BIS CCA-001-2010
May 2010

A new look into credit procyclicality: International panel evidence

A paper prepared for the BIS CCA Conference on

"Systemic risk, bank behaviour and regulation over the business cycle"

Buenos Aires, 18–19 March 2010

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A New Look into Credit Procyclicality: International Panel Evidence (*)

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The goal of this paper is to provide up-to-date worldwide evidence on the short-term relationship between credit changes and output changes. Standard correlation methods, state-of-the-art panel Granger causality tests, and panel regressions were applied on a maximum sample of 144 countries over the period 1990-2007. Our results openly clash with two popular economic statements, namely, that credit is procyclical and that changes in credit have strong effects on private expenditure. According to the evidence produced, credit procyclicality -in the sense that the simple correlation coefficient is positive and significant at 10% or less- prevails in just 45% of the countries when annual data are used (23% with quarterly data). As for time precedence, our work suggests, for the full sample and a number of random subsamples, that Granger causality runs from GDP to credit in an overwhelming majority of cases, while the often claimed causality from credit to GDP is a feature observable much less frequently. In turn, panel regressions reveal a much stronger effect on GDP growth on credit growth than the other way around. Furthermore, after testing for endogeneity, we contend that our results uncover not just mere Granger causality but economic causality. As an adittional issue, we explore the linkage of bank capital with economic activity and credit, without finding, for a sample of 16 OECD countries, any significant relationship. All in all, these findings have vast academic and policy implications.

This version: February 18, 2010

(*) The paper has benefited from the sharp comments of Horacio Aguirre, Eduardo Corso, Arturo Galindo, Federico Gandelman and Marcelo Oviedo on a previous version. The usual disclaimer applies. This work does not necessarily reflect the opinion of the Central Bank or its authorities.

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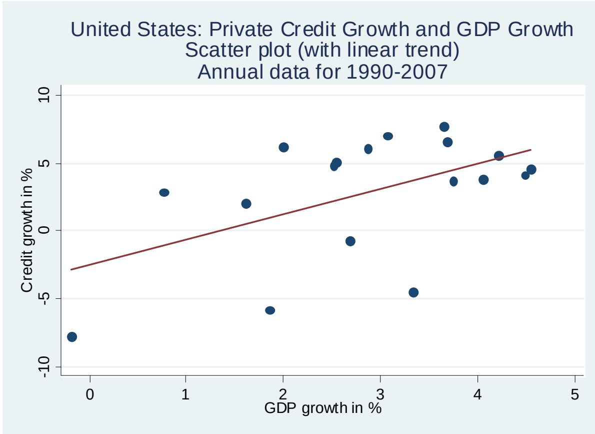
1 Introduction

There seems to be a broad consensus in both academic and policy circles that financial systems are procyclical. For instance, in the context of the Basel II debate, critics have pointed out that procyclical capital requirements would trigger an automatic reduction in bank credit in bad times with deleterious consequences on investment and consumption. As put by Taylor and Goodhart (2004), *"…if risk-sensitive regulation requires banks to hold a higher capital ratio during economic downswings, reflecting the increased potential credit losses in their portfolios, then they may respond by reducing their loan book, o by passing on the funding costs of raising capital. The resulting rationing of credit, or its higher cost, may lead to real effects through reduced investment and consumption"*. In turn, Rochet (2008) claims that *"The subprime crisis is a perfect illustration of the "procyclicality" of financial systems....Financial history abounds with examples of such financial cycles, with an alternation of credit booms fuelled by "exuberant" optimism during growth phases, followed by dramatic episodes of credit "crunches" triggered by relatively moderate negative shocks but ultimately generating major downturns in economic activity"*

Despite its popularity, our sense is that a categorical verdict on this matter has not been passed so far. At the end of the day, we wonder whether the correlation and causality between credit growth and output growth is an empirical regularity around the globe. Just to motivate our discussion, we next display scatter plots of annual GDP growth-private loan growth pairs over 1990-2007 for four countries: United States, Canada, Chile, and Uruguay. Under the traditional hypothesis, reflected in the opening statements, we would expect the points to be aligned around a line with a positive and rather large slope. This is not what we see in these countries (and many others, as well). Actually, we observe a positive trend in the US and Chile, yet with a very poor goodness-of-fit and quite different slopes. In contrast, the correlation in Canada and Uruguay is outright negative.


United States: Private Credit Growth and GDP Growth
Scatter plot (with linear trend)
Annual data for 1990-2007
Credit growth in %
GDP growth in %
-10
-5
0
5
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0
1
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4
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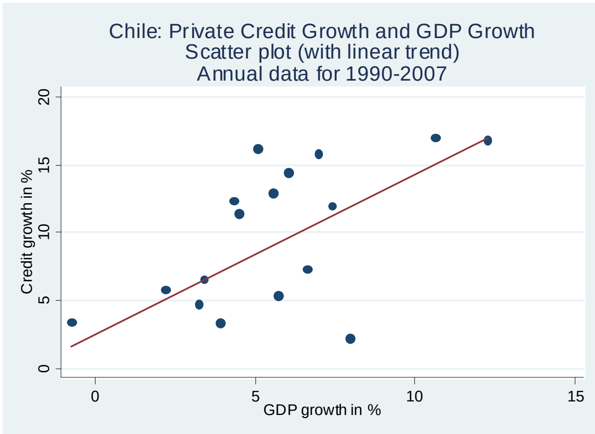
Chile: Private Credit Growth and GDP Growth
Scatter plot (with linear trend)
Annual data for 1990-2007
Credit growth in %
GDP growth in %
0
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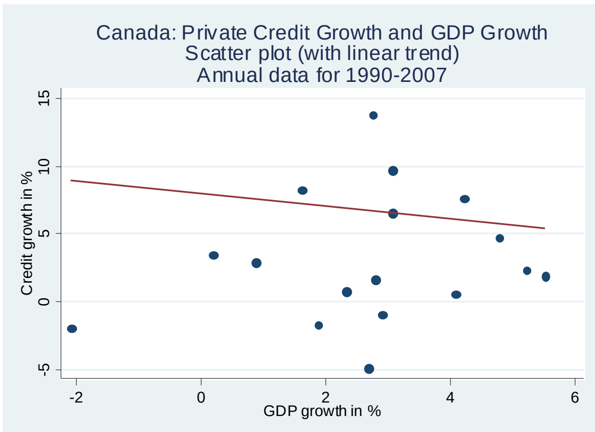
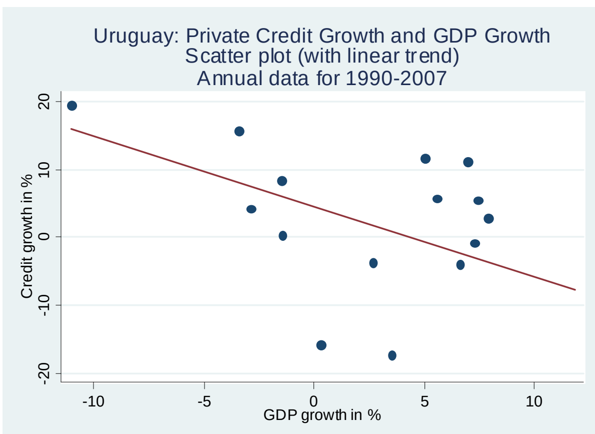

Canada: Private Credit Growth and GDP Growth
Scatter plot (with linear trend)
Annual data for 1990-2007
Credit growth in %
GDP growth in %
-5
0
5
10
15
-2
0
2
4
6


Uruguay: Private Credit Growth and GDP Growth
Scatter plot (with linear trend)
Annual data for 1990-2007
Credit growth in %
GDP growth in %
-20
-10
0
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20
-10
-5
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Our main goal is to produce international evidence on the short-term relationship between credit changes and output changes. Operationally, we will distinguish two concepts: procyclicality (whether credit and output growth, both at time *t*, move synchronically) and Granger causality (whether past credit growth affects current values of output growth, and viceversa). As a secondary topic, we will extend the analysis to the nexus between bank capital, output and credit changes. We will tackle these empirical questions by means of standard econometric measures and by applying state-of-the-art panel Granger causality tests designed to unveil common and idiosyncratic behavior across countries. To this end, we will use techniques developed by Im, Pesaran and Shin (IPS) (2003) for panel unit roots and later adapted to a Granger causality framework by Hurlin (2008).[1] Moreover, we will discuss economic causality via panel regressions. Our unbalanced panel sample will cover, at maximum, a broad sample of 144 countries over 1990-2007. Different robustness checks will be performed to reassure the validity of our results.

Beyond its academic interest, our research issues have far-reaching policy implications on some heatly debated areas. For one, it may provide additional evidence on the potential real effects of procyclical capital regulations, as those embedded in Basel II capital requirements. Secondly, and somehow related to the previous point, the analysis may offer a fresh perspective on the channels of transmission from the banking system to the macroeconomy in both tranquil and crisis times.

Anticipating our results, we find that, contrary to common belief, (1) Credit is not procyclical in a majority of countries, and (2) GDP growth Granger-causes credit growth in most cases (even after random resampling), while the reverse causality direction is much more infrequent. Additionally, we empirically reject any linkage of bank capital with GDP growth and credit. Thus, our main findings pose doubts on the widely accepted conception underlying a strong and deleterious effect of credit movements on business cycles.

The rest of the paper is structured as follows. In Section 2, we advance a brief review of the literature. Section 3 describes the data and the econometric strategy. Section 4 presents and discusses the results. The association between Granger and economic causality takes place in Section 5. We close with some conclusions.

[1] As part of our research, and given that the test is not available in any econometric package, we implement this new Granger test in Stata. The resulting do files are available upon request.

2 Literature Review

The following paragraphs summarize previous theoretical and applied work on the link between business cycles, bank capital and credit. This succinct review builds our skepticism and disinclination to accept at face value the notion that changes in loans, caused by changes in bank capital or other factors, precede changes in economic activity.

Regardless of its widespread acceptance, the relationship between business cycles and credit is still controversial from a theoretical standpoint regarding both the *direction of causality* and then the *sign* of such correlation. Concerning the direction of causality, common wisdom favors a lending-to-growth view, based on a simple flow-of-funds argument: financially constrained units will be able to spend more as more credit is granted. Nevertheless, it is equally sensible that economic conditions determine credit availability, driven by either demand or supply factors. In the first case, a growing economy stimulates a stronger demand for investment and consumption, which encourages more credit applications to finance the additional expenditure. Supply-side forces, on the other hand, may be at play whenever booming cycles lead to asset price inflation and stronger borrower balance sheets, which in turn boost bank lending through the so-called financial accelerator (see Bernanke and Gertler (1989) and Kiyotaki and Moore (1997)).[2]

As for the sign of the correlation between credit and economic activity, one can envisage that the positive sign assumed in the previous accounts is not the only possible outcome - credit may or may not be procyclical on theoretical grounds. For instance, the desire to smooth consumption over time would lead households to apply for credit in (temporarily) bad times, giving rise to a countercyclical credit pattern. Likewise, the corporate pecking order theory (Myers and Majluf (1984)) suggests that firms choose to shrink their leverage in economic upswings, as larger profits allow them to rely on their own funds – in the presence of adverse selection and intermediation costs, internal funds are less expensive than external sources.

Recent empirical work around procyclicality tends to find support for a positive co-movement between credit and economic activity. These studies, however, are not fully

[2] In practice, though, it is cumbersome to disentangle supply and demand effects on credit volumes.

comparable to each other because they differ in (i) the variables used to quantify financial system procyclicality, (ii) the country and time coverage, (iii) the econometric approach, and (iv) even more importantly, in which variable is on the right- and which on the left-hand side. Bikker and Hu (2002) focus on the relationship between the change in loans to total bank assets and GDP growth in assessing procyclicality in multivariate panel regressions for 26 industrial countries. Alternatively, they test the bank profitability-GDP growth nexus. Brambilla and Piluso (2007) examine the link between the rates of growth of GDP and of bank assets in Italy over the very long run (1890-1973) with a VAR approach. Eickmeier, Hofmann and Worms (2006), in turn, also run a VAR model to study real GDP and nominal loans to the private sector in Germany and the Euro area over the 1985-2005 period. Goodhart, Hofmann and Segoviano (2004) correlate the GDP and loan growth rates to characterize procyclicality, while Saurina and Jimenez (2006) substitute loan growth rate by loan losses. A positive impact of quarterly credit growth on GDP growth in the U.S. is uncovered by Greenlaw, Hatzius, Kashyap and Shin (2008) with both OLS and IV regressions. A study by Jeong (2009) on procyclicality of Korean banks since the 1997 crisis regresses corporate loan growth on GDP growth and some additional controls, estimating a positive and significant coefficient.

Of course, the long-term relationship between finance and growth is another related strand of literature. In this case there exists solid evidence about a positive influence from the stock of private credit to GDP to per capita GDP growth in a multivariate setting (see Levine (2005) for a survey). However, even this finding has been challenged: for instance, Hurlin and Venet (2008) detect a robust Granger causality from economic growth to financial development for 63 countries in 1960-2000.

Turning now to the theory connecting capital and credit, it posits that increases in regulatory capital forces banks to curtail lending in order to comply with minimum requirements. In particular, under-estimation of risks in booms and over-estimation of risks in downturns would cause required capital to move in a countercyclical fashion, thus inducing credit to expand in the positive phase and fall otherwise. For this hypothesis to hold, nevertheless, it would be necessary that minimum capital requirements be binding. In practice, due to risk management decisions, market discipline and reputational motives, banks usually hold excess capital, breaking the alleged link between capital and credit (see Peek and Rosengren (1995), Alfon, Argimon and Bascuñana-Ambrós (2004), and Gropp and Heider (2008)). As a matter of fact, the available evidence for a few developed countries has been unable to detect any systematic and noticeable impact of capital changes on lending (see for example Jackson et al. (1999) and Driscoll (2004)).

3 Econometric Methodology

This section develops the econometric approach adopted in this paper. As mentioned in the Introduction, we want to test both the *sign* and the *direction of causation* between credit growth and GDP growth. For the sign, we will rely on simple contemporaneous correlations, while for the causal direction we will resort to Granger tests adapted to a panel framework. Since the chief statistical novelty of our paper is the implementation of this technique, we will next explain its foundations. After presenting our main results in the following section, we will resume in Section 5 the discussion about the link between Granger causality and underlying causality.

Hurlin (2008) proposes a panel Granger statistic test based on the methodology developed by Im, Pesaran and Shin (2003) for panel unit root tests in heterogeneous panels. The main benefit is that Granger non-causality hypothesis can be evaluated with more statistical efficiency in a panel data framework than in individual cross-section units. Nevertheless, the cross-sectional dimension implies that heterogeneity across the N countries in the panel should be accounted for in the definition of the causal relationship.

The Granger non-causality test for heterogeneous panels developed by Hurlin (2008) takes into account the heterogeneity of the data generating process (DGP) associated to the dynamic model specification of each country, and also the heterogeneity of the causal relationship from X to Y arising from the multiple countries included in the analysis. Actually, the DGP could be different for each country despite the fact that the causal relation from X to Y can exist for all countries in the panel. On the other hand, a Granger causality relation can be present just for a subgroup of countries. These sources of heterogeneity give rise to two extreme causal relationships, namely: (i) **H**omogeneous **N**on **C**ausality (HNC), in which no country causality relation exists from X to Y; and (ii) **H**omogeneous **C**ausality

(HC), whereby N causal relationships exist with the same dynamics for every country in the sample. Between these two cases we have two additional hypotheses, both related to the heterogeneous nature of the panel. In between, however, evidence can be found supporting (iii) **HE**terogeneous **C**ausality (HEC), where, as in the HC hypothesis, we have N casual relationships from X to Y, but the dynamics differs across countries, or (iv) **HE**terogeneous **N**on **C**ausality (HENC), which assumes that there exists at least one and at most N-1 countries for which there does not exist any causal relation in the sample. As can be seen, in the HEC hypothesis the heterogeneity comes from different dynamics across countries, while in the HENC hypothesis the heterogeneity originates from the causal relation from X to Y, because there is a group of countries for which X does not Granger cause Y.

The statistical test consists of the simple average of the individual Wald statistics of Granger non-causality tests for each country. Under the null hypothesis, i.e. HNC hypothesis, there is no causal relationship for all countries in the panel. However, under the alternative we have different possibilities. The reason is that the test is conducted in a heterogeneous panel, which means that parameters would be different from one country to another. Under this alternative, there are two kinds of country groups: one group revealing Granger causal relationships from X to Y (but not necessarily with the same dynamic specification), and another country group where there is no Granger causality.

Suppose we have two stationary variables, X and Y, for N countries. For each country i = 1…N, we observed T_i observations with k lags. Then we estimate the following model:

$$Y_{it} = \alpha_i + \sum_{k=1}^{K} \gamma_i^k Y_{it-k} + \sum_{k=1}^{K} \beta_i^k X_{it-k} + \varepsilon_{it} \qquad (1)$$

The autoregressive parameters γ_i^k and the coefficients β_i^k can differ across countries and the innovations ε_{it} are i.i.d. (0, σ_i^2) for i = 1,…,N and are independently distributed across countries. The null hypothesis of homogeneous non causality can be written in the following way:

$$H_0 : \beta_i = 0 \quad \forall i = 1,..., N \quad with \;\; \beta_i = (\beta_i^1,...,\beta_i^K)' \qquad (2)$$

Under the alternative hypothesis, there is a causal relationship from X to Y at least for one country and some of them but not all show β_i's equal to 0. This means that the alternative can be written as:

$$\begin{aligned} H_1 : \beta_i &= 0 \quad \forall i = 1,..., N_1 \\ \beta_i &\neq 0 \quad \forall i = N_1 + 1, N_1 + 2,..., N \end{aligned} \qquad (3)$$

where N_1 is unknown but satisfies the condition that $0 \leq N_1 / N < 1$.

It is clear that whenever the null hypothesis is not rejected, the X variable does not Granger cause Y for all countries in the panel. On the contrary, if the HNC hypothesis is rejected, the conclusions are different depending on the N_1 value. If $N_1 = 0$, then X Granger-causes Y, which means that we observe the homogeneous causality relationship for all countries in the sample. However, N_1 can take any value below N and greater than zero. In these cases we obtain the heterogeneous hypothesis, which results from either different model specifications or different causal relationships for the countries in the panel.

The Granger causality test for heterogeneous panels suffers from the same weakness as the panel unit root tests designed by IPS. As Enders (2004) argues, rejection of the null hypothesis means that at least one β_i is different from zero. Then, we could be rejecting the hypothesis of non causality just because of one or two countries. Besides this, there is no way to know which of the β_i are statistically different from zero.

4 Econometric Results

In this section we present our empirical findings. The analysis will be divided in two parts. The first one will encompass a sample of 16 OECD countries for which there exists complete data so as to carry out our tests involving bank capital (defined as the sum of Tier 1 and Tier 2 items) as well as private credit and output (broken down by their households and non-financial corporations components). Motivated by the relevance and controversy of the link between credit and output, the second part will focus exclusively on the link between these two variables, but extending the sample to a total of 144 countries. In all cases the time span covers since the 1990s through 2007.

Before proceeding, let us clarify basic issues about variable definitions, sample dimensions, time frequency, and lag structure. Recall that our working hypotheses are put forward in terms of flows (as opposed to stocks), which justifies why we use the changes in credit, capital, and GDP. To check the sensitivity of the results to the way the credit variable is defined, we will work with two alternative specifications for the change in credit: the percentage change of real private sector loans (which is the most traditional measure in the literature) and the interannual absolute change of private sector loans to GDP, both in nominal terms. The change in GDP will be measured by the real GDP growth rate. The change in bank capital will be scaled by bank assets. Before conducting the Granger equations, all involved times series were tested to ensure that they were stationary, as required by the application of this methodology.

We chose to work with yearly data because the credit procyclicality story is clearly a short-term story. The central question in deciding our data frequency is how long it takes for a change in the volume of credit to translate into changes in household or corporate expenditure (or for changes in the economic phase to affect bank capital and credit). Based on this reasoning, we can easily discard low frequency data (i.e., 5 or more years). By the same token, only one lag is employed in our annual database, once we do not expect that right-hand side variables may have an economically meaningful effect after two or more years.[3] Shorter than annual intervals may also be acceptable, but in this case the effect may

[3] Apart from this, OECD data, which are the key input in Section 4.1, is only available at annual frequency.

spread over more than just one quarter. Later on we will do over our main estimations using quarterly instead of yearly data to provide some check for robustness.

4.1 Credit, capital and business cycles in OECD countries

In this section we will deal with the linkage between changes in bank capital, private credit and GDP for a set of 16 OECD countries with available information over the 1990-2007 period.

A first shocking finding from Table 1 is that the correlation between changes in private credit and changes in GDP (the customary measure of procyclicality) is substantially less stronger than usually thought. The mean and median correlation for this country sample lie well below 0.06, reaching negative values in some cases. This conclusion is robust to the use of real credit growth (not shown in the table), Spearman correlations, lagged and lead credit variables, and to the split of credit and expenditure at the level of households and of non-financial corporations. Table 2 looks at the correlation between bank capital, private credit and GDP, revealing a similar pattern of weak to nil association. Table 3 displays Granger tests on the nexus between GDP, capital and loans, which invariably fail to reject the null hypothesis of homogeneous non-causality. Summing up the results for this OECD sample, we may say that credit is not procyclical on average and that neither capital affects credit nor credit causes, in a Granger sense, output. These findings blatantly contradict the conventional view.

4.2 Credit and business cycles in 144 countries

We now turn to a much broader sample of 144 countries in order to reassess the linkage between credit and GDP, leaving aside the reference to bank capital due to lack of data. Table 4 repeats the calculation of simple and Spearman correlations, rendering a similar picture as before in terms of the low average correlation between credit (measured both by the change in private loans to GDP and the growth rate of real private loans) and real GDP growth rate.[4] Instead of working with averages, Graph 1 displays the contemporaneous

[4] In unreported exercises, we investigated whether these correlations are asymmetric around the business cycle, increasing in negative phases and decreasing in upswings. To this end, we proceeded to calculate the real GDP trend with a Hodrick-Prescott filter and to compute the correlation for years above and below this trend, but

correlation decomposing the full sample in three groups: countries with negative correlation, countries with positive and statistically significant correlation at 10% or less, and countries with positive or negative but non-significant correlations. When credit change is measured through the percentage change in real loans, we find that 55% of the sample (79 out of 144 countries) has non-significant correlations and the remaining 45% do have positive and significant correlations, with a mean value of 0.58. When the alternative credit change definition is adopted, the proportion of countries with significant correlations drops to 12.5% (18 countries). This evidence implies that procyclicality is not a widespread phenomenon around the globe but just applies to a subset of developed and developing nations.

Table 5 presents the Granger causality tests for the full sample. For both credit change definitions, the non-causality hypothesis from GDP to credit is rejected, while non-causality from credit to GDP is rejected only for the change in loans to GDP. To provide additional evidence, we constructed 100 random samples of 20 countries with reposition and computed the percentage of cases rejecting the null hypothesis of HNC. We then replicated the experiment with groups of 30, 40, 50 and 60 countries. As can be seen in Table 6 and 7 for each of the credit variables, the HNC from credit to GDP growth is rejected in 26% of the cases (country size = 20) to 49% of the cases (country size = 60), while the non-causality from GDP growth to credit is rejected in 98% of the cases when taking 20 countries, and is rejected in the 100% of the cases in the other four exercises (with samples from 30 to 60 countries). This reinforces the impression that Granger causality from GDP to credit is noticeably more frequent than the other way around. Finally, in Table 8, we ran the Granger exercises solely for the subset of countries displaying positive and significant contemporaneous correlations. Once again the Granger non-causality from GDP to credit is strongly rejected under each of the credit variables, while the reverse causality is rejected (at 5%) only for the real loan growth definition.

results were not significantly different. As an alternative, we concentrated in countries that went through financial crises and dropped the peak year and the two following years, and then recomputed the correlation. Again the value was not modified in any noteworthy fashion.

4.3 Additional Robustness Checks

In order to strengthen our claim that credit procyclicality and credit-led growth are far from international stylized facts, we conducted a battery of additional exercises for different country samples, data periods, and time frequencies.

To start, we abandoned for a moment the panel structure to work with individual country Granger tests, in order to see if they convey a similar message. In particular, we focused on the coefficient of the lagged independent variable, which is the crucial one to test the anticipated or delayed effect underlying Granger estimations. Using the real credit growth variable, we found that 29 out of the 144 countries display a significantly positive coefficient in the GDP-to-credit equations, whereas 19 did it in the credit-to-GDP equations. In Graphs 2 (with change in loans to GDP) and 3 (with real loans growth) we plotted the distribution of the t-statistics from these 144 Granger tests, and a quick visual inspection convinces us that these significance tests are clustered around zero in the credit-to-GDP equations but have, as expected, a larger mean value in the GDP-to-credit relationship. Considering that the panel tends to support the GDP-to-credit rather than the credit-to-GDP causal link, these results are in line with the rest of our findings.[5]

Another innovation is the use of quarterly data as an alternative to the annual data employed so far. As a first step, we seasonally adjusted the quarterly time series downloaded from the IMF's International Financial Statistics database, and then we redid our correlations and Granger tests for all countries with available information (totaling 65).[6] As discussed previously, unlike annual data, we may expect that more than one lag to have a meaningful economic interpretation, so we allowed for a heterogenous lag structure across countries. Specifically, we allowed for as many lags as needed to make sure that the estimated residuals become a white noise.[7]

[5] In any case, it is interesting to note that panel estimations may be leaned towards one hypothesis that it is not rejected in only 24 out of 144 groups. This issue, however, is common to any panel regression and will not pursued here.
[6] We used the X-12 ARIMA method to get the seasonally adjusted data in those cases where the original IMF data was not previously corrected for seasonality.
[7] For most of the countries, only one lag was enough to satisfy this condition, but in about 20% of the sample two or more (up to six) lags were included.

Reassuringly, and despite the change in time frequency (from annual to quarterly), the number of units (from 144 to 65) and the lag structure, our conclusions appear to entirely hold. Graph 4 reveals that the earlier correlation pattern remains largely unaltered when higher frequency data are used. In particular, we observe that in 77% of the countries the correlation between credit growth and GDP growth is not significant (58% when considering the change in loans-to-GDP ratio). Correlation coefficients are positive and significant in 22% of the countries for the loan growth rate and 2% for the change in loans to GDP, with modest coefficients of 0.4 and 0.3, respectively. Granger causality tests, presented in Table 9, again show that we can confidently reject the Granger non-causality from GDP to credit, but not from credit to GDP.[8]

Starting with the seminal Im, Pesaran and Shin (2003) paper, several authors have the raised the criticism that panel unit roots, and as a result the panel Granger tests that derive from them, may be distorted by the presence of correlation between the disturbances of the different cross-section units (see for example Jönsson (2006) and Fleissig and Strauss (2000)). Macroeconomic interdependence may certainly be a priori an issue in our international panel. Assuming there exists a time-specific effect, the practical recommendation in this case is to demean the data, that is, substracting the cross-sectional mean for each time period from each observation at that date. We do that on our core annual dataset, as reported in Table 10, drawing additional confirmatory evidence. As before, non-causality from GDP to credit is comfortably rejected for the whole 144-country sample and for the restricted 65-sample of countries with positive and significant correlation between both variables. In contrast, the reverse Granger non-causality is rejected at a 10% significance level, but not at 5%. In Table 11 we replicate the country resampling procedure developed in Table 7, and the results again decidedly lean towards the GDP-to-credit relationship.

[8] Given the particular economic and financial conditions prevailing during the current decade, in unreported regressions, we constrained the estimation to the 2000-2007 period to assess whether the credit-output nexus was modified (without this change being captured by our time dummies). The results, however, were robust to this sample split as well.

5. Granger causality between credit and GDP growth: How far from economic causality?

5.1 Granger causality vs. economic causality

We firmly believe that Granger causality is the right tool to address the research questions at hand. Let us first recall that our primary goal is to put to the test the enormously influential paradigm stating that changes in credit are followed by changes in economic activity. Although Granger tests are all we need to accept or reject this hypothesis, we would like to go one step further and briefly entertain the discussion about the implications of Granger causality for economic causality in the context of our particular problem. Our claim is that, even after acknowledging the econometric complexity of testing causality, a closer evaluation provides compelling reasons for expecting Granger tests to be a sound and dependable shortcut into economic causality when it comes to the short-run association between credit and output, counter to the usual stance that Granger causality is invariably just a pale and flawed approximation.

In a first-order specification, like the one adopted for the yearly dataset, our model would take the following form:

$$
\begin{aligned}
y_{i,t} &= \delta_1 y_{i,t-1} + \delta_2 x_{i,t} + \delta_3 x_{i,t-1} + \delta_4 z_{i,t} + \varepsilon_{y,t} \\
x_{i,t} &= \delta_5 x_{i,t-1} + \delta_6 y_{i,t} + \delta_7 y_{i,t-1} + \delta_8 z_{i,t} + \varepsilon_{x,t}
\end{aligned}
$$

where *y* and *x* stand for GDP growth and credit growth, respectively, *z* is a vector of other variables potentially affecting *y* and *x*, and ε are error terms. In turn, the subindex *i* refers to countries and *t* to time periods. Three conditions would be sufficient to guarantee the absence of endogeneity and thus the estimators' unbiasedness and consistency: (a) the error terms are white noise, (b) δ_4 and δ_8 are zero, and (c) δ_2 and δ_6 are also zero.[9,10] What is more important for our purposes, under these conditions Granger causality would also imply economic causality.

[9] In principle, Granger non-causality is neither a necessary nor sufficient condition for weak exogeneity, understood as the irrelevance of an additional equation explaining the variable *x* for a consistent estimation of *y* in terms of *x*. For further discussion, see the seminal paper on exogeneity by Engle, Hendry and Richard (1983) and the textbook treatment of Davidson and McKinnon (1993).

[10] Actually, if δ_8 is zero, it suffices that δ_6 is zero to have weak exogeneity.

Condition (a) was satisfied in the quarterly Granger regressions, where we explicitly included as many lags of the explanatory variables as needed to make the residuals (from individual-country Granger equations) a white noise.[11]

Condition (b) imposes that no relevant variable is omitted from the analysis. The skeptical reader might legitimately be concerned about the presence of an endogeneity bias in our bivariate Granger experiments. Our reply is fourfold:

(1) We insist that our focus is to validate or reject the popular notion spelled out at the beginning, by which credit crunches sternly hampers GDP growth. The financial experts cited there, joined by the bulk of the profession, do think in terms of a bivariate short-term relationship between credit and business cycles, without ever highlighting any major role for third variables;

(2) We happen to agree with this "bivariate worldview" in regard to our short-term credit/GDP model, based on a simple, uncontestable truth: the link from lending to expenditure relies on an accounting identity, where extraneous behavioral equations and hence third variables are assumed away by definition.[12]

(3) Even if one were determined to specify a fuller model, there exists no agreement whatsoever about the relevant variables to be included, letting alone the obvious colinearities across right-hand side variables prone to arise in a short-run model involving credit and GDP growth; and

(4) Let us recall that the surprising absence of a significant short-term impact of credit on GDP was the norm (with some exceptions) across our Granger estimations. This implies that, in order to claim that these results are an artifact of endogeneity bias, one should be able to identify, even without having a complete model specification, a relevant omitted variable covarying strongly and negatively with credit growth (and displaying at the same

[11] As mentioned before, in the annual dataset the inclusion of more than one lag would not be economically meaningful, as we are dealing with a short-run phenomenon. However, the first-order Granger regressions gave rise to white noise residuals in most countries.

[12] We would certainly repeal this idea in a long-run study linking financial deepening and economic growth, where the incorporation of third variables to isolate the effect of credit is inescapable as various and complex effects need to be taken into account.

time a positive correlation with the dependent variable).[13] We could not think of such a variable.[14]

Finally, the lack of a contemporaneous feedback relationship between credit growth and GDP growth, condition (c), is more difficult to overcome. To start, by solely looking at the issue of time anticipation, the Granger methodology neglects this sort of link between y_t and x_t. Furthermore, our succinct literature review underscored arguments from credit to GDP and from GDP to credit. However, this should not jeopardize the robustness of our previous findings. If anything, both variables correlate positively, which means that the estimates would be biased upward. But our empirical problem is not that we reject too often the Granger non-causality from credit to GDP, but that we reject it too rarely. In other words, the potential endogeneity of our Granger coefficients does nothing but to reinforce our earlier conclusions.

5.2 Some exploratory panel regression analysis

Even after factoring in the limitations of standard regression analysis that we discussed at length in the above subsection, regressions have two appealing features vis-à-vis our previous Granger tests: (a) They produce numerical estimates that can give a rough idea of economic significance, which may be an asset for policy purposes, and (b) They allow to control for other factors beyond the history of the main variables of interest.

However, as we argued before, we think that, for the particular issue under study, regression analysis is unlikely to yield results any more reliable than the Granger tests did so far. The limitations of the multivariate regression analysis come from (1) the absence of a well-established and fully specified empirical model linking credit flows, output growth, and other macroeconomic variables. Besides, since output growth is at the center of all economic developments, all candidate explanatory variables (inflation, interest rates, real state prices, and others used in different papers) are unlikely to be exogenous to GDP growth; and (2) the

[13] The bias of the credit growth coefficient depends not only on the correlation between credit growth and the omitted variable but also on the effect that the omitted variable has on the dependent variable, GDP growth. Thereby, the sign and magnitude of the bias depends of the interaction of both estimators.

[14] It goes without saying that our correlations to capture procyclicality are free from this potential caveat, once the concept of correlation does not require any additional controls. Much less controversial and more pressing is the need to control for endogeneity in a long-run GDP growth framework at the time of examining the impact of the stock (not the flow) of credit, but this analysis is quite different from ours. See for example World Bank (2001).

lack of a bullet-proof econometric technique to deal with the potential endogeneity arising from the joint determination of credit and GDP underlined in this document. With these caveats in mind, we carried out a series of regressions, from credit to GDP and viceversa, chiefly aimed to challenge our core, Granger-based findings. In all cases, we controlled for country fixed effects, time-varying (annual) effects and for financial crises.[15] We restrict our reported estimations to those including the private credit growth (as opposed to the change in loans to GDP), because of the easier interpretation of the coefficient as an elasticity.

In the first two columns of Table 12 we show the results for the annual database of 144 countries in 1990-2007. We find that one percentage point of contemporaneous (lagged) GDP growth is associated with an increase of 0.75 (0.627) percentage points in private credit growth. The full effect adds to about 1.38, which is 27 times larger than the increase in GDP growth (0.051) associated to a one-percentage-point increase in private credit. This evidence of an allegedly much more potent effect from GDP to credit than the other way around is also consistent with the main message of this paper.

Seeking to uncover a possible asymmetric effect around output trend, we implemented a Hodrik-Prescott filter to identify years above and below long-run GDP trend, and then we created a dummy variable taking value 1 for years above the GDP trend, which we subsequently interacted with the independent variable of interest. The rationale of this question is that the public and the analysts seem to be more concerned about credit procyclicality when the economy goes through bad times, but not that much in tranquil periods. This in turn may respond to myopia or other behavioral biases, but it also might be due to purely economic factors. For example, credit contractions might be more harmful to the economy during downturns, as households and firms find it more difficult to make up for the lack of credit with their internal funding or other sources. As for the GDP-to-credit link, it might be the case that the supply of credit is more sensitive to economic conditions in economic slumps. As can be seen in Table 12, columns 3 and 4, we encounter that the slope coefficient of GDP growth in the credit growth regression lowers and that of credit growth in the GDP growth rises. These slope changes, however, are not big enough to modify our overall conclusions: for instance, the full effect of GDP on credit falls from the previous

[15] Financial crises are dated using the chronology assembled by Laeven and Valencia (2008). A dummy with value 1 on the peak year is used in our estimations.

1.38 to 1.32 in above-GDP trend periods, while the credit effect on GDP goes up from 0.051 to 0.069. In sum, the impact asymmetry is hardly noticeable.

The last two columns in Table 12 scrutinize the prior that the flows of credit might have a more perceptible impact in countries having beforehand a deeper financial system. Estimated values prove this hypothesis wrong on empirical grounds, as the interaction of credit growth with the credit to GDP ratio turned out to be economically and statistically non-significant.

Besides the estimation for the full sample, in Table 13 we isolate the subsample of the 65 countries with a positive and significant correlation between the two variables, under the presumption that results for the full sample might somewhat be biased downward by the 55% of the sample displaying no correlation whatsoever.[16] As expected, estimates become larger, with the full effect (contemporaneous plus lagged) now reaching 2.22 in the GDP to credit regression, against 0.11 in the credit to GDP equation. Beyond that, the only other change is that the asymmetric effect of GDP growth on credit growth becomes non-significant.

Going back to the endogeneity issue, let us recall that a positive two-way relationship between credit and GDP may bias both estimated coefficients upward. The verification from Tables 12 and 13 that there exists a contemporaneous feedback between GDP growth and credit growth in the regressions (δ_2 and δ_6 are indeed positive and significant) indicates that this source of endogeneity might be present. The two-step Arellano-Bond GMM technique, based on internal instruments, is often employed to deal with this potential bias.[17] It can be checked in Tables 14 and 15 that results remain largely unaltered, removing the concern about endogeneity giving rise to unreliable estimators in previous exercises.[18]

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[16] In addition, we examined this subsample of countries in search of a common profile in terms of economic, institutional or financial development, but we were unable to find a systematic pattern across the members of this group.

[17] We use the original Arellano-Bond two-step estimator rather than the system estimator because the latter is recommended for cases in which the original variables are highly persistent, and thus the level variables are poor instruments for the differenced variables. However, this is not the case in our present application.

[18] As shown at the bottom of Tables 14 and 15, our GMM estimations pass the usual Arellano-Bond tests of no serial error correlation of first-order (from the original equation) and second-order (from the first-differenced equation). The latter is needed to guarantee the consistency of the GMM estimator, provided the original equation's disturbances are already proved to be non-serially correlated. The traditional Sargan / Hansen statistics test the validity of the instruments. The Hansen and Sargan tests coincide when the variance-covariance matrix is spherical. Otherwise, the Hansen statistic for the second step Arellano-Bond estimator is theoretically superior.

6 Conclusions and Discussion

The aim of this paper was to provide up-to-date worldwide evidence on whether changes in credit precede (and eventually cause) changes in economic activity in the short-run. Standard correlation methods (to assess procyclicality between credit and business cycles) and state-of-the-art panel Granger causality tests (to examine time anticipation) were applied on a maximum sample of 144 countries over 1990-2007. Taking advantage of these tools, we also extended the analysis to the linkage between bank capital, output and credit changes in OECD countries.

Our results openly clash with two popular economic statements, namely, that credit is procyclical and that changes in credit have strong effects on private expenditure. According to the evidence produced, credit procyclicality, in the sense that the simple correlation coefficient is positive and significant at 10% or less, prevails in just 45% of the countries when annual data are used, and 23% with quarterly data. As for the second question, the response offered by our work for the full sample and a number of random subsamples is that Granger causality runs from GDP to credit in an overwhelming majority of cases, while the often claimed causality from credit to GDP is a feature observable much less frequently. In regard to the linkage of bank capital with economic activity and credit, our estimations for a sample of 16 OECD countries did not pick up any significant relationship. In turn, our panel regressions reveal a much stronger effect on GDP growth on credit growth than the other way around. Furthermore, after testing for endogeneity, we contend that our results uncover not just mere Granger causality but economic causality.

These controversial findings have vast academic and policy implications, inviting to revisit some long-established and possibly misleading notions on bank capital regulation, the nexus between banking and the macroeconomy, and financial crisis prevention and resolution. In the first place, the alleged pitfall of Basel II in exacerbating credit procyclicality should be revisited in light of our evidence against bank capital as neither being affected by economic conditions nor affecting credit flows. In the second place, the paper puts in doubt the claim that the chief transmission channel from banking crisis to the real economy runs through the credit contraction and the associated retrenchment in private expenditure. In turn, the severity of this lending channel would justify massive bailouts of the financial system. Our results suggest that the macroeconomic impact of credit changes on GDP growth is questionable, undermining the classic argument for public interventions in the event of a financial crisis. Needless to say, there might exist other motives for such intervention, but it would enrich the policy discussion to clarify which ones are the most relevant on empirical grounds. Finally, a critical topic for future research agenda should be to unveil the ultimate causes of credit procyclicality (or the lack of it) by studying the behavior of bank managers, depositors, borrowers and regulators over the cycle.

Before closing, we are aware that our provocative results beg the question as to why is there such a widespread belief among policymakers and academics in a strong influence of credit on economic activity, and why the evidence we have just documented departs from conventional wisdom. To our understanding, the existing consensus has theoretical roots, which are hard to dismiss, and practical roots, which are manifestly misled. At the theoretical level, analysts have internalized the financial accelerator model developed in the 1980s, which claims that economic cycles deeply affect and are affected by credit movements. Not only has this theory been integrated into an elegant and compelling modeling framework but it also makes perfect sense to those familiarized with the dynamics of everyday financial markets. According to this model, a feedback relationship should be uncovered by any econometric exploration, which for the most part is not the case in the present paper.

While the theory looks flawless, its advocates seem to overestimate the role of credit as a source of finance for the private sector, as clearly shown by the following quotes. Ben Bernanke (2007), one of leading voices in the field of credit and macroeconomics, states that: *"To expand and modernize their plants and increase their staffs, most firms must turn to financial markets or to financial institutions to secure this essential input. Families rely on the financial markets to obtain mortgages or to help finance their children's educations."* Greenlaw, Hatzius, Kashyap and Shin (2008) maintain that *"…when capital markets are imperfect…access to financing is not assured. Furthermore, if some borrowers are dependent on intermediaries for financing, then any factors that disrupt the supply of financing from intermediaries will have real effects."*

In both cases a strong assumption is made about the intense credit-dependency of firms and households, and here resides the mistaken practical stand in favor of a strong link between credit and output. In reality, a strong reliance on own funds happens to be the case, as revealed by corporate-level data (see Bebczuk (2003)) and macroeconomic figures (see Bebczuk and Schmidt-Hebbel (2007)). Actually, this should not be surprising after pondering the very financial frictions that the financial accelerator is built upon: given the informational opacity prevalent in financial markets, external funds are much more expensive, if available at all, than internal funds, inducing firms to prefer self-financing. To prove the point with some back-of-the envelope figures, let us consider the following accounting flow of funds identity for the private sector: Consumption + Investment = ΔLoans +ΔStock+ΔBonds+ΔOwn Funds, where Δ stands for change (what matters here is the flows of finance, not the stocks). A straightforward measure of credit dependence is therefore the ratio ΔLoans/(Consumption+ Investment). Table 16 reports this ratio for a set of 23 countries with available information.[19] According to the table, private credit contributes a mere 9.5% of total financing in industrial countries and 7.9% in developing countries on average over 1990-2005. These plain figures help rationalizing our findings, which at first glance might appear as a gross violation of a sound theory buttressing a strong influence of credit over economic activity. Two practical points can be made: (1) An increase in loans does not need to automatically translate into higher private spending, as more credit can just substitute for other financing sources. In particular, with easier access to credit, the private sector might decide to save less and thus reduce self-financing;[20] and (2) In light of the low average incidence of credit on spending, it might well be the case that loan changes have an indiscernible statistical effect in many countries, especially taking into account that the credit dependence ratio is quite variable over time.

Although these considerations should weaken the almost blind belief in credit procyclicality, they do not mean that credit does not matter for the economy. Much to the contrary, they reinforce the argument that credit has a crucial role on overall productivity via a correct allocation of financial resources, a conclusion that has already found convincing empirical backing (see for instance World Bank (2001) and Bebczuk and Garegnani (2007)).

Finally, and compounding the problem, observers of financial phenomena seem to display some bias in extrapolating the behavior of credit markets around crisis to tranquil times.

[19] The binding data constraint is the private gross investment figure. National accounts usually publish total investment aggregating the private and public components. Our data comes from United Nations.

[20] Also, as our spending variable is GDP growth, the change in loans may be accompanied by changes in different directions in the various items comprising GDP, without any detectable change in GDP as a whole.

Specifically, during episodes like the recent subprime crisis, it is rather typical to watch a rapid credit expansion on the eve of the crisis along with strong GDP growth, and a credit crunch accompanied by an economic contraction in the aftermath. This simultaneity may not be the rule over longer periods, yet such traumatic changes appear to shape beliefs on the relationship between credit and output, as if they were permanent instead of temporary features of financial markets. This perception bias has been labeled as saliency by the flourishing literature on behavioral finance (see Barberis and Thaler (2003)).

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Table 1
Correlation between
credit flows and GDP growth by institutional sector
Data for 16 OECD countries
with annual data for 1995-2007

Simple correlation between GDP growth and:	*Mean*	*Median*	*Maximum*	*Minimum*
Change in (Household Loans/GDP)	-0.304	-0.251	0.516	-0.898
Lagged Change in (Household Loans/GDP)	0.060	0.084	0.630	-0.828
Lead Change in (Household Loans/GDP)	-0.106	-0.039	0.664	-0.869
Change in (Corporate Loans/GDP)	-0.069	-0.083	0.700	-0.651
Lagged Change in (Corporate Loans/GDP)	-0.013	0.040	0.598	-0.605
Lead Change in (Corporate Loans/GDP)	0.174	0.328	0.675	-0.728
Change in (Private Loans/GDP)	-0.172	-0.128	0.607	-0.836
Lagged Change in (Private Loans/GDP)	0.025	0.016	0.647	-0.726
Lead Change in (Private Loans/GDP)	0.086	0.256	0.803	-0.778
Spearman correlation between GDP growth and:	*Mean*	*Median*	*Maximum*	*Minimum*
Change in (Household Loans/GDP)	-0.279	-0.209	0.552	-0.868
Lagged Change in (Household Loans/GDP)	0.058	0.078	0.706	-0.868
Lead Change in (Household Loans/GDP)	-0.087	-0.105	0.545	-0.907
Change in (Corporate Loans/GDP)	-0.044	-0.077	0.665	-0.682
Lagged Change in (Corporate Loans/GDP)	0.004	-0.007	0.545	-0.670
Lead Change in (Corporate Loans/GDP)	0.207	0.341	0.676	-0.709
Change in (Private Loans/GDP)	-0.150	-0.168	0.587	-0.790
Lagged Change in (Private Loans/GDP)	0.054	0.055	0.671	-0.736
Lead Change in (Private Loans/GDP)	0.123	0.308	0.772	-0.797

Table 2
Correlation between bank capital,
private credit flows and GDP growth

Data for 16 OECD countries with annual data for 1990-2007

Simple correlation between change in (Bank Capital/Bank Assets) and:	*Mean*	*Median*	*Maximum*	*Minimum*
GDP growth	-0.023	-0.049	0.459	-0.427
Lagged GDP growth	0.010	0.092	0.346	-0.408
Lead GDP growth	0.062	0.052	0.503	-0.580
Change in (Private Loans/Bank Assets)	0.103	0.112	0.848	-0.508
Lagged Change in (Private Loans/Bank Assets)	0.024	0.157	0.629	-0.653
Lead Change in (Private Loans/Bank Assets)	0.051	0.023	0.408	-0.301
Spearman correlation between change in (Bank Capital/Bank Assets) and:	*Mean*	*Median*	*Maximum*	*Minimum*
GDP growth	-0.005	-0.054	0.429	-0.536
Lagged GDP growth	-0.025	-0.021	0.333	-0.473
Lead GDP growth	0.067	0.004	0.482	-0.573
Change in (Private Loans/Bank Assets)	0.244	0.190	0.900	-0.279
Lagged Change in (Private Loans/Bank Assets)	0.077	0.055	0.595	-0.382
Lead Change in (Private Loans/Bank Assets)	0.013	-0.024	0.491	-0.292

Table 3
Panel Granger causality tests between capital, credit and output
Data for 16 OECD countries with annual data for 1990-2007

Homogeneous Non-Causality	p-value (*)
A. Changes scaled by Bank Assets	
From change in [Bank Capital/Assets] to change in [Private Loans/Assets]	0.317
From change in [Private Loans/Assets] to change in [Bank Capital/Assets]	0.933
From change in [Bank Capital/Assets] to GDP growth	0.881
From GDP growth to change in [Bank Capital/Assets]	0.284
B. Real growth rates	
From Bank Capital growth to GDP growth	0.568
From GDP growth to Bank Capital growth	0.668
From Bank Capital growth to Private Loans growth	0.127
From Private Loans growth to Bank Capital growth	0.484
From Private Loans growth to GDP growth	0.361
From GDP growth to Private Loans growth	0.128

(*) p-values below 0.05 mean that the null hypothesis of Granger non-causality is rejected.

Table 4
Correlation between
private credit flows and GDP growth
Data for 144 developed and developing countries
with annual data for 1990-2007

Simple correlation between GDP growth and:	*Mean*	*Median*	*Maximum*	*Minimum*
Change in (Private Loans/GDP)	0.013	0.045	0.804	-0.731
Lagged change in (Private Loans/GDP)	0.025	0.047	0.754	-0.671
Lead change in (Private Loans/GDP)	0.304	0.308	0.917	-0.557
Growth rate of real Private Loans	0.337	0.344	0.905	-0.427
Lagged growth rate of real Private Loans	0.172	0.192	0.843	-0.541
Lead growth rate of real Private Loans	0.326	0.349	0.839	-0.436
Spearman correlation between GDP growth and:	*Mean*	*Median*	*Maximum*	*Minimum*
Change in (Private Loans/GDP)	0.024	0.014	0.786	-0.822
Lagged change in (Private Loans/GDP)	0.044	0.038	0.939	-0.719
Lead change in (Private Loans/GDP)	0.286	0.288	0.934	-0.543
Growth rate of real Private Loans	0.351	0.350	0.843	-0.286
Lagged growth rate of real Private Loans	0.185	0.169	0.770	-0.559
Lead growth rate of real Private Loans	0.309	0.317	0.829	-0.526

Graph 1: Contemporaneous correlations between credit and GDP

Data for 144 developed and developing countries
with annual data for 1990-2007

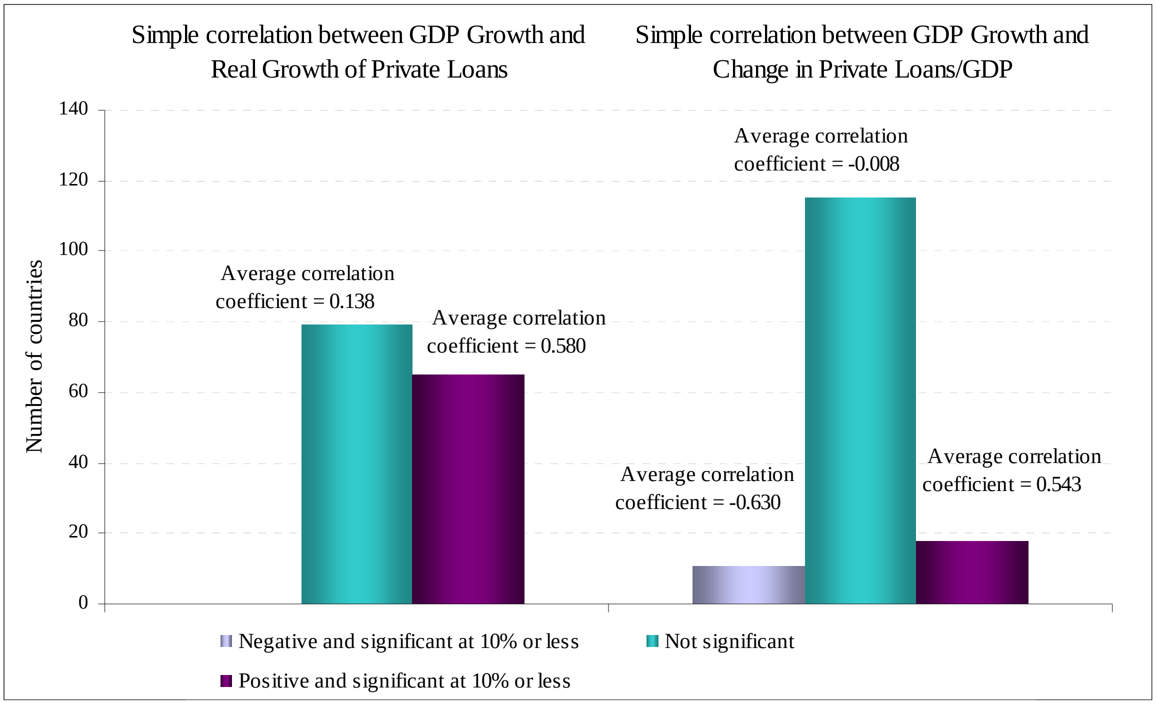


Table 5
Panel Granger causality tests between GDP and credit
Total sample of 144 countries with annual data for 1990-2007

H_0: Homogeneous non-causality	p-value
From GDP growth to real Private Loans growth	0.000
From real Private Loans growth to GDP growth	0.179
From GDP growth to change in [Private loans / GDP]	0.000
From change in [Private loans / GDP] to GDP growth	0.000

Table 6
Panel Granger causality tests between GDP growth and the change in Private Loans/GDP for 100 random samples of different country size
Total sample of 144 countries with annual data for 1990-2007

Number of countries	**H_0: Homogeneous non-causality**	**% of cases rejecting H_0**
20	From GDP growth to change in [Private loans / GDP]	98%
	From change in [Private loans / GDP] to GDP growth	26%
30	From GDP growth to change in [Private loans / GDP]	100%
	From change in [Private loans / GDP] to GDP growth	27%
40	From GDP growth to change in [Private loans / GDP]	100%
	From change in [Private loans / GDP] to GDP growth	38%
50	From GDP growth to change in [Private loans / GDP]	100%
	From change in [Private loans / GDP] to GDP growth	36%
60	From GDP growth to change in [Private loans / GDP]	100%
	From change in [Private loans / GDP] to GDP growth	49%

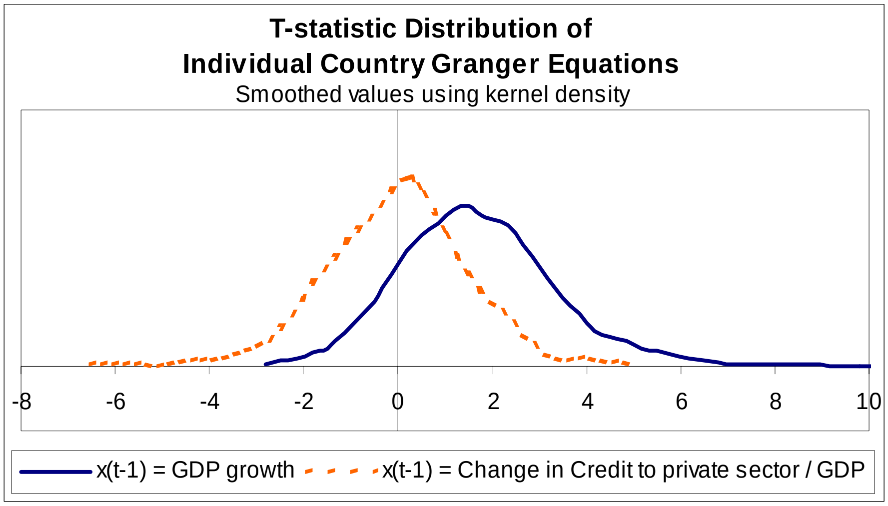
Table 7
Panel Granger causality tests between GDP growth and private credit growth for 100 random samples of different country size
Total sample of 144 countries with annual data for 1990-2007

Number of countries	H_0: Homogeneous non-causality	% of cases rejecting H_0
20	From GDP growth to real Private Loans growth	88%
	From real Private Loans growth to GDP growth	19%
30	From GDP growth to real Private Loans growth	97%
	From real Private Loans growth to GDP growth	5%
40	From GDP growth to real Private Loans growth	100%
	From real Private Loans growth to GDP growth	10%
50	From GDP growth to real Private Loans growth	100%
	From real Private Loans growth to GDP growth	8%
60	From GDP growth to real Private Loans growth	99%
	From real Private Loans growth to GDP growth	7%

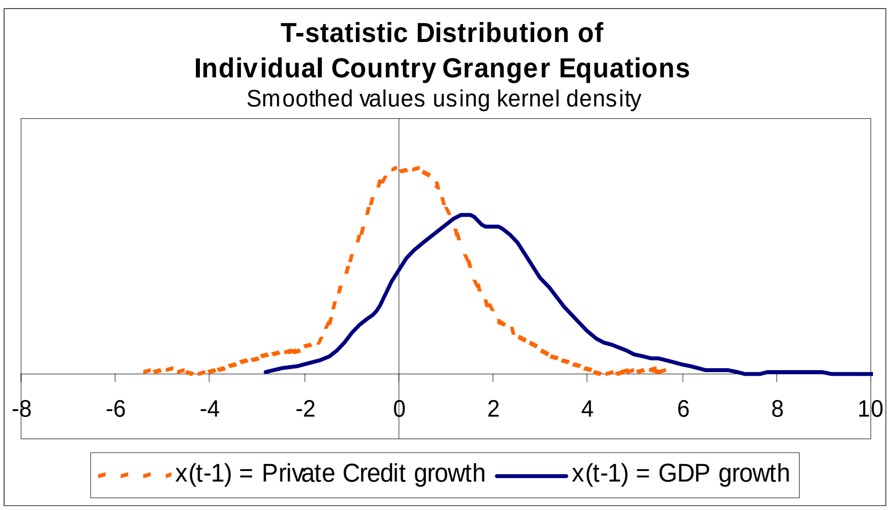
Table 8
Panel Granger causality tests between GDP and credit
Countries with positive and significant (at 10% or less) correlations
with annual data for 1990-2007

Sample	H_0: Homogeneous non-causality	p-value
65 Countries	From GDP growth to real Private Loans growth	0.000
	From real Private Loans growth to GDP growth	0.017
18 Countries	From GDP growth to change in [Private loans / GDP]	0.000
	From change in [Private loans / GDP] to GDP growth	0.084

Graph 2



Graph 3



Graph 4: Contemporaneous correlations between credit and GDP

Data for 65 developed and developing countries
with quarterly data for 1990-2007

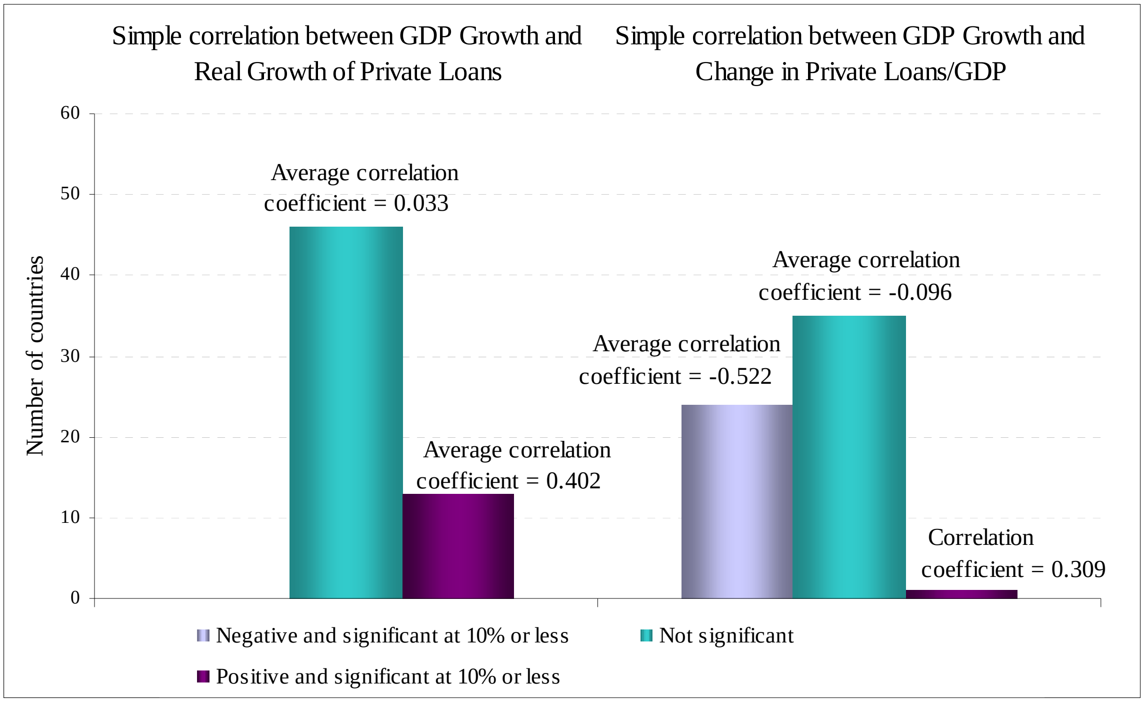


Table 9
Panel Granger causality tests between GDP and credit
Total sample of 65 countries with quarterly data for 1990-2007

H_0: Homogeneous non-causality	p-value
From GDP growth to real Private Loans growth	0.004
From real Private Loans growth to GDP growth	0.667
From GDP growth to change in [Private loans / GDP]	0.025
From change in [Private loans / GDP] to GDP growth	0.911

Table 10
Panel Granger causality tests between GDP and credit
Demeaned Data
Annual data for 1990-2007

Sample	Ho: Homogeneous non-causality	p-value
Total sample **(144 countries)**	From GDP growth to real Private Loans growth	0.000
	From real Private Loans growth to GDP growth	0.058
Countries with positive correlation at 10% or less **(65 countries)**	From GDP growth to real Private Loans growth	0.000
	From real Private Loans growth to GDP growth	0.074

Table 11
Panel Granger causality tests between GDP growth and private credit growth for 100 random samples of different country size
Demeaned data
Total sample of 144 countries with annual data for 1990-2007

Number of countries	Granger Causality	% of cases rejecting Granger non-causality
20	From GDP growth to real Private Loans growth	82%
	From real Private Loans growth to GDP growth	21%
30	From GDP growth to real Private Loans growth	95%
	From real Private Loans growth to GDP growth	27%
40	From GDP growth to real Private Loans growth	99%
	From real Private Loans growth to GDP growth	21%
50	From GDP growth to real Private Loans growth	100%
	From real Private Loans growth to GDP growth	22%
60	From GDP growth to real Private Loans growth	100%
	From real Private Loans growth to GDP growth	24%

Table 12
Panel regressions with two-way fixed effects (1)
Full sample of 144 countries with annual data for 1990-2007

Explanatory Variables	Dependent variable					
	Private credit Growth	GDP Growth	Private credit Growth	GDP Growth	Private credit Growth	GDP Growth
	(1)	(2)	(3)	(4)	(5)	(6)
GDP Growth	0.750***		0.914***		0.785***	
	[0.131]		[0.153]		[0.171]	
Lagged GDP Growth	0.627***	0.130***	0.728***	0.105***	0.615***	0.126***
	[0.104]	[0.0337]	[0.114]	[0.0359]	[0.103]	[0.0337]
Private credit Growth		0.0512***		0.0305***		0.0492***
		[0.00983]		[0.0111]		[0.0122]
Lagged Private credit Growth	0.179***	0.00445	0.178***	0.00373	0.181***	0.00423
	[0.0305]	[0.00699]	[0.0324]	[0.00747]	[0.0311]	[0.00695]
GDP Growth*Dummy=1 if GDP above Trend (2)			-0.323**			
			[0.137]			
Credit Growth*Dummy=1 if Credit above Trend (2)				0.0387***		
				[0.00990]		
GDP growth * (Private Credit/GDP)					-0.113	
					[0.287]	
Private Credit growth * (Private Credit/GDP)						0.0131
						[0.0198]
Constant	6.433***	3.893***	6.461***	3.682***	6.573***	3.861***
	[1.383]	[0.303]	[1.509]	[0.312]	[1.372]	[0.305]
Observations	2259	2259	2024	2024	2250	2250
Number of countries	144	144	129	129	144	144
Financial Crisis dummy	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
F-test for fixed effects - H0: all $\mu i=0$	0,009	0,000	0,010	0,000	0,008	0,000
F-test for time dummies - H0: all $\lambda_t=0$	0,000	0,000	0,000	0,000	0,000	0,000
R-squared	0.192	0.136	0.194	0.134	0.193	0.138

(1) Robust standard deviations reported between brackets. ***Significant at 1%, **Significant at 5%, *Significant at 10%.
(2) Trend extracted with Hodrik-Prescott filter using a smoothing parameter of 1600.

Table 13
Panel regressions with two-way fixed effects [1]
Sample of 65 countries with GDP growth-credit growth correlation
positive and significant at 10% or less
Annual data for 1990-2007

Explanatory Variables	Dependent variable					
	Private credit Growth	GDP Growth	Private credit Growth	GDP Growth	Private credit Growth	GDP Growth
	(1)	(2)	(3)	(4)	(5)	(6)
GDP Growth	1.583***		1.780***		1.657***	
	[0.206]		[0.202]		[0.261]	
Lagged GDP Growth	0.640***	0.174***	0.834***	0.136***	0.638***	0.174***
	[0.174]	[0.0471]	[0.159]	[0.0456]	[0.172]	[0.0467]
Private credit Growth		0.109***		0.0901***		0.107***
		[0.0165]		[0.0164]		[0.0198]
Lagged Private credit Growth	0.178***	-0.0167	0.163***	-0.0108	0.180***	-0.0173
	[0.0440]	[0.0107]	[0.0458]	[0.0108]	[0.0448]	[0.0107]
GDP Growth*Dummy=1 if GDP above Trend [2]			-0.239			
			[0.201]			
Credit Growth*Dummy=1 if Credit above Trend [2]				0.0264**		
				[0.0106]		
GDP growth * (Private Credit/GDP)					-0.265	
					[0.358]	
Private Credit growth * (Private Credit/GDP)						0.0104
						[0.0232]
Constant	2.099	3.285***	1.461	3.016***	2.481	3.238***
	[1.854]	[0.357]	[2.198]	[0.360]	[1.754]	[0.355]
Observations	1024	1024	897	897	1020	1020
Number of countries	65	65	57	57	65	65
Financial Crisis dummy	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
F-test for fixed effects - H0: all $\mu i=0$	0,000	0,000	0,000	0,000	0,000	0,000
F-test for time dummies - H0: all $\lambda_t=0$	0,052	0,000	0,263	0,000	0,045	0,000
R-squared	0.382	0.327	0.379	0.322	0.383	0.328

(1) Robust standard deviations reported between brackets. ***Significant at 1%, **Significant at 5%, *Significant at 10%.
(2) Trend extracted with Hodrik-Prescott filter using a smoothing parameter of 1600.

Table 14
Two-step Arellano-Bond Regressions (1)
Full sample of 144 countries with annual data for 1990-2007

Explanatory Variables	Dependent variable					
	Private credit Growth	GDP Growth	Private credit Growth	GDP Growth	Private credit Growth	GDP Growth
	(1)	(2)	(3)	(4)	(5)	(6)
GDP Growth	0.986***		1.118***		1.293***	
	[0.180]		[0.234]		[0.264]	
Lagged GDP Growth	0.660***	0.0662	0.772***	0.0499	0.656***	0.0728
	[0.123]	[0.0587]	[0.125]	[0.0668]	[0.127]	[0.0602]
Private credit Growth		0.0459***		0.0311**		0.0551***
		[0.0113]		[0.0135]		[0.0150]
Lagged Private credit Growth	0.207***	0.00286	0.206***	0.00124	0.207***	0.00246
	[0.0434]	[0.00624]	[0.0448]	[0.00676]	[0.0443]	[0.00649]
GDP Growth*Dummy=1 if GDP above Trend (2)			-0.344**			
			[0.163]			
Credit Growth*Dummy=1 if Credit above Trend (2)				0.0191		
				[0.0139]		
GDP growth * (Private Credit/GDP)					-1.115**	
					[0.491]	
Private Credit growth * (Private Credit/GDP)						-0.0468
						[0.0314]
Observations	2089	2089	1870	1870	2079	2079
Number of countries	144	144	129	129	144	144
Financial Crisis dummy	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of instruments	22	22	23	23	23	23
Arellano-Bond test for AR(1)	0,000	0,000	0,000	0,000	0,000	0,000
Arellano-Bond test for AR(2)	0,616	0,686	0,908	0,547	0,641	0,614
Sargan test of overid. Restrictions - $\chi^2(2)$	0,201	0,509	0,464	0,501	0,189	0,462
Hansen test of overid. Restrictions - $\chi^2(2)$	0,439	0,514	0,701	0,469	0,411	0,446

(1)Standard errors in brackets corrected by Windmeijer finite-sample correction. ***Significant at 1%, **Significant at 5%, *Significant at 10%.
(2) Trend extracted with Hodrik-Prescott filter using a smoothing parameter of 1600.

Table 15
Two-step Arellano-Bond Regressions (1)
Sample of 65 countries with GDP growth-credit growth correlation
positive and significant at 10% or less
Annual data for 1990-2007

Explanatory Variables	Dependent variable					
	Private credit Growth	GDP Growth	Private credit Growth	GDP Growth	Private credit Growth	GDP Growth
	(1)	(2)	(3)	(4)	(5)	(6)
GDP Growth	1.737***		1.883***		1.979***	
	[0.249]		[0.302]		[0.367]	
Lagged GDP Growth	0.598***	0.176***	0.768***	0.215***	0.607***	0.186***
	[0.190]	[0.0518]	[0.192]	[0.0566]	[0.189]	[0.0518]
Private credit Growth		0.104***		0.0886***		0.118***
		[0.0165]		[0.0172]		[0.0228]
Lagged Private credit Growth	0.237***	-0.0124	0.251***	-0.0111	0.237***	-0.0135
	[0.0674]	[0.0111]	[0.0737]	[0.0113]	[0.0676]	[0.0111]
GDP Growth*Dummy=1 if GDP above Trend (2)			-0.248			
			[0.219]			
Credit Growth*Dummy=1 if Credit above Trend (2)				-0.00710		
				[0.0178]		
GDP growth * (Private Credit/GDP)					-0.935*	
					[0.519]	
Private Credit growth * (Private Credit/GDP)						-0.0692
						[0.0454]
Observations	949	949	830	830	944	944
Number of countries	65	65	57	57	65	65
Financial Crisis dummy	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of instruments	22	22	23	23	23	23
Arellano-Bond test for AR(1)	0,000	0,000	0,000	0,000	0,000	0,000
Arellano-Bond test for AR(2)	0,130	0,510	0,194	0,363	0,138	0,548
Sargan test of overid. Restrictions - $\chi^2(2)$	0,015	0,197	0,034	0,143	0,012	0,204
Hansen test of overid. Restrictions - $\chi^2(2)$	0,092	0,317	0,143	0,278	0,081	0,316

(1)Standard errors in brackets corrected by Windmeijer finite-sample correction. ***Significant at 1%, **Significant at 5%, *Significant at 10%.
(2) Trend extracted with Hodrik-Prescott filter using a smoothing parameter of 1600.

Table 16
Ratio of private loan flows
to private spending (consumption plus investment)
in developed and developing countries
Average value for 1990-2005, in descending order

Country	Loan Flows / Private Spending
Developed Countries	
Netherlands	25.8%
Sweden	15.4%
Spain	15.3%
UK	14.4%
Canada	12.2%
Australia	11.4%
Austria	9.9%
Norway	9.5%
Italy	8.0%
Belgium	6.3%
Germany	5.5%
France	4.1%
Finland	3.7%
US	3.6%
Japan	-3.2%
Mean	***9.5%***
Developing Countries	
Chile	13.4%
Korea	12.3%
Brazil	12.3%
Poland	8.5%
Mexico	5.9%
Colombia	4.7%
Slovak Rep.	4.3%
Czech Rep.	1.8%
Mean	***7.9%***

Source: Own elaboration based on UN National Accounts.

Data Annex

Variable	Source
Private Credit (Claims on Private Sector)	IMF - International Financial Statistics
Consumer Price Index	IMF - International Financial Statistics
Real GDP growth	World Bank - World Development Indicators
Bank Capital and Assets, OECD countries	Income Statement and Balance Sheet of the Banking System of OECD Countries
Household and Corporate Loans, OECD countries	National Accounts of OECD Countries - Financial Accounts Flows