \\ = \left(\lambda_1 - \lambda_0 + \delta\right) - \left(\lambda_1 - \lambda_0\right) = \delta$$

where δ is the causal effect of interest. To obtain consistent standard errors on the estimated δ I follow the methodology proposed by Bertrand, Duflo and

[7] Note that constant observable variables – as for instant constant free float – do not cloud the estimated impact of the ban, δ – I would just explicitly include them in the model. A bias exists if there are hard-to-measure or unobservable variables that may impact stocks liquidity (think for instance of the hard-to-measure "enforcement of insider trading regulation"). Although not accurately measurable, these variables cannot be omitted (see ANGRIST J.D. and PISCHKE J.S., 2009 for an extensive treatment of the issue). However, if in the time span under inspection, such variables can be confidently considered as constant, then the fixed-effects specification allows to get rid of these unobserved confounders.

Mullainathan (2004): the authors have indeed shown that conventional difference-in-difference standard errors obtained with an OLS on a panel dataset largely understate the standard deviation of the estimated treatment effect, thus over-estimating *t*-statistics and significance levels. AR(1) corrections -which have been used in past short selling ban studies- have been shown to fare poorly too. In my study I therefore follow the solution proposed by the authors: namely I apply a correction that collapses the time series information into a *pre* and *post* period, which has been shown to produce the correct level of significance for studies with numbers of treated individuals (stocks, here) N>50 and with individuals receiving treatment at exactly the same time, as it is the case here.

Specification *(2)* could be improved adding additional controls, namely additional time-varying variables that may affect stocks' bid-ask spreads [8]. To do so, I build on theoretical models of the determinants of bid-ask spread and I include in the model specification the matrix of cohort-and-time-varying covariates X_{ict} that captures the stocks market capitalization and price[9]. Therefore:

$$S_{ict} = \gamma_{ic} + \lambda_t + \delta D_{ct} + X'_{ict}\beta + \varepsilon_{ict} \tag{3}$$

It follows that the only assumption on which this model rests, is common λ_t across the two cohorts of banned and unbanned financial stocks. Differently from previous literature, the assumption in my model is not stringent: indeed, while it is debatable whether financial and non-financial stocks do react similarly to shocks in the market, it is instead highly likely that very interconnected financial stocks comove.

In fact, the interconnectedness among financial stocks and its implications have been discussed at length in the literature. Starting from Chordia, Roll and Subrahmanyam (1999); Forbes and Rigobon (2002); Croux, Forni and Reichlin (2001); Campbell, Koedijk and Kofman (2002); up to Kritzman, Li, Page, Rigobon (2010) and Billio, Getmansky, Lo, Pellizzon (2010), this literature agrees

[8] See GLOSTEN L.R. and HARRIS L. (1988); MCINISH T.H. and WOOD R.A. (1992) and SCHWARTZ R. (1988) for an in-depth review of bid-ask spread determinants.

[9] Theory identifies trading activity as a determinant of stocks liquidity. Yet, although it might be argued that trading activity, captured by volumes of shares traded- should be included as a regressors, if there is reason to think that there exists a causal relationship of the ban on volume, then inserting the latter as controls would be econometrically incorrect and it would bias the estimates on the ban dummy (See ANGRIST J.D. and PISCHKE J.S., 2009) for an extensive treatment of the bad control problem). Results in section 7 show that the impact of the ban on prices is not significant so I include it in my matrix of cohort and time varying covariates.

on findings of high level of financial markets co-movement during both crises and stable periods. In particular Bekaert, Hodrick, Zhang (2009) finds that, in examining time trends, there has been a significant increase in stock return correlations within Europe, giving strong support to my assumption.

Besides relying on past literature, to check the robustness of my assumption I also carry out a parametric and a non-parametric test. First I run a falsification test: for the period before the introduction of the ban, I estimate my regression DD adding cohort specific time trends to the regressors of the model. I write:

$$S_{ict} = \gamma_{0c} + \gamma_{1ct}\sigma + \lambda_t + \varepsilon_{ict} \tag{4}$$

where γ_{0c} is the cohort-specific intercept and γ_{1ct} is the cohort-specific trend coefficient that multiplies the time-trend variable *t*. In other words, γ_{1ct} is an interaction variable: it interacts the day dummy with the cohort dummy[10]; adding this variable allows banned and unbanned control stocks to follow different trends in the time span considered. Checking the significance of the coefficient σ estimated on the interaction variable therefore means to check whether, in the pre-ban period, banned and unbanned stocks have experienced statistical significantly different trends in liquidity. As reported in Table 2, I find that no estimated coefficient is significant; this means that the spreads of the unbanned stocks that I use as controls appear to have reacted to events in the market in a similar way the spreads of banned stocks have, for the period before the ban was introduced. This result provides strong suggesting evidence in favour of the goodness of the control groups and their ability to mimic the behaviour of the banned stocks in absence of treatment.

I also run a second test: I build on the work of Kritzman, Li, Page, Rigobon (2010) and Billio, Getmansky, Lo, Pellizzon (2010), and use the non-parametric method of Principal Components Analysis to estimate the extent of co-movements due to common factors. For the *pre*-ban period, I derive the matrix of eigenvectors of the matrix of the stocks returns, and the vector of eigenvalues in descending order. In particular, I extract the principal components from the matrix of banned stocks' returns; and I do the same for the matrix of unbanned stocks returns. The *first* principal component (PC) captures the direction of maximum variance, or in other words it is the common factor on which all the variables load. If the first principal component of the banned stocks and the first PC of the unbanned stocks are strongly correlated, then this is an indicator that the

[10] The cohort dummy takes value 1 if the stock will be banned and 0 otherwise.

two groups of stocks co-move, as the common factor that affects all stocks is the same – in a statistical sense – for both groups. I find that the first principal component of the banned stocks and the first PC of the unbanned stocks almost entirely overlap with their correlation being as high as over 0.74. This test gives additional supportive evidence to the assumption in my paper.

Table IIIA report the results of my estimations. Column 1 reports results estimated under specification *(2)*, while column 2 reports estimates under specification *(3)*. Column 1 shows that the ban is associated with an increase of 0.59 percentage points in the quoted bid-ask spread. This result is significant at 1% significance level. This effect is quite large if one considers that the mean spread in the sample is 2.27 and the median spread is 1.76. Column *(2)* reports a smaller but still positive coefficient estimated at the 10% significance level, confirming a significant increase in bid-ask spreads.

This result is particularly important for two reasons: first it is estimated under very mild identification assumptions when compared to previous literature, therefore it is less likely to be driven by confounding factors. Secondly, this result is economically reasonable if one compares it to the much harsher impact on liquidity estimated during the 2008-2009 experience, when the ban was associated with an increase in the bid-ask spread of financial firms of up to 2.75 percentage points: the 2011 European bans did not prohibit short sales *tout court*; what was prohibited under the ban was taking *net* short positions, as explained in section 3. This means that trades in this direction were not banned altogether; instead a long balancing position was required, even when in cash-settlement. Hence, the 2008 stricter ban disrupted liquidity more than the 2011 looser ban.

5.2.2 *Differential Liquidity Effects*

After establishing the average treatment effect on banned stocks' liquidity, I now assess whether the ban had differential liquidity effects by considering: *i)* the presence of options; *ii)* free float.

i. Traded Options

First of all, I inquire whether the presence of an option market has allowed traders to synthetically express bearish positions on the underlying stock. If this is the case, stocks with traded options might have suffered less from the imposed restrictions than stocks without options. This question is particularly interesting from a policy perspective, since this ban differed from the previous ones in so far as restrictions were imposed also on any shorting position expressed through the derivatives market; in other words, a short position synthetically reproduced by, say, buying a put on the banned stock, would be included in the counting of the trader's net short positions. Analysing differential liquidity effects for the stocks with traded options would henceforth provide a check on the effectiveness of the ban and its enforceability.

In order to cast light on this issue, I classify stocks into two groups: those that have traded options and those that don't. To do so, I cross information from Bloomberg and the national exchanges, so to be able to identify the two groups. I expect that no significant differences should arise between the two groups, in the scenario of the correct enforcement of the ban. To assess differential liquidity effects, I run two separate regressions (namely: banned and controls with options; banned and controls without options) and then test whether the difference in the estimated coefficient is significantly different form zero. To obtain a correct estimate, since I cannot compare two separate regressions estimated on independent distributions, I run a seemingly-unrelated estimation that corrects for variance and covariance, producing reliable significance of the test. Results are reported in Table 4. As expected, the difference in liquidity effects for stocks with traded options and stocks without is not significant, so we cannot reject the null hypothesis that the ban was able to prevent traders from entering into *net* short positions through the use of the derivatives market, as instead had been the case for the 2008 bans.

ii. Free Float

As explained in Section 3.2, restrictions to shorting activity exist also in the absence of legal restrictions or bans due mainly to the nature of the market, to the possibility for the lender to recall the stock and to the presence of strategic investors. While the decentralized nature of the market and the right to recall do affect all stocks alike, the presence of strategic investors in certain groups of assets creates differential conditions for those stocks. Indeed, there may be stocks held largely by retail investors or investors with a direct stake in the firm, and hence harder to borrow and trade even in the absence of a ban. In this section, I focus on this class of stocks to inspect whether the short-selling ban has disrupted their liquidity *less* than it did for easy-to-trade stocks. To shed light on this possible effect, I inspect the presence of a differential liquidity effect for stocks with above vs. below mean free float[11]. The free float indicates the percentage of shares that

[11] Free float is calculated by subtracting the shares held by insiders and those deemed to be stagnant shareholders from the shares outstanding. Stagnant holders include ESOP's, ESOT's QUEST's, employee benefit trusts, corporations not actively managing money, venture capital companies and shares held by Governments.

investors can freely negotiate in secondary market, as they don't belong to controlling interest investors: the higher the float the easiest it is to locate and trade the stock.[12] It follows that, since the ban significantly widened banned stocks' spreads, I expect the liquidity of high free float banned stocks to have been disrupted even more than the liquidity of banned stocks that are mainly in the hands of strategic investors.

Analogously to the analysis conducted for traded options, I run a seemingly-unrelated estimation that corrects for variance and covariance, producing reliable significance of the test. Results are reported in Table 5 and show that the difference between the two coefficients is significantly different from zero. In particular, the ban appears to have dramatically disrupted the liquidity of banned stocks with high free float, while its impact appears non significant on low-free float stocks. This might be due to the fact that low free float stocks are mainly in the hands of controlling interest shareholders who are known for being reluctant (or sometimes even forbidden) to lend their shares; it follows that low free float stocks are already difficult for traders to locate and short, even in the absence of a ban. This is instead not the case for high free float stocks for which short positions are easy and frequent to enter into, and are therefore the group that is more likely to have been hit by the ban.

5.3 *Matching Estimation*

In Table 3A Column (3) I further check the robustness of my estimation by performing a more refined diff-in-diff where I show that for each banned stock I can find an unaffected stock that is similar along all important dimensions- sensitivity to manipulation, size, leverage, volatility, price- to the banned one. The rationale is as follows.

[12] To allow for comparability across stocks and in time, it is common to use proxy of lending supply in percentage terms (see SAFFI P. and SIGURDSON H., 2011). In a time series analysis in fact using absolute free float would be a biased measure- over time the free float might increase in absolute terms; however the effective lending base might be larger or smaller according to whether the free float is growing respectively at higher or lower rate than the total capitalization. Also, consider that a purely speculative trader needs to be able to push the price in his favour (down) to avoid losses. This is very hard to achieve when, in percent terms, a big slice of the stock equity is in the hands of strategic investors. Therefore, even when stock A has higher absolute float than stock B, if stock A has lower percentage float, then stock A is harder to manipulate: the pool of optimistic non-lending investors is in relative terms much bigger, and can counteract any speculative attempt. The speculative short seller will therefore not target a low-percentage free float stock.

Unless the regulator wanted to discriminate between stocks, thus introducing a layer of unfair competition by protecting only *selected* assets, it could be that the regulator deemed the banned stocks to be an easy target for abusive short sellers, while it must have deemed the stocks not under ban to be more difficult to manipulate, and thus not in need of a regulatory protection. The first thing to check is therefore whether banned and unbanned stocks show different degree of shorting sensitivity.

I therefore rely on the literature (D'Avolio, 2002 and Asquith, Pathak, Ritter, 2005 among others) and use free float as a measure of sensitivity. For a stock to have a low free float, it means that the majority of the shares are in the hands of non-lending optimistic investors or investors with vested interests; it is therefore very hard for an abusive short seller to start a downward spiral in the price of those stocks. High free float is thereby capturing ease of uninformed shorting.

Beside sensitivity to manipulation, I also rely on the asset pricing literature and consider stock specific characteristics that could have led to a differential treatment with respect to the ban. In particular I focus on size, leverage, volatility and price.

I then rely on the matching methodology proposed in Wurgler and Zhuravskaya (2002): for each banned stock I create a portfolio of substitutes that constitute my new group of controls; the portfolio is made of three unbanned stocks from the financial sector that are closest to the banned stock in terms of the above specified characteristics. More specifically, for each banned stock I select the three unbanned stocks within the financial sector that minimize the sum of the squared percentage difference of size, stock price, leverage, volatility, and float. The matching procedure is done with replacement so that a control stock can be used as match for more than one treated stock. Being the countries in my sample all European and subject to harmonized trading regulation, I choose not to condition the matching on the country of the firm incorporation, on the basis that an investor can easily substitute, say, a banned Italian stock for a French non-banned one. Data used for the matching procedure are computed before the event window under scrutiny, so as at May 2011.

Conditional on all these information, there is no reason why a stock should receive a short sale ban or not. In other words, after controlling for size, leverage, volatility, price and sensitivity to manipulation, it is really hard to argue that there is a source of omitted heterogeneity driving selection; the two groups, banned and controls, are perfectly comparable. I run my regression on this matched subsample and report results in Table 3A Column (3). The regression results are virtually unchanged, and they show that the ban has disrupted liquidity of banned stocks at the 5% significance level.

6. - Notes on Endogeneity

As mentioned earlier, looking back at the previous literature, the possible existence of an endogeneity bias has resulted in a main limitation in the interpretation of results. Indeed, previous papers have tried to capture market-wide variability using two-way fixed effects or selecting control groups from non-financial industries, implicitly assuming that market-wide variability in the financial sector is similar to, and can be captured by, the variability appreciated in other sectors. However, it is hard to think that trends in the financial stocks can be controlled for using counterfactuals belonging to very different industries, such as retail and manufacturing; this left researchers with a possibly spurious effect estimated on the ban. Given these premises, this paper makes a substantial contribution to the identification strategy. Indeed, the only assumption made here is common time trends, λ_t across the two cohorts of financial stocks.

It is worth noting that this loose identification assumption is supported by both theoretical and empirical arguments: Chordia, Roll and Subrahmanyam (2000) first inspected common trends in liquidity across assets, and found that, quoted spreads tend to co-move with their respective industry-wide liquidity, even after accounting for individual determinants of liquidity. More recently Brunnermeier and Pedersen (2009) have shown that commonality in liquidity across securities can be explained in the light of funding restraints for speculators: more precisely, they start by considering the fact that trading requires existing capital in the form of a margin, and then show that when an exogenous shock imposes funding constraints on speculators, such traders become reluctant to take position in capital-intensive stocks thus reducing liquidity at the market-wide level. The authors also show that this market-wide liquidity trap effect is harsher during a crisis, when capital available is already diminished and the risk of hitting margin constraints is higher. Moreover, recent research on contagion and systemic risk has cast light on the high level of interconnectedness of players in the financial sector: Acharya and Yorulmazer (2008) have shown that banks have an incentive to herd and undertake correlated investments because this increases the portion of risk that is systemic and hence maximize the chances of them being rescued at times of turbulence; on this note, Brown and Dinc (2011) show that government running large budget deficit are reluctant to let a failing bank close when the financial sector is weak, thus giving even more credit to the idea that *ex-post* regulation might be time-inconsistent and might indeed foster herding behaviour in the financial arena.

Considering the results by these authors together, it follows that previous research does give reason to believe that very interconnected financial stocks do behave similarly in terms of trends in market liquidity, so that it can be confidently assumed that the λ_t is common across the two cohorts of banned and unbanned stocks.

This argument is even more sound if one considers the increased degree of connectedness among different players in the financial sector coming from the diversification of core services: in the past 10 years, financial innovation has blurred the distinctions between business types, with financial players moving aggressively into previously non-core activities, and therefore increasing their linkages[13].

Finally, as explained in detail in Section 5.2.1, I have also conducted two tests, a parametric and a non-parametric one, and both tests have given strong suggestive evidence in favour of the goodness of my assumption.

7. - Stock Prices

Short sellers have frequently been pointed out as being the cause to the stocks' sharp decline in prices. As it is possible to read from the coordinated statement release[14], the expectations of the regulators were to stop, though the ban, any abusive short selling from driving down the price of financial instruments to a distorted level. The European 2011 ban was in fact accompanied by claims from the regulatory authorities that such restrictions would prevent any speculator to gain from spreading false or misleading information.

The theory on short selling has been ambiguous on this matter: on the one side, this trading strategy has been seen as contributing to efficient pricing, by uncovering distressed companies; yet on the other side, it has recently been questioned for its ability to price manipulate and negatively impact firms even when trading in the absence of information.[15]

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[13] See for instance the Geneva Association Systemic Risk Working Group Report (2010).

[14] CESR/10-298

[15] See Goldstein I. and Guembel A. (2007) and Shkilko A., Van Ness B. and Van Ness R. (2011) for an extensive treatment of the issue.

The predicted impact on prices of a short selling ban is hence not clear. European regulators seem to have embraced the view expressed by Brunnermeier and Oehmke (2008), according to which the ban would prevent predatory shorting and hence the liquidation of financial institutions that consequently end up falling short of their capital requirements. If stocks are under-priced because of a group of bearish traders acting on false information, the regulators' reasoning goes that by preventing that part of traders from spreading misleading information, stocks prices would then be able to go back up to their true value.[16]

Evidence gathered during the 2008-2009 worldwide bans seems to suggest that restrictions on short sales have done little to prevent the decline of stock prices. With the exception of the US, where results may have been affected by a multitude of concurring confounding events such as the TARP, short selling bans in Europe have appeared to have had a non-significant impact on shares prices. I therefore investigate whether this time regulators have been successful in achieving their goal and bringing back the values of the stocks higher to their fundamentals.

I begin by visually inspecting the behaviour of the cross-sectional average cumulative excess returns of banned versus unbanned financial stocks. Excess returns are defined as the difference between the stock returns and the respective country index returns. Graph 3 displays the behaviour of the two groups and shows that the two move very closely together, with banned stocks never experiencing an upsurge in terms of cumulative excess returns; this suggests the ban did not do much to support stock values.

I further move to panel regressions to analyse the effect of the ban in more detail. The model specification is equivalent to the specification *(2)*, namely I estimate the impact of the ban using difference-in-difference analysis and a standard error correction, with the dependent variable being here weekly excess returns. The choice of the weekly frequency is justified by the fact that previous literature (Bris, Goetzmann and Zhu, 2007) has found that this frequency strikes the right balance between noise and information. Estimates are reported in Table 3B Column (1). Column (2) instead reports results estimated on the matched subsample. Both results confirm the visual evidence in Graph 3: although the estimated coefficient on the ban is positive, it is not significantly different from zero.

[16] However, if the market believes that the short sellers are indeed informed and it has rational expectations as in DIAMOND D.V. and VERRECCHIA R.E. (1987), this prediction may not be correct: investors are aware of the impact of the ban on the composition of the pool of investors, so stocks are not overvalued on average.

8. - Conclusions

In this paper I study the impact of the European short selling ban of the summer of 2011, with the aim to shed light on the impact of the ban on market liquidity as well as on the effectiveness of the ban on stock prices performance. Studying the ban is particularly interesting because it differs in many aspects from previously imposed short-selling restrictions; moreover, the very nature of this ban gives the opportunity to identify the effect of the regulation on the market with more precision and under a milder assumption.

The 2011 regulatory measure against short selling appears to have significantly increased bid-ask spreads, thus amplifying the strains in traders' quests for liquidity at a time of already great hardship. Moreover, stock prices did not appear to have benefited from short selling restrictions; the impact of the ban is indeed not significant. Gathering results from previous literature on short-selling bans and the most recent experience analysed in my thesis, it is evident that imposing short selling constraints may not be the right regulatory response: very stringent bans like the ones imposed worldwide in 2008 proved to be extremely detrimental for the market; loosely defined bans, like the recent European ban, hurt liquidity and fail to reach the target of the regulator.

A final note is worth pointing out. It is a good exercise asking whether the results of this paper are consistent when considered together, and whether these results are coherent with the view of short sellers held by the regulators. Can a short-selling ban dramatically disrupt market liquidity, but at the same time significantly increase stock prices? In fact the results of this paper, along with previous empirical evidence, seem to point to a negative answer: decreased market liquidity caused by the ban is paired with no effect on stock prices. The theoretical explanation behind these paired results may then lie in the argument brought forward by Brunnermeier and Pedersen (2009): when market liquidity goes down, market risk increases, thus making investors require a premium to trade in those stocks that are experiencing higher market risk; this premium is then reflected into prices that hence decline. It follows that any possible positive impact on stock prices may therefore be completely offset by the indirect effect that the regulation has on prices through market liquidity. Policy makers should therefore consider this before imposing bans that harshly impair market liquidity, especially at times of already peaking spreads.

TABLE 1

STRUCTURE OF THE BAN

The ban was introduced on the same day – 12th August 2011 – in France, Italy and Spain. The ban was initially intended to last 15 days only, but it was then rolled over to September 30, and once again, up to November 11. It affected only selected stocks in the financial sector, regardless of the trading venue where transactions are executed (MTFs and OTC were henceforth included in the calculation of net short positions).

Country	Ban start date	Scope	Financial stocks	Banned stocks
France	12-Aug-11	Selected financial	139	10
Italy	12-Aug-11	Selected financial	65	29
Spain	12-Aug-11	Selected financial	52	16

TABLE 2

DIFFERENCE-IN-DIFFERENCE: COMMON TIME EFFECTS ACROSS COHORTS BEFORE BAN

I inspect whether the differences in trends over time in the liquidity of the banned stocks compared to their controls, for the pre-ban period, are significantly different from zero.

Specification	$S_{ict} = \gamma_{0c} + \gamma_{1ct}\sigma + \lambda_t + \varepsilon_{ict}$	
Interaction Variable γ_{1ct}	Average coefficient	0.472
	Number of estimates	70
	Number of significant estimates at 1 (5-10) % level	0 (0 – 0)

TABLE 3A

DIFFERENCE-IN-DIFFERENCE REGRESSION ESTIMATES - BID ASK SPREAD

All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. Coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5-10) percentage level. The number in parenthesis indicates the *t*-statistics.

	(1)	*(2)*	*(3)*
Countries	All	All	All
Dependent variable	Bid-Ask Spread	Bid-Ask Spread	Bid-Ask Spread
Ban	0.59***	0.17*	0.17**
	(3.74)	(1.67)	(2.26)
Stock Fixed Effects	yes	yes	yes
Time Effects	yes	yes	yes
Methodology	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff
	(specification (2))	(specification (3))	(Matching Estimation)
Number of stocks	223	223	80

TABLE 3B

DIFFERENCE-IN-DIFFERENCE REGRESSION ESTIMATES - EXCESS RETURNS

All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. Coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5-10) percentage level. The number in parenthesis indicates the *t*-statistics.

	(1)	*(2)*
Countries	All	All
Dependent variable	Excess Returns	Excess Returns
Ban	0.005	0.004
	(1.34)	(1.14)
Stock Fixed Effects	yes	yes
Time Effects	yes	yes
Methodology	Diff-in-Diff	Diff-in-Diff
	(specification (2))	(Matching Estimation)
Number of stocks	223	80

TABLE 4

BID-ASK SPREADS AND SHORT-SELLING BANS: DIFFERENTIAL LIQUIDITY EFFECTS - OPTIONS

All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. Below, I run a seemingly-unrelated estimation to test the presence of any differential liquidity effect. The coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5) percentage level. The number in parenthesis indicates the *t*-statistics.

	(1)	*(2)*
Countries	All	All
Ban	0.70***	0.56***
	(11.35)	(2.36)
Sample	Stocks with traded options	Stocks without traded options
Test of Difference in Coefficients	*Not significant:* Pr>Chi2 = 0.254	

TABLE 5

BID-ASK SPREADS AND SHORT-SELLING BANS: DIFFERENTIAL LIQUIDITY EFFECTS - FREE FLOAT

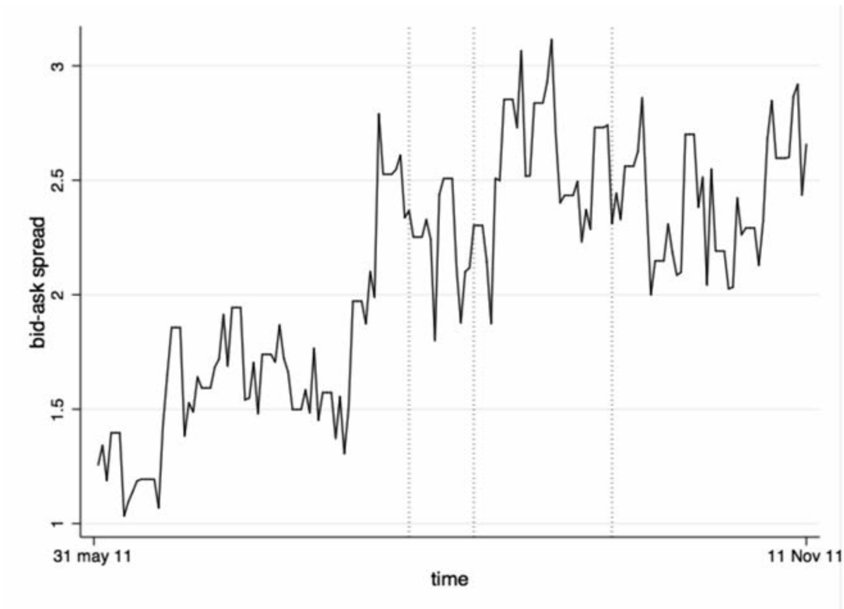
All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. Below, I run a seemingly-unrelated estimation to test the presence of any differential liquidity effect. The coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5) percentage level. The number in parenthesis indicates the *t*-statistics.

	(1)	*(2)*
Countries	All	All
Ban	1.56***	-0.19
	(8.42)	(-1.48)
Sample	Stocks with high free float	Stocks with low free float
Test of Difference in Coefficients	*Significant:* Pr>Chi2 = 0.000	

GRAPH 1

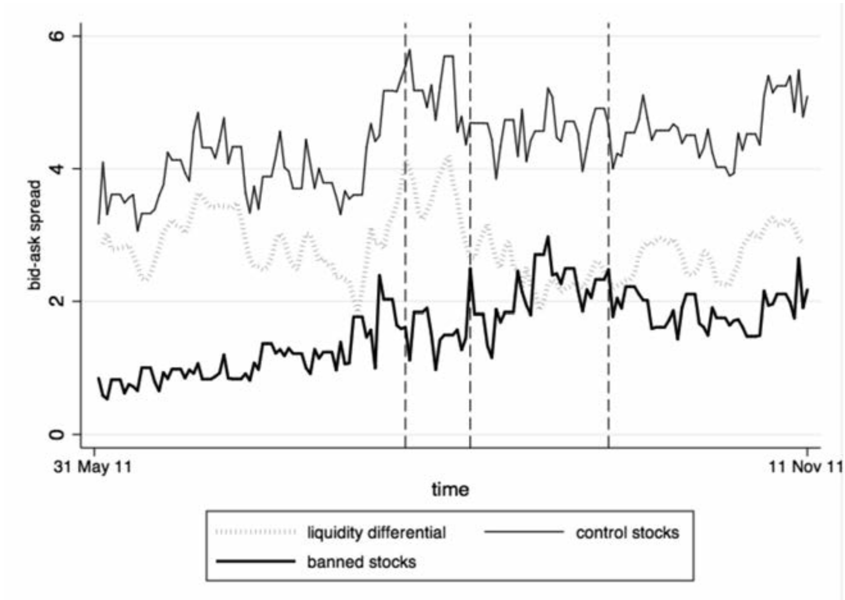
EVOLUTION OF THE EUROPEAN AVERAGE PERCENTAGE QUOTED BID-ASK SPREADS

Evolution of equally weighted cross-sectional average quoted bid-ask spreads for all financial stocks, both banned and non-banned, for the matched sample, in the three countries considered; the dashed lines correspond to the introduction of the bans and their extensions.



GRAPH 2

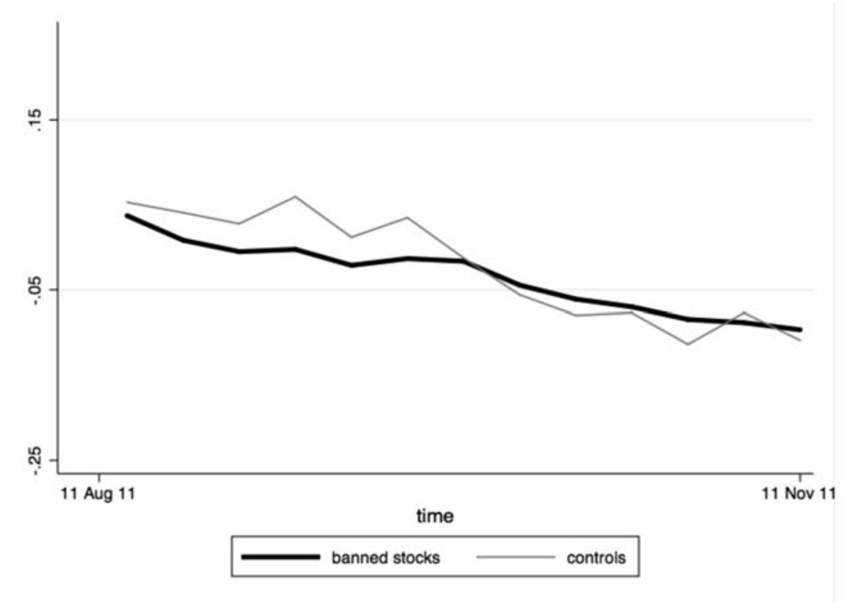
AVERAGE PERCENTAGE QUOTED BID-ASK SPREADS FOR THE BANNED STOCKS AND THEIR CONTROLS



GRAPH 3

CUMULATIVE EXCESS RETURNS

Cumulative excess returns of banned and unbanned stocks for the post-ban period. Cross-sectional average cumulative excess returns of banned stocks compared to unbanned stocks for the post-ban period. Excess returns are defined as the difference between the stock returns and the respective country index returns.



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Are Sovereign Credit Ratings Pro-Cyclical? A Controversial Issue Revisited in Light of the Current Financial Crisis

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With the present work I aim to shed new light on the debate on possible pro-cyclicality of the foreign sovereign credit ratings issued by credit rating agencies (CRAs), considering their behavior in the current financial crisis. I find that the assumption of a pro-cyclical behavior of CRAs appears to be groundless when referring to the period 2001-2011. Actually, even though the ratings issued throughout the pre-crisis period (2001-2006) are generally higher than the predicted ones, there is no evidence of CRAs assigning unduly worse ratings during the years of the crisis (2007-2011).
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1. - Introduction

In the context of the global financial crisis, that erupted in 2007 and impacted strongly the euro area, credit rating agencies (CRAs) have been frequently blamed for exacerbating negative market developments, due to the alleged inability to make appropriate judgments on sovereign creditworthiness resulting in unduly severe downgrades.

The question addressed in this paper is whether the rating agencies tend to assume a pro-cyclical behavior, issuing ratings during the pre-crisis period (crisis period) more favorable (worse) than those justified by the analysis of the actual macroeconomic fundamentals and qualitative factors of a country, thus resulting in an unduly amplification of the business cycle.

This issue brings to light earlier debates that emerged during the Asian crisis of 1997-1998. Even then, rating agencies were widely criticized for having assigned, after the outbreak of the crisis, excessively severe sovereign credit ratings. In particular, Ferri *et* al. (1999) denounce the "conservative approach" taken by the CRAs after failing to forecast the build-up to the crisis, with the assignment of ratings lower than those strictly deriving from the analysis of countries' macroeconomic fundamentals. Doing so, they may have contributed to the worsening of countries' fundamentals and, eventually, to the deepening of the financial turmoil.

On the contrary, Mora (2004) partially revisits Ferri *et* al.'s conclusions by substantially refusing to hold the credit rating agencies responsible for unduly impacting on market expectations with their assessments and for eventually aggravating the Asian crisis. Indeed, on the basis of her findings Mora talks of stickiness of sovereign credit ratings rather than of pro-cyclicality.

With the present work I aim to shed new light on the debate on possible pro-cyclicality of the foreign sovereign credit ratings issued by CRAs considering their behavior in the current financial crisis with reference to advanced economies[1]. The exercise refers to Standard and Poor's and Moody's, as they hold around 80% of the relevant market.

In the development of the thesis, I build on the models of Ferri *et* al. and Mora and then I introduce some innovations with reference to the width of the sample of countries examined, the explanatory variables considered and the modalities adopted for the analysis. Moreover, as I mentioned above, the main contribution

[1] The exercise focuses on developed countries as the current crisis originated in advanced economies, leaving many emerging market economies relatively untouched.

to the literature is to switch the analysis of CRAs' sovereign ratings pro-cyclicality from the context of the Asian crisis to the context of the current financial and economic crisis.

With regard to the sample, while Ferri *et* al. and Mora take into account respectively 17 and 88 countries, I extend it to 96 countries (34 developed and 62 emerging, on the basis of the IMF classification) in order to increase its information value.

As for the explanatory variables, unlike Ferri *et* al., who regress the ratings issued by Moody's only against the macroeconomic fundamentals of the countries concerned, in my work I follow Mora's approach in using econometric models that, by referring also to non-macroeconomic variables, aim at capturing both the qualitative and quantitative criteria adopted by the CRAs in their assessment of countries' creditworthiness.

The modalities I adopt for the analysis are as follows:

- firstly, with a linear transformation I assign to each of the 21 rating categories of S&P's and Moody's a number ranging from 100 (AAA/Aaa) to 0 (C/SD) and, with respect to each individual country, I take the average of the ratings issued by the two CRAs for every year in the period 2001-2011;
- secondly, with a linear and an ordered probit models I regress across the whole sample the average of the CRAs' ratings against the basket of variables used as proxies of the determinants of the foreign sovereign credit ratings; furthermore, I conduct an analysis articulated in two sub-periods (pre-crisis and crisis) and two sub-samples of countries (developed and emerging economies) in order to make sure that the results are robust to different specifications and, if not, to understand the causes;
- finally, in order to verify the assumption of CRAs' ratings pro-cyclicality, I build the benchmark against which to judge the assigned ratings. This benchmark consists of the predicted ratings calculated firstly on the basis of the coefficients obtained from the linear regressions across the full sample and secondly, as a further exercise, using the coefficients calculated separately for the pre-crisis period (2001-2006) and for the full-blown crisis period (2007-2011).

According to the literature, the conclusion that a pro-cyclical behavior has been adopted by rating agencies would depend on two conditions: *1)* in the pre-crisis period, assigned ratings (AR) are on average better than predicted ratings (PR) and, on the contrary; *2)* during the crisis, AR are on average worse than PR; in other words, pro-cyclicality is an expression of "over-reactive" behavior.

The results of the analysis lead me to the conclusion that no pro-cyclicality can be detected in the CRAs behavior during the current crisis with reference to the basket of developed countries on which I focus the exercise.

The rest of the paper is organized as follows: Chapter 2 contains a literature review; Chapter 3 is devoted to explaining in detail the main features of the dataset; Chapter 4 hosts the development of the models and the overall results of the study; Chapter 5 concludes.

2. - Literature Review

The foreign sovereign credit rating issued by CRAs pertains to a sovereign ability and willingness to service financial obligations to non-official, in other words commercial, creditors.

The factors that lead a government to declare default on its debt, which must be taken into account by CRAs when formulating their assessments, are numerous and complex[2], so that their reconciliation in a quantitative model is extremely problematic.

Numerous studies attempt to identify the intricate set of key factors used by rating agencies. The first systematic work is that by Cantor and Packer (1996) who, in analyzing the determinants of sovereign credit ratings, extrapolate eight key variables: *per capita* income, GDP growth, inflation, fiscal balance, external balance, external debt, economic development, default history. Considering a sample of 49 countries, the authors regress with an ordinary least squares the numerical equivalents of the ratings issued in 1995 by S&P's and Moody's against the eight explanatory variables previously identified. The results show that six of the eight variables have a systematic relationship with the ratings assigned (for example, lower inflation and lower external debt are consistently related to higher ratings) and the model predictive ability is quite impressive (R^2 = 92%) .

Subsequent researches by other authors have extended the range of the explanatory variables underlying the determination of ratings and further refined the econometric models used.

Juttner and McCarthy (1998) found that the relation used by Cantor and Packer was unstable if estimated with reference to a multiple year period; indeed, the authors reestimated Cantor and Packer's equations with data from 1995,

[2] See S&P's (2011).

1996, 1997 and 1998 and found that while the estimation results for both 1996 and 1997 are similar to the 1995 results, the number of significant variables and the proportion of the variation in the ratings explained by the regression declined significantly for 1998. This leads the authors to state that rating behavior changes during crisis and cannot be predicted.

Monfort and Mulder (2000), building on Juttner and McCarthy's conclusions, introduce changes to Cantor and Packer's static specification estimating a dynamic model that allows for lags in ratings and in the set of explanatory variables. The authors assume that if credit rating agencies see through the cycles, ratings would be constant and react only to unexpected innovations in variables thus following a random walk. Their results show that this condition for a random walk is almost fulfilled but not quite to suggest that CRAs see through the cycles: indeed, while it is true that the coefficient of the lagged dependent variable is close to one and the change in ratings responds to innovations (especially in the share of investment in GDP and inflation), it is also true that the variables lagged export growth and, to some extent, lagged debt over exports appear to contribute to current ratings.

Ferri *et* al. (1999), following Cantor and Packer, propose an empirical study to determine whether the credit rating agencies were pro-cyclical during the 1997-98 Asian crisis and, if so, to what extent. They run an unbalanced panel with random effects considering the variables specified by Cantor and Packer and using a sample of 17 countries for a period of 10 years (1989-1998). They find the expected sign for the coefficients of all the explanatory variables taken into consideration and a confirmation of their statistical significance (with the exception of GDP *per capita* and inflation rate). In order to demonstrate their hypothesis of pro-cyclicality of CRAs behavior, the authors compare the predictions arising from their model to the effective ratings. The results show that, in the pre-crisis period, actual ratings were consistently above the predicted ones and, during the first year of the crisis, dropped much more than what their model predicts. They also find that in 1998 the model-predicted ratings converged to the actual ratings, so reflecting – according to the authors – the endogeneity of macroeconomic fundamentals to ratings through the ratings effect on investors' capital outflow and the subsequent damage to fundamentals.

Ferri *et* al.'s conclusion of excessively conservative behavior of ratings for the East Asian economies was revisited by Mora (2004). In her analysis she starts by discussing some technical limitations to Ferri *et* al.'s setting (among these, the use of the minimum Moody rating in a year, the use of a linear model with random effects and the neglect of the influence of non-macroeconomic variables[3]) in order to propose new solutions aimed at improving the quality of the results. The sample is enlarged to 88 countries and the period extended to 13 years (from 1989 to 2001) considering as a dependent variable the average of Moody's and S&P's ratings over each year. In addition to the classical macroeconomic fundamentals she also uses Eurobond spreads or EMBI spreads as proxy for market sentiment. As alternative to the linear model, an ordered probit specification is introduced. The latter, indeed, should better fit the rating behavior since it is a model for ordinal dependent variables. The results lead Mora to adopt a more cautious view with respect to the supposed pro-cyclical effect of ratings. While the predicted ratings were lower than the assigned ones during the pre-crisis period, there is little support for Ferri *et* al.'s findings during the crisis since in this period the assigned ratings seem to track predicted ratings for most of the Asian countries. Therefore, ratings should be considered sticky rather than pro-cyclical. Another important conclusion, that derives from the extension of the sample to the post-crisis period and that is coherent with the sticky view, is the inertia in ratings. In other words, CRAs appear to be slow in changing their assigned rating once the crisis is over.

The global financial turmoil of these years and the dramatic shifts in the ratings assigned to many developed countries give us the opportunity to update the analysis concerning the behavior of the two main CRAs and their assumed pro-cyclicality.

3. - Data

I consider a balanced panel of 96 countries, in the period from December 2001 to December 2011. In order to build the dependent variable, I use the long term foreign currency ratings published by the two rating agencies Standard & Poor's and Moody's. While for some of countries in the sample CRAs have issued multiple ratings per year, I consider only the rating in place at the end of each year, since the explanatory variable used in my regressions are available only on a yearly frequency. The twenty-one grading notches expressed in letters (from AAA to SD by S&P's and Aaa to C by Moody's) are linearly converted to a scale ranging from 100 to 0, with 100 being AAA/Aaa and 0 being SD/C (see Table 1). As both CRAs use the same number of grading notches, it is possible to use

[3] Such as Eurobond spreads and a country's default history.

the average of their values for each year. In this way, I can obtain a measure of a country's average assessment by the two leading credit agencies.

The correspondence between the number of grading notches makes it also possible to compare the ratings released by S&P's and Moody's throughout the observed period. Spearman's rank correlation coefficient is quite high (0.98) suggesting that CRAs' issued ratings are quite aligned. Specifically, as shown in Table 2, around 50 per cent of all the observations have the same pair-wise ratings and around 88 per cent of the differences between S&P's and Moody's ratings lie within the range of one notch.

These results might imply that the two rating agencies give substantially similar weights to various factors in their assessment of a country's default risk.

As for the explanatory variables, among those mostly labeled in the academic literature as the determinants of the sovereign credit ratings, I select the ones enabling to capture, as much as possible, both CRAs' quantitative and qualitative criteria[4].

Macroeconomic data are taken from the International Monetary Fund's World Economic Outlook Database 2012, from the World Bank's World Development Indicators 2011, from Datastream and from National Banks statistics.

The set of the explanatory variables and their relationship with the foreign sovereign credit ratings are described below:

1) GDP *per-capita* (PPP): higher GDP *per-capita* should positively influence ratings since a broader potential tax and funding base upon which to draw makes a country less vulnerable to exogenous shocks;
2) GDP growth: also in this case, it can be expected that higher GDP growth has a positive impact on ratings, since it strengthens the sovereign's capacity to reimburse outstanding debts. On the other hand, Cantor and Packer (1996) obtain a negative relationship, justifying the result with the explanation that developing countries usually have a growth rate higher than developed economies;
3) Inflation: generally, high inflation is associated to lower ratings because it could be symptomatic of problems at the macroeconomic policy level;

[4] In issuing their ratings CRAs take into consideration also the long-term perspectives of the economies. Nevertheless, as also stated by Afonso A. *et al.* (2011), CRAs' projections are strongly based on current information, which is just what my modeling focused on. Moreover, I have decided not to consider financial spreads and other "asset-price" variables, that are meant to be forward-looking, in order to avoid any influence from external factors, possibly driven by market sentiment, that could bias an analysis based on countries' macroeconomic fundamentals.

4) Unemployment rate: possible changes in the economic environment can be better faced by a country with lower unemployment in the context of a more flexible labour market. Higher labour taxation and reduction of the social benefits that accompany lower unemployment are also elements that contribute to explain the expected negative relation with ratings;
5) General government gross debt (percent of GDP): a higher debt burden should correspond to a higher risk of default. Therefore, the expected relationship is negative;
6) External Debt (over export): a higher level of external indebtedness increases the risk of additional fiscal burdens. The impact on ratings should be negative;
7) Current Account[5] (percent of GDP): a large current account deficit indicates that the public and private sectors together rely heavily on funds from abroad. If this deficit persists over time, the resulting growth in foreign indebtedness may become unsustainable;
8) Government Effectiveness: this variable contributes to define CRAs' political score. Better socio-political conditions of a country are at the base of a higher rating;
9) Default History: reputation in sovereign debt is an important element to consider. Indeed, CRAs try to capture not only the ability but also the willingness of a country to service its financial obligations. Therefore, the impact on ratings should be negative since the credit risk raises for those countries that have defaulted in the past.

4. - Methodology and Results

The econometric approaches used in the literature on ratings can be conveyed by two main streams: linear regression methods and ordered response models.

Due to its good predictive power and good fit, the linear regression model based on a linear numerical representation of the ratings has often been used in the studies on the determinants of the sovereign credit ratings either in its simplest form, represented by the ordinary least squares (Cantor and Packer, 1996; Afonso, 2003; Butler and Fauver, 2006), or in its slightly more complex version deriving from the generalization of the cross section analysis to panel data by doing fixed or random effects estimations (Monfort and Mulder, 2000; Eliasson, 2002; Canuto *et* al., 2004).

[5] Current Account variable is also intended to capture, even if only partially, the level of a country's competitiveness which plays an important role in the assessment of countries' economic prospects during the current financial crisis.

Nevertheless, many critiques on the adequacy of the above-mentioned model have been raised because of its cardinality measure, implying the strong hypothesis that CRAs consider the difference between two rating categories identical for any two adjacent categories.

On this issue, different quantitative transformations have been proposed: for example, Reisen and Maltazan (1999) and Afonso (2003) respectively apply a logistic transformation and both logistic and exponential transformation of the ratings.

The second main stream of the literature makes reference to the ordered response model. This is the generally preferred method as it defines the size of the differences between each category according to the ratings' qualitative ordinal measure. Among the authors that use this procedure there are Hu *et* al. (2002); Mora (2004); Bissoondoyal-Bheenick (2005); Bissoondoyal-Bheenick *et* al. (2005) and Afonso *et* al. (2011).

In order to get a more comprehensive view of the observed phenomenon, the empirical analysis performed in this study estimates the determinants of sovereign debt ratings with both a linear regression model based on a linear numerical representation of ratings[6] (specifically, fixed effects and random effects estimations) and an ordered probit with fixed effects.

Nevertheless, because of space constraints, my comments will focus specifically only on the results from the linear regression model[7].

4.1. *Linear Regression Framework*

In the linear regression framework, I run two balanced panels respectively with fixed effects and with random effects. Let's firstly consider the following model:

(1) $$y_{it} = \alpha + \beta x_{it} + u_{it}$$

[6] FERRI G. *et* AL. (1999) apply both a linear and a nonlinear transformation but the conclusions from the models are similar. AFONSO A. *et* AL. (2011) use both linear and logistic transformations. They find out that the linear transformation is "quite adherent to the data" and that, more in general, the basic results and point they make are preserved in both models. Furthermore, according to S&P's (see BEERS D.T. and CAVANAUGH M., 1998) no such difference between the two types of transformations exists. For all these reasons, in this study a linear transformation will be used.

[7] In general (see Table 3), the results from the ordered probit estimation confirm those from the linear models for what concerns the statistical significance and the sign of the coefficients on the explanatory variables. The main difference pertains to their lower order of magnitude in comparison to those obtained with the linear models.

where y_{it} is the dependent variable (average of S&P's and Moody's foreign currency ratings); α is the intercept term; β is a k x 1 vector of coefficients on the explanatory variables; x_{it} is a 1 x k vector of explanatory variables and u_{it} is the disturbance term; $t = 1,...,T$; $i = 1,...,N$.

Specifically, taking into consideration the independent variables presented in Chap. 3, model *(1)* can be rewritten as:

(2)
$$y_{it} = \alpha+\beta_1 GDPpercapita_{it}+\beta_2 GDPgrowth_{it} + \beta_3 inflation_{it} + \beta_4 unemployment_{it} + \beta_5 grossdebt_{it} + \beta_6 externaldebt_{it} + \beta_7 currentaccount_{it} + \beta_8 gov.effectiveness_{it} + \beta_9 defaulthistory_{it} + u_{it}$$

The first approach I use to estimate equation *(1)* is the fixed effects regression. This method proves useful for controlling for omitted variables in panel data when the omitted variables vary across entities (countries) but do not change over time. The fixed effect regression can be obtained from equation *(1)* by decomposing the disturbance term, u_{it}, into an individual specific effect, μ_i, that encapsulates all of the variables that affect y_{it} cross-sectionally but do not vary over time, and the "remainder disturbance", v_{it}, that varies over time and entities (capturing everything that is left unexplained about y_{it}).

(3)
$$u_{it} = \mu_i + v_{it}$$

Therefore, equation *(1)* can be rewritten by substituting in for u_{it} from *(3)*, thus obtaining the fixed effects model:

(4)
$$y_{it} = \alpha + \beta x_{it} + \mu_i + v_{it}$$

Unlike the random effects specification, as we will see below, for fixed effects analysis, $E(\mu_i|x_{it})$ is allowed to be any function of x_{it}. Therefore, the fixed effects model achieves one of the main purposes of applying panel data analysis, that is to allow for the omitted variable, μ_i, to be arbitrarily correlated with the x_{it}.

The second approach consists in estimating equation *(1)* by using random effects. Similarly to the fixed effects, the random effects estimation implies slopes estimates that are assumed to be the same both cross-sectionally and temporally. However, differently from the previous case, the intercepts of the random effects model are assumed to be composed of two parts: *1)* a common intercept α which is the same for all countries and over time and *2)* a random variable ε_i that, on the other hand, changes cross-sectionally but is constant over time. The random effects panel model can be written as:

$$(5) \qquad y_{it} = \alpha + \beta x_{it} + \omega_{it}, \qquad \omega_{it} = \varepsilon_i + v_{it}$$

where x_{it} is the same 1 x k vector of explanatory variables, ε_i is the new cross-sectional error term that measures the random deviation of each entity's intercept term from the "global" intercept term α; and v_{it} is the individual observation error term.

It is important to highlight that the random effects model relies on the strong assumptions that ε_i is independent of v_{it} and x_{it}, has zero mean and constant variance σ^2_ε. Therefore this framework, unlike the previous one, assumes $E(\varepsilon_i|x_{it}) = E(\varepsilon_i) = 0$.

Now, the question one should ask is which of the two models is preferable. In general, when the composite error term, ω_{it}, is uncorrelated with the regressors, $E(\omega_{it}|x_{it}) = 0$, a random effects approach is preferable to the fixed effects one. On the other hand, if this condition does not hold, it is better to use fixed effects that lead to inefficient but consistent estimates.

In order to unravel this problem, I adopt the Hausman specification test after equation *(1)* has been estimated by using random effects. The Qui-Square statistic is quite high ($\chi^2(8) = 35.63$) and the null hypothesis of no correlation is rejected with a p-value <5%; therefore, since the condition $E(\omega_{it}|x_{it}) = 0$ does not hold, the fixed effects estimation turns out to be the most suitable model.

Nevertheless, for completeness and comparison purposes, the results for both specifications are reported in Table 3.

In the fixed effects and random effects models, as for the factors that contribute to determine a sovereign economic score by CRAs, GDP *per capita*, unemployment and GDP growth have all the expected sign even if only the first two variables are statistically significant.

The inflation rate variable, as part of the monetary score, enters both the models significantly and with a negative sign. Therefore, this supports the interpretation of the negative effect that higher inflation – evidence of problems at macroeconomic level – may have on ratings.

Looking at the fiscal variable, general government gross debt (percent of GDP) is significant and has the expected negative sign both in the fixed effects and in the random effects models.

Considering the variables underpinning CRAs' external score, I find for both models that the external debt is statistically significant and has the expected negative sign, while the current account (percent of GDP) has a negative sign[8] and it is not significant. The absence of a strong and systematic relationship between this variable and the ratings may not be so startling: actually, while it is true that a high current account surplus is positively valued by the CRAs as a factor of strength of a country's economy with respect to the rest of the world, it is also true that only better rated countries are able to run current account deficits and borrow more easily from abroad.

Finally, as for the qualitative factors considered in CRAs' political score, government effectiveness and default history are statistically significant and assume respectively a positive and a negative sign in both models.

To conclude, most of the variables considered turn out to be both economically meaningful and statistically significant in explaining sovereign credit ratings. Moreover, as it can be seen from the statistics at the end of the Table 3, the explanatory power of the two models is relatively high with an overall *R-square* of 74% in the fixed effects estimation and 85% in the random effects one.

4.2. *Rating Regressions Across Sub-Periods*

In order to detect a possible evolution in the CRAs' rating methodology induced by the financial crisis, highlighted by changes in the value of the coefficients on the rating determinants, I divide the full sample into two sub-periods: the pre-crisis (2001-2006) and the crisis period (2007-2011).

As shown in Table 4, the coefficients for the sub-period 2001-2006 are generally in line with those for the full estimation period in both linear models. As for the sub-period 2007-2011, the coefficients estimates mostly feature the same signs as those calculated for the full period, but their significance and orders of magnitude are substantially different. In particular, both models suggest an increased importance assigned to gross debt and inflation rate and, at the same time, a decline in the significance level of the estimated coefficients on external debt, current account and default history. The linear regression with fixed effects also displays, differently from the random effects one, a lower weight and/or a lower statistical significance of unemployment and government effectiveness variables. These changes suggest that, in the context of the crisis, CRAs focused their at-

[8] MONFORT B. and MULDER C., (2000); ELIASSON A. (2002); MORA N. (2004) and AFONSO A. *et* AL. (2011) find a negative relationship between ratings and current account as percentage of GDP.

tention more than in the previous years on the governments' fiscal area and on the countries' problems at the macroeconomic policy level, as roughly measured by the level of the inflation rate.

In addition to this outcome, the exercise shows that, although the number of observations on the two sub-samples are fairly similar, the explanatory power of the models drops from 81% for the pre-crisis period to 58% for the crisis period, when we consider fixed effects, and from 89% to 79% in case of random effects. This may suggest that the classical rating determinants identified in the literature do not fully explain the rating variations occurred during the crisis.

However, the more CRAs move away from quantitative variables to give heavier weight to variables not measurable or otherwise difficult to quantify, the more they expose themselves to many criticisms, such as lack of transparency, lagging behind the markets and conflicts of interest deriving from rating agencies being paid by the assessed issuer itself.

4.3. *Rating Regressions across Sub-samples (Developed vs. Emerging Countries)*

As we have seen, the previous analysis reveals some differences, between the two periods analyzed, in weights and in statistical significance of the coefficients on the explanatory variables; this points to an adjustment of the methodology adopted by the CRAs in relation to the evolution of the business cycle.

Moreover, given that the full sample has a considerable degree of heterogeneity in the level of development of various countries and considered that «one of the main features of the recent financial turmoil is that it originated in advanced economies, with many emerging market economies relatively insulated»[9], I perform an additional analysis. In particular, I investigate whether the above-mentioned variations in the coefficients have affected all countries in the sample or mainly the advanced economies. For this purpose, I split the sample into two groups: "developed countries" and "emerging countries" according to the IMF definition.

In order to track more closely the evolution of the business cycle over the years, I conduct a recursive estimation of the coefficients for $t + i$ sub-periods where t = 2001-2005 and i = 2006, 2007, 2008,…, 2011. Therefore, starting from the period 2001-2005, I successively add all the observations for one year at a time till the full sample is covered. This procedure is applied to both sub-samples of developed and emerging countries.

[9] See Imf (2010, page 102).

In general, the results for the separate regressions confirm the overall results from the full sample (see Tables 5 and 6). Actually, some sign reversals can be observed across sub-periods in the two linear models for the explanatory variables GDP growth, unemployment, external debt and current account but only in case of insignificant or scarcely significant (10% level) coefficient estimates.

If, on the one side, the results of this analysis substantially corroborate the robustness of the outcomes from the full sample, on the other they uncover some differences (as for the level of statistical significance and for the size of the coefficients) between the two sub-samples in the various periods taken into account; this outcome offers interesting insights.

CRAs seem to give a different level of importance to the variables used for determining the ratings according to whether they refer to developed or emerging countries.

In particular, the factors linked to the commercial and financial relations of a specific country with the rest of the world – in my exercise current account and external debt – result in having a much more of an impact on the ratings of the emerging economies.

On the other hand, the determinants relating to inflation, unemployment and gross debt (the latter only starting from the sub-period 2001-2008) contribute more to define ratings assigned to advanced countries than those assigned to emerging countries, as shown, in general, by the higher weight and the higher level of statistical significance assumed by these determinants for developed economies in both models.

As for the evolution over time of the estimated coefficients, the exercise suggests that in the wake of the crisis the CRAs, when assessing developed countries, started assigning to gross debt and external debt variables greater importance than in previous sub-periods: indeed, the recursive estimation highlights that the coefficients assumed by the above-mentioned variables for developed countries become statistically significant only from the 2001-2008 sub-period and tend to progressively increase in absolute value in the following sub-periods.

This latter behavior also characterizes the coefficient of the inflation variable, with the only difference that it starts to be already statistically significant in the sub-period 2001-2005.

In contrast, for emerging countries a substantial stability of the weights of the same variables can be noticed throughout all the sub-periods.

Finally, looking at the predictive power of the individual regressions, a progressive worsening can be noticed, in particular, starting from the sub-period

2001-2008 only for the set of developed countries. This finding seems to better explain the results of the exercise conducted in paragraph 4.2: indeed, it indicates that the reduction of the predictive power of the models in the transition from the pre-crisis to the crisis period is mainly due to changes intervened in the assessment process of developed countries, with the CRAs using in their rating methodologies new variables in addition to those already used.

In conclusion, the analysis conducted in the present paragraph seems to suggest that in the decade 2001-2011 CRAs, in assessing countries' creditworthiness, have adopted differentiated methodologies for developed and emerging countries. Also, in the face of the burst of the crisis and of its evolution over the years, the agencies have progressively adjusted the rating methodology used for advanced economies – the ones mostly hit by the financial turmoil – leaving basically unchanged that for emerging countries.

4.4. *Comparison between Predicted Ratings and Assigned Ratings for Developed Countries*

The purpose of this analysis is to ascertain whether an evidence of pro-cyclicality in the assessments of rating agencies emerges in the period investigated.

As mentioned in the introduction, according to the literature, the conclusion that a pro-cyclical behavior has been kept by rating agencies would depend on two conditions: *1)* in the pre-crisis period, assigned ratings (AR) are on average better than predicted ratings (PR) and, on the contrary; *2)* during the crisis, AR are on average worse than PR; in other words, pro-cyclicality occurs when the mean of the distribution of distances[10] between predicted ratings (PR) and actual ratings (AR) is negative in the pre-crisis period and positive during the crisis.

Since the financial turmoil has prevalently hit advanced economies, the verification of the existence of pro-cyclicality in sovereign ratings issued by the CRAs is conducted with reference only to the sample of developed countries; for this purpose, the assigned ratings are compared with those predicted for the sample of 34 developed countries identified by the IMF, with a specific focus on the percentage distribution of PR<AR, PR=AR and PR>AR, separately for the pre-crisis period (2001-2006) and for the crisis period (2007-2011).

Following Mora's approach, I calculate the predicted ratings for the advanced economies taking into account the coefficients (identified in section 4.1) esti-

[10] Distance = Predicted ratings – Assigned ratings.

mated with the linear regressions across the larger set of 96 countries[11] and over the entire sample period 2001-2011 (long-run coefficients).

It could be argued that the calculation of the predicted ratings using the coefficients obtained from the regressions across the entire sample period may not be apt to detect possible adjustments in the methodology adopted by the CRAs for the assignment of ratings; indeed, these coefficients, capturing at the same time both methodologies for the pre-crisis and for the crisis period, tend to "average" the two methods, thus preventing us from appreciating potential differences between them. This could eventually lead to less accurate conclusions when comparing the predicted ratings with the assigned ones in each of the two sub-periods.

In order to tackle this issue, I perform a further exercise: I estimate the ratings also on the basis of the coefficients (identified in paragraph 4.2) obtained by separately regressing across the two sub-periods 2001-2006 and 2007-2011 (short-run coefficients).

4.4.1. *Comparison between PR and AR Using Long-Run Coefficients*

Table 7 shows for each of the two linear models outlined in the previous chapters the distribution of distances, expressed in notches, between assigned ratings and predicted ratings calculated with the long-run coefficients and a summary of the same distribution achieved through the descriptive statistics mean, standard deviation and skewness.

What emerges is that during the pre-crisis period, respectively 68.33% and 60.56% of the predictions obtained using the linear models with fixed effects and with random effects coincide perfectly with the assigned ratings.

It also emerges that the prediction errors are very limited since the positive differences remain within only one notch and that, with regard to the negative differences, only 2.78% and 5% of the ratings predicted respectively with the linear fixed effects and random effects models are lower than the assigned ratings by two notches. Therefore, in general, given the narrow dispersion of errors, it can be said that the models approximate the ratings assigned by CRAs.

[11] It is noteworthy that differences in the coefficient estimates of the linear regressions between the sub-samples of developed and emerging countries have emerged in the analysis conducted in paragraph 4.3. Nevertheless, the higher predicted power of considering the entire sample of 96 countries has convinced me to utilize this larger sample instead of the one referred only to the 34 developed countries. Moreover, the use of the larger set of countries allows also to consider the explanatory variable default history, that on the contrary, it is omitted from the linear regression with fixed effects across developed countries because of multicollinearity problems.

However, a bias emerges analyzing both the mean of the distribution of distances, that assumes values -1.27 and -1.94 respectively in the linear model with fixed effects and in the linear model with random effects, and the *p*-value associated with the *t*-test, showing that the mean for the two models are significantly different from zero at a 1% level.

This bias would confirm what Ferri *et* al. and Mora argued about CRAs: that they tend to assign better ratings than those predicted in the periods in which no signs of crisis have yet emerged.

On the contrary, during the crisis the percentage of PR=AR decreases while there is an increase of the percentages of PR differing from AR by two or more notches, as indicated also by the value of the standard deviation, that in the transition from the pre-crisis to the crisis period increases from 2.9 to 5.85 with the fixed effects model and from 3.14 to 5.92 with the random effects model: this can be interpreted as a sign of a reduction in the predictive power of the models.

The zero-centered average of the distribution of distances – suggested by the relative *t*-test – can be assumed as a sign of the absence of pro-cyclicality, although in some cases a high positive distance (>3 notches) is found between PR and AR. This fact, along with the high positive value of the skewness (3.50 and 3.36 respectively for fixed effects and random effects models), would indicate a behavior of CRAs somehow oriented, during the crisis, to issue for some countries ratings much lower than what would be justified by the economic fundamentals and qualitative factors considered in my regressions.

4.4.2. *Comparison between PR and AR Using Short-Run Coefficients*

In this exercise, whose results are reported in Table 8, I conduct the same analysis developed in the previous paragraph using the short-run coefficients for calculating the predicted ratings.

The comparison with the results of the previous exercise shows a higher predictive ability of the new specification: Table 8, indeed, highlights for the pre-crisis period a significant increase in the percentage of PR=AR (from 68.33% to 80.00% for the linear model with fixed effects and from 60.56% to 75.56% for the linear model with random effects) and a reduction to a single notch of the maximum distance between PR and AR, as also evidenced by the lower value assumed by the standard deviation (from 2.9 to 2.1 and from 3.14 to 2.29, respectively for the linear models with fixed effects and with random effects).

The bias emerged in the previous exercise decreases. Indeed, the mean of the distribution of distances in the two models lowers in absolute value (in particular, from –1.27 to –0.77 for the linear model with fixed effects and from –1.94 to –0.94 for the random effects one) maintaining, in any case, a level of statistical significance at 1%.

This confirms, albeit not as strongly as in the long-run coefficients exercise, the tendency of CRAs to issue, during the periods of financial and economic stability, more favorable ratings than those strictly suggested by countries' fundamentals.

As for the full-blown crisis period, the same comparison with the results obtained in the previous exercise shows a reduction of the percentage of PR>AR with a distance greater than or equal to a notch and a slight increase in the percentage of PR lower than AR by three notches; this trend is confirmed by the lower value of the skewness of the distribution of distances (2.44 in the linear model with fixed effects and 2.15 in the random effects one). In other words, there is a reduction of the number of cases in which assigned ratings are much lower than those predicted.

On the contrary, the mean of the distribution of distances in both models increases in absolute value (from -0.65 to -1.18 in the linear model with fixed effects and from -0.53 to -0.96 in the linear model with random effects) and it also becomes statistically significant.

These last results indicate that, during the crisis period, in assessing developed countries the CRAs have adopted a methodology aimed mainly at the stability of ratings rather than at their maximum accuracy, avoiding to translate into timely downgrades the cyclical fluctuations, as highlighted by the predicted ratings. Actually, this behavior is in line with the "smoothing rules" that CRAs publicly state to intentionally apply in order to promote stability. Cantor and Mann (2007) find, for example, that agencies tend to change ratings only if the anticipated rating change is expected to be persistent, and/or higher than one notch.

In conclusion, also the present exercise does not provide evidence of a pro-cyclical behavior of rating agencies in assessing sovereigns in the current financial and economic crisis.

***

Summing up the conclusions reached in both exercises conducted above, it emerges that, during the pre-crisis period, CRAs expressed assessments that, tend to be better than those justified by the economic fundamentals and qualitative factors considered in my models (such evidence is attenuated in the exercise based on the short-run coefficients). This confirms the point of view of Ferri *et* al. and Mora.

With regard to the crisis period, the tendency, observed by Ferri *et* al. with reference to the Asian crisis, of CRAs issuing ratings worse than those predicted appears not to be confirmed, since the mean of the distribution of distances is centered in zero in the exercise based on the long-run coefficients and it even takes a negative and statistically significant value in the exercise based on the short-run coefficients.

Ultimately, the analyses carried out so far provide strong evidence in favor of the absence of pro-cyclicality in agencies' behavior regarding the set of developed countries in the current financial and economic crisis.

However, it must be noted that, during the financial turmoil, more numerous and higher prediction errors are found. In particular, the high positive value assumed in both the linear models by the skewness of the distribution of distances for the period 2007-2011 suggests that CRAs over-reacted in their assessments for several countries. Therefore, in the next paragraph I examine in more detail those countries for which there has been the greatest distance (equal to or higher than 3 notches in absolute value) between PR and AR. In this context, I will also briefly make reference to the ratings' record of some of the major developed countries. For the sake of simplicity, the analysis will be conducted according to the model based on the short-run coefficients which, by certain indicators (skewness and standard deviation), seems able to approximate the ratings assigned by the agencies more accurately than the long-run coefficients model.

4.4.3. *Country-Specific Evidence*

In both linear models with fixed effects and with random effects, Greece and Portugal are the countries showing, for the period 2007-2011, assigned ratings, which significantly diverge from those estimated by the models.

As shown in Graphs 1 and 2, in which the model's predictions are plotted along with actual ratings, the results for Greece and Portugal are very similar and can be described jointly: during the pre-crisis period the models substantially track the ratings; in 2007 and 2008 the countries are overrated since the assigned ratings are higher than the predicted ones; in 2009 the distances are reduced as a consequence of the CRAs downgrades that involved both countries; it is only in 2010, following further downgrades, that AR and PR substantially coincide; in 2011 the downgrades lead the assigned ratings to being significantly lower than the predicted ones (three notches for Portugal and even more for Greece).

In the model with random effects, besides Greece and Portugal, also Ireland turns out to feature a considerable difference between AR and PR (in 2009 a negative distance of 3 notches), showing for the entire crisis period a trend quite similar to the other two countries' (Graph 2).

At first glance, the results for the three countries in question could lead to the conclusion that the agencies, in the face of an economic scenario which in 2007-2008 appeared already deteriorated, didn't react promptly. On the contrary, from a more careful analysis of the historical events, we could argue that CRAs may have behaved properly, given the perceived high probability, at the time, of a bailout of countries in financial distress. In 2009, when the worldwide nature of the financial and economic crisis became apparent, they resolved to cut the ratings assigned to these countries, after doubts were also raised about the likelihood of bailout interventions.

In 2009-2010, the signs became visible of a deteriorating economic situation for some other major developed countries (Italy, Spain, UK, US), as evidenced by the reduction in the relative ratings predicted with both linear models utilized (Graphs 3 and 4). Moreover, despite the common reduction of one notch in the predicted ratings for all those countries, only Spain was downgraded in the same year 2009. For Italy, the first downgrade occurred only two years later, in 2011; at the end of this same year also the United States were affected by a downgrade. No downgrade, on the contrary, was issued for the United Kingdom.

In 2010 a reduction in the level of ratings is suggested by the model estimations also for France and Germany[12] (for the latter only in the model with fixed effects); nevertheless, no downgrade has been subsequently issued for those countries.

These results could suggest – and actually some authors[13] do argue – that, in many cases, the agencies are slow in changing their ratings in the face of a worsening of the economic situation of countries. It has to be said that this behavior must not necessarily be linked to CRAs being prone to react to events rather than to anticipate them, as many critics say. Indeed, CRAs themselves publicly state their effort to avoid volatile ratings by using smoothing practices: in particular, they prefer to rate "through the cycle" basing their assessments on the issuer's *ex-ante* perceived ability to survive cyclical troughs, which provides a cushion against the impact of economic downturns[14].

Nevertheless, my exercise supports the 2010 IMF's view on CRAs, *i.e.* that they have a preference for stability, that possibly leads (Portugal and Greece rat-

[12] Actually, for what concerns Germany, the predicted rating in the linear model with FE decreases by one notch in 2010 and increases again in 2011.

[13] See Eijffinger S.C.W. (2011).

[14] See Imf (2010).

ings' records could be two examples – Graphs 1 and 2) to the so-called "pro-cyclical cliff effects": in plain words, with this approach oriented to the stability of ratings, if the expected reversal in the negative economic trend of the country doesn't materialize and, on the contrary, its creditworthiness continues to worsen, then CRAs find themselves in a position to proceed to a more abrupt downgrade.

5. - Conclusive Remarks

In the present work I have tried to shed new light on the debate on possible pro-cyclicality of the foreign sovereign credit ratings issued by CRAs, considering their behavior with reference to the current financial crisis.

The main outcome of the exercise I have conducted is that the assumption of a pro-cyclical behavior of the CRAs appears to be groundless when referring to the period 2001-2011. Actually, even though the ratings issued throughout the pre-crisis period (2001-2006) are generally higher than the predicted ones, there is no evidence of CRAs assigning unduly worse ratings during the years of the crisis (2007-2011). Indeed, for this period, the exercise based on the long-run coefficients shows that the predicted ratings mostly match the assigned ones; moreover, the exercise based on the short-run coefficients suggests that assigned ratings on average are even higher than those predicted.

The results of this last exercise seem to be coherent with the "smoothing rules" that CRAs publicly state to apply in order to promote stability and to avoid exacerbating cyclical fluctuations.

Nevertheless, according to IMF (2010), this behavior can lead to the so called "pro-cyclical cliff effects", as my exercise based on the short-run coefficients suggests to have happened for Greece and Portugal in the current crisis.

Another important finding of the present work is that the methodology used by CRAs in assessing countries has not been crystallized over the years and for different typologies of economies: the analysis of the determinants of the sovereign ratings shows that CRAs take into consideration a broad (and, to some extent, variable) array of fundamental factors and weigh them dynamically. Indeed, I have found signs of CRAs changing the weights given to some macroeconomic fundamentals, for example, inflation and government gross debt, in particular when assessing developed countries, coherently with their major involvement in the current financial and economic crisis.

In conclusion, the findings of the present work suggest an excessive severity of the criticisms addressed to CRAs for having, in the current financial and economic crisis, contributed to amplify its effects with their assessment activity of sovereigns' creditworthiness.

More agreeable are the criticisms about the lack of transparency of the criteria used by CRAs to issue their ratings; this substantial opacity, making it difficult to understand and share the methodologies adopted by rating agencies, undoubtedly contributes to fuel allegations addressed by many against them and to question their own legitimacy and *raison d'être*.

Therefore, it is of utmost importance that policymakers, as also stated by IMF (2010), continue their efforts to push CRAs to «be transparent about the quantitative measures they calibrate in the rating process and how they validate their ratings».

TABLE 1

NUMERICAL TRANSFORMATION OF CREDIT RATINGS

Interpretation	Alphanumeric		Numeric linear transformation
	Moody's	S&P	
INVESTMENT-GRADE RATINGS			
Highest Quality	Aaa	AAA	100
High Quality	Aa1	AA+	95
	Aa2	AA	90
	Aa3	AA–	85
Strong Payment Capacity	A1	A+	80
	A2	A	75
	A3	A–	70
Adequate Payment Capacity	Baa1	BBB+	65
	Baa2	BBB	60
	Baa3	BBB–	55
SPECULATIVE-GRADE RATINGS			
Likely to fulfill obligations,	Ba1	BB+	50
ongoing uncertainty	Ba2	BB	45
	Ba3	BB–	40
High-risk obligations	B1	B+	35
	B2	B	30
	B3	B–	25
DEFAULT GRADE	Caa1	CCC+	20
	Caa2	CCC	15
	Caa3	CCC–	10
	Ca	CC	5
	C	SD	0

TABLE 2

COMPARISON BETWEEN S&P'S AND MOODY'S RATINGS

S&P's and Moody's Differences in Notches	Observations
>2	4
2	38
1	143
0	484
–1	216
–2	58
<–2	18
Total	961
% of differences within +/–1 notch	88%
% of differences within +/–2 notches	98%

TABLE 3

ANNUAL RATING REGRESSIONS

	Linear FE	Linear RE	Ordered Probit FE
REGRESSORS			
GDP pc (PPP)	0.0004***	0.0005***	0.0001***
	(0.000)	*(0.000)*	*(0.000)*
GDP growth	0.066	0.078	0.029*
	(0.051)	*(0.052)*	*(0.017)*
Inflation rate	–0.239***	–0.265***	–0.057**
	(0.044)	*(0.044)*	*(0.027)*
Unemployment rate	–0.688***	–0.680***	–0.146***
	(0.106)	*(0.099)*	*(0.048)*
G. gross debt	–0.185***	–0.155***	–0.051***
	(0.018)	*(0.016)*	*(0.012)*
External Debt	–0.673***	–0.562***	–0.208**
	(0.199)	*(0.178)*	*(0.094)*
Current account	–0.030	–0.029	–0.008
	(0.048)	*(0.046)*	*(0.019)*
G. effectiveness	6.663***	12.641***	2.287***
	(1.285)	*(0.955)*	*(0.482)*
Default history	–9.281***	–11.348***	–2.459***
	(1.804)	*(1.604)*	*(0.520)*
Constant	74.021***	65.547***	
	(1.919)	*(1.855)*	
Observations	785	785	785
R^2 overall (pseudo if O.P)	0.743	0.848	0.622
Hausman specification test[$]		Chi^2 = 35.63	
		(0.000)	

* Significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in parentheses.
[$] The null is that RE estimation is consistent and therefore preferable to FE.

Table 4

ANNUAL RATING REGRESSIONS ACROSS SUB-PERIODS

	Linear FE		Linear RE	
	2001-2006	2007-2011	2001-2006	2007-2011
REGRESSORS				
GDP pc (PPP)	0.001***	0.001***	0.001***	0.001***
	(0.000)	*(0.000)*	*(0.000)*	*(0.000)*
GDP growth	0.105	–0.045	0.010	–0.024
	(0.075)	*(0.069)*	*(0.075)*	*(0.071)*
Inflation rate	–0.228***	–0.441***	–0.259***	–0.448***
	(0.042)	*(0.101)*	*(0.042)*	*(0.103)*
Unemployment rate	–0.565***	–0.331	–0.469***	–0.774***
	(0.147)	*(0.204)*	*(0.128)*	*(0.165)*
G. gross debt	–0.118***	–0.382***	–0.092***	–0.220***
	(0.022)	*(0.037)*	*(0.019)*	*(0.026)*
External Debt	–0.770***	0.482	–0.754***	–0.222
	(0.291)	*(0.367)*	*(0.244)*	*(0.282)*
Current account	–0.113**	–0.129	–0.114**	–0.095
	(0.055)	*(0.082)*	*(0.052)*	*(0.075)*
G. effectiveness	4.400***	5.973*	10.401***	13.072***
	(1.349)	*(3.207)*	*(1.030)*	*(2.002)*
Default history	–12.265***	4.162	–13.092***	–6.563**
	(1.618)	*(4.913)*	*(1.505)*	*(3.090)*
Constant	69.204***	60.980***	58.942***	63.972***
	(2.505)	*(6.910)*	*(2.202)*	*(2.931)*
Observations	424	361	424	361
R^2 overall	0.818	0.586	0.892	0.793

* Significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in parentheses.

TABLE 5

RATING REGRESSIONS. COMPARISON BETWEEN DEVELOPED AND EMERGING COUNTRIES FOR DIFFERENT PERIODS (FIXED EFFECTS)

	Linear Specification FE													
	2001-2005		2001-2006		2001-2007		2001-2008		2001-2009		2001-2010		2001-2011	
REGRESSORS														
GDP pc	.0004***	.0018***	.0003***	.0015***	.0002***	.0014***	.0003***	.0012***	.0003***	.0011***	.0004***	.0009***	.0004***	.0007***
GDP growth	–.077	–.143	–.026	–.137	.044	–.105	–.179**	.036	–.085	.095	–.087	.095	–.045	.060
Inflation	–.554***	–.210***	–.616***	–.182***	–.560***	–.191***	–.766***	–.194***	–.807***	–.177***	–.771***	–.178***	–.832***	–.184***
Unemployment	–.441*	–.3068	–.515***	–.175	–.562***	–.062	–.499***	.046	–.510***	–.0092	–.462***	–.147	–.987***	–.290**
G. gross debt	–.070	–.0619	–.0410	–.084**	–.009	–.068**	–.143***	–.066**	–.151***	–.067**	–.201***	–.072***	–.288***	–.089***
External Debt	.0373	–2.133*	–.017	–2.307**	–.0184	–2.45***	–.323**	–1.963**	–.633***	–2.24***	–.809***	–2.18***	–.364*	–2.03***
Current account	–.001	–.2142**	–.019	–.161**	–.023	–.1162*	.108*	–.086	.010	–.137**	–.061	–.149***	.055	–.142**
G. effectiveness	5.359***	2.743	4.648***	3.397	4.944***	3.570	6.474***	5.104**	5.688***	5.116***	6.361***	5.318***	6.103***	5.785***
Constant	77.460***	43.680***	81.039***	45.160***	81.264***	44.170***	85.466***	43.199***	87.761***	44.460***	88.110***	46.970***	95.232***	50.920***
Observations	147	202	180	244	213	287	246	331	279	375	312	419	340	445
R^2	0.512	0.534	0.519	0.576	0.535	0.589	0.364	0.622	0.283	0.638	0.236	0.650	0.251	0.662

Legend: ■ developed □ emerging

TABLE 6

RATING REGRESSIONS. COMPARISON BETWEEN DEVELOPED AND EMERGING COUNTRIES FOR DIFFERENT PERIODS (RANDOM EFFECTS)

	Linear Specification RE													
	2001-2005		2001-2006		2001-2007		2001-2008		2001-2009		2001-2010		2001-2011	
REGRESSORS														
GDP pc	.0005***	.001***	.0004***	.0011***	.0003***	.0011***	.0003***	.0010***	.0004***	.001***	.0004***	.001***	.0004***	.0008***
GDP growth	–.132	–.113	–.111	–.112	–.057	–.099	–.199**	.009	–.039	.087	–.051	.093	.003	.063
Inflation	–.518***	–.218***	–.611***	–.199***	–.581***	–.204***	–.858***	–.211***	–.863***	–.186***	–.835***	–.180***	–.965***	–.182***
Unemployment	–.389**	–.188	–.478***	–.151	–.527***	–.109	–.513***	–.034	–.568***	–.039	–.606***	–.130	–1.213***	–.238**
G. gross debt	–.013	–.085***	–.013	–.100***	–.001	–.086***	–.078***	–.082***	–.096***	–.080***	–.134***	–.084***	–.164***	–.099***
External Debt	.053	–4.153***	.078	–3.48***	.079	–3.00***	–.266*	–2.29***	–.562***	–2.48***	–.743***	–2.38***	–.480**	–2.22***
Current account	–.039	–.203**	–.010	–.159**	–.001	–.1129*	.102*	–.061	.020	–.104*	–.047	–.120**	.006	–.117**
G. effectiveness	7.005***	11.030***	6.107***	10.670***	6.270***	9.963***	7.270***	10.260***	6.881***	9.720***	7.802***	9.410***	7.975***	9.890***
Constant	71.060***	52.300***	75.903***	51.090***	77.460***	48.570	80.536***	46.52***	82.670***	46.13***	83.030***	47.43***	88.790***	50.550***
Observations	147	202	180	244	213	287	246	331	279	375	312	419	340	445
R^2	0.620	0.735	0.585	0.720	0.576	0.701	0.490	0.692	0.410	0.690	0.366	0.69	0.371	0.690

Legend: developed emerging

TABLE 7

SUMMARY OF PREDICTION ERROR (LONG-RUN COEFFICIENTS)

Models	**Prediction Error, Pre-crisis (Notches)**								
	<–3	**–3**	**–2**	**–1**	**0**	**1**	**2**	**3**	**>3**
Linear FE	0.00% 0	0.00% 0	2.78% 5	24.44% 44	68.33% 123	4.44% 8	0.00% 0	0.00% 0	0.00% 0
Linear RE	0.00% 0	0.00% 0	5.00% 9	31.67% 57	60.56% 109	2.78% 5	0.00% 0	0.00% 0	0.00% 0
Models	**Prediction Error, Crisis (Notches)**								
	<–3	**–3**	**–2**	**–1**	**0**	**1**	**2**	**3**	**>3**
Linear FE	0.00% 0	0.63% 1	5.63% 9	20.63% 33	64.38% 103	4.38% 7	1.88% 3	1.25% 2	1.25% 2
Linear RE	0.00% 0	0.63% 1	5.63% 9	20.00% 32	63.75% 102	5.00% 8	2.50% 4	1.25% 2	1.25% 2

Models	**Mean**		**Skewness**		**Standard Deviation**	
	Pre-crisis	**Crisis**	**Pre-crisis**	**Crisis**	**Pre-crisis**	**Crisis**
Linear FE	–1.277*** *(0.000)a*	–0.656 *(0.158)*	–0.769	3.504	2.9	5.857
Linear RE	–1.944*** *(0.000)*	–0.531 *(0.258)*	–0.694	3.369	3.144	5.923

Note: a) The numbers in parentheses below the means are the *p*-values.

TABLE 8

SUMMARY OF PREDICTION ERROR (SHORT-RUN COEFFICIENTS)

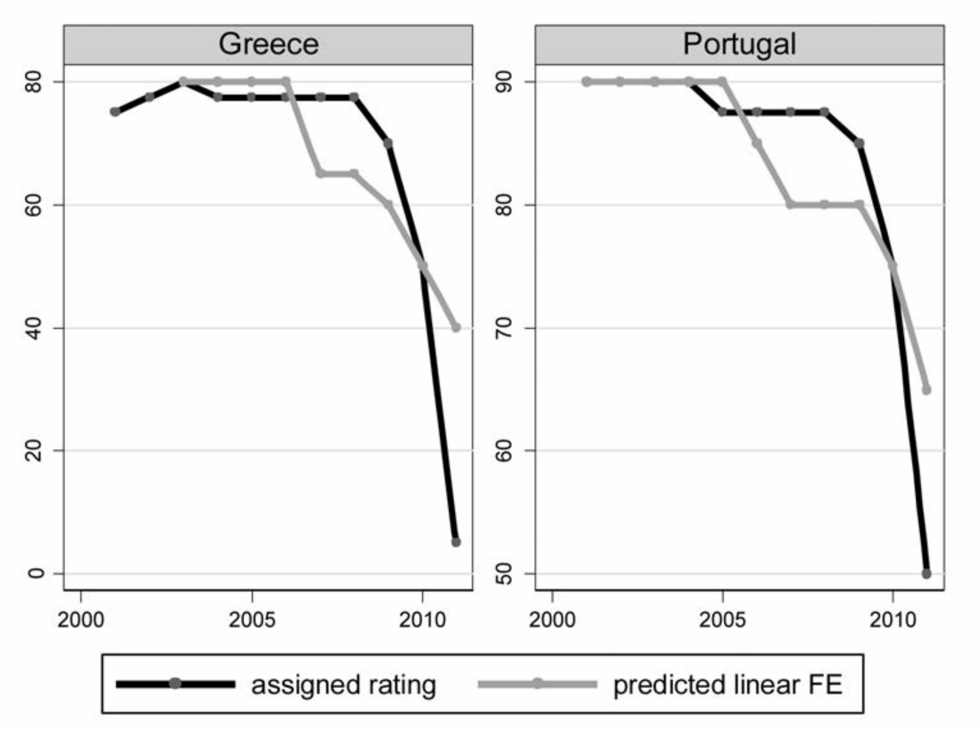
Models	**Prediction Error, Pre-crisis (Notches)**								
	<–3	**–3**	**–2**	**–1**	**0**	**1**	**2**	**3**	**>3**
Linear FE	0.00% 0	0.00% 0	0.00% 0	17.78% 32	80.00% 144	2.22% 4	0.00% 0	0.00% 0	0.00% 0
Linear RE	0.00% 0	0.00% 0	0.00% 0	21.67% 39	75.56% 136	2.78% 5	0.00% 0	0.00% 0	0.00% 0
Models	**Prediction Error, Crisis (Notches)**								
	<–3	**–3**	**–2**	**–1**	**0**	**1**	**2**	**3**	**>3**
Linear FE	0.00% 0	1.25% 2	5.00% 8	21.88% 35	66.25% 106	3.13% 5	1.25% 2	0.63% 1	0.63% 1
Linear RE	0.00% 0	1.88% 3	4.38% 7	20.63% 33	65.00% 104	5.00% 8	1.88% 3	0.00% 0	1.25% 2

Models	**Mean**		**Skewness**		**Standard Deviation**	
	Pre-crisis	**Crisis**	**Pre-crisis**	**Crisis**	**Pre-crisis**	**Crisis**
Linear FE	–0.777*** *(0.000)a*	–1.18*** *(0.002)*	–0.946	2.44	2.1	4.8
Linear RE	–0.944*** *(0.000)*	–0.968** *(0.018)*	–0.669	2.152	2.29	5.12

Note: a) The numbers in parentheses below the means are the *p*-values.

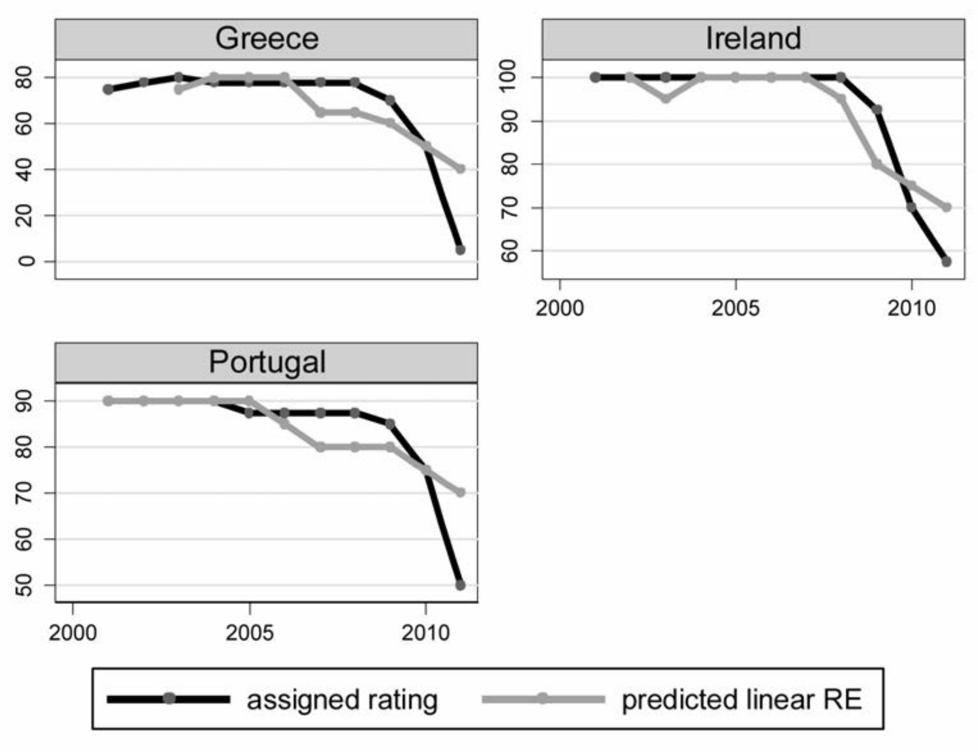
Graph 1

ASSIGNED RATING AND PREDICTED RATING BY ADVANCED COUNTRY WITH THE LARGEST DISTANCES BETWEEN PR AND AR (SHORT-RUN COEFFICIENTS) (LINEAR REGRESSION WITH FE)



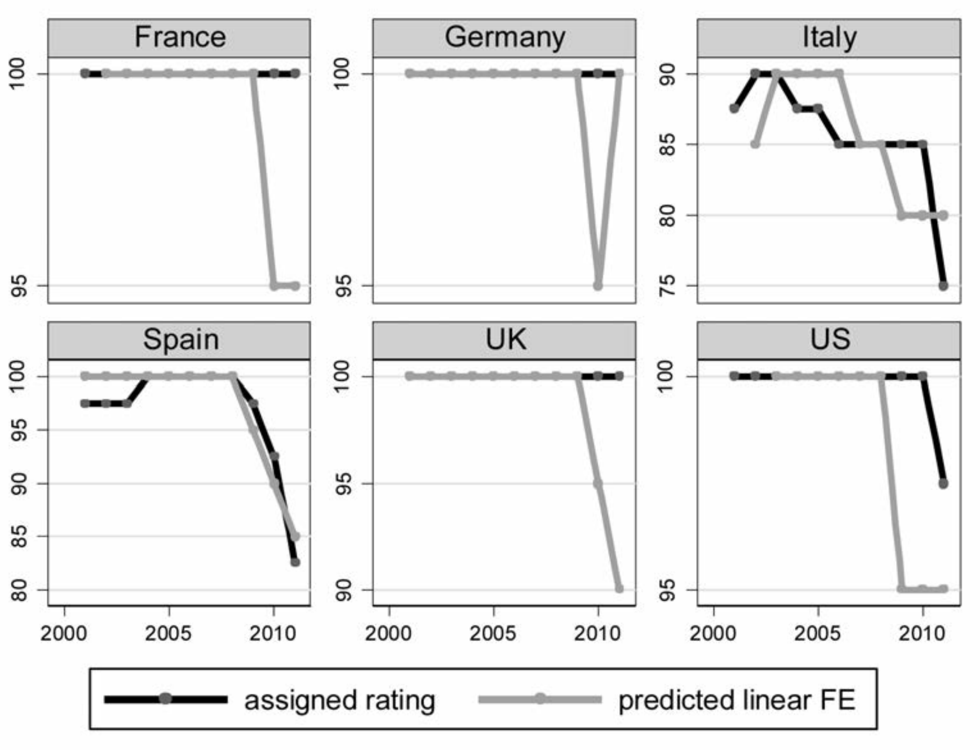
GRAPH 2

ASSIGNED RATING AND PREDICTED RATING BY ADVANCED COUNTRY WITH THE LARGEST DISTANCES BETWEEN PR AND AR (SHORT-RUN COEFFICIENTS) (LINEAR REGRESSION WITH RE)



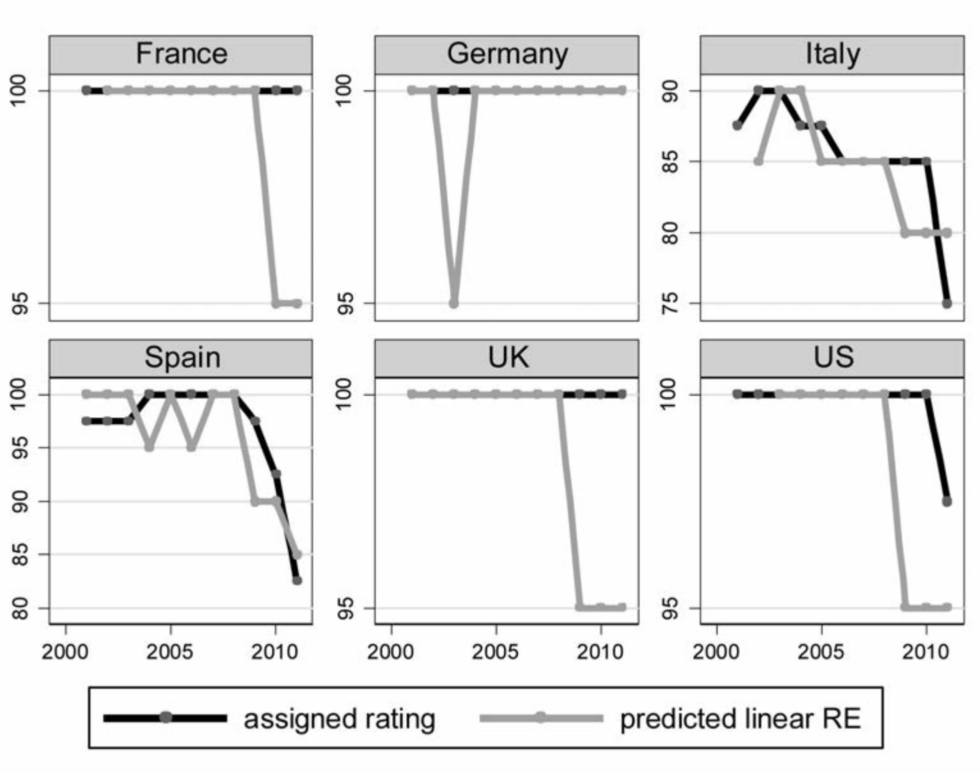
GRAPH 3

ASSIGNED RATING AND PREDICTED RATING BY ADVANCED COUNTRY (SHORT-RUN COEFFICIENTS) (LINEAR REGRESSION WITH FE)



GRAPH 4

ASSIGNED RATING AND PREDICTED RATING BY ADVANCED COUNTRY
(SHORT-RUN COEFFICIENTS) (LINEAR REGRESSION WITH RE)



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The Spread of Mafia in Northern Italy: The Role of Public Infrastructure

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This paper analyzes the spread of mafia in Northern Italy. In particular this study aims at showing that public funding of infrastructure attracts the transplantation of such criminal organizations. I assemble a new dataset on mafia-related crimes, which provide information at provincial level over the period 1985-2010. By applying a differences-in-differences strategy, I show that public investment for renewal of A4 motorway increased mafia-related crimes with economic connotation in those provinces that received roadwork. [JEL Classification: K42; C52; R1].

Keywords: public investment in infrastructure; mafia expansion; differences-in-differences.

* <*lavinia.piemontese@gmail.com*>. I am grateful to my supervisors Professor Carlotta Berti Ceroni and Professor Giovanni Prarolo for their patience and their indispensable guidance. I would also like to thank the four anonymous referees and Professor Gustavo Piga for their helpful comments.

1. - Introduction

This thesis investigates the economic causes of the spread of mafia in Northern Italy registered in the last few decades. The Northern regions are characterized by a thriving economic activity that offers mafia organizations a wide range of profitable opportunities.

A preliminary examination of data about mafia expansion toward new prosperous territories suggests that mafia is attracted by new business chances offered the North of the country. In fact available data show a sharp increase of mafia presence in regions not historically affected by such phenomenon. Moreover, other descriptive statistics indicate that the Northern judicial system was not so used to mafia presence to face its expansion. This piece of information is consistent with the sociological study provided by Varese (2011), which aims at identifying factors that facilitate the movement of Italian mafia towards new territories. The author indeed concludes that an appealing territory for a successful transplantation needs to be characterized by a fault in law enforcement.

The purpose of this thesis is to enhance the understanding of mafia expansion by empirically investigating whether public funding of infrastructure is a possible causing effect of mafia expansion. In particular, I analyze whether the renewal works of A4 motorway approved between the 2000 and the 2002 have pulled mafia presence. In fact, public works have already been argued to attract and increase mafia phenomena. For example, according to a recent inquiry published by the newspaper La Repubblica[1], A3 motorway (known as Salerno-Reggio Calabria) that crosses territories with historical mafia presence, such as Campania and Calabria, is considered as a “mafia-building site”.

Given the difficulties met in searching for a proper method that allows to investigate causal relationships, the starting point in such choice is the examination of the data. I assemble a new dataset that provides information on mafia-related crimes made available from the yearbook of criminal statistics published by the Italian Statistical Institute (ISTAT). The dataset collects information across statistical units and time periods, in fact it includes data on reports at provincial level of mafia-murders, mafia-type nature association and extortions, over the period 1985-2010.

After providing a discussion of descriptive statistics, I argue that, given the feature of the data, the differences-in-differences method (DD) is the best strategy

[1] «L’autostrada delle cosche», *www.inchieste.repubblica.it.*

to find out whether the public funding of infrastructure has attracted the recent expansion of mafia in Northern Italy. In particular, the DD strategy aims at estimating the causal effect that the renewal works of A4 motorway approved over the period 2000-2002 have on mafia expansion. This method compares the number of mafia related crimes reports before and after the roadwork-approval and between the treatment group, formed by the provinces crossed by the renewed A4 and the control group, the remaining Northern Italy provinces.

While the reports of mafia-type association do not lead to statistically significant estimates, reports of mafia-type murders and reports of extortions give statistical significant results. The former estimate is negative, the latter is positive. These findings suggest that public funding of infrastructure, in this case funding for the renewal of A4 motorway, may have increased mafia activity in Northern regions. Moreover, mafia organizations infiltrated in this area seem to prefer activities with economic connotation to crimes that characterize mafia presence in Southern Italy, *i.e.* murders.

The reminder of the paper is structured as follows. The next section provides a brief review of the existing economic literature. Section 3 describe the background and motivates the performed empirical analysis. Section 4 reviews the DD methodology. Section 5 describes the data, the treatment. Moreover it presents the main estimation results and some robustness checks. Section 6 concludes.

2. - Related Literature

Although a complete review of the economic literature on mafia is beyond the scope of the present paper, I hereby provide a brief review of the relevant work on the emergence, the consequences and the diffusion of mafia.

Italian mafia is a criminal organization involved in a wide range of criminal activities, which impact on both private and public sectors. Mafia businesses indeed include supply of illicit goods and services such as drugs smuggling and prostitution trafficking; activities against individuals operating in the economy like loan sharking; actions against public officials and local politicians aimed at stating the control over the territories where this organization operates. Further, according to Shelling (1971), the core business of criminal organizations is the use of the violence, which guarantees them the monopoly power in those legal and illegal markets they control. Indeed, in order to stabilize its activities and grow, Italian mafia performs a peculiar intimidator behavior. All these distinctive features have been detailed in 1983 by Article 416-*bis* of the Penal Code that defines mafia nature criminal organization[2].

Starting from its origins, Sicilian mafia, the oldest branch of this criminal organization, rose in the nineteenth century after the demise of the Sicilian Feudalism as an industry of private protection (Gambetta, 1996). This phenomenon can be seen as a response to increased demand of protection from predatory attacks, lower degree of property-right enforcement and decreased interpersonal trust after the end of the Feudalism. These facts led indeed to the emergence of independent groups, *i.e.* private protection enforcers, which filled the occurred gap between the establishment of private property rights and the lack of adequate law enforcement.

In a recent contribution, Buonanno *et* al. (2012) explain that the economic origins of mafia activity are linked to specific geographical features. This industry of private protection was indeed more active in those areas that produced and exported particular goods, hence characterized by a higher demand for protection of goods and economic transactions. In particular, the authors collect a unique dataset, which combine information about historical presence of mafia in Sicilian municipalities over the second half of the nineteenth century with information about geographical and climatic conditions of the investigated territories. The empirical analysis shows that Sicilian mafia developed in the Western part of the region, area prone to sulphur extraction and citrus fruits cultivation. Given that sulphur and citrus fruits were the most valuable export goods, the Western Sicily was extremely vulnerable to predatory attacks and hence characterized by a high degree of demand for private protection.

Mafia activity entails deep economic consequences. Bandiera (2003) analyses this issue by showing that historical mafia groups, providing landlords with private protection, worsened the economic condition of the landowning class as a whole. In particular, the author, using a common-agency model, argues that the services supplied by mafia groups generate a negative externality. In fact each landlord, buying private protection from predatory attacks, deflects thieves on other properties. Hence, for a given extension of land, an increase in the number of landowners increases the competition for protection and then the profits of the protection providers, *i.e.* mafia groups. Moreover, Bandiera's analysis shows that, other things equal, mafia was more likely to thrive in areas were the land was more fragmented. Finally, the author concludes that mafia groups gained le-

[2] I report the entire text of the article in the following section.

gitimacy and reputation from the services they provide and then they exploited the acquired power engaging in other illegal activities. For that reason, this contribution discusses both definitions of criminal organization proposed by Fiorentini (2000): mafia groups emerge as governance structures, *i.e.* providers of public goods such as private protection, and then expand as ordinary illegal firms, supplying private goods in illegal markets.

The present analysis aims at inspecting one possible reason of the recent mafia migration towards Northern Italy; it does not provide a description of the mechanisms that allow mafia groups to thrive in new territories. This paper does not inspect the reasons why a firm or an individual could find mafia services profitable, and hence demands these services as the Sicilian landlords did during the nineteenth century. Instead, in this study the attention is focused on the event that probably trigged the movement of mafia organizations towards new territories, not historically affected by this phenomenon.

Concerning the economic literature that analyzes the impact that mafia has nowadays, Barone and Narciso (2013) measure the impact of mafia on economic outcomes throughout an empirical investigation of the relationship between mafia activity and allocation of public transactions. The authors build an econometric specification using a unique dataset which collect information over 390 municipalities located in Sicily about mafia related crimes, public funding and socio-economic conditions such as unemployment over the period 2004-2009. Their analysis confirms that «... mafia diverted 35% of the total amount of public transfers».

Mafia activity can affect the socio-economic everyday life generating measurable losses. In this perspective Pinotti (2011) performs an analysis aimed at estimating the actual loss of economic activity mafia causes. The author investigates mafia activity over the time period 1951-2007. In particular, he performs an empirical study focused on two Italian southern regions, Apulia and Basilicata, which experienced the presence of this phenomenon after the 1970s. Pinotti's analysis is relevant for two reasons: first, he finds out that the aggregate loss implied by presence of mafia amounts to 16% of GDP *per capita*, over the analyzed time period[3]; secondly the author applies an interesting procedure, the synthetic control method which allows to distinguish the impact of mafia activity on the economic growth from impact of any other source.

Crucial for the present analysis is the sociological study provided by Varese (2011), which aims at identifying factors that facilitate the movement of Italian

[3] From its transfer to these new regions in the middle 70's to 2007, the last year analyzed.

mafia from its native environment toward new territories denoted by an high degree of economic and financial development such as Northern Italy. The author presents two different attempts of transplantation[4] of 'Ndrangheta, the mafia branch developed in Calabria: a successful one in Bardonecchia, Piedmont and a failed one in Verona, Veneto. This research suggests that mafia succeed in transplantations in territories characterized by high demand of criminal protection, associated to huge presence of unemployed immigrant workers, willing to accept illegal employment, waiving the right to each form of legal protection, e.g. trade unions. Hence, according to this analysis, an appealing territory for a successful mafia migration must present several factors together: a thriving environment and a fault in law enforcement.

Although these arguments are appealing, the empirical investigation of the economic causes of the expansion of mafia in Northern regions remains relatively unexplored in the literature. This thesis provides a step in this direction by empirically investigating the determinants of mafia expansion in Northern Italy. In particular, this analysis aims at checking whether public funding of major works generates scope for mafia activity. The following section motivates the ground for this empirical investigation the choice of the empirical strategy and the variables used for the analysis.

3. - Motivation

On 11th March, 2011 Mario Draghi, the former Governor of Bank of Italy held a conference at University of Milan about mafia expansion in Northern Italy, focusing in particular on the rise and the spread of this phenomenon in economically developed regions such as Lombardy, Piedmont and Veneto.

In this speech, Draghi posits that there exists a positive relationship between the increased rate of mafia activity observed in the last decade and the prosperous economic and financial situation of such territories. It is worth noticing that mafia activities in the North of the country seem to differ to a significant extent with respect to Southern areas. In particular, an increase in crimes related to business and economic activity, as money laundering and extortion is observed, rather than mafia-type murders or terroristic attacks. These facts point to a possible ex-

[4] Event defined as: «... the ability of a mafia group to offer criminal protection over a sustained period of time outside its region of origin and routine operation» (Varese F., 2011, page 414).

planation of the expansion of mafia: these organizations have migrated to different regions because of new (profit) opportunities.

The following examination of the empirical evidence about the recent spread of mafia is consistent with Draghi's speech. Moreover, this analysis can finalize the switch to the practical part of this study.

Table 1 reports data at regional level acquired by ABCN (National Agency for Administration of Seized Goods to Criminal Organizations) about seized real estates and firms in two time periods, years 2000 and 2010.

Real estates include goods such as apartments, building plots or agricultural plots, which belong to people involved actively in mafia groups, e.g. "bosses". They represent in a concrete way the power that the mafia-people can exert on the surrounding territory. Seized firms are the main sources of money laundering activity; these enterprises operate mainly in the building sector and in services industry. Thereby in the present context we can safely assume that the seizure of a real estate or a firm is not just a signal of "passive investment" from the South to the North of the country; instead it can be viewed as an actual indication of mafia presence in the territory of interest.

TABLE 1

SEIZED REAL ESTATES AND SEIZED REAL FIRMS

	Panel 1: Seized Real Estates		
Region	Real estates 2000	Real estates 2010	Growth rate
Piedmont	39	82	110.26%
Aosta Valley	0	0	
Lombardy	63	600	852.38%
Trentino A.A.	0	16	
Veneto	7	71	914%
Friuli V.G.	0	9	
Liguria	4	22	450%
Emilia Romagna	24	55	129%
Tuscany	3	32	967%
Umbria	0	0	
Marche	0	7	
Lazio	100	255	155%
Abruzzo	4	40	900%
Molise	0	2	
Campania	497	896	80%
Apulia	129	595	361%
Basilicata	2	7	250%
Calabria	389	969	149%
Sicily	604	2,063	242%
Sardinia	35	82	134%
North	137	855	524%
Centre	103	294	185%
South (Mezzogiorno)	1,660	4,574	175%
Italy	1,900	5,803	205%

Source: ABCN (National Agency for Administration of Seized Goods to Criminal Organizations) Regional level data.

Panel 2: Seized Firms			
Region	Firms 2000	Firms 2010	Growth rate
Piedmont	4	12	200%
Aosta Valley	0	0	
Lombardy	61	203	233%
Trentino A.A.	0	0	
Veneto	2	4	100%
Friuli V.G.	0	1	
Liguria	3	8	167%
Emilia Romagna	6	25	317%
Tuscany	1	11	1,000%
Umbria	1	1	0%
Marche	3	3	0%
Lazio	42	117	179%
Abruzzo	0	1	
Molise	0	0	
Campania	152	318	109%
Apulia	23	119	417%
Basilicata	0	3	
Calabria	20	40	100%
Sicily	164	573	249%
Sardinia	0	3	
North	76	253	232%
Centre	47	132	180%
South (Mezzogiorno)	359	1,057	194%
Italy	482	1,442	199%

Source: ABCN (National Agency for Administration of Seized Goods to Criminal Organizations) Regional level data.

On one hand this table shows a hard struggle against mafia: over the period 2000-2010 the numbers of seized real estates and seized firms have tripled. On the other hand, it also suggests a sharp increase of mafia presence in Northern Italy. In fact, it can be immediately noticed that the growth rates of both seized real estates and firms are higher in Northern Italy rather than in the Centre and in the South of the country.

According to descriptive statistics relative to 2010, notice that Lombardy is the fourth region, after Sicily, Calabria and Campania, in terms of number of seized real estates (600 over a total of 5,803) and seized firm (203 over a total of 1,442). Moreover, Piedmont and Veneto show sharply rising numbers of seized real estates: in Piedmont from 39 to 82 in 2010, in Veneto from 7 to 71 in 2010. Table 2 reports information provided by the yearbook of criminal statistics published each year by the Italian Statistical Institute (ISTAT) available online at *giustiziaincifre.istat.it.* This table shows additional descriptive statistics (means, standard errors, min values, max values) of reports of three mafia-related crimes: mafia-type murders, mafia-type association (Article 416-*bis*) and extortions; the statistics are computed on data at provincial level in three time periods: 1990, 2000, 2010.

TABLE 2

DESCRIPTIVE STATISTICS FOR MAFIA-RELATED CRIMES

Panel 1: Mafia Activity in all Regions						
Variable	Year	Obs.	Mean	S.e.	Min	Max
Murders	1990	95	5.86	2.08	0	155
	2000	103	1.43	0.62	0	58
	2010	104	0.66	0.23	0	16
Art. 416-*bis*	1990	95	1.98	0.59	0	50
	2000	103	2.26	0.6	0	37
	2010	104	1.44	0.44	0	33
Extortions	1990	95	27.56	4.01	0	192
	2000	103	33.42	4.15	2	229
	2010	104	58.06	8.08	3	619

Source: Yearly Book of Criminal Statistics, published by ISTAT (Italian Statistical Institute) Provincial level data.

Panel 2: Mafia Activity in Northern Regions						
Variable	Year	Obs.	Mean	S.e.	Min	Max
Murders	1990	41	0.92	0.48	0	19
	2000	46	0.04	0.03	0	1
	2010	46	0.04	0.03	0	1
Art. 416-*bis*	1990	41	0.59	0.24	0	7
	2000	46	0.36	0.1	0	3
	2010	46	0.08	0.6	0	3
Extortions	1990	41	16.17	2.93	1	97
	2000	46	22.96	4.64	2	169
	2010	46	44.11	9.13	3	386

Source: Yearly Book of Criminal Statistics, published by ISTAT (Italian Statistical Institute) Provincial level data.

This table reports descriptive statistics for two distinct samples: one includes provinces located in all Italian regions (Panel 1); the other provide information about Northern regions, *i.e.* Piedmont, Aosta Valley, Lombardy, Trentino Alto Adige, Veneto, Friuli Venezia Giulia, Liguria, Emilia Romagna (Panel 2). All the values are meant as the yearly average of reports in each province. Note first the increasing means of the extortions variable: in Panel 1 the mean value doubled and in Panel 2 it tripled. Second, Panel 2 displays descriptive statistics of extortions close to the ones computed in Panel 1, *i.e.* including territories with historical mafia presence. Third, both Panel 1 and Panel 2 report decreasing mean values of mafia-type murders. In the restricted sample, *i.e.* Northern regions, these values are smaller: this fact suggests that mafia-type murders are representative of mafia's native environment. Finally, Panel 2 shows very few cases of mafia-type association, ten times smaller than the values reported in Panel 1 (which include mafia historical settlements). These values are also decreasing. Article 416-*bis* defines a well-structured crime, describing how mafia organizations act in a very precise way. Thus these few reports of mafia-type association may be linked to investigation ability of the judicial system (in territories not historically affected by the phenomenon) in finding such cases. Table 3 provides statistics computed at regional level, every 100,000 inhabitants about mafia-type homicides (Panel 1), cases *ex* Article 416-*bis* (Panel 2), reports of extortions (Panel 3). It can be easily noticed that this table shows the same patterns as Table 2.

TABLE 3

DESCRIPTIVE STATISTICS FOR MAFIA-RELATED CRIMES - EVERY 100.000 INHABITANTS VALUES

Panel 1: Reports of Mafia-Type Murders Every 100,000 Inhabitants			
Region	Murders 1990	Murders 2000	Murders 2010
Piedmont	0.19	0.02	0
Aosta Valley	0	0	0
Lombardy	0.24	0.01	0.01
Trentino A.A.	0	0	0
Veneto	0.07	0	0
Friuli V.G.	0	0	0
Liguria	0.18	0	0
Emilia Romagna	0.08	0	0.02
Tuscany	0.14	0	0
Umbria	0	0	0
Marche	0	0	0
Lazio	0.16	0.08	0
Abruzzo	0	0	0
Molise	0.61	0	0
Campania	3.58	1.28	0.31
Apulia	0.25	0.52	0.37
Basilicata	0.16	0	0
Calabria	6.78	1.68	1.19
Sicily	3.02	0.26	0.2
Sardinia	0.06	0	0

Source: Yearly Book of Criminal Statistics, published by ISTAT (Italian Statistical Institute) Provincial level data.

Panel 2: Reports of Mafia-Type Association Every 100,000 Inhabitants			
Region	Art. 416-*bis* 1990	Art. 416-*bis* 2000	Art. 416-*bis* 2010
Piedmont	0	0.05	0
Aosta Valley	0	0.85	0
Lombardy	0.12	0.03	0.03
Trentino A.A.	0	0	0
Veneto	0.07	0.11	0
Friuli V.G.	0	0.04	0
Liguria	0.41	0.09	0
Emilia Romagna	0.08	0.15	0.02
Tuscany	0.2	0.09	0.03
Umbria	0	0.24	0
Marche	0.07	0	0.06
Lazio	0.37	0.04	0.02
Abruzzo	0.16	0.08	0
Molise	0	0	14.99
Campania	1.18	1.09	0.14
Apulia	0.25	0.55	0.02
Basilicata	1.47	0	6.96
Calabria	0.77	2.18	1.14
Sicily	0.68	1.61	0
Sardinia	0	0	0

Panel 3: Reports of Extortions Every 100.000 Inhabitants			
Region	Extortions 1990	Extortions 2000	Extortions 2010
Piedmont	4	6.8	9.2
Aosta Valley	4.37	2.54	2.35
Lombardy	2.55	3.71	8.11
Trentino A.A.	1.58	2.02	4.86
Veneto	1.97	11.91	5.56
Friuli V.G.	3.33	0.94	5.11
Liguria	2.54	5.11	9.53
Emilia Romagna	1.97	4.89	6.6
Tuscany	1.9	4.66	8.5
Umbria	1.61	3.3	7.22
Marche	2.46	4.82	7.89
Lazio	4.91	5.2	9.1
Abruzzo	3.06	4.78	12.17
Molise	1.51	7.78	8.43
Campania	6.08	8.99	17.53
Apulia	10.89	9.32	13.83
Basilicata	18	8.52	8.66
Calabria	6.54	11.05	15.48
Sicily	9.42	10.41	12.89
Sardinia	3.3	6.19	8.55

Source: (panel 2-3): *Yearly Book of Criminal Statistics*, published by ISTAT (Italian Statistical Institute) Provincial level data.

Table 4 displays data at regional level on reports of money laundering activities registered during the 2010. Here, the statistics do not show much variation among regions. However, recall that money laundering is not exclusively related to mafia organizations; hence these statistics could not exactly display the analyzed mafia expansion.

According to this preliminary examination, in the last decade an increase of mafia presence in Northern Italy has occurred. This is clear from the observation of data about seized real estates and seized firms and extortions. Nevertheless, very few cases *ex* Article 416-*bis*, *i.e.* of mafia-type association, are observed. This evidence suggests that the judicial system in the North of the country was not familiar enough with the phenomenon to conduct appropriate controls in order to face it.

Table 4

REPORTS OF MONEY LAUNDERING ACTIVITIES

Region	Every 100,000 inhab.	Absolute values
Piedmont	1.73	77
Aosta Valley	0.78	1
Lombardy	1.37	135
Trentino A.A.	0.97	10
Veneto	1.08	53
Friuli V.G.	3.72	46
Liguria	9.22	149
Emilia Romagna	1.82	80
Tuscany	1.5	56
Umbria	1.99	18
Marche	0.89	14
Lazio	2.87	164
Abruzzo	1.49	20
Molise	0.62	2
Campania	3.21	187
Apulia	2.76	113
Basilicata	1.19	7
Calabria	2.69	54
Sicily	2.48	125
Sardinia	1.97	33
Italy	2.22	1,344
North	1.99	551
Centre	2.11	252
South (Mezzogiorno)	2.59	541

Source: Yearly Book of Criminal Statistics, published by Istat (Italian Statistical Institute) Provincial level data.

In this context, the discussed mafia expansion is consistent with the sociological study provided by Varese (2011) that suggests that a thriving economic environment must be linked to a lack in the law enforcement in order to be considered as a key factor for a successful mafia migration. In order to implement Varese's arguments, I take fervent activity in public sector in Northern regions as an indicator of a thriving environment. In particular, I consider the renewal of A4 motorway (Turin-Trieste), approved between 2000 and 2004. Moreover, I collect a unique dataset which provides information about mafia activity in Northern Italy, *i.e.* provincial level data about three mafia-related crimes over the period 1985-2010. Thus, the dataset reports information across statistical units, *i.e.* Northern provinces, and across time periods. The nature of the data and the information about the approved public works for the renewal of A4 motorway suggest that the implementation of the DD method is an appropriate tool to check whether public funding of infrastructure has attracted the recent movement of mafia and to estimate this possible causal effect. Indeed, the DD strategy is a proper method to measure the effect that changes in policy have on the economic environment.

4. - Empirical Strategy

The following section introduces the differences-in-differences (DD) approach implemented for the empirical analysis.

The DD method is applied in the panel data framework when two groups of observations can be distinguished: one is exposed to a treatment and the other one is not. This strategy aims at estimating the causal effect of the treatment on the treated units, *i.e.* the effect of the causing variable on the exposed group.

A compelling example of such analysis is provided by Duflo (2001), the author estimates the effect of building schools on education and future earnings in Indonesia, exploiting the school construction program realized between 1973 and 1978. The dataset exploited by Duflo provides data before and after the examined policy. Moreover, having information about the date place of birth and the place of residence of each individual, the sample can be divided between individuals exposed to the policy and those who were not.

In the present context, the focus is on the effect of public investments in infrastructure on the expansion of mafia in new territories. The dataset collects information at provincial level about mafia activity over the period 1985-2010, in eight Italian regions, *i.e.* Piedmont, Aosta Valley, Trentino Alto Adige, Liguria, Lombardy, Veneto, Friuli Venezia Giulia and Emila Romagna, none of which is historically interested by the phenomenon. Important road-works were executed between the years 2000 and 2002 on highway A4 (Turin-Trieste), a primary road crossing several but not all provinces in Piedmont, Lombardy, Veneto and Friuli Venezia Giulia (see Graph 2). The described framework and the features of the dataset suggest that a DD strategy is well suited for the present context. Indeed two groups of provinces can be identified. The first group, *i.e.* treatment group, includes provinces crossed by the renewed A4 highway. The second group, *i.e.* control group, the other Northern provinces not crossed by renewed A4. Moreover these provinces are comparable to each other and observations about mafia activity are available for all provinces over a wide time interval.

The main difference between these two groups is the exposure to the road works, *i.e.* the treatment. It may thus be convincingly suggested that, under the key assumption that the same trend of mafia activity would prevail in all provinces if renewal works had been executed everywhere (*i.e.* in both groups) or no province had been exposed to roadwork, the DD estimator measures the causal effect of the intervention (public works for maintenance of infrastructure) on the observed mafia expansion.

More formally, the expected value of mafia activities in province p at time t, in the absence and in the presence of roadwork respectively, is:

- $E(Y_0|p,t)$
- $E(Y_1|p,t)$

The identifying assumption in order to apply DD is:

- $E(Y_0|p,t) = \beta_t + \gamma_p$
- $E(Y_1|p,t) = \beta_t + \gamma_p + \delta$

Where β_t is a time fixed effect equal for each province, γ_p is a province fixed effect equals for all time periods and δ can be identified as the effect on mafia spread given by the execution of public works.

In other words, the DD strategy allows counterfactual levels for treated and non-treated to be different, but relies on the fundamental assumption that the variation over time is similar. This means that we assume that in the absence of the treatment, treated outcomes would have moved just as untreated outcomes.

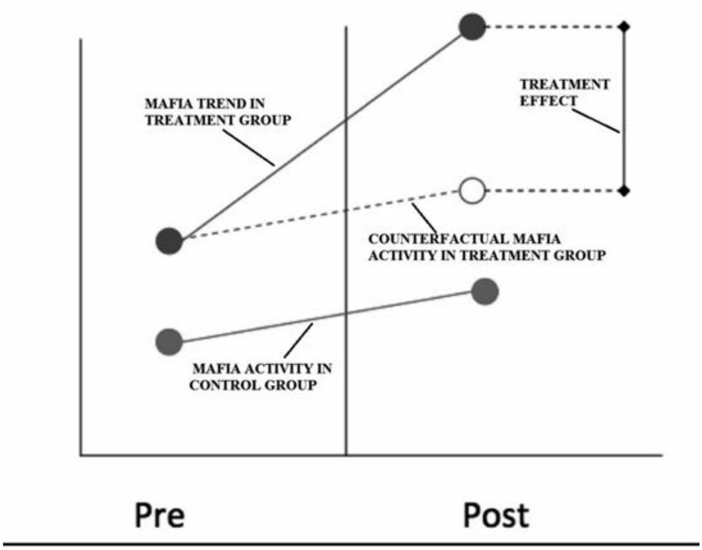
Typically DD is performed assuming also that the effect is additive, namely that in the absence of the treatment the value of the change in the treatment group would have been equal to the change in the control group (*i.e.* there is a parallel trend).

Then, the causal effect can be computed comparing the sample analogues of the expected values of mafia activity before and after the roadwork and between the treatment and the control groups.

Graph 1 shows graphically how this analysis works.

GRAPH 1

DIFFERENCES-IN-DIFFERENCES STRATEGY



Source: econometricsense.blogspot.com.es

For sake of completeness, we should recall that the DD strategy does not allow to control for the reaction of the society to the increased criminal activity. In the economic literature, the determination of crime is viewed as a non-cooperative game where players are potential criminals and "affected" society. In this game, potential criminals choose the level of criminal activity according to the degree to which opportunities for crimes are present in the territory of interest and the severity of sanctions, *i.e.* the risk of arrest and conviction generated by the criminal justice system. On the other hand, society chooses the severity of sanctions, re-acting in part to the prevalence of criminal behavior, *i.e.* reporting the presence of the criminal activities to the qualified authorities. The result is an equilibrium crime rate and an equilibrium level of severity of sanctions.

The DD strategy does not express this game-theoretic equilibrium, because it does not provide a mechanism for society to react to increased criminal activity. Moreover it does not allow to analyze whether a certain degree of societal reaction can affect the level of criminal activity; *i.e.* analyze whether the criminal behavior can be reverted by society[5].

The present analysis aims at inspecting a possible cause of mafia expansion in Northern Italian regions. Thereby it can be viewed as a first stage of an extensive investigation of mafia spread toward new territories. Other steps would include other actors, such as the affected society and the consequence of such expansion.

5. - Empirical Analysis

5.1 *Data*

The source of data is a dataset that collects information made available from the yearbooks of criminal statistics published yearly by the Italian Statistical Institute (ISTAT)[6].

The dataset reports provincial level data about mafia activity, between years 1985 and 2010. The sample considered in the DD analysis includes Piedmont, Lombardy, Aosta Valley, Trentino Alto Adige, Liguria, Veneto, Friuli Venezia Giulia, Emilia Romagna, *i.e.* Northern regions, not historically interested by mafia activity and crossed by highway A4[7].

In the present context, mafia activity is measured as the number (absolute value) of reports of three mafia-related crimes: mafia-type murders, cases of mafia-type association, extortions. Judicial-based measurements of this kind are often subjected to some degree of underreporting. In particular, mafia related issues as *omertà*, the use of violence and a peculiar system of intimidations, which hamper

[5] I would like to thank the referee for this valuable comment. In pursuing this research, my goal is to provide a model that allows to take into account this “non-cooperative game aspect”.

[6] *giustiziaincifre.istat.it*

[7] For a group of provinces established in 1996, observations have been merged to the ones of the provinces of origin. In particular, observations for Lecco have been merged to the observations for Como; observation for Lodi to observations for Milan; observations for Rimini to Forlì-Cesena; observations for Verbano-Cusio-Ossola to Novara; and observation for Biella to Vercelli.

a proper judicial investigation, largely affect crime reporting. Hence, the following analysis may be prone to an attenuation bias, and likely to lead to downward-biased estimated effects compared to the actual one.

The first variable, mafia-type murders, has been used as a mafia activity indicator in several economic studies. However, as Pinotti (2011) emphasizes, in Northern regions very few cases are observed. The analysis of the descriptive statistics (reports of mafia-related crimes) provided in the previous section indeed confirms that murders may be little used by mafia in recently penetrated environments.

The second variable is the incidence of cases ex Article 416-*bis* of Italian Penal Code, which provides the following definition of a mafia-type association: «when those belonging to the association exploit the potential for intimidation which their membership gives them, and the compliance and *omertà* which membership entails and which lead to the committing of crimes, the direct or indirect assumption of management or control of financial activities, concessions, permissions, enterprises and public services for the purpose of deriving profit of wrongful advantages for themselves or others». As already remarked, very few observations are available for such cases. According to Table 2-Panel 2, for instance, in 2010 this crime has been reported at most only three times at provincial level. Low figures may be related to the judicial system characterizing the area of interest, which perhaps is not used to mafia phenomenon.

Third, reports of extortions are collected. The extortion is a kind of tax payment imposed by criminal organization for corresponding services. The payment can be either a sum of money or made in-kind. Through extortions, mafia organizations pursue a two-fold aim. On one hand, they guarantee themselves a significant flow of income. On the other hand, they strengthen the control of territories they try to establish in. The number of extortions indeed seems the most promising measure in the present analysis, which assumes that mafia organizations are attracted by new business opportunities in Northern Italy and that expansion in these territories is mainly profit-oriented.

Finally, the dataset includes reports of thefts, a crime specifically not related with mafia activities. Using such dependent variable may help in proving that public funding in infrastructure leads to mafia-related crimes expansion rather than an overall increase in crime offenses[8]. Here, the expected sign must be thus zero or negative.

[8] In fact, if mafia establishes as a private protection provider, territories affected by a high degree of mafia presence could display fewer cases of crimes, such as thefts or robberies.

5.2 *Exogeneity of Treatment and other Identifying Assumptions*

As explained in the previous section, the DD strategy estimates the causal effect in panel data analysis when only a certain group of observation is exposed to the causing variable. In this analysis renewal works of A4 highway are exploited in order to distinguish among two groups of observations: the Italian provinces crossed by the renewed A4 compose the treatment group; the control group includes all the other provinces located in Northern Italy.

First of all, in order to allow the DD strategy to measure the causal effect of the A4 renovation on the mafia expansion in the treated provinces, we need the treatment to be exogenous. In the present context, the treatment, *i.e.* public funding of infrastructure, has not to be influenced by mafia presence. In this perspective the exogenous treatment employed in such investigation is the renewal works executed on the highway A4 (Turin-Trieste) between years 2000 and 2002. As Graph 2 shows, the A4 highway starts from Turin, Piedmont and crosses four regions and fourteen provinces terminating in Trieste, Friuli Venezia Giulia.

Graph 2

A4 MOTORWAY TURIN-TRIESTE

Source: Author's computation using AutoCAD. Grey line: A4 motorway; Striped area: treatment group; White areas: control groups; Bold lines: regional boundaries.

The highway is composed by five stretches: first from Turin to Milan; second from Milan to Brescia; third from Brescia to Venice; fourth Mestre-link; fifth from Venice to Trieste. During the 2000 renewal works have been approved for the first and fifth stretches of the road. Other renewal works for the second stretch have been approved later, in 2002. The renewal works have been approved for a reason that is independent of any kind of mafia oppression, indeed the roadwork has been approved for the most travelled stretches, which have a high likelihood of road accidents occurrence.

Furthermore A4 motorway exists since the 1927; hence the municipalities selected for the renewal investment did not decide whether execute the works, possibly influenced by mafia coercion. Hence this analysis is not affected by reverse causality, the hypothesis that mafia diverted the renewal works can be excluded and the exogeneity of the treatment is warranted.

This public investment in major works has been chosen as the causing variable of the mafia spread because mafia organizations are likely to penetrate in the economic activity exploiting the Italian subcontract system. For each infrastructure of national importance a representative public agency tenders for public contract. The firm that gets the contract has the right to split it in several subcontracts and, in turn, tender for them.

This branched system is likely to attract mafia organizations that can expand in new territories attracted by new business opportunities exploiting their peculiar intimidator actions in order to obtain the contract or some subcontracts, *i.e.* influencing the choice of firms involved. In other words, mafia organizations, whose legal activities are mainly linked to the building sector, can exploit public investment in infrastructures[9] in order to move towards new settlements. Once stabilized (e.g. during or after the execution of the work on order), these groups can thrive in the new attacked territories both in legal and illegal markets, pursuing profit aims.

Finally we have to highlight that, even if we are focusing on a unique project, the validity of the implemented strategy is not compromised. Indeed, during the analyzed time period, in the Northern provinces that forms the control group have not been approved other major works. The choice of the treatment, and thus of the two groups, has been made following a precise and careful consultation of sources linked to the Ministry of Infrastructures and Transportation and Ministry of Internal Affairs and their activities during the analyzed time period.

As mentioned in the previous section the implementation of the DD model relies on the validity of the "parallel trend" assumption. In the present analysis is assumed that the evolution over time of mafia activity would have followed the

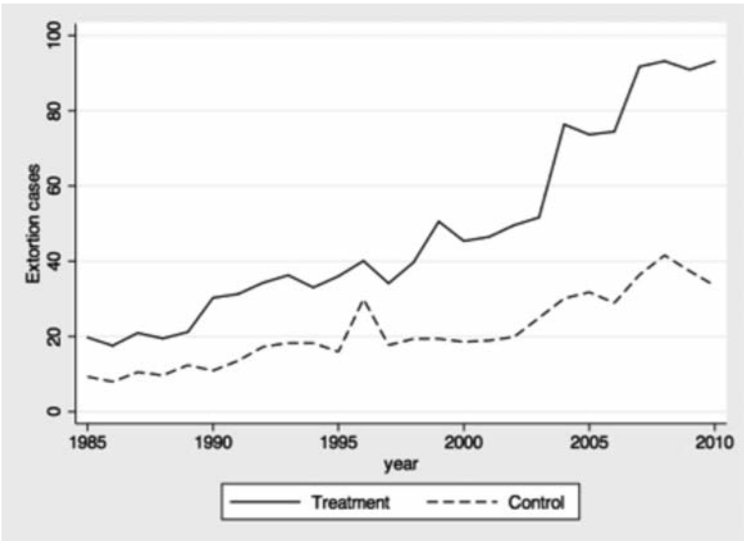
[9] Recall that, from the description provided above, by definition (Article. 416-*bis* Penal Code) a mafia organization undertakes «management or control of financial activities, concessions, permissions, enterprises and public services for the purpose of deriving profit…».

same pattern in all the Northern provinces if renewal works were not approved. In the present context, this assumption is likely to hold. Indeed there are no reasons to suspect that some provinces are more prone to mafia groups' infiltrations than others. Northern territories are characterized by an homogeneous economic conditions (e.g. we can consult ISTAT reports time series that show very close values of GDP *per capita* of Northern regions) that and by an entrenched rejection of mafia phenomena; hence there must be other factors or events, as the public investments we are considering, that attract these groups and allow them to establish and thrive.

As indicative example, Graph 3 plots the yearly average of extortion cases[10]. In practice, we would like to observe a parallel movement of mafia activity in both groups up to the time of the implementation of the treatment (years 2000-2002), and then a change of the trend for the treatment group should be remarked. The Graph shows a parallel trend of both groups up to 2000-2002, then a sharp increase of mafia activity is observed only for the treatment group.

Graph 3

PARALLEL TREND ASSUMPTION



Source: Author's computation using STATA *Yearly Book of Criminal Statistics,* published by Istat (Italian Statistical Institute) Provincial level data.

[10] We are interested in this criminal activity because of its economic connotation.

As a final remark, it is reasonable to think that Northern provinces have experienced the same macro shocks over the studied time period (1996-2008), indeed there have not been observed localized shocks (e.g. natural disaster such as earthquake) that affected only a specific subarea.

Putting all this things together, if we focus our attention on extortions, *i.e.* crime with economic connotation, we can safely assume the validity of the implemented strategy. The implementation of DD model can provide an estimate of the average treatment effect of the renewal works of A4 motorway on the treated provinces.

5.3 *Regressions*

The empirical investigation applies the DD regression presented above, which estimate the causal effect of infrastructures on the mafia spread in the provinces that received public works, allowing for time fixed effect and province fixed effect.

The following regression has been performed:

$$mafia_{pt} = \beta_t + \gamma_p + \delta D_{pt} + \varepsilon_{pt} \qquad (1)$$

Where: β_t is a time fixed effect equal for each province; γ_p is a province fixed effect equals for all time periods; $mafia_{pt}$ measures the number of reports of the considered crime and D_{pt} is a dummy variable which takes value 1 if the examined province belongs to the treatment group and the observation is made after the treatment (for instance, $D_{pt} = 1$ if an observation is taken for Bergamo territory after 2002), hence the associated coefficient δ can be identified as the effect on mafia spread given by the execution of public works.

5.4 *Results*

Table 5 presents estimation results of the OLS regressions of equation *(1)* with standard errors robust to heteroschedasticity (only the coefficients of the variable D_{tp} are reported).

Table 5

RESULTS DD METHOD

Variable	Obs.	Coef.	S.e.	*t* statistic	*p*-value
Murders	1,066	-0.448***	0.152	-2.96	0.003
Art 416-*bis*	1,066	-0.116	0.130	-0.89	0.371
Extortions	1,066	27.012***	4.903	5.51	0.000
Thefts	1,066	2,312.383	-3,126.24	1,730.196	0.182

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors in brackets.
Source: Yearly Book of Criminal Statistics, published by Istat (Italian Statistical Institute) Provincial level data.

Regression results obtained using extortions as dependent variable bolster the idea that infrastructures represent an attractive business chance for mafia expansion. In fact the regression gives a positive and statistically significant estimate: the execution of the renewal works has determined an increase of about 27 extortion cases per treated province.

Table 6 inspects the results obtained regressing reports of extortions. All the values are meant as the average extortions in each province, over the periods before and after the treatment. Here, the causal effect of interest is estimated using the sample analogue of the population means. This table shows average extortions in the treated and in the control group before and after the renewal works on the motorway A4. The margins show group differences in each period, the change over time in each group and the differences-in-differences.

Table 6

AVERAGE EXTORTION CASES

Variable	Treatment group	Control group	Difference
Average extortions before	32.18***	15.55***	16.63***
	(3.00)	(0.93)	(1.45)
Average extortions after	75.14***	30.33***	44.81***
	(9.06)	(1.26)	(6.02)
Change in mean extortion	42.99***	14.78***	28.21***
	(5.89)	(1.03)	(3.26)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors in brackets.
Source: Yearly Book of Criminal Statistics, published by Istat (Italian Statistical Institute) Provincial level data.

The regression that uses mafia-type murders as dependent variable generates a statistically significant negative coefficient. The estimated effect, though, is very small. Such result suggests that mafia organizations, in order to succeed in their migrations towards Northern Italy, have operate differently, avoiding crimes which characterize mafia presence in its historical settlements. Migrating mafia organizations seem to prefer crimes with economic connotation, rather than crimes such as murders or terroristic attacks, because the former are better suited for a successful transplantation in the economic environment characterizing Northern territories[11]. This conclusion is consistent with the analysis provided by Pinotti (2011) that argues that mafia-type murders denote mafia presence in Southern regions.

Using cases *ex* Article 416-*bis* of Penal code as dependent variable, the analysis does not lead to statistically significant coefficient. Indeed, very few cases of mafia-type association have been observed. Such outcome seems to be linked to the features of the judicial system in the North of the country. Probably, a judicial system located in an environment not historically interested by the phenomenon, was not able to conduct appropriate controls in order to recognize mafia presence and face its expansion.

Finally, once thefts are used as dependent variable, the coefficient of interest is not significant, confirming the idea that public works do not increase general crime offences but only affects mafia-related criminal activities.

Overall, these findings suggest that public renewal works of A4 motorway may have contributed to increase mafia activity in Northern regions and that mafia organizations seems to be attracted by the business opportunities of thriving areas. Moreover, is seems that migrating mafia groups prefer activities with economic connotation in new territories, while crimes which characterize mafia presence in Southern Italy, such as murders are barely affected.

5.5 *Robustness Checks*

In this section I present the results of some robustness checks, performed modifying the basic regression. If the estimated coefficients are robust, *i.e.* do not significantly change, this is interpreted as evidence in favor of the results so far obtained.

The first robustness check estimates the OLS regressions on an alternative sample. The dataset includes information about three Central regions, namely

[11] Recall that the present analysis relies on the assumption that mafia groups moved because attracted by the business opportunities offered by the North of the country. The migrating groups are motivated by profit objectives, thus they are likely to prefer activities with economic connotation such as money loundering, extortions loan sharking, *etc*. Thus, in the present context, we can exclude the hypothesis that mafia type murdes are less important because, given that the migration is recent, there has been so far little competition among mafia groups.

Tuscany, Marche and Umbria, not historically interested by the phenomenon and hence comparable to Northern regions.

As a second check the time periods considered for the treatment variable are lagged one and two years respectively. In fact there exists a time interval between the approval and the execution of the renewal works, this period is likely to allow mafia organizations to infiltrate in the subcontract system and stabilize in the new territory. Hence, these robustness checks assume that the causing event of mafia expansion is the execution of the renewal works of A4 motorway rather than their approval. Table 7 presents the new estimates under alternative specifications.

TABLE 7

ROBUSTNESS CHECKS

Robustness Check 1: The Sample Includes Observations of Tuscany, Marche and Umbria

Variable	Obs.	Coef.	S.e.	*t* statistic	*p*-value
Murders	1,545	-0.457***	0.151	-3.03	0.002
Art. 416-*bis*	1,545	0.077	0.123	-0.61	0.542
Extortions	1,545	24.682***	4.76	5.19	0
Thefts	1,545	-2,241.816	1,706.633	-3,126.24	0.189

*** *p*<0.01, ** *p*<0.05, * *p*<0.1 Robust standard errors in brackets.
Source: Yearly Book of Criminal Statistics, published by ISTAT (Italian Statistical Institute) Provincial level data.

Robustness Check 2: Time Periods for the Treatment Variable are Lagged One Year

Variable	Obs.	Coef.	S.e.	*t* statistic	*p*-value
Murders	1,066	-0.429***	0.15	-2.87	0.004
Art. 416-*bis*	1,066	-0.089	0.133	-0.66	0.507
extortions	1,066	28.298***	5.31	5.33	0
thefts	1,066	469.817	1,288.623	0.36	0.715

*** *p*<0.01, ** *p*<0.05, * *p*<0.1 Robust standard errors in brackets.
Source: Yearly Book of Criminal Statistics, published by ISTAT (Italian Statistical Institute) Provincial level data.

Robustness Check 3: Time Periods for the Treatment Variable are Lagged Two Years

Variable	Obs.	Coef.	S.e.	*t* statistic	*p*-value
Murders	1,066	-0.397***	0.145	-2.75	0.006
Art. 416-*bis*	1,066	-0.147	0.121	-1.22	0.225
extortions	1,066	29.165***	5.769	5.06	0
thefts	1,066	767.242	1,231.17	1,452.48	0.533

*** *p*<0.01, ** *p*<0.05, * *p*<0.1 Robust standard errors in brackets.
Source: Yearly Book of Criminal Statistics, published by ISTAT (Italian Statistical Institute) Provincial level data.

Results are mainly unchanged, suggesting that the estimated coefficients of public works on mafia-related crimes are rather robust. As before, when extortions are used as indicator of mafia activity, the execution of public works determines a significant increase in activity in the province in the following period.

Instead, the effect on mafia-association cases, thefts and murders is small or insignificant. Again, these results may indicate that mafia expansion in new regions attracted by thriving economic activities does not necessarily involve an increase in typical mafia offenses, such as mafia-type murders, or incidence of mafia associations, but rather an upward movement in mafia-related crimes with economic connotation, such as extortions.

6. - Conclusion

This thesis provides an economic analysis of the recent mafia expansion in Northern Italy and contributes to the literature by investigating empirically the possible effect of public investment in infrastructure. Using a new dataset which reports information about three mafia-related crimes (mafia-type murders, mafia-type association, cases of extortions) at provincial level over the period 1985-2010, evidence is provided that the renewal works of A4 motorway approved between 2000 and 2002, determined an increase of mafia-related crimes with economic connotation on the treated provinces. In particular, taking extortions as an appropriate indicator of mafia presence in Northern Italy, estimation results show that the public renewal works in A4 motorway implied an average increase of extortion cases of about 27 per treated province.

Instead, public investment in infrastructure does not seem to determine increased incidence of murders or mafia-type associations, suggesting that mafia does not necessarily use the same strategies in newly colonized territories.

Finally, other crimes such as thefts are not affected by the execution of public works, supporting the view that the outcome extortions captures a more specific effect, such as the expansion of illegal mafia-type organizations in provinces where public investment is concentrated, rather than a generalized increase in illegal activities in these areas.

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The Granularity of the Stock Market: Forecasting Aggregate Returns Using Firm-Level Data

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This paper draws on the theoretical concept of granularity to forecast aggregate stock market returns using firm-level data. When applied to stock market returns, granularity suggests that fluctuations in the returns of individual firms can be used to forecast future aggregate returns. This paper finds that a model including firm-level data outperforms a benchmark model based on aggregate variables alone. Furthermore, a real-time investment strategy based on our model beats a buy-and-hold strategy on the stock market either in terms of cumulative returns or in terms of risk-adjusted excess returns or in both, depending on the forecast horizon.
[JEL Classification: G11; G17].

Keywords: granularity; forecasting; stock market returns.

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1. - Introduction

This paper proposes an empirical model to forecast US aggregate stock market returns using firm-level data.

The main motivation lies in the recent theoretical finding of the "granular nature" of the fluctuations of many economic variables. Gabaix (2011) calls "granularity" the fact that the volatility of an aggregate economic variable (e.g. GDP) is mostly explained by the volatility of the largest "grains" that form the aggregate. This holds provided that the dimension of the "grains" has a fat-tailed distribution, *i.e.* a distribution with an infinite variance. Gabaix (2011) shows that this is the case for US GDP, as he claims that idiosyncratic movements in the production of the largest 100 firms explain about one third of the variations in output and in the Solow residual. Carvalho and Gabaix (2013) go even further and claim that the so-called "great moderation", a significant fall in the volatility of GDP that began in the 1980's, is mostly due to a change in the fluctuations of the output of the biggest firms in the US. Finally, two recent papers by Acemoglu, Carvalho, Ozdaglar and Tahbaz-Salehi (2012) and Carvalho (2010) use the theory of networks to establish a link between the propagation of idiosyncratic shocks and the topology of the sectors that compose an economy.

Empirically, di Mauro, Fornari and Mannucci (2011) exploit the hypothesis of granularity to provide forecasts for industrial production using data on the equity prices of firms. They find that the returns and variances of a relatively small number of firms significantly help in forecasting industrial production for several countries, including the US.

This paper draws on the work of di Mauro, Fornari and Mannucci (2011) to provide forecasts for aggregate stock market returns in the US.

We use the daily stock prices of 1,000 firms listed in the US to produce end-month returns and monthly variances for each firm. Next, we set up a regression of h-month aggregate stock market returns on relevant aggregate information (market-wide dividend yields and market variance) and on firm-level data (individual returns and variances). Importantly, firms enter the equation one at a time. Finally, the forecasting horizon h takes the following values: 1, 3, 6, 12 and 24 months.

We use this set-up to forecast aggregate returns in real time.

Standing in period t, we rank firms according to their forecasting performance for aggregate returns in t. We pick the top ten firms and we use them to produce ten forecasts for aggregate returns in $t+h$. Finally, we average them out. The outcome is a time series of forecasts for aggregate returns that boasts some good properties. First, it easily passes Hansen (2005)'s test for Superior Predictive Ability when confronted with the forecasts produced without using firm-level data. Secondly, when we set up a model-based investment strategy on the stock market, it turns out that for relatively short forecast horizons, *i.e.* for 1, 3 and 6 months, our strategy beats a buy-and-hold strategy on the stock market either in terms of average returns or in terms of average risk-adjusted excess returns or, most frequently, in both. This result holds for different investment horizons.

We conclude that, for relatively short forecast horizons, our model provides valid and useful forecasts of aggregate stock market returns.

The rest of the paper is organized as follows. Section 2 reviews the relevant literature; Section 3 describes the data we use; Section 4 presents our model and some preliminary results on its forecasting potential; Section 5 applies our model to forecasting aggregate returns in real time. Section 6 concludes.

2. - Review of the Literature

This section is divided into two parts. The first one describes the relevant theoretical literature that inspires this paper. The second one is concerned with empirical applications of that theory, as well as other empirical approaches to forecasting aggregate stock market returns.

2.1 *Theoretical Literature*

The core theoretical concept that informs our research is the concept of "granularity". According to Gabaix (2011) granularity holds when the fluctuations of an aggregate variable can be traced back in large part to the fluctuations of the largest "grains" that form the aggregate. More specifically, his basic result is that when the size of firms is distributed according to a power law, idiosyncratic shocks to the volatility of large firms do not average out. On the contrary, they affect the whole economy.

To fix ideas, imagine an economy with only idiosyncratic shocks to firms. $S_{i,t}$ represents the production of firm i in year t. Its growth rate can be expressed as a random function of its volatility:

$$\frac{\Delta S_{i,t+1}}{S_{i,t}} = \frac{S_{i,t+1} - S_{i,t}}{S_{i,t}} = \sigma_i \varepsilon_{i,t+1} \tag{1}$$

where $\varepsilon_{i,t+1}$ are uncorrelated random variables with mean 0 and variance 1. Total GDP is just the sum of the individual productions. Therefore, its growth rate can be written as follows:

$$\frac{\Delta Y_{t+1}}{Y_t} = \frac{1}{Y_t}\sum_{i=1}^{N} \Delta S_{i,t+1} = \sum_{i=1}^{N} \sigma_i \frac{S_{i,t}}{Y_t}\varepsilon_{i,t+1} \tag{2}$$

where N is the number of firms in the economy. The volatility of GDP, defined as the standard deviation of its growth rate is:

$$\sigma_{GDP} = \sqrt{\sum_{i=1}^{N} \sigma_i^2 \left(\frac{S_{i,t}}{Y_t}\right)^2} \tag{3}$$

Thus, the square volatility of GDP is a weighted average of the individual square volatilities. For simplicity, imagine that the production of each firm has the same volatility: $\sigma_i = \sigma$. Then the volatility of GDP becomes:

$$\sigma_{GDP} = \sigma \sqrt{\sum_{i=1}^{N} \left(\frac{S_{i,t}}{Y_t}\right)^2} \tag{4}$$

The term under square root can be thought of as a transformer of individual volatilities into aggregate volatilities. It plays a crucial role in what follows.

Now, a common argument among macroeconomists is that an idiosyncratic shock to volatility has a very small effect on GDP in sufficiently big economies. To see why, consider the simple case of a number N of firms with equal size. The volatility of GDP becomes:

$$\sigma_{GDP} = \frac{\sigma}{\sqrt{N}} \tag{5}$$

This equation means that at the aggregate level an idiosyncratic shock to the volatility of a given firm is multiplied by $1/\sqrt{N}$. Thus, individual volatilities decay at a very fast rate, which depends on the number of firms that populate an economy. As a consequence, they cannot account for the observed volatility of GDP and this is why macroeconomists resort to aggregate shocks. Gabaix (2011) shows that this result holds even more generally provided that the size of firms is distributed with finite variance. In that case, assuming they all have the same volatility , we would have

$$\sigma_{GDP} = \frac{\sqrt{E(S^2)}}{E(S)} \frac{\sigma}{\sqrt{N}} \tag{6}$$

Therefore, the rate of decay is $\sqrt{N}$, as it happens in the case of firms with equal size.

This result hinges on the crucial assumption that the size of firms has a distribution with finite variance. However, the empirical evidence doesn't support this assumption. Among others, Axtell (2001) and Gabaix (2011) prove that the size of firms in the US follows a power law.

A power law is a probability distribution f such that

$$f(x) = \frac{\zeta a}{x^{\zeta+1}} \tag{7}$$

for $x>a^{1/\zeta}$, $\zeta \geq 1$ In other words, a power law characterizes the tails of a distribution and makes them particularly "fat". This implies that the variance of the distribution becomes infinite. A power law can be applied to many economic and social phenomena. Pareto (1896) was the first to discover that the income distribution follows a power law. Furthermore, Zipf (1949) and Gabaix (2009) apply this distribution to the size of cities while Gabaix, Gopikrishnan, Plerou and Stanley (2006) to the size of mutual funds.

When the power law is used to describe the distribution of the sizes of firms, Gabaix (2011) claims that an exponent $\zeta = 1$ provides the best approximation of the data.

Most importantly, Gabaix (2011) shows that when the size of firms follows a power law, idiosyncratic shocks to firm-level volatilities decay at a far slower rate than $\sqrt{N}$. This happens because the central limit theorem no longer holds. Specifically, he proves that:

$$\sigma_{GDP} = \frac{v_{\zeta}}{\log N} \sigma \quad \text{for} \quad \zeta = 1 \tag{8}$$

(9)
$$\sigma_{GDP} = \frac{v_\zeta}{N^{1-1/\zeta}}\sigma \quad \text{for} \quad 1 < \zeta < 2$$

(10)
$$\sigma_{GDP} = \frac{v_\zeta}{N^{1/2}}\sigma \quad \text{for} \quad \zeta \geq 2$$

where v_ζ is a random variable independent from N and σ. For $\zeta \geq 2$ the distribution no longer has infinite variance, therefore the standard result applies and the decaying rate is $\sqrt{N}$. Otherwise, as the parameter ζ decreases, $1/N^{1-1/\zeta}$ increases and when $\zeta = 1$, which is the case for the size of US firms, idiosyncratic shocks decay on aggregate at the very slow rate of $\log N$.

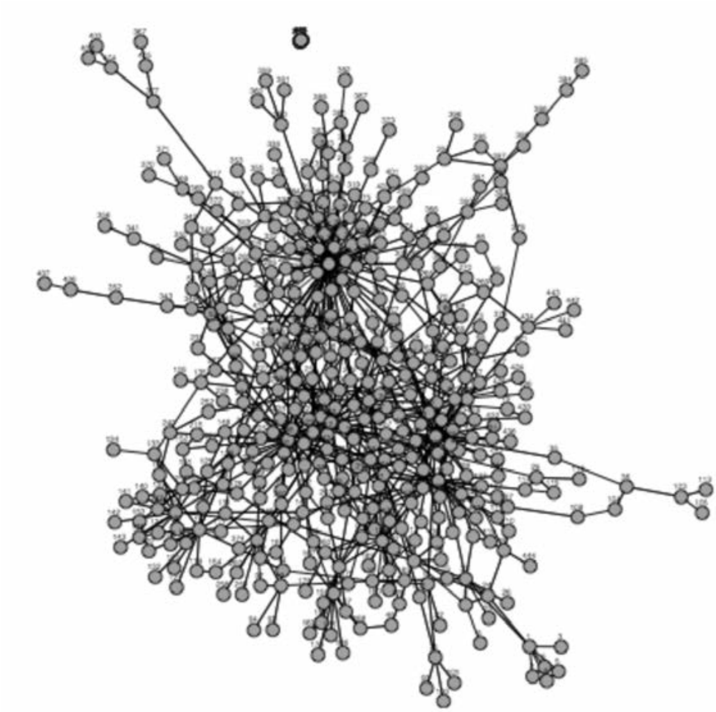
This insight is extremely important as it underlines that movements in the production of large firms can cause significant volatility at the aggregate level. Of course, the same argument holds for any aggregate economic variable whose components have a fat-tailed distribution.

In order to apply this theory to US GDP, Gabaix (2011) selects the largest 100 firms in terms of sales for each year between 1951 and 2008. Then, he defines a "granular residual" as the weighted average of the growth rate in the labor productivity of these firms, purged from the aggregate labor productivity. Thus, the granular residual is a proxy for the idiosyncratic growth in productivity of a given firm. Finally, he regresses GDP growth and the Solow residual on the granular residual. About one third of fluctuations in GDP growth and in the Solow residual are due to the granular residual, *i.e.* to the movements in productivity growth of the largest 100 firms each period. Thus, his empirical findings confirm the theory.

Gabaix (2011) concentrates on the distribution of firm sizes to explain the relevance of idiosyncratic shocks. Acemoglu, Carvalho, Ozdaglar and Tahbaz-Salehi (2012) tackle the issue from a different point of view. They build on the theory of networks to show that the topology of the interconnections between the sectors of an economy matters for the propagation of idiosyncratic shocks. Their argument is appealing and intuitive and it can be grasped by looking at the network representation of input/output linkages among US sectors in 1997, shown in Figure 1.

FIGURE 1

NETWORK OF INPUT/OUTPUT LINKAGES OF US SECTORS IN 1997



The authors derive this graph using data on the US input/output matrix taken from the Bureau of Economic Analysis. The network shows that there exist some "core" sectors, which have many linkages to relatively isolated "peripheral" sectors. In order to give a formal characterization of the interconnections of sectors, the authors examine the input/output matrix. Entry (i,j) of this matrix contains the share of sector j's product in sector i's production technology. The sum of every column j, *i.e.* the "degree" of sector j, is then the share of j's product in the production function of the whole economy. Using the input/output matrix, the authors define and characterize two propagation mechanisms for micro-level shocks. The first is by first-order interconnections. When the degrees of sectors (*i.e.* their contribution to the production function of the whole economy) have a fat-tailed distribution, idiosyncratic sectoral shocks propagate to the aggregate economy at a rate lower than $N^{1-1/\zeta}$. This result is similar to the one in *(9)*, with the difference that Acemoglu, Carvalho, Ozdaglar and Tahbaz-Salehi (2012) only provide a lower bound for the rate of decay, which will be slower than the one predicted by *(9)*. The second and most important propagation mechanism is by second-order interconnections. The authors define the second-order degree of sector *j* as the weighted sum of the degrees of sectors that demand input from *j*, with weights equal to the share of *j*'s product in their production function. Thus, second order degrees measure the interconnectedness of each sector with the other ones. The main result here is that if second order degrees are distributed according to a power law, then, once again, aggregate volatility decays at a rate slower than $N^{1-1/\xi}$. Finally, the authors prove that for balanced networks where there is a uniform bound to the degree of every sector, idiosyncratic sectoral shocks average out at the fast rate of $\sqrt{N}$. In conclusion, the key messages of the paper are two. First, idiosyncratic shocks matter for aggregate volatility under certain conditions. Second, the structure of the interconnections between the sectors of an economy matters for the propagation of sectoral shocks. In particular, asymmetry is important: the extent to which idiosyncratic shocks propagate throughout the economy depends on the asymmetry of the interconnections between the sectors.

Carvalho (2010) further explores the role of networks for the propagation mechanism of micro-level shocks. First, he identifies the topological characteristics of the input/output network of US sectors. He finds that sectors are relatively homogeneous in their role as input-demanders. Every sector relies on a relatively small number of key inputs and this feature is robust across different sectors. However, he finds a great heterogeneity across sectors in their role as input-suppliers. While each sector takes inputs form a few different sectors, a few sectors provide inputs to a large proportion of sectors. Specifically, the number of sectors to which every sector is a supplier follows a power law. The author then proposes a calibrated multi-sector dynamic model that is able to replicate these two features of the network of US sectors. Most importantly, the model is able to generate considerable aggregate volatility from idiosyncratic shocks.

Finally, Malevergne, Santa-Clara and Sornette (2009) provide an application of the concept of granularity to asset pricing. The authors claim that a power law in the capitalization of the firms that are present in a well-diversified portfolio causes the risk of the portfolio to be higher than market risk. To account for this additional risk, the authors define a "Zipf factor" and they proxy it with the difference between the returns of the equal-weighted portfolios and those of the value-weighted portfolios.

The theory exposed so far is the motivation for the empirical work pursued by this paper. In particular, we consider important the extension and enrichment of granularity through the theory of networks. We believe that the structure of the connections between the grains of an economy is fundamental to understand the mechanism that turns micro-level shocks into aggregate shocks. The resulting theory is insightful and further research along this line promises to be extremely fruitful.

2.2 *Empirical Literature*

As the previous Section shows, the concept of granularity is far-reaching and it can be empirically applied to a variety of contexts. Our paper is closely related to di Mauro, Fornari and Mannucci (2011). The authors use the granular hypothesis to produce forecasts for industrial production. Their methodology is relatively simple. They set up a one-equation model where the *h*-month growth rate of industrial production is regressed on two aggregate variables (term spread and market variance) and on two firm-level variables (the end-month returns and the monthly variances of the equity prices of a given firm *j*). They consider four forecast horizons (6, 12, 18 and 24 months) and a wide sample of firms listed in the stock markets of different countries, including the US. These firms enter the model only one at a time, so that the authors can test which firm provides the best forecast in every period. They start with an in-sample analysis and they find that the R^2 of the model using most of the firms is much higher than a benchmark based on aggregate variables only. This result is robust for all the countries and all the forecast horizons. Next, they do an out-of-sample analysis using a 10-year rolling sample and testing the rolling *h*-month-ahead forecasting performance of each firm. They find that few firms provide sizeable improvements in the forecasts and they conclude that this is evidence of the presence of granular effects. Furthermore, they show that firms listed in foreign countries do provide useful information for forecasting the industrial production of a given country. This paper is an important empirical application because it shows that granularity is applicable to many economic concepts. Our paper draws much on this insight as it applies part of the methodology of di Mauro, Fornari and Mannucci (2011) to aggregate stock market returns.

Blank, Buch and Neugebauer (2009) provide an empirical application of Gabaix (2011)'s granular residual to German banks. Their aim is to understand the impact of idiosyncratic shocks to large banks on the probability of distress of smaller banks. First, they construct a measure of idiosyncratic shocks to large banks, which they call "banking granular residual". To do so, they take the 10 biggest banks in Germany in terms of total operating income (a proxy of their size) and they compute a weighted average of the difference between the growth rate of the cost-to-income *ratio* of each bank and the average growth rate of the same variable. The weights are given by the sizes of the banks proxied by operating income. Finally, they include this granular residual into a stress-testing model for the German banking system composed of both a micro and a macro level. The micro level explains the distress probabilities of banks, while the macro level is a vector autoregressive model. By including the banking granular residual as an additional explanatory variable for the probability of distress of banks, the authors test the relevance of idiosyncratic shocks to large banks for the stability of the German financial system. Indeed, they find that positive shocks to large banks decrease the probability of distress of smaller banks and *vice versa*.

Gabaix and Carvalho (2013) use granularity to explain the so-called "great moderation", *i.e.* the structural break occurred in the volatility of GDP in the 1980's. While before the 1980's GDP growth showed ample movements, after that period and until the recent financial crisis, the pattern of GDP growth became smoother. Gabaix and Carvalho (2013) claim that changes in GDP volatility were largely, if not exclusively, due to idiosyncratic shocks to the volatility of some sectors. These, in turn, propagated to the whole economy. They define "fundamental volatility" the volatility derived exclusively from idiosyncratic shocks to firms. It is exactly the same volatility defined by equation *(4)*. The difference is that Gabaix and Carvalho (2013) don't assume that fundamental volatility is the only component of GDP volatility, as in equation *(4)*. The literature has identified the start of the great moderation by testing for the presence of a structural break in the level of GDP volatility. Using data from 1947:Q1 to 2009:Q4, Gabaix and Carvalho (2013) show that, once they control for fundamental volatility, they no longer find any breaks. Thus, the authors claim that changes in the volatility of firms, as opposed to aggregate shocks to volatility, determined the structural change of GDP volatility in the 1980's.

Finally, leaving granularity aside, there have been many other approaches to forecasting aggregate stock market returns in the literature. Pesaran and Timmermann (1995) provide a good critical review of a wide range of models based on a set of different aggregate variables, ranging from dividend yields to interest rates to monetary growth rates and others. Specifically, they simulate the effectiveness of these models in real time, adding uncertainty over the exact set of variables that should be used in each period. Their analysis underlines that the set of best predictors changes much over time and it tends to vary with the volatility of the stock returns. Moreover, they find that the predictability of the stock market increases with its volatility.

A more recent approach to forecasting stock market returns is the one pursued by Ferreira and Santa-Clara (2011). Their strategy is to divide stock market returns into their three basic components, *i.e.* dividend-price *ratios*, earnings growth and price-earnings *ratio* growth, and to forecast each part separately. The authors justify their methodology by the different time series persistence of these components. The dividend-price *ratio* is very persistent and it can be forecasted using the current *ratio*. Since earnings growth is almost unpredictable in the short run but it has a low-frequency predictable component, the authors forecast it using its 20-year moving average. Finally they assume no growth in the price earnings *ratio*, so that the final return forecast is equal to the 20-year moving average of the earnings growth plus the current dividend-price *ratio*. The authors show that using these forecasts to set up an investment strategy yields a Sharpe *ratio* gain of 0.3 over a trading strategy based on the historical mean.

In conclusion, the existing approaches to forecasting aggregate stock market returns rely on the information contained in a varying set of aggregate variables. The novel contribution of this paper lies in exploiting firm-level data to derive forecasts for aggregate returns and in showing that micro-level data matter even on top of aggregate information.

3. - Data

The data we use are taken from Thomson Financial Datastream and from the database of the Federal Reserve Bank of St. Louis.

We derive our results using US data: for the sample we consider, the US stock market leads the other stock markets and we can study it in isolation without any major biases.

Datastream computes many different indexes at different levels of aggregation. A level 1 index is computed for a market in its entirety, while levels 2 to 6 span from industry-wide to subsector-wide indexes. For each level of aggregation, Datastream computes different indicators and each of them summarizes a different piece of information.

For the purposes of this paper, we use:

- the daily and monthly economy-wide Price Index for the United States;
- the monthly economy-wide Dividend Yield for the United States;
- the daily equity prices of 1,000 firms listed in the United States and belonging to all the sectors of the economy, with the industrial and financial sectors being the most represented;
- the annualized constant maturity rate of 1-month, 3-month, 6-month, 12-month and 24-month US Treasuries.

The sample size of the data goes from 5/1977 to 12/2009, with two important exceptions. First, the 1,000 firms are a sub-sample of the US listed companies in December 2009. Hence, for each of them the sample size of daily equity prices is different, as it depends on when the company first entered the stock market. The firm-level data are the same as those used by di Mauro, Fornari and Mannucci (2011). Second, maturity rates for 1-month treasury bills are only available from 7/2001 onwards.

What follows is a more detailed description of the construction of our variables of interest.

Datastream computes the price index (*PI*) at any level of aggregation as follows:

$$PI_t = PI_{t-1} \frac{\sum_{c=1}^{C} P_t^{(c)} N_t^{(c)}}{\sum_{c=1}^{C} P_{t-1}^{(c)} N_t^{(c)} f^{(c)}} \tag{11}$$

where $P_t^{(c)}$ is the unadjusted price on working day t for constituent c, $N_t^{(c)}$ is the number of shares in issue on day t for constituent c, f is an adjustment factor for a capital action occurring on day t and C is the number of constituents of the index. Sector and market aggregations are weighted by market value and are calculated using a representative list of shares.

From the daily *PI*, we compute the monthly market variance:

$$MktVar_t = \frac{\sum_{s \in S_t} (PI_s - \overline{PI}_t)^2}{w_t - 1} \tag{12}$$

where t indexes the month of reference, S_t is the set of working days in month t, w_t is the number of working days in month t and $\overline{PI}_t$ is the monthly average of the daily *PI* for month t.

From the monthly *PI*, instead, we compute the aggregate returns in 1, 3, 6, 12 and 24 months:

$$Agg\,\mathrm{Re}\,t_{t+h} = \log(PI_{t+h}) - \log(PI_t) \tag{13}$$

where h = 1, 3, 6, 12, 21 and t is the month of reference.

Using individual equity prices, *i.e.* individual *PI*'s, we compute:

- the individual end-month returns for each firm *j*:

$$\mathrm{Re}t_t^{(j)} = \frac{(PI_t^{(j)} - PI_{t-1}^{(j)})}{PI_{t-1}^{(j)}} \tag{14}$$

where $PI_t^{(j)}$ is the equity price of firm *j* at the end of month *t*;

- the individual monthly variances of the equity prices for each firm *j*:

$$Var_t^{(j)} = \frac{\sum_{s \in S_t} (PI_s^{(j)} - \overline{PI}_t^{(j)})^2}{w_t - 1} \tag{15}$$

where *t* indexes the month of reference, S_t is the set of working days in month *t*, w_t is the number of working days in month *t* and $\overline{PI}_t^{(j)}$ is the monthly average of the daily $PI^{(j)}$ for month *t* and firm *j*.

The constant maturity rates of 1-month, 3-month, 6-month, 12-month and 24-month US Treasuries that we take form the dataset of the Federal Reserve Bank of St. Louis are in annualized terms. We transform each of them into h-month rates to make them comparable to *h*-month stock market returns.

4. - The Model

The purpose of this section is to set up our model and to present some preliminary results about its forecasting potential. We regard these results as a necessary condition for forecasting aggregate returns in real time, which we do in Section 5.

We consider a regression of aggregate returns in *h* periods on a constant, two aggregate variables and two firm-level variables. The aggregate variables are the market-wide dividend yield and the monthly variance of the market-wide price index, while the two firm-level variables are the individual end-month returns and the monthly variance of the individual price index.

$$Agg\,\mathrm{Re}t_{t+h} = \beta_0 + \beta_1 DY_t + \beta_2 MktVar_t + \beta_3 \mathrm{Re}t_t^{(j)} + \beta_4 Var_t^{(j)} + \varepsilon_t \tag{16}$$

Thus, firms j enter the equation only one at a time and we can elicit the marginal effect of a single firm compared to a model with aggregate regressors only:

$$AggRet_{t+h} = \beta_0 + \beta_1 DY_t + \beta_2 MktVar_t + \varepsilon_t \qquad (17)$$

Model *(17)* is going to be our benchmark.

As shown by Pesaran and Timmermann (1995), after the 1970's dividend yields always enter the set of best aggregate predictors of stock market returns. Aggregate market variance, instead, conveys information about the riskiness of financial markets. In a riskier environment, firms typically under-hire (Bloom, 2009), which affects their production and their performance in the stock market. Moreover, the uncertainty linked to the volatility of the market induces firms to postpone their investments.

Model *(16)* contains an index for firms, j, and an index for periods, h. For each month beginning in 4/1987+h, for h equal to 1, 3, 6, 12 or 24 months, we repeatedly estimate model *(16)* for each firm j and each forecast horizon h using a 10-year rolling sample. This means that standing in month t and in order to use all the possible information, the sample size of the dependent variable goes from t-119 to t. As a consequence, the explanatory variables go from t-119-h to t-h.

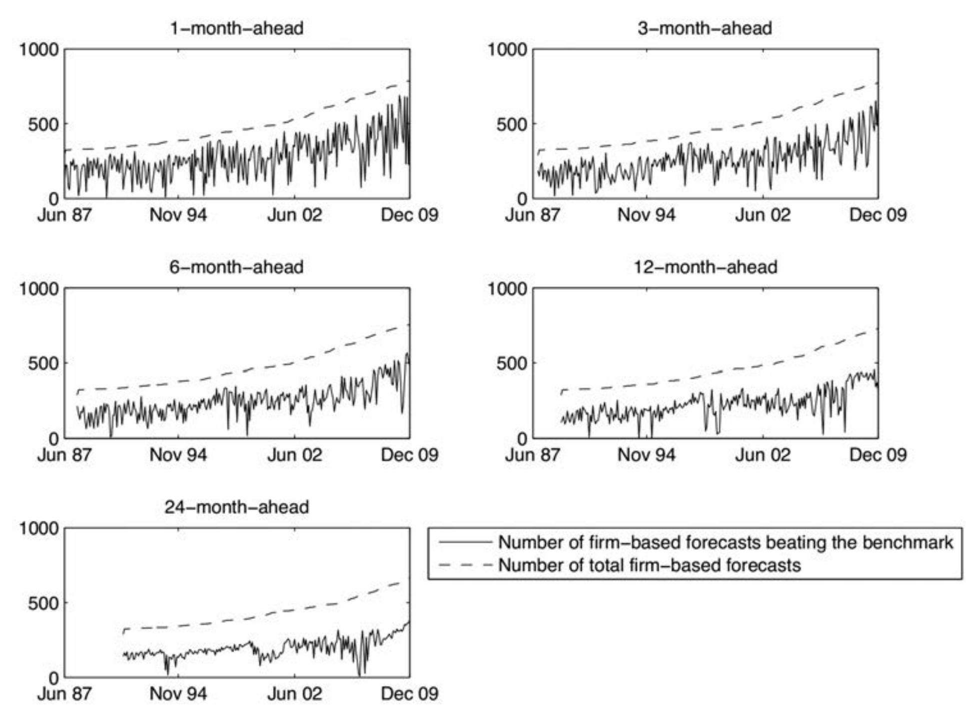
An important remark is in order here. As mentioned in Section 3, the sample sizes of firm-level prices are different. Every period, we consider only the firms that have a 10-year history, so that each rolling window includes exactly 120 monthly observations. Because of the way our dataset is constructed, the closer we are to 12/2009, the larger the set of firms we can use. This is suboptimal, as we would have preferred to have a larger pool of firms to choose from when forecasting aggregate returns in the 80's and 90's. However, since our concern is exclusively that of forecasting, we are confident that including a larger set of firms in our model could only improve our results. Therefore, what follows is robust to our data selection.

For each forecast horizon, at time t we have a cross section of forecasts for time $t+h$, each one of them derived by including a different firm into model *(16)*. *Ex post*, *i.e.* standing in $t+h$, it is possible to determine which and how many of the firm-based forecasts delivered a good forecasting performance in terms of forecast error.

Graph 1 shows the number of firm-based forecasts that beat the benchmark in every point in time and for every forecast horizon h.

GRAPH 1

NUMBER OF FIRM-BASED FORECASTS BEATING THE BENCHMARK EACH PERIOD



This number varies considerably over time and it should be compared to the number of firm-based forecasts available every period, which is also a function of time. Every period a large fraction of the firms available do increase the forecasting performance of a model based on aggregate information alone. Averaging over time, the percentage of firms that do so is 56% for a 1-month forecast horizon, 53% for h=3 months, 52% for h=6 months, 48% for a 6-month forecast horizon and 45% for h=24 months.

Keeping in mind that roughly half of the firms yield improvements to forecasting power and for the sake of visualizing this result, we can look at the very best firm-based forecasts every period. Graph 2 plots the series of best firm-based forecasts, together with the benchmark forecasts and the observed aggregate returns, for the subsample 1/2008 - 12/2009.

Graph 2

BEST-*EX-POST* FIRM-BASED FORECASTS, BENCHMARK FORECASTS AND AGGREGATE RETURNS

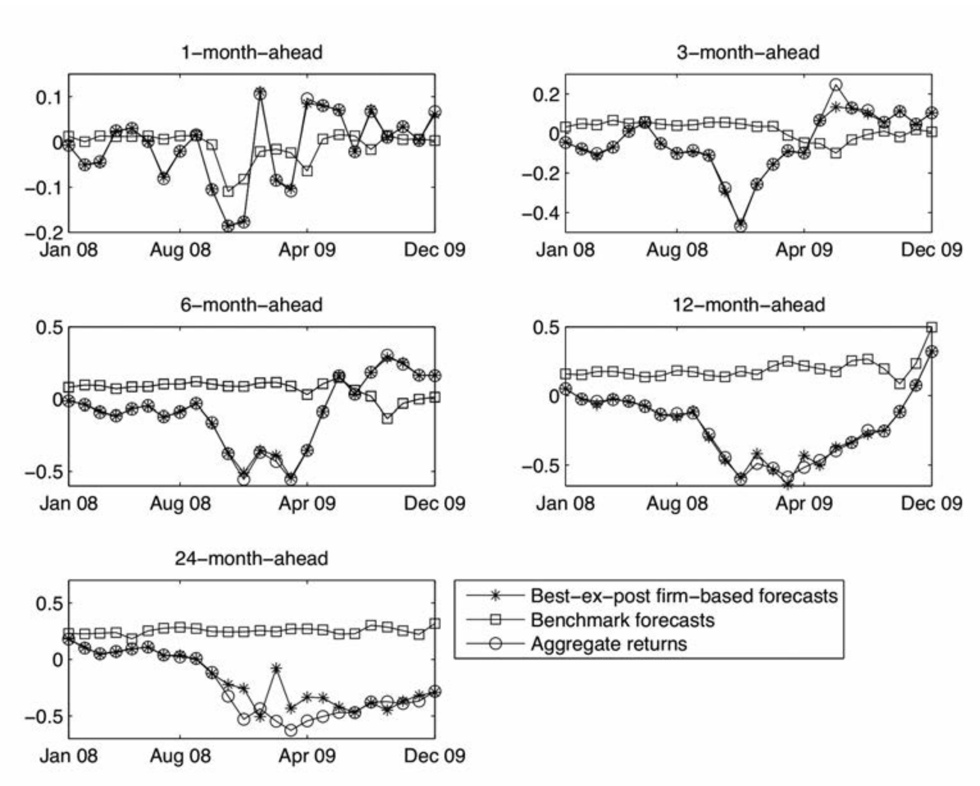


TABLE 1

NAMES OF THE TOP PERFORMING FIRMS FROM 1/2008 TO 12/2009, h=6 MONTHS

Dates	Names									
	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th
1/08	Strayer Education Inc.	ITT Corporation	Energen Corp.	ConocoPhillips	Fiserv, Inc.	John Wiley & Sons Inc.	Green Mountain Coffee Roasters, Inc.	Hess Corporation	Reliance Steel & Aluminum Co.	Questar Corporation
2/08	MarkWest Energy Partners, L.P.	FactSet Research Sy-stems Inc.	Leucadia National Corporation	Energy Transfer Partners, L.P.	FLIR Systems, Inc.	FTI Consulting, Inc.	Fossil Group, Inc.	Donaldson Company, Inc.	Washington Federal Inc.	Alliant Techsystems Inc.
3/08	Cliffs Natural Resources Inc.	Murphy Oil Corporation	Precision Castparts Corp.	Ecolab Inc.	Sempra Energy	Green Mountain Coffee Roasters, Inc.	Freeport-McMoRan Copper & Gold Inc.	Exelon Corporation	Harman Internatio-nal Industries, Inc.	Xtreme Oil & Gas, Inc.
4/08	Old Republic Inter-national Corp.	Ball Corporation	Tesoro Corporation	Stericycle, Inc.	Ansys, Inc.	Green Mountain Coffee Roasters, Inc.	Apple Inc.	Oshkosh Corporation	Itron, Inc.	Jacobs Engineering Group Inc.
5/08	Donaldson Company, Inc.	Roper Industries Inc.	Citigroup, Inc.	Raymond James Financial, Inc.	Essex Property Trust Inc.	Patterson Companies, Inc.	Ametek Inc.	Albemarle Corporation	Precision Castparts Corp.	BRE Properties Inc.
6/08	United States Steel Corp.	FLIR Systems, Inc.	Steel Dynamics Inc.	CR Bard Inc.	Cliffs Natural Resources Inc.	Perrigo Company	Archer Daniels Midland Company	Ansys, Inc.	FactSet Research Systems Inc.	Occidental Petro-leum Corporation
7/08	PPL Corporation	Questar Corporation	ITT Corporation	Airgas, Inc.	Freeport-McMoRan Copper & Gold Inc.	Itron, Inc.	Silver Wheaton Corp.	Sempra Energy	Valero Energy Corporation	Southern Copper Corp.
8/08	Jacobs Engineering Group Inc.	Southern Copper Corp.	Hess Corporation	Xtreme Oil & Gas, Inc.	Occidental Petro-leum Corporation	Walter Energy, Inc.	Stericycle, Inc.	Steel Dynamics Inc.	EQT Corporation	Southwestern Energy Co.
9/08	Kansas City Southern	HCP, Inc.	Federal Realty Investment Trust	FTI Consulting, Inc.	Steel Dynamics Inc.	EQT Corporation	Prologis, Inc.	Occidental Petro-leum Corporation	NHPC Ltd.	Public Service Enter-prise Group Inc.
10/08	Jacobs Engineering Group Inc.	Southwestern Energy Co.	Occidental Petro-leum Corporation	Flowserve Corp.	Energen Corp.	National Oilwell Varco, Inc.	Cabot Oil & Gas Corporation (COG)	Raymond James Financial, Inc.	Noble Energy, Inc.	ITT Corporation
11/08	Airgas, Inc.	Denbury Resources Inc.	Activision Blizzard, Inc.	Perrigo Company	Occidental Petro-leum Corporation	National Oilwell Varco, Inc.	Murphy Oil Corporation	Stericycle, Inc.	Comstock Resources Inc.	Loews Corporation
12/08	Cliffs Natural Resources Inc.	SM Energy Company	Oshkosh Corporation	Carbon Energy	Walter Energy, Inc.	The Mosaic Company	Cabot Oil & Gas Corporation (COG)	Denbury Resources Inc.	Stericycle, Inc.	FTI Consulting, Inc.
1/09	Flowserve Corp.	Kansas City Southern	United States Steel Corp.	Atwood Oceanics, Inc.	BOK Financial Corporation	National Fuel Gas Company	Forest Oil Corporation	Pioneer Natural Resources Co.	Helmerich & Payne, Inc.	Murphy Oil Corporation
2/09	Walter Energy, Inc.	Nordson Corporation	Comstock Resources Inc.	Cliffs Natural Resources Inc.	Carbon Energy	Bio-Rad Laboratories, Inc.	Flowserve Corp.	Cerro Grande Mi-ning corporation	United States Steel Corp.	Southwestern Energy Co.
3/09	Comstock Resources Inc.	United States Steel Corp.	AK Steel Holding Corporation	Weatherford Interna-tional Ltd.	MarkWest Energy Partners, L.P.	FMC Corp.	Carbon Energy	Morgan Stanley	SPX Corporation	EQT Corporation
4/09	AptarGroup, Inc.	Oceaneering Interna-tional, Inc.	Corporate Office Properties Trust	SM Energy Com-pany	Hormel Foods Corporation	Highwoods Properties Inc.	EQT Corporation	Questar Corporation	BancorpSouth, Inc.	Kinder Morgan En-ergy Partners, L.P.
5/09	Green Mountain Coffee Roasters, Inc.	Corporate Office Properties Trust	Iron Mountain Inc.	Northern Trust Corporation	Markel Corp.	BOK Financial Corporation	Covance Inc.	Realty Income Corp.	Alleghany Corporation	Citigroup, Inc.
6/09	Cerner Corporation	Rovi Corporation	UGI Corporation	DeVry, Inc.	American Electric Power Co., Inc.	PPL Corporation	Varian Medical Systems, Inc.	Microchip Technology Inc.	ACS Motion Control, Ltd.	St. Jude Medical Inc.
7/09	SAN LEON ENERGY	KLA-Tencor Corporation	Owens-Illinois, Inc.	American Express Company	Linear Technology Corporation	IDEXX Laboratories, Inc.	Westinghouse Air Brake Tech. Corp	FedEx Corporation	Iron Mountain Inc.	Pepsico, Inc.
8/09	Freeport-McMoRan Copper & Gold Inc.	Reliance Steel & Aluminum Co.	AK Steel Holding Corporation	Stericycle, Inc.	DENTSPLY Inter-national Inc.	Regions Financial Corporation	Great Plains Energy Incorporated	McDermott Interna-tional Inc.	Arch Coal Inc.	United States Steel Corp.
9/09	Torchmark Corpora-tion	United States Steel Corp.	Ashland Inc.	Carbon Energy	Citigroup, Inc.	TransCanada Corp.	Steel Dynamics Inc.	Bank of America Corporation	AGCO Corporation	Cliffs Natural Re-sources Inc.
10/09	DDR Corp.	Domtar Corporation	National Oilwell Varco, Inc.	Occidental Petro-leum Corporation	XL Group plc	Helix Energy Solu-tions	Steel Dynamics Inc.	Carbon Energy	AGCO Corporation	United States Steel Corp.
11/09	Helmerich & Payne, Inc.	United States Steel Corp.	Lincoln National Corporation	American Internatio-nal Group, Inc.	Carbon Energy	AGCO Corporation	Unit Corporation	Occidental Petro-leum Corporation	Steel Dynamics Inc.	National Oilwell Varco, Inc.
12/09	Icahn Enterprises,	Washington Federal Inc.	Prosperity Bancshares	Steel Dynamics Inc.	United States Steel Corp.	Oshkosh Corporation	Legg Mason Inc.	Stericycle, Inc.	Bank of America Corp.	NXT Energy Solu-tions, Inc.

The series of observed returns (circles) and that of the best firm-based forecasts (asterisks) are almost indistinguishable in the graph, especially at short forecast horizons. Indeed, the resulting root mean square errors are as follows: 0.009 for a forecast horizon of 1 month, 0.066 for 3 months, 0.040 for 6 months, 0.032 for 12 months and 0.061 for 24 months.

Finally, as a further way to visualize the underlying mechanism of the model, Table 1 reports the names of the top ten performing firms for the same subsample, 1/2008 to 12/2009 and for a 6-month forecast horizon.

5. - Forecasting Aggregate Returns in Real Time

This Section builds on the model and on the evidence described in Section 4 to forecast aggregate stock market returns in real time.

The previous Section suggests that firm-level data can improve forecasts of aggregate returns h periods ahead in a remarkable way. However, in real time, *i.e.* standing in period t and wanting to forecast $t+h$, choosing one single firm, or a subset of firms, to construct the h-month-ahead forecast is not straightforward. In fact, it constitutes the main difficulty of our approach. In this Section we propose a simple methodology for making this choice and we test its results at different forecasting horizons. Moreover, we build a simulation whereby we compare a buy-and-hold strategy on the stock market with an investment strategy based on the real-time forecasts we produce.

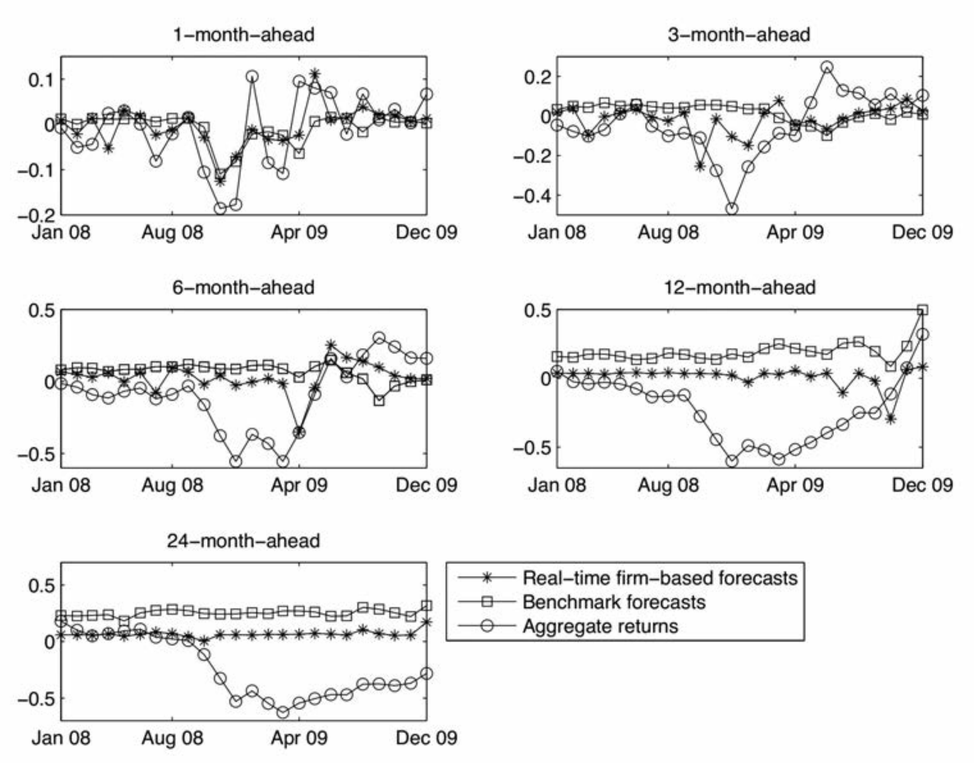
Standing in period t, we rank the many firm-based forecasts of aggregate returns in t according to the absolute value of their forecast error. Clearly, these forecasts were computed in period $t-h$. Next, we select the ten firms that boast the best performance, *i.e.* the minimum forecast error in absolute value, and we compute ten forecasts for aggregate returns in $t+h$ by putting those firms one at a time into model *(16)*. Finally, we compute a simple average to get the final forecast of period $t+h$.

The rather straightforward idea is that the forecasting performance in period t is a good indicator of the forecasting performance in period $t+h$. Ideally, for each firm the sample size would span at least $120+2h$ periods, *i.e.* the periods needed to perform one regression ($120+h$), to identify the top-performing firms and to compute the relevant forecast ($120+2h$). Since this is not the case for a few firms, we only do the above for firms with a sufficient sample size.

For each forecast horizon h equal to 1, 3, 6, 12 and 24 months, Graph 3 shows the series of firm-based forecasts, the benchmark forecasts based on aggregate variables only and the observed aggregate returns.

GRAPH 3

REAL-TIME FIRM-BASED FORECASTS, BENCHMARK FORECASTS AND AGGREGATE RETURNS



The longer the forecast horizon, the less accurate the forecasts, especially in some periods. This is due to the simple fact that when the forecast horizon gets bigger, there is a wider gap between the forecasting performance of firm *j* measured by the forecast error in time *t* and the forecasting performance *h* periods later. When *h*=24 months, the forecast error at time *t* for firm *j* contains little information about firm *j*'s performance 2 years later. For shorter forecast horizons, instead, the firm-based forecasts track aggregate returns reasonably well and better than the benchmark forecasts.

In order to have a quantitative measure of the validity of our forecasts we first look at four standard statistics. Table 2 shows the root mean square error and the mean absolute errors of the different forecasts, together with the correlation between forecasts and observed returns and the R^2 from a regression of the returns on the forecasts.

TABLE 2

RMSE'S, MAE'S, SIMPLE CORRELATIONS WITH OBSERVED RETURNS AND R^2 FROM A REGRESSION OF OBSERVED RETURNS ON THE FORECASTS

Forecast Horizon	Root Mean Square Errors		Mean Absolute Errors		Simple Correalations		R^2	
	Real-time firm-based forecasts	Benchmark forecast	Real-time firm-based forecasts	Benchmark forecast	Real-time firm-based forecasts	Benchmark forecast	Real-time firm-based forecasts	Benchmark forecast
1 month	0.038	0.045	0.168	0.184	0.518	0.160	0.268	0.026
3 months	0.082	0.094	0.242	0.255	0.248	-0.135	0.062	0.018
6 months	0.120	0.144	0.289	0.318	0.349	-0.129	0.122	0.017
12 months	0.192	0.234	0.389	0.410	0.099	-0.314	0.010	0.098
24 months	0.313	0.383	0.311	0.523	-0.038	-0.563	0.002	0.316

Both the RMSE and the MAE suggest that our model performs better than the benchmark for all forecast horizons. Looking at the firm-based forecasts, the correlation with observed returns is relatively high for 1, 3 and 6-month horizons, with a peak at h=1. For 12 and 24-month horizons, instead, it is very close to zero. The R^2 reveals a similar picture, as the variation in aggregate returns is best matched by the variation in firm-based forecasts for a 1-month forecast horizon. Overall, these four statistics provide good evidence of the quality of our forecasting model. However, we wish to consider two further ways of assessing its validity.

First, we test against the presence of so-called data snooping problem. Under data snooping, the superior predictability of a given model, compared to other available models, is driven by the randomness in the data, rather than by the validity of the model itself. The issue was first raised by White (2000) and further discussed by Hansen (2005), who proposes a test for "Superior Predictive Ability" (SPA). Hansen (2005)'s test allows to compare the predictive power of a given model against a finite number m of models to determine whether any of the alternatives has a grater forecasting power with respect to the benchmark. Let $L(Y_t, \hat{Y}_t)$ represent the loss from predicting $\hat{Y}_t$ when the true value is Y_t. The performance of model k at time t, relative to the benchmark model can be expressed as $d_{k,t} = L(Y_t, \hat{Y}_{t,k}) - L(Y_t, \hat{Y}_{t,0})$, $k = 1,\ldots,m$, $t = 1,\ldots,n$. We want to test the hypothesis that, on average, the benchmark model is better than any of the alternatives proposed. Therefore the null hypothesis is $H_0{:}E(d_{k,t}) \leq 0$ for all $k = 1,\ldots,m$. Hansen shows that the proper statistic to be used is $T_n^{SPA} = \max_k n^{1/2} \bar{d}_k \hat{w}_k^{-1}$, where $\bar{d}_k = n^{-1} \sum_{t=1}^{n} d_{k,}$ and where $\hat{w}_k^2 = \hat{\mathrm{var}}(n^{1/2} \bar{d}_k)$ is a consistent estimator of the asymptotic variance $w_k^2 = \lim_{n\to\infty} \mathrm{var}(n^{1/2}\bar{d}_k)$. The distribution of the test statistic under the null is the most important problem, as testing multiple inequalities implies that the distribution is not unique under the null. Within this framework, Hansen (2005) provides a bootstrapped derivation of the distribution T_n^{SPA} of under the null, together with consistent *p*-values.

For the purposes of our paper, we wish to compare only two models, the real-time firm-based one and the benchmark. We derive our results by running the test using the mean absolute error as a loss function.

The *p*-values obtained by testing the null hypothesis that the predicting power of the benchmark is greater are as follows: 0.000 for a forecast horizon of 1 month, 0.082 for a forecast horizon of 3 months, 0.003 for 6 months, 0.000 for 12 months and 0.001 for 24 months.

As a final way of testing the validity of our model, we use the real-time firm-based forecasts to set up a simulation of portfolio management in real time.

Our investment strategy is as follows. Standing in month *t*, if the real-time firm-based forecast for month *t+h* predicts stock market returns above those of an *h*-month Treasury Bill, then we invest all our money in the stock market. Conversely, if the return of an *h*-month Treasury Bill is above the one our model predicts for the stock market, then we invest all our money in government bonds. Every *h* months we repeat the procedure until the end of our investment horizon.

As a benchmark, we consider a buy-and-hold strategy on the stock market. This strategy implies investing in the stock market at a given date and keeping the money there until the end of the investment horizon. We choose it as a benchmark to show whether it would be possible to beat the market using our forecasting model in real time. The efficient market hypothesis clearly states that if stock markets are efficient, then they will incorporate all the publicly available information and it shouldn't be possible to use a piece of such information to systematically beat the market. As a further check, we also look at the results of a buy-and-hold strategy on the bond market.

For *h*=1 month, the series of real-time firm-based forecasts starts in 7/1987 and ends in 12/2009. However, the US government has issued Treasury bills with a 1-month maturity only from 7/2001. Therefore, given this limitation in sample size, we consider a maximum investment horizon of 96 months and a minimum of 60 months, or 5 years. For *h*=3 months, we consider a maximum investment horizon of 87 quarters, 43 semesters for *h*=6 months, 20 years for *h*=12 months, 18 years for *h*=24 months.

Of course, within each of these investment horizons different starting dates are possible. Considering again *h*=1 month, when planning to invest for 92 months, the starting date can be any between 6/1987 (*i.e.* one month ahead of the first forecast) and 10/1987. However, when planning to invest for 60 months, the starting dates can go from 6/1987 to 12/1999. Given each forecast horizon and each investment horizon considered, we test our portfolio strategy against a buy-and-hold benchmark for all the possible starting dates.

Table 3 shows average cumulative returns and Sharpe *ratios* of different investment strategies for varying forecast and investment horizons. Sharpe *ratios* are risk adjusted excess returns relative to a buy-and-hold bonds strategy. In order to save space, we include in the table only a subsample of the possible investment horizons, taken at precise intervals.

Looking at *h*=1 month, the average returns of a model-based strategy are higher than those of a buy-and-hold strategy for all the investment horizon considered. When *h*=6 months this is true for investment horizons greater than 18 months.

Moreover, for 1 and 6-month forecast horizons, the average Sharpe *ratio* of a model-based strategy is always higher than the benchmark. In other words, the risk-adjusted excess return of a model-based strategy is higher, regardless of the investment horizon.

Looking at *h*=3 months, a model-based strategy cannot beat the market in terms of average returns. However, the Sharpe *ratio* of the former is higher provided that the investment horizon is at least 70 quarters.

As for longer forecast horizons, *i.e.* 12 and 24 months, a model-based strategy is clearly outperformed buy a buy-and-hold strategy, both in terms of average returns and in terms of risk-adjusted excess return.

The general picture reveals that our model delivers a good performance when the forecast horizon is relatively short, especially at 1 and 6 months. However, as the horizon increases, the model-based forecasts become less accurate.

Graph 4 shows the cumulative returns from different strategies at selected investment horizons as a function of the starting date. For each forecast horizon, the investment horizons considered in the figure are the shortest and the longest among those presented in Table 3.

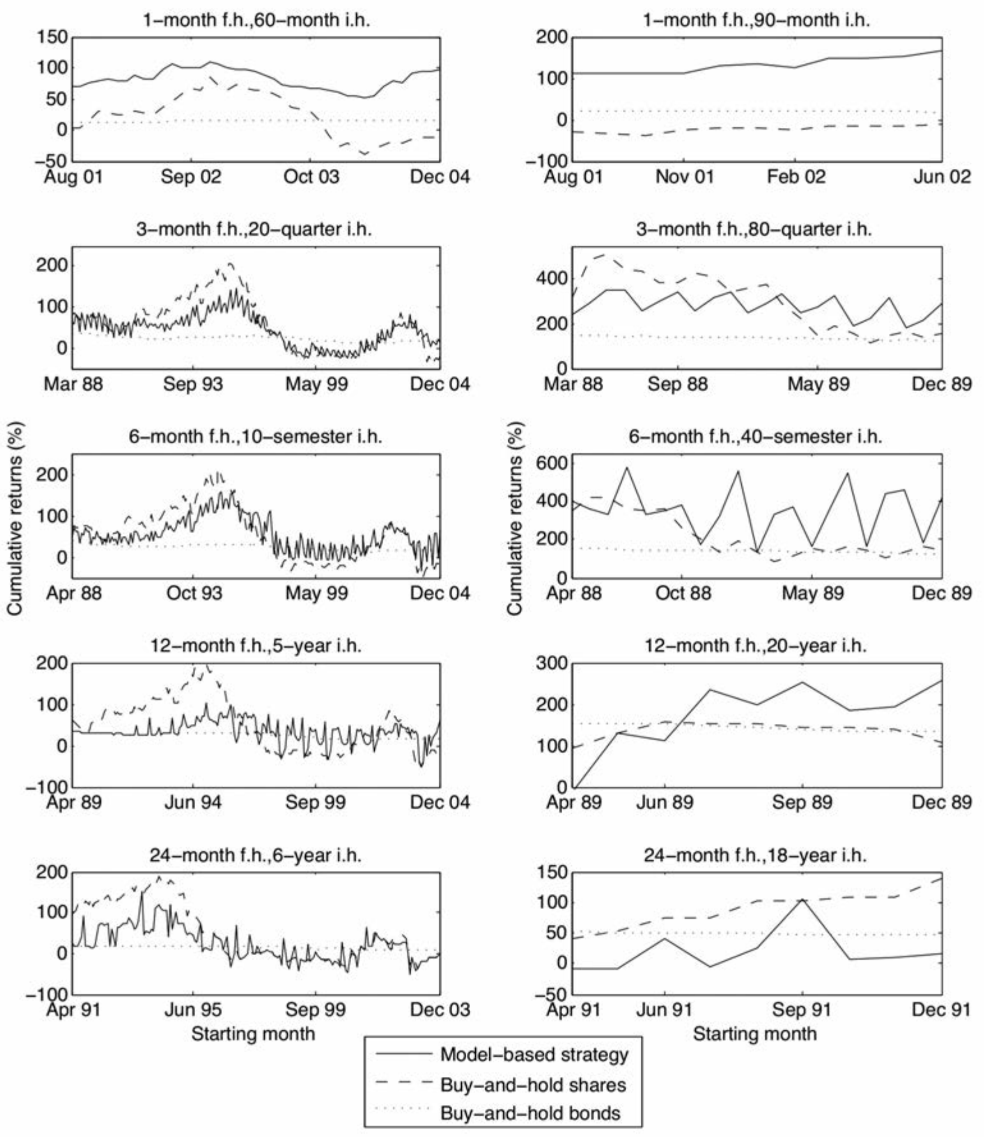
TABLE 3

AVERAGE CUMULATIVE RETURNS FROM BUY-AND-HOLD STRATEGIES AND FROM A MODEL-BASED STRATEGY

Forecast Horizon	Investment Horizon	Buy-and-Hold Bonds	Buy-and-Hold Shares		Model-Based Strategy		# of Possible Starting Dates
		Average cumulative returns	Average cumulative returns	Average Sharpe *ratio*	Average cumulative returns	Average Sharpe *ratio*	
1 month	60 months	14.27 %	22.37 %	0.22	81.80 %	4.14	41
	65 months	15.35 %	21.28 %	0.18	90.07 %	5.94	36
	70 months	16.32 %	17.77 %	0.05	99.44 %	13.05	31
	75 months	17.16 %	11.52 %	-0.34	109.07 %	8.24	26
	80 months	17.84 %	2.40 %	-0.82	116.68 %	4.89	21
	85 months	18.43 %	-11.08 %	-1.69	123.80 %	4.36	16
	90 months	18.93 %	-22.87 %	-4.50	130.70 %	5.71	11
3 months	20 quarters	22.89 %	58.17 %	0.58	44.08 %	0.62	202
	30 quarters	36.75 %	96.62 %	0.71	68.93 %	0.70	172
	40 quarters	52.84 %	154.49 %	0.93	109.27 %	0.89	142
	50 quarters	69.75 %	188.94 %	1.22	138.51 %	1.17	112
	60 quarters	85.67 %	209.41 %	2.10	161.82 %	2.05	82
	70 quarters	105.95 %	280.91 %	2.02	222.24 %	2.80	52
	80 quarters	136.31 %	299.97 %	1.32	281.27 %	2.96	22
6 months	10 semesters	23.39 %	54.71 %	0.51	53.72 %	0.83	201
	15 semesters	37.57 %	90.61 %	0.63	89.34 %	1.15	171
	20 semesters	54.06 %	139.50 %	0.77	143.65 %	1.49	141
	25 semesters	71.36 %	161.81 %	1.10	190.37 %	2.03	111
	30 semesters	87.84 %	195.28 %	1.61	238.07 %	2.13	81
	35 semesters	109.04 %	250.08 %	1.42	308.34 %	2.21	51
	40 semesters	139.18 %	214.80 %	0.71	349.88 %	1.62	21
12 months	5 years	23.85 %	50.11 %	0.44	32.14 %	0.34	189
	8 years	41.87 %	87.42 %	0.52	57.78 %	0.45	153
	11 years	62.35 %	118.80 %	0.67	91.07 %	0.61	117
	14 years	82.70 %	150.77 %	1.21	117.00 %	0.56	81
	17 years	107.28 %	200.25 %	1.01	148.37 %	0.56	45
	20 years	143.19 %	134.58 %	-0.37	172.36 %	0.32	9
24 months	6 years	14.55 %	50.05 %	0.52	22.09 %	0.19	153
	8 years	20.18 %	56.64 %	0.47	25.05 %	0.11	129
	10 years	25.90 %	58.99 %	0.62	26.50 %	0.01	105
	12 years	31.30 %	87.05 %	1.07	38.87 %	0.18	81
	14 years	36.88 %	118.46 %	1.36	48.89 %	0.24	57
	16 years	42.99 %	127.98 %	1.03	49.16 %	0.10	33
	18 years	49.45 %	89.93 %	1.21	20.15 %	-0.79	9

GRAPH 4

CUMULATIVE RETURNS FROM DIFFERENT INVESTMENT STRATEGIES AT VARYING FORECAST HORIZONS (F.H.) AND AT SELECTED INVESTMENT HORIZONS (I.H.), AS A FUNCTION OF THE STARTING MONTH OF THE INVESTMENT



For all the forecast horizons, the first fact that hits the eye is that returns vary considerably depending on the starting date. As a general pattern, model-based cumulative returns are more volatile than those coming from a buy-and-hold strategy for shares. However, the higher model-based Sharpe *ratios* suggest that this volatility is more than compensated by the higher excess return of a model-based strategy.

Furthermore, looking at short investment horizons, buy-and-hold returns are higher for earlier starting dates and lower for later ones. This might suggest that in times of limited volatility, *i.e.* before the years 2000's in our sample, a buy-and-hold strategy beats a model-based one. The opposite seems to be true when volatility rises, as evidenced by cumulative returns at later starting dates. This, in turn, suggests that the real-time predictability of the excess returns of the stock market is higher in periods of high volatility and *vice versa*, consistently with the similar finding of Pesaran and Timmermann (1995).

Moreover, a related feature of model-based cumulative returns is that they are stationary. When the investment horizon is longer, the risk associated with a buy-and-hold strategy for shares increases because there is uncertainty over future volatilities. Therefore, as reflected by the increasingly higher difference between the Sharpe *ratios* of the two strategies, the longer the forecast horizon, the more convenient it is for a risk-averse investor to rely on a model-based strategy.

Finally, we discuss possible alternatives to our model and the rationale behind some our choices.

First, we compute an average of the top ten forecasts because just picking the best one would yield much more volatile forecasts, which cannot beat the benchmark of a model based on aggregate information alone. In particular, research (see Hansen, 2008 for a quick review) has shown that a simple average of forecasts can hardly be outperformed by more elaborated models based on Bayesian model averaging (see Hoeting *et* al., 1999 for a review) or on forecast combination *à la* Granger (1989). A more recent advance in the field is the one of Hansen (2008) who proposes a "least square forecast averaging". However, this procedure is only applicable to nested models, thus resulting unfit for the specification pursued by this paper.

Second, we could have taken an average of a different number of forecasts, instead of considering the top ten. We tried to take averages of the first 5, 15, 20 and 30 forecasts and feed the results to the Hansen (2005)'s Superior Predictive ability Test. When confronted with the benchamark of a model based on aggregate information alone, none of the models based on the alternative averages can out-perform a model based on the average of the top ten forecasts. The *p*-value of the latter is lower than any of the alternatives for all the forecast horizons considered.

Third, instead of just looking at the forecast error in *t*, we could have looked at a wider measure of the recent performance. Following di Mauro, Fornari and Mannucci (2011), in each period *t* we compute a ranking of the firms that boast the best Root Mean Square Error (RMSE) calculated using the forecast errors dated from *t*-5 to *t* obtained using a particular firm in model *(16)*. Next, we compute *m* forecasts for *t*+*h* using the top *m* firms and we average them out. We consider the following values for *m*: 1, 5, 10, 15, 20, 30. Once again, when compared to a benchmark based on aggregate information using the Superior Predictive Ability test, none of the possible values of *m* yields a lower *p*-value than a model based on selecting firms by their forecast errors and based on averages of the top ten firms. The only exception is for an investment horizon of 3 months, where a top-10, RMSE-based model beats a top-10, forecast-error-based model.

6. - Conclusion

Using US data, this paper shows that firm-level data matter for forecasting US aggregate stock market returns at several forecast horizons. In particular, one can use our real-time firm-based forecasts of aggregate returns to set up an investment strategy that beats a buy-and-hold strategy on the stock market, either in terms of average cumulative returns or in terms of risk-adjusted excess returns or in both, depending on the forecast horizon.

These findings are evidence of "granular effects" in forecasting stock market returns. The movements in the equity prices of a limited number of firms help forecasting much of the future movements of aggregate returns, even controlling for other aggregate variables.

Beyond the scope of this basic result, there is room for further research. One route could explore the potentially different relevance of individual returns as opposed to individual variances for forecasting. Indeed, the model we propose makes use of both.

A somewhat more demanding issue is that of fine-tuning an algorithm for choosing one firm in real time to forecast aggregate returns *h* periods later. We propose a simple mechanism based on the most recent forecasting performance of firms. The resulting forecasts are a good match for US data when the forecast horizon is relatively short but results become less compelling for other forecast horizons. Making the real-time choice of a given firm dependent on a more complex evaluation of its forecasting performance may prove fruitful.

In conclusion, this paper uncovers the great potential of the granular hypothesis for forecasting aggregate stock market returns and it proposes a valid model for making forecasts in real time. Further research along this line promises to be a very good investment.

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Profile of the 2013 Winners

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RAMONA DAGOSTINO (*rdagostino@london.edu*) holds a M.Sc. in Economic and Social Sciences and a B.Sc. in International Economics and Management from "L. Bocconi" University of Milan, both *summa cum laude*. While an undergraduate, she spent a semester at Wharton, UPenn. She is currently a 2^{nd} year Ph.D. student in Finance at London Business School (LBS). As an undergraduate, she was awarded the title of Valedictorian, being the youngest *cum laude* graduate of the graduating class of 2009. She is also the recipient of the Merit Award Scholarship for academic excellence, the ESRC Doctoral Scholarship, and the LBS Financial Award. Before starting her Ph.D., Ramona spent one year in Brussels working as a Research Assistant for professor Estelle Cantillon at ECARES, Université Libre de Bruxelles. Ramona's research interests are in the field of international macro-finance.

PAOLO GIACOMINO (*paolo.giacomino@gmail.com*), graduated *summa cum laude* in Finance and Banking in 2012 at "Tor Vergata" University of Rome, after completing *cum laude* in 2010 a first level degree in "Financial Markets and Institutions" at the same University. In 2013, he was awarded a Master in Economics at Luiss "G. Carli" University of Rome.
He is currently engaged as a consultant and researcher in Economics, focusing on the analysis of macroeconomic scenarios and quantitative analysis and assessment of commodities price dynamics.

LAVINIA PIEMONTESE (*lavinia.piemontese@gmail.com*) holds – *summa cum laude* – a B.A. in Law and Economics and a M.Sc. in Economics from the University of Bologna. After the graduation in July 2013, she moved to Spain and enrolled at Barcelona Graduate School of Economics, where she successfully completed the M.Sc. in Economics of Public Policy in July 2013.
She is currently enrolled in her first year of the Ph.D. program in Economic Analysis at Universitad Autonoma de Barcelona. During this year, she was involved in intense coursework and a job experience as Research Assistant of Professor Hannes Muller at Institut d'Analisi Economica (at Universitad Autonoma Barcelona). Her research interests lie in the fields of Economics of Education and Economics of Crime. She would like to focus her research on the recidivism of young criminals. In particular, she wants to investigate the impact that education and job-training programs have on the likelihood of committing new crimes, through an empirical approach.

Stefano Schiaffi (*stefano.schiaffi@gmail.com*) holds a B.Sc. and a M.Sc. *summa cum laude* in Economics and Social Sciences from "L. Bocconi" University, where he graduated under the supervision of professor Carlo Ambrogio Favero. He also holds a M.Sc. in Economics from the Barcelona Graduate School of Economics and, during his undergraduate studies, he spent a semester at the University of California, Berkeley, where he took courses in behavioural economics and finance. He is currently enrolled in the Ph.D. in Economics and Finance at "L. Bocconi" University. His research interests lie in the fields of macroeconomics and finance, with specific reference to the role of psychological factors in explaining and forecasting macro-financial aggregates. During his M.Sc. at Bocconi, he worked as a trainee for the European Central Bank, where he co-authored a paper on short-term forecasting of real GDP using confidence indicators.

Professional Growth of «Angelo Costa» Winners

The Winners of the XVth Edition (2012)

Barbara Biasi (*bbiasi@stanford.edu*) holds a B.Sc. in Management and Economics and a M.Sc. in Economic and Social Sciences, *summa cum laude*, from "L. Bocconi" University of Milan. She is currently a second-year Ph.D. student in Economics at Stanford University. Her research interests lie in the fields of labour and public economics, and her current projects are related to the dynamics of returns to education, the determinants of intergenerational mobility and the diffusion of innovation. She is currently working as a research assistant to professor Petra Moser. In the past, she spent 3 months working as a researcher at IGIER Bocconi under the supervision of Prof. Eliana La Ferrara and one year at *Fondazione Rodolfo de Benedetti* under the supervision of professors Tito Boeri and Michele Pellizzari.

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Giuliano Pirrone (*giuliano.pirrone@gmail.com*) graduated from the University of Palermo in "Economics and Finance" in November 2008 and specialized *summa cum laude* from the same University in "Economic and Financial Sciences" in March 2011. He is currently engaged in deepening issues of industrial territory in order to provide valuable support to local productive activities. Aspiring freelancer, business consultant and advisor for Sicilian economic realities.

ISOLINA ROSSI (*isolinarossi@gmail.com*) After graduation she worked as a Research Analyst at Italia Futura, a leading Italian *think tank* devoted to the promotion of liberal and progressive policy making. She is currently enrolled in a Master in Public Administration (MPA) at Columbia University, specializing in Economic Policy Management. Her interests include innovation and public policies. At Columbia University she is a member of Woman in Leadership, a student network aiming to build a lifelong resource center for professional and personal development for existing woman leaders.

The Winners of the XIVth Edition (2010/2011)

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MARCO NIEDDU (*mg_nieddu@hotmail.com*) attended a M.Sc. in Economics at the University of Essex. His interests, apart from social capital, deal with time use and adult care. His M.Sc. thesis, done under the supervision of Prof. Alison Booth, refers to the role of providing care to elderly relatives in affecting spouses' job supply. Currently Marco is a Ph.D. student in Economics at the University of Cagliari, and he started working on this topic and on time allocation in general.

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The Winners of the XIIIth Edition (2009)

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The Winners of the XIIth Edition (2008)

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The Winners of the XIth Edition (2007)

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MURRO P., «The Determinants of Innovation: What is the Role of Risk?», *The Manchester School*, no. 81(3), 2013; BARTOLI F., FERRI G., MURRO P., ROTONDI Z., «SME Financing and the Choice of Lending Technology in Italy: Complementarity or Substitutability?», *Journal of Banking and Finance,* no. 37(12), 2013;

BARTOLI F., FERRI G., MURRO P., ROTONDI Z., «Bank-Firm Relations and the Role of Mutual Guarantee Institutions at the Peak of the Crisis», *Journal of Financial Stability,* no. 9, 2012.

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The Winners of the Xth Edition (2006)

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The Winners of the IX^th^ Edition (2005)

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Recent Publications:
VIANI F., SÁ F., «Shifts in Portfolio Preferences of International Investors: An Application to Sovereign Wealth Funds», *Review of International Economics*, 2013; VIANI F., CORSETTI G., DEDOLA L., «The International Risk Sharing Puzzle is at Business Cycle and Lower Frequency», *Canadian Journal of Economics*, 2012; VIANI F., CORSETTI G., DEDOLA L., «Traded and Nontraded Goods Prices, and International Risk Sharing: An Empirical Investigation», *NBER Chapters,* in *NBER International Seminar on Macroeconomics 2011*, 2011.

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Recent Publications:
MORRESI O., « Gli effetti della crisi finanziaria del 2007 sulle imprese italiane», *Rivista di Studi Politici,* no. 25(2), 2013, pages 11-62; MORRESI O., «Debt and Expropriation: The Role of the Ultimate Largest Shareholder», *International Review of Applied Financial Issues and Economics,* no. 4(4), 2012.

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Recent Publications:
MELINDI GHIDI P., «A Model of Ideological Transmission with Endogenous Parental Preferences», *International Journal of Economic Theory*, no. 8(4), 2012, pages 381-403; MELINDI GHIDI P., CHABE-FERRET, B., «Fertility Differentials and Uncertainty: An Economic Theory of the Minority Status Hypothesis», *Journal of Population Economics*, no. 26(3), 2013, pages 887-905.

The Winners of the VIIIth Edition (2004)

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The Winners of the VII^th Edition (2003)

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The Winners of the VIth Edition (2002)

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The Winners of the V^th^ Edition (2001)

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Recent Publications:
SCHIAVO S., BEE M., RICCABONI M., «The Size Distribution of US Cities: Not Pareto, Even in the Tail», *Economics Letters*, no. 120(2), pages 232-237; SCHIAVO S., CHINAZZI M., FAGIOLO G., REYES J., «Post-Mortem Examination of the International Financial Network», *Journal of Economic Dynamics and Contro*l, no. 37(8), 2013, pages 1692-1713; SCHIAVO S., RICCABONI M., ROSSI A., «Global Networks of Trade and Bits», *Journal of Economic Interaction and Coordination*, no. 8(1), 2013, pages 33-56.

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Daniela Iorio (*daniela.iorio@uab.cat*) is assistant professor of economics at the Universitat Autonoma de Barcelona, which she joined in the fall of 2007. Currently she is working on topics related to political economy, labor and health economics.

The Winners of the IVth Edition (2000)

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Recent Publications:
Argenziano R., Gilboa I., «History as a Coordination Device», *Theory and Decision*, 2012, no. 73(4), pages 501-512; Argenziano R., Schmidt-Dengler P., «Inefficient Entry Order in Preemption Games», *Journal of Mathematical Economics*, 2012, no. 1(1), pages 17-52; Argenziano R., Schmidt-Dengler P., «Competition, Timing of Entry and Welfare in a Preemption Game», *Economics Letters*, 2013, no. 120, pages 509-512.

Riccardo Bonci (*riccardo.bonci@ecb.int*) After spending three years at the Regional Economic Research Department at the Perugia branch of Banca d'Italia, Riccardo is now senior economist at the European Central Bank. As part of the Monetary Policy stance Division, his tasks and responsibilities cover the analysis of monetary and credit developments in the euro area and the forecast of financial aggregates. His studies are currently focused on private sector debt, the link between deleveraging and economic activity and on sectoral imbalances, especially from a flow-of-funds perspective.

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The Winners of the III^rd Edition (1999)

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Recent Publications:
Valente G., Thornton D.L., «Out-of-Sample Predictions of Bond Excess Returns and Forward Rates: An Asset Allocation Perspective», *Review of Financial Studies,* no. 25, 2012; Valente G., Nucera F., «Carry Trades and the Performance of Currency Hedge Funds», *Journal of International Money and Finance*, no. 33, 2013.

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Recent publications:
FLABBI L., CHECCHI D., «Intergenerational Mobility and Schooling Decisions in Germany and Italy», *Rivista di Politica Economica*, no. VII-IX, 2013; FLABBI L., MORO A., «The Effect of Job Flexibility on Women Labor Market Outcomes: Estimates from a Search and Bargaining Model», *Journal of Econometrics*, no. 168, 2012, pages 81-95.

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MICHELE TROVA (*michele.trova@venetobanca.it*) consolidated his activity in the field of pricing of structured financial products, collateral management and the parameterization of the front office systems currently in use by the Financial Department of Veneto Banca S.C.p.A., where he acted as Head of the Analysis & Financial Controlling unit. Starting from December 2012 he has also been appointed as Head of the Quantitative Financial Advisory unit for Veneto Banca S.C.p.A. He continued his research activity at the University of Venice where he was confirmed instructor for the course of Investments at the IMEF Master.

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The papers submitted for publication in the Rivista di Politica Economica are evaluated by anonymous referees who do not know the identity of the authors. The role of these experts is fundamental to ensure the quality of the papers that will then be published in the journal.
Our particular thanks go to all the referees who – in a cooperative spirit – helped us selecting the works submitted to the editorial office of the Rivista di Politica Economica in the year 2012.

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