Inequality, credit expansion and financial crises

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Abstract

In the three decades leading up to the financial crisis of 2008/09, income inequality rose across much of the developed world. This has led to a vigorous debate as to whether widening inequality was somehow to blame for the crisis. At the heart of this debate is the question of whether rising inequality leads to private sector credit booms, which are, in turn, widely accepted as a macroeconomic risk factor. Despite growing interest, empirical evidence on an inequality-fragility relationship is limited. That which does exist fails to tip the balance of evidence conclusively one way or the other. This research adds to this scarce body of evidence. Based on an econometric analysis of a panel of eighteen OECD countries covering the period 1970-2007, this study finds a statistically significant, positive relationship between income concentration and private sector indebtedness when controlling for conventional credit determinants. The implications of such a relationship are twofold. First, the view that the distribution of income is irrelevant to macroeconomic outcomes (implicit in mainstream economic thought) needs a second look. Second, if policy makers wish to make the financial system more robust, they should cast the net wider than regulatory and monetary policy reforms, and consider the effects of changes to the distribution income.

Keywords: Income inequality, Credit booms, Financial crises, Financial de-regulation

JEL Classification: D31, G01, E51

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1. Introduction

In the three decades leading up to the financial crisis of 2008/09, economic inequality rose across much of the developed world (Atkinson et al. 2011; OECD, 2011). This rise was nowhere more pronounced than in the United States – the country at the epicentre of the crisis – where, by the mid-2000s, income concentration reached magnitudes not seen since the period immediately preceding the Great Depression. Against this backdrop, a lively debate has re-emerged as to whether inequality may play, directly or indirectly, some destabilising role in the economy.

Central to this debate is the question of whether high or widening inequality contributes to the excessive accumulation of debt, which, in turn, is widely recognised as being the ultimate driver behind episodes of financial instability. The latter notion can be traced back to Fisher (1932; 1933) who argued that "all great booms and depressions" are ultimately caused by two dominant factors, *"[...] namely, over indebtedness to start with and deflation following soon after"* (Fisher, 1933, p. 341). Building on this view, Minsky (1975; 1982; 1986) placed the expansion of corporate debt at the heart of his financial instability hypothesis. He argued that an inherent feature of capitalist economies is the propensity for the financial system to swing between periods of extreme robustness and extreme fragility. Paradoxically, it is precisely the environment of economic prosperity and stability itself that shows the seeds of the ensuing financial collapse. During prosperous times, when corporate incomes are high (in excess of what is needed to pay off debt) a speculative euphoria develops and lending surpasses what borrowers can possibly repay from future incoming cash flows. The eventual result is widespread default, shortly followed by a liquidity crisis and asset price deflation. Lending contracts sharply and even those businesses that are creditworthy are denied access to finance, leading to a contraction in the real economy. Kindleberger (1978) tells a similar story, whereby a benign economic environment creates a sense of optimism for the future. As a result, asset prices rise, leading to yet further optimism. Key in this narrative is investors' use of credit to gain increased exposure to rising asset prices, driving prices up further. A self-reinforcing mania develops and profit expectations depart significantly from their fundamental potential, all the while debt to income ratios rise and capital ratios fall. The crisis reveals itself when something occurs to expose the true extent of this over-optimism, and a panic ensues. Asset prices crash as investors rush to liquidate their positions at the same time. A substantial body of empirical research, mostly developed in the last decade, has confirmed that episodes of financial instability are indeed precipitated by excessive levels of debt, in some form or other (see, for example, Borio and White, 2003; Mendoza and Terrones, 2008; Elekdag and Wu, 2011; Reinhart and Rogoff, 2009; Shularick and Taylor, 2012).

Accordingly, the investigation of the roots of financial instability needs to focus on the drivers of credit/debt expansion. The existing body of theories (see Mendoza and Terrones, 2008, for a review) provides explanations related to: herd behavior by banks (Kindleberger, 2000); information problems that lead to bank-interdependent lending policies (Rajan, 1994); the underestimation of risks (Borio, et. al., 2001) and the loosening of lending standards (Dell'Ariccia and Marquez, 2006); the presence of government guarantees (Corsetti, et. al., 1999); limited commitment on the part of borrowers (Lorenzoni, 2005); business cycles and financial accelerators (e.g., Bernanke et al., 1999; Kiyotaki and Moore, 1997). On the empirical side, the factors established as being key drivers of credit expansion include: deregulation of the financial system (Demirguc-Kunt and Detriagiache, 1998; Kaminsky and Reinhart, 1999;Ranciére, Tornell and Westermann, 2006; Decressin and Terrones, 2011); accommodative monetary policy (Borio and White, 2003; Elekdag and Wu, 2011; Mendoza and Terrones, 2008); rapid economic growth (Mendoza and Terrones, 2008); and inflows of foreign capital (Elekdag and Wu, 2011; Mendoza and Terrones, 2008; Decressin and Terrones 2011). The key question that this paper seeks to address is whether the redistribution of income should also be added to this list.

Despite a growing interest and theoretical debate into the inequality/credit/crisis relationship (see section 2), the empirical research is still scanty. The purpose of this paper is to add to this scarce evidence with an empirical model derived from a critical discussion of the existing literature. We are able to present the results of an econometric analysis for a panel of 18 OECD developed economies covering the period 1970-2007, which shows a statistically significant, direct, positive relationship between income concentration and private sector indebtedness, when controlling for other credit determinants. In the same sample, private sector indebtedness is shown, as a preliminary exercise, to increase the probability of financial crisis.

The contribution of the paper to the existing knowledge is twofold. On the conceptual side, besides providing a thorough review and discussion of the existing literature on the topic, we provide an organised view of the channels through which widening inequality favours credit expansion. On the empirical side, we complement the only cross-country evidence so far available, by Bordo and Meissner (2012), which is our main empirical inspiration and concluded for the inexistence of an inequality/credit nexus. However, we depart from this reference work in many instances, regarding: (i) the choice of the measure of credit (a broader, more inclusive metric); (ii) the estimation of the model in levels rather than in changes (assigning relevance to the amount of credit in relation to the size of the economy in determining the risk of a crisis); (iii) the explicit consideration of the threats posed by endogeneity and reverse causation issues; (iv) the explicit consideration of the institutional drivers of credit expansion (by means of an indicator of financial deregulation); (v) the consequent restriction of the time span of the analysis to the last four decades, even though with the addition of four countries. Our more limited time coverage is, however, not to be regarded as a major drawback, since it corresponds to the period in which credit started to remarkably decouple from broad money as a result of increased leverage and augmented funding via the nonmonetary liabilities of banks. A period in which most developed economies entered an age of unprecedented financial innovation, risk and leverage, which eventually undermined their stability (Schularich and Taylor, 2012).

The remainder of this paper is structured as follows. Section 2 reviews the literature on inequality, credit and financial fragility and provides the conceptual framework on which our empirical model relies. Section 3 describes the data, provides some preliminary descriptive evidence, and presents the methodology and the findings of the econometric model. Section 4 concludes.

2. Inequality, Indebtedness and crisis: theoretical explanations and empirical evidence

The notion that inequality may be linked to economic instability is not new. Writing on the causes of the Great Crash of 1929 and the ensuing Great Depression, Marriner Eccles, chairman of the Federal Reserve during that period, argued that:

> "a giant suction pump had by 1929-1930 drawn into a few hands an increasing portion of currently produced wealth. This served them as capital accumulations. But by taking purchasing power out of the hands of mass consumers, the savers denied themselves the kind of effective demand for their products that would justify a reinvestment of their capital accumulations in new plants. In consequence, as in a poker game where the chips were concentrated in fewer and fewer hands, the other fellows could stay in the game only by borrowing. When their credit ran out, the game stopped" (Eccles, 1951, p. 76, cited in Reich, 2010)

John K. Galbraith argued in similar vein in his best seller 'The Great Crash', wherein he highlighted the "bad distribution of income" as being the first of "five weaknesses [that] seem to have had an especially intimate bearing on the ensuing disaster" (1992, p.97; original edition 1954).

Following the onset of the global financial crisis of 2007-2008, interest in this notion has been rekindled, with a number of analyses suggesting that widening inequality may have played a key role in the recent crisis. These include a number of popular books (e.g. Rajan, 2010; Reich; 2010; Galbraith, 2012; Stiglitz, 2012; Palley, 2012), policy-focused papers (e.g. Stiglitz, 2009; IMF-ILO, 2010; UN commission of experts, 2012; Krueger, 2012), and opinion editorials penned by prominent economic commentators (e.g. Milanovic, 2009; Wade, 2010; Roubini, 2011). In addition to these somewhat informal contributions, there is also a small (but growing) body of academic research that has attempted to more formally analyse the relationship empirically and theoretically (e.g. Atkinson and Morelli, 2010, 2011; Kumhof and Rancière, 2010; Fitoussi and Saraceno, 2011; Kumhof *et al.*, 2012; Tridico, 2012; Bordo and Meissner, 2012; van Treek, 2013).

Rajan's book 'Fault lines' (2010) contributed much of the momentum to the current debate. Argues that rising inequality in the U.S. pressured governments of all political persuasions to enact policies aimed at improving the lot of those low- and middle-income voters being left behind. However, in the polarised world of American politics, the usual recourse of governments in such circumstances – the redistribution of income via taxes and social spending – is politically toxic. Instead successive governments chose to placate those voters by enacting policies that would expand their access to credit—a solution that attracted far less political attention, and hence was far more palatable to both sides of the political divide. These policies included the deregulation of credit markets and the encouragement of state-owned mortgage agencies to expand lending to low-income households. This created a glut of credit, which households obligingly guzzled as a substitute for rising incomes as they sought to attain the standard of living they had come to expect. The resulting credit bubble laid the foundations for the subsequent crisis.

However, Acemoglu (2011) suggests that this explanation may misinterpret the true cause and effect relationship. He posits an alternative hypothesis whereby – instead of rising inequality pressuring a political response that then causes a crisis – it was politics that drove both inequality and the financial crisis. There is concomitance, not causation. Citing evidence from Bartels (2008) and Gilens (2005), Acemoglu argues that the policies of politicians over the period in question were, in fact, more closely aligned to the preferences of a minority of high-income voters than they were to the preferences of the majority of low and middle-income voters. Instead of redistributive policies favouring low- and middle-income constituents, politicians implemented financial deregulation policies favouring influential high-income constituents (many of whom worked in, or directly benefited from, the financial sector).

Key to this narrative is the political influence that wealth brings. Yet, Acemoglu does not link rising income concentrations to increased political influence amongst the affluent (perhaps he leaves this for his audience to infer) despite this being a central tenet of Bartels's study, on which his argument is founded. With the addition of this component, inequality may once more be seen to lead to increased financial instability through a self-reinforcing process: 1) rising inequality leads to increased political influence amongst the wealthy; 2) this is used to promote policies of financial market deregulation; 3) this leads to both financial instability and rising inequality; and back to1). The possibility of higher income concentrations affording those at the top of the distribution greater political influence with which to promote policies of financial deregulation in the pursuit of personal interest is also explored by Krugman (2012). The key difference between this hypothesis and Rajan's hypothesis, then, is the end of the distribution from which deregulatory pressure comes.

To summarise, the above narratives suggest that widening income disparities in the U.S. led to financial instability via the political process as a result of increased pressure on politicians to enact polices of financial deregulation. Depending on the account, this pressure came from opposing ends of the distribution of income. From the bottom, growing discontent pressured politicians into providing low- and middle-income households with access to credit as a substitute for rising household income. From the top, an increasingly wealthy – and hence increasingly influential – elite pressured politicians into liberalising financial markets for their own personal gain.

The theories outlined above relate specifically to recent events in the United States. It is the interplay between rising inequality and U.S. politics that caused the credit boom and the subsequent crisis, with the deregulation of financial markets being an intermediate output. This indirect explanation is therefore compatible with the US experience only and there is no suggestion that the relationship should hold in a different place or time. Alternative, more general lines of reasoning are therefore needed to explain the mechanism via which rising inequality might be linked to an abnormal increase in household indebtedness.

In the spirit of Kalecki (1942) and Kaldor (1955), Stiglitz (2009; 2012) and Fitoussi and Saraceno (2010; 2011) argue that rising income inequality in run-up to the crisis redistributed income from households with a high propensity to consume to those with a low propensity to consume, weighing on consumption expenditure and supressing aggregate demand. The policy response from modern inflation/output targeting central banks was to loosen monetary conditions to support demand. This propped up consumption for a while, but it could not go on forever; private sector debt eventually reached unsustainable levels and the credit bubble burst. Adding strength to this under-consumption argument is the empirical evidence on the cross-sectional relationship between relative income (i.e. position within the income distribution) and the marginal propensity to save, which tends to find a positive correlation (see e.g. Dynan, Skinner, and Zeldes, 2004).

All preceding explanations are political economy ones, in the sense that higher income inequality does not, *per se*, result in higher levels of indebtedness and increased financial fragility. Rather, in certain institutional settings, high levels of inequality provoke a political or monetary response, which are then responsible for the expansion of credit, the excessive build up of debt, and the corresponding financial instability. Although intellectually appealing, these approaches appear of a limited explanatory power, since policy actions are always the complex outcome of a convergence of economic, political and social forces; reducing them to be primarily driven by inequality patterns seems therefore an over simplification.

An alternative theory is that a more direct link between inequality and indebtedness (and hence risk of crisis) exists, i.e. one that does not rely on very specific institutional arrangements, and so holds in a more general sense. Taking a more formal approach than those discussed above, Kumhof and Rancière (2010) develop a closed economy DSGE model in which a financial crisis is the endogenous result of rising income inequality. They take as stylised facts the correlation between rising income inequality and credit growth in the US, in both the periods preceding the 1929 market crash and in the run up to recent financial crisis. They argue that in both instances credit growth was an equilibrium outcome. When the model is calibrated to US data, simulations show how increased income inequality can endogenously lead to credit growth, higher leverage and increased probability of a financial crisis. The model has at its heart two classes of economic agent: investors (defined as the top 5% of earners) who own all of the capital, earn only capital income, and save and invest as well as consume; and workers (everyone else) who earn only wage income and use this only for consumption. A key assumption is that workers have some minimal consumption level that they must attain, which is a function of some previously attained level of consumption, and that they will turn to credit markets, which are assumed to be perfect, if necessary in order to attain this. When a shock reduces the bargaining power of workers relative to investors, the workers, faced with declining real wage growth, borrow in order maintain their desired level of consumption. On the other side of the transaction, investors lend to the workers out of their rising incomes via financial intermediaries, which they own. As inequality increases, workers become increasingly indebted to investors, who amass claims on them. The saving and borrowing behaviour of these two groups leads to increased demand for financial intermediation, and the size of the financial sector grows in relation to the rest of the economy. All this while, leverage of the household and financial sector increases, thus increasing the probability of a financial crisis. A key feature of this model is that consumption inequality rises much more slowly than income inequality due to the substitution of loans for income at the bottom of the distribution. This is consistent with documented trends concerning the relative evolution of income and consumption inequalities in the U.S. and elsewhere in the run-up to the recent crisis (e.g. for the U.S. see Kruger and Perri, 2006; for the UK see Blundell and Etheridge, 2010; for Canada see Brzozowski et al., 2010; and for Italy, see Jappelli and Pistaferri, 2009)

Thus, in this theoretical framework an increase in income inequality leads to both an increase in supply of credit from those at the top of the distribution, and an increase in demand for credit from those at the bottom of the distribution. On the supply side, higher income inequality favours savings (and thus credit availability) due to the rich having a higher propensity to save (in this model the bottom 95% do not save at all). Such a transmission mechanism, from increasing income concentration at the top of the distribution to increased availability of household credit, is also proposed by Fitoussi and Saraceno (2010; 2011) and Milanovic (2009). The latter argues that rising inequality in the U.S. led to vast accumulations of wealth at the top of the income distribution, which led to a glut of funds seeking profitable investment. The financial sector, overwhelmed by the volume of funds seeking investment relative to profitable opportunities in the productive sector (but ever keen to earn the associated transaction fees, nonetheless) became increasingly more inventive and reckless, "basically throwing money at anyone who would take it" (Milanovic, 2010, p.194).

On the demand side, higher inequality causes those lower down the income distribution to borrow more in order to maintain consumption expenditure as their income falls. This finding is consistent with previous U.S. based studies on the relationship between income inequality and household debt by Iacovello (2008) Blundell, Pistaferri, and Preston (2008) and Krueger and Perry (2006). However, a key point of contention is the extent to which observed increases in measured income inequality reflects widening dispersion of permanent (i.e. lifetime) income or widening dispersion of transitory (i.e. current period) income. Kruger and Perry find that income inequality increased significantly over the period 1980 – 2003, both between and within groups of households with the same characteristics (e.g. age, gender, and race) and that, whilst between-group income and consumption inequality followed a similar path, within-group consumption inequality rose much more slowly than between-group consumption inequality. They argue that the increase in within-group inequality can be explained by increased volatility of idiosyncratic labour income (i.e. increased *transitory* income dispersion), which increased demand for credit for insurance. This view fits comfortably within mainstream modelling frameworks based on the permanent income and lifecycle models (Friedman, 1957; Modigliani and Brumberg, 1954). In their strictest form, these hypotheses state that (under certain assumptions) a household's consumption expenditure in a given period will be an annuity of its permanent income, irrespective of fluctuations in transitory income. Any deviation of temporary income from this annuity will be smoothed via recourse to credit markets. In a good year, a household will save any excess; in a bad year, they will borrow to cover any shortfall. However, whilst this paradigm allows that increased variance in transitory income might lead to higher borrowing, it denies the possibility of rising inequality of permanent income leading to higher borrowing—any change in permanent income would simply cause a corresponding change in consumption. Yet in Kumhof and Ranciere's model, households at the bottom of the income distribution borrow to maintain consumption after a shock effects the distribution of *permanent* income. Moreover, insofar as the U.S. experience is concerned, recent empirical evidence strongly suggests that the observed rise in measured income inequality in recent decades has been predominantly driven by increased dispersion in permanent income, with increased variability of transitory income playing a much smaller role, if any (see e.g. Kopczuk, Saez and Song, 2010; Debacker et al., 2013). Van Treek (2013, p.10) argues that in order to properly understand the effects of the distribution of permanent income on consumption and savings decisions, the permanent income hypothesis must be abandoned in favor of the lesser known relative income hypothesis, originally formulated by Duesenberry (1947). This hypothesis posits that a household's consumption expenditure in a given period is a function of some previously attained maximum level of consumption expenditure, and of the consumption expenditure of reference households. Thus, this theory explicitly allows for a relationship between the redistribution of permanent income and aggregate consumption behavior. As permanent income is redistributed in favour of those at the top of the distribution, those households that have been affected negatively will run down savings or dissave in an attempt to maintain consumption in proportion to some previously achieved level, and in an attempt to emulate the consumption expenditure of other reference households who benefitted positively from the redistribution. The latter channel is emphasized by Frank *et al.* (2010) who build a theoretical model of consumer behaviour with the concept of relativity of consumption at its very foundations. They argue that rising inequality leads to expenditure cascades, "whereby increased expenditure by some people leads others just below them on the income scale to spend more as well, in turn leading others just below the second group to spend more, and so on". In support of this hypothesis, they present empirical evidence of a positive relationship between the level of income inequality in the 100 most densely populated U.S. counties and three different measures of financial distress. Bertrand and Morse (2013) provide further empirical support via an econometric analysis of the relationship between the consumption of "non-rich" households (those below the 80th percentile of the income distribution) and the income and consumption of the "rich" (percentile) within US states in a given year. Their key finding is that the consumption expenditure and the income of rich households within each US state in a given period of time is a significant predictor, both statistically and in a material economic sense, of the consumption expenditure of non-rich households within that state, holding the income of those middle-income households constant. According to their results, a 1 percent rise in the consumption expenditure of rich households increases the consumption expenditure of non-rich households by 0.18 percent. As a next step, they attempt to test the extent to which the permanent income hypothesis can explain these findings, given the possibility that, in states where top income levels are higher, non-rich households may rationally expect their incomes to rise in future thus increasing their present-day consumption. They do this by regressing future non-rich household income on rich-household income (along with various other control variables) and but in no specifications do they find the relationship to be significant.

Whilst theoretical explanations of an inequality-crisis relationship abound, empirical evidence in the relationship is limited. Kumhof *et al*. (2012) build on Kumhof and Rancière (2010) by opening up the model to the international sector. They begin by documenting the simultaneous rise in inequality, current account imbalances, and household indebtedness across the globe. To the original closed economy model, they add foreign agents who both work and invest. As before, following a bargaining shock that causes the income share of workers to decline at the expense of investors, the latter react by lending a portion of their increased income back to workers who seek to maintain their relative consumption. In an open economy, investors also profit from being able to intermediate the savings of foreigners to domestic workers. Calibrating the model to UK data, simulations show that increased inequality endogenously leads to credit expansion, increased leverage and increased current account deficits, which in turn increase the probability of a systemic financial crisis. The model is also calibrated to investigate the situation where the policy response to rising inequality is financial deregulation (as per the "Thatcher years") finding that, although this helps to smooth workers' consumption in the short-run, this comes at the expense of even higher household indebtedness and higher debt service payments—resulting lower consumption in the long run. Another key effect of introducing deregulation is to encourage investors to channel much more of their additional income into financial investments over real investments. This further stimulates aggregate demand whilst at the same time constraining aggregate supply by slowing down capital accumulation. As a complement to this theoretical model, Kumhof and co-authors also conduct an econometric analysis using a panel of 18 OECD countries over the period 1968-2006. They find that income concentration (measured by the top 1% and top 5% income share) is a statistically significant predictor of external deficits. For example, a one percentage point rise in the share of income going to the top 1% results in a deterioration of the current account by 0.6% of GDP. They note that a coefficient of this magnitude aligns broadly with the experiences of the UK over the last thirty years.

Using data from 25 countries over the period 1911-2010 Atkinson and Morelli (2010, 2011) look for patterns of rising inequality in advance of 'systemic' banking crises (as defined by a combination of sources: Laeven and Valencia, 2010; Bordo *et al*., 2001; Reinhart and Rogoff, 2008, 2009; and Reinhart 2010). Consistent with Rajan's hypothesis, they find significant increases in income inequality in the US prior to both the 1929 crash and the recent financial crisis. However, they find that this pattern is far from universal. Whilst a number of the banking crises in their sample were preceded by notable increases in income inequality (measured in terms of Gini coefficient and top income shares), many more were not. They conclude that banking crises vary a great deal in their nature and causes, and that, even if their analysis did reveal consistently rising inequality prior to banking crises, "causality is not easy to establish" (2011, p.49). However, they also highlight that their methodology focuses solely on *changes* in inequality, and so is silent about the effect of *levels* of inequality on financial fragility – an avenue they earmark for further research.

Another study that contests the universality of a link between income inequality and crises is that of Bordo and Meissner (2012). They conduct an econometric investigation into the relationship using a panel of 14 mainly advanced countries from 1920 to 2008. In the first stage of their research, they investigate the link between credit growth and financial crises using a series of logit regressions (with a binary dependent variable for financial crises). They find a statistically significant, positive relationship between credit growth (over five-year periods) and the occurrence of a financial crisis, in line with the existing literature on credit growth and crises. In the second stage of their research, they use a series of fixed effects OLS regressions to investigate the determinants of credit growth. As a dependent variable they take the five-year change in annual bank lending and regress this on the five-year change in the top 1% income share and various additional determinants of credit growth that are theoretically and empirically linked to credit growth in the literature. In various specifications of the model, they fail to reject the null hypothesis that growing inequality has no statistically significant relationship with credit growth. Based on these findings they reject what they call the 'RKR' (Rajan, Kumhof and Renciére) hypothesis, in favour of the more traditional determinants of credit growth and crises.

3. Inequality, Deregulation and Credit: Empirical Analysis

The variety of possible interpretative frameworks just summarised suggest different reasons underpinning the cause/effect relations between inequality, policy variables, indebtedness and the outburst of a crisis. Consistent with the most general explanations of the inequality/credit growth link discussed in the previous section, our objective here is to provide empirical evidence on the existence of a direct relationship between income inequality and the size of credit once other possible drivers, including deregulation of financial markets, are accounted for. We therefore test here empirically the idea of inequality and deregulation driving independently credit growth and whether the effect, if any, of inequality is reinforced or not by increasing deregulation of financial markets. Our approach shares many similarities with and is mainly inspired by Bordo and Meissner's (2012) study; however, it significantly departs from it in terms of methodology, time/country coverage and variables used. This offers new cross-country empirical evidence and additional ground for discussion on the topic.

Our research objective poses several issues on both the empirical and the econometric side. First of all, as usual when dealing with inequality in a panel dimension, gathering the information needed for the dataset is a challenging task. For the reasons explained in section 3.1 we limit our dataset to 18 OECD countries, over the period 1970-2007. Compared to Bordo and Meissner's study, we therefore have more cross-country observations but a shorter time dimension. This assures higher comparability/homogeneity of data and better availability of explanatory variables, while allowing to focus on the period in which major developments of interest here (particularly the increase of inequality and the extensive wave of deregulation) took place. This comes at the cost of a shorter run perspective which, however, (i) would have been based on very fragmentary and heterogeneous empirical materials; and (ii) would be not able to considere (as in B&M) and control for the major institutional changes occurred on the side of financial markets, due to lack of data before the '70s.

A second major point to be clarified, related to the empirical model specification dealt with in section 3.2, is that a credit expansion, although regularly preceding and determining the conditions of a financial crisis, is *per se* not necessarily negative for the economy, when it is driven by factors related to the real economy or to the *normal* developments of macroeconomic aggregates. For this reason, we need to analyse the relationships of our interest in the framework of a more general model of credit drivers, derived on the basis of the relevant literature. Once other potential factors are controlled for, we are able to isolate the effects of other drivers of credit growth (particularly, inequality) potentially conducive to financial instability. A third group of problems that need to be addressed, also dealt with in section 3.2, relates to the complexity of the relationships among the variables considered, which are far from being univocally determined. On the one side, a typical problem of endogeneity related to potential reverse causality exists between credit growth and the factors used in the model as its drivers. As the literature summarized in the following subsection emphasises, they include the investment rate, GDP growth and the level of development, besides inequality. On the other side, our explanatory variables of main interest might not be independent each other. Rather, a causality link has been hypothesised both from inequality to deregulation (as in Rajan's explanations) and *vice versa* (as in Acemoglu's view); similarly, rising inequality could drive monetary expansion (Stiglitz's hypothesis), which is also obviously in the set of regressors. All these aspects are accounted for by proper econometric treatments, namely by instrumenting the potentially endogenous variables. On the descriptive side, the direction of causality has been preliminarily tested by means of the usual Granger analysis (section 3.2).

The remainder of this section goes on to discuss the data (section 3.1), before presenting some preliminary descriptive evidence (3.2), the econometric models (3.3) and our findings (3.4).

3.1. Data and variables

Our analysis exploits an unbalanced panel of annual data from eighteen OECD economies[4] over the period 1970-2007. This represents a shorter time period compared to B&M and is dictated by the choice of variables used, which it is argued are conceptually more appropriate. Despite the shorter timeframe, this panel covers the key period of interest in which income inequality, deregulation and household indebtedness rose in tandem across much of the developed world.

The dependent variable used in all model specifications is the level of domestic credit to the private sector as a percentage of GDP, from World Development Indicators database (World Bank, 2012), which includes credit from banks and other financial institutions. This is in contrast to B&M, who use the log of real bank loans to the private sector. Conceptually, it can be argued that the measure used here is preferable because focusing on bank credit only can be misleading in our context here. As Elekdag and Wu (2011) maintain, the choice of the credit aggregate is important when attempting to understand financial fragility. A variable that includes credit extended by non deposit-taking institutions is preferable, as credit booms can arise owing to funds provided by these institutions, especially in periods of high rates of financial innovation and deregulation, as it is the one under scrutiny here. The choice of considering the amount of total credit (as a % of GDP) in levels, rather than in terms of changes (as in Bordo and Meissner, 2012), is motivated by the fact that all the literature emphasises how it is the *excessive* credit available in the economy that leads to financial crisis. On the contrary, whether higher rates of credit growth lead to a financial crisis or not

[4] To the 14 countries considered by Bordo and Meissner (2012) (Australia, Canada, Denmark, France, Germany, Italy, Japan, Netherlands, Norway, Spain, Sweden, Switzerland, UK, US), we add Portugal, Finland, Ireland and New Zealand.

depends on the initial level of credit available in the economy, since the same growth rate might translate into very different levels of credit and risk. Also, increasing debt levels do not necessarily lead to instability *per se*: other things equal, credit growth accompanied by similar income/productivity growth need not entail increased macroeconomic risk. Similarly, unchanged debt levels may involve increased macroeconomic risk if income/productivity is falling. This is the reason why it is also necessary to control for per capital GDP levels and growth, besides standardising credit on GDP.

There is one very obvious limitation to both of the credit measures discussed above. This is the fact that they comprise household debt (in which we are interested) *and* the debt of businesses and other private organisations (in which, strictly speaking, we are not interested). To get around this, as per Bordo and Meissenr (2012), a measure of gross fixed capital formation in % of GDP is also included as an additional independent variable in all regressions to account for credit extended to the non-household private sector and demanded by the productive system. Similarly, in the framework of a truly Keynesian investment function, the inclusion of GDP growth serves as an additional control for credit demand driven by the expectations of firms about future levels of demand.

As a proxy for income concentration and inequality more generally, we follow B&M in using the share of total income going to the top 1% of earners, from the World Top Income Database. This data has been obtained from historical income tax records by several different teams of researchers following the methodology of Piketty (2001). Observation units are individuals, households, or tax units (depending on the country) and income includes labour, business and capital income. In some cases, the income concept also includes realised capital gains. As a result there are some cross-country comparability issues (although small). Some comparability issues may also exist over time resulting from changes to tax legislation (Atkinson et al., 2011). Aside from comparability issues, it should be noted that there are also a number of conceptual shortcomings of this indicator as a measure of income concentration and income inequality. For example, the income concept is market income (i.e., pre tax and transfers), whereas in this context it would be preferable to use disposable income (i.e., post tax and transfers), which bears more significantly on household/individual budget constraints and thus on consumption, investment and borrowing decisions. However, this indicator provides an excellent insight into income concentration at the very top of the distribution and it is particularly suitable to represent the side of inequality we consider important here (related to the ideas of relative income and consumption cascade effects as drivers of credit); by its own nature it offers little information as to what is happening at the bottom of the distribution, except for the fact that, if the income share of the top 1% rises, the income share of the remaining 99% must perforce fall. This shortcoming of offering a partial view on the distribution is common to all measures of inequality and empirical evidence shows top income shares to be strongly correlated with broader inequality measures, such as the Gini coefficient (e.g., Leigh, 2007). The top 1% was also chosen since it guaranteed greater coverage and comparability across countries and over time and to the precedent in the existing inequality-crisis literature (e.g., Bordo and Meissner, 2012; Kumhof et al., 2012).

The third key variable of our analysis is the indicator of Credit Market (de)regulation (code 5A) supplied by the Fraser Institute in the Economic Freedom of the World database (Gwartney et al., 2010). The measure, ranging from 0 to 10 in ascending order of deregulation, is a summary indicator of four dimensions related to: (i) ownership of banks (measured as the % share of deposits held in private banks); (ii) foreign bank competition (computed on the rate of denial of foreign bank applications and of their share of the banking sector assets); (iii) private sector credit (as a proxy of the extent to which government borrowing crowds out private borrowing; (iv) interest rate controls/negative interest rates (based on the extent of credit market controls, market determination of interest rates, stable monetary policy, positive real deposit and lending rates). The summary indicators has been extensively used in the existing empirical literature (e.g., Giannone et al., 2011, Dawson, 2006; Stankov, 2012;); the alternative credit market regulation index by Abiad et al. (2008), also widely employed (e.g., Mendoza and Terrones, 2008; Azzimonti et al., 2012), is more limited over time (1973-2005) and would remarkably reduce the number of observations especially in the most recent years, of great interest here. An extensive literature has emphasis that deregulation of financial markets triggers credit expansion, for example due to increased aggregate supply of financial instruments and credit (Bordo and Meissner, 2012), to the consequences of increased competition (Gosh, 2010; Dell'Ariccia and Marquez, 2006), to the emergence of implicit guarantees (Gourinchas at al., 2001; Corsetti et al., 2009), to the increase in opportunistic behaviours by bankers (Demirguc Kunt and Detragiache, 1998).

Other variables used in various specifications as controls and identified on the basis of the existing literature are, besides the proxy for investments already described, a measure of portfolio investments (as a % of GDP) to control for credit demand driven by transactions in equity and debt securities, including external liabilities (except those constituting foreign authorities' reserves). This variable also accounts for financial capital inflows usually considered in the literature as related to credit growth (Mendoza and Terrones, 2008; Elekdag and Wu, 2011). An alternative option to include (again indirectly) this information was to use a measure of current account balance (as a % of GDP), which however never turn out significant in the estimates and was therefore excluded to keep the model as parsimonious as possible. Other two variables, real interest rate (lending rate adjusted by the GDP deflator) and broad money supply (M2 over GDP), act as proxies for the monetary policy environment[5]. As emphasised by Elekdag and Wu (2011, p. 9), the interest rate alone may not be able to accurately represent the level of global liquidity at all times, especially more recently due to nonconventional monetary policies. To address this issue, we follow their recommendation of complementing the interest rate series by a metric of broad money supply. The use of a lending rate (in the place of a policy rate) allows including in the analysis the complexity of institutional arrangements on the financial markets which shape cross-country differences in interest rate pass-through effects (e.g., ECB, 2009; Cottarelli and Kourelis, 1994); in the use of a broad concept of money supply we

[5] An alternative, indirect approach was to include variables emphasising the role of monetary stabilisation programs, such as the rate of inflation or a real exchange rate (Gaouringhas et al., 2001). These two variables, included in the model, did not turn out significant and were excluded due to multicollinearity problems they generated.

follow Schularick and Taylor (2012). All the variables just described are from the World Bank WDI database.

Many studies find that the overall level of economic development, often measured by per capita income or income growth measures, is the strongest predictor of financial progress and credit availability (see, for example, Adarov and Tchaidze, 2011, and the many references cited therein). We therefore include among the regressors the level of real per capita GDP and the annual GDP growth rate (both from: Angus Maddison - Statistics on World Population), which also act as further controls for the pro-cyclicality of credit (Borio et al., 2001). This is, in facts, already accounted for by standardizing the amount of credit on GDP; however, as emphasised by Mendoza and Terrones (2008), this measure can ambiguously represent credit expansion, since its growth might be simply due to a GDP decline, with credit being constant. Including controls for GDP level and growth contributes addressing this issue.

3.2 Preliminary and Descriptive Evidence

As a preliminary step to the main empirical model, we provide evidence on the link between credit expansion and the outburst of financial crises. Although credit expansion might be due to financial deepening shown to support growth (Levine, 2005) or normal cyclical upswings, rapid credit growth episodes are typically associated with growing financial imbalances, and tend to end abruptly, often in the form of financial crises (Elekgag and Wu, 2011). As emphasised in the previous sections, an extensive literature agrees on the existence of this relationship, which is extensively confirmed empirically. Following Bordo and Meissner (2012) and Schularick and Taylor (2012) we explore this relationship in our database for the years and countries considered here by estimating the probability of a banking crisis as a function of credit expansion in the following form:

$$\Pr(banking\ crisis) = f\left(Cred_{i,t} + \alpha_i + \tau_t + \varepsilon_{i,t}\right) \qquad [1]$$

where subscripts *i* and *t* refer to countries and years, respectively (*i = 1, ..., 18; t = 1970, ..., 2010)*; α_i and τ_t are the country and the time specific effects and ε_{it} is an idiosyncratic error term for each country and each period. The dependent variable is coded as binary indicator equal to 1 when a banking crisis occurred according to Laeven and Valencia (2013) and zero otherwise; the explanatory variable *Cred* describes the amount of credit over GDP as described in the previous sections.

Figure 1 provides graphical evidence for a selection of countries for which the relationship is particularly clear-cut (the dotted vertical lines identify banking crises episodes).

Figure 1. Private credit/GDP and crises: select countries, 1970-2010

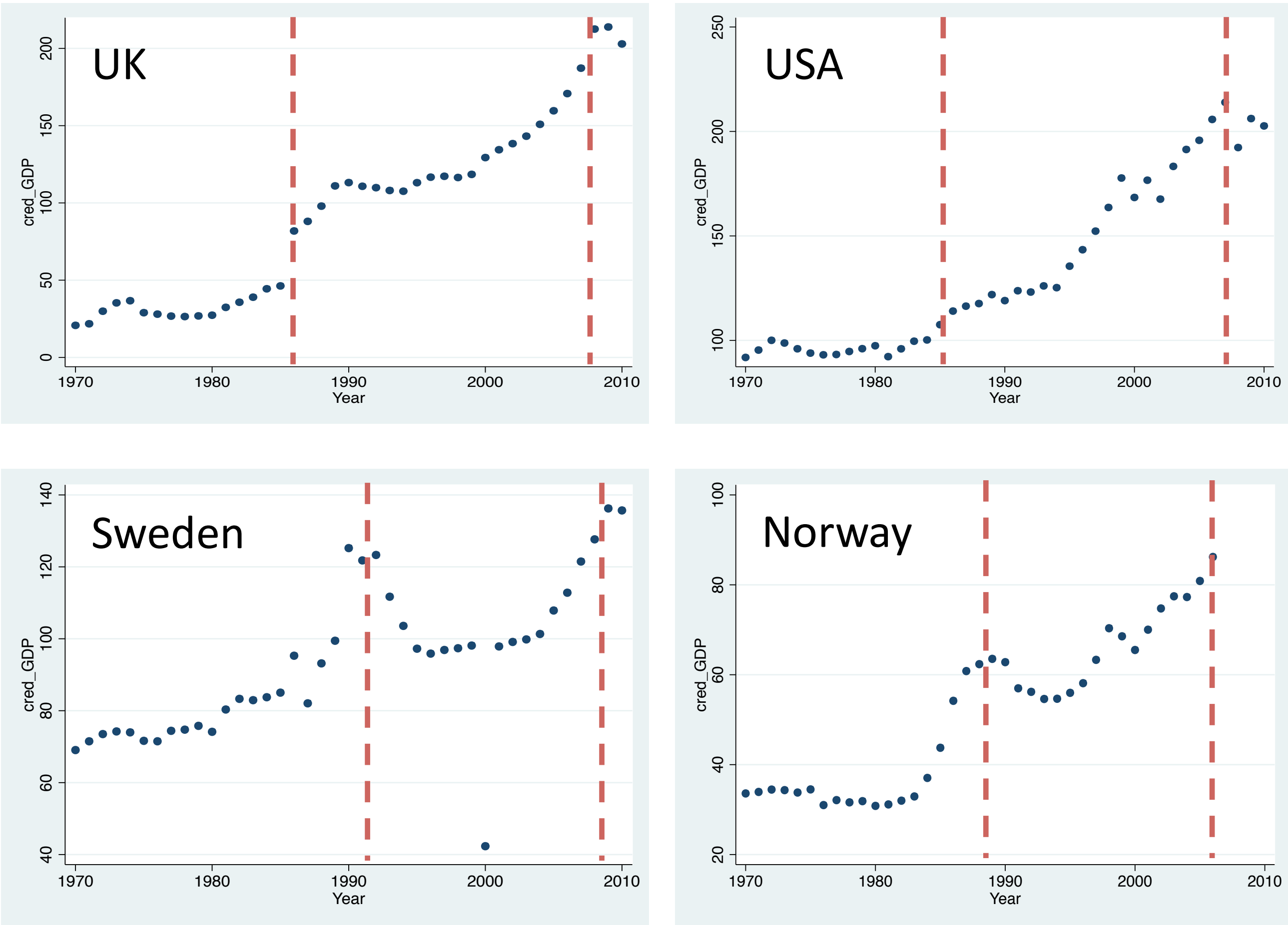


Table 1 presents the outcomes, which confirm the existence of a strong, positive, statistically significant and robust relationship between the amount of credit available and the probability of a banking crisis. These results obtained from our sample, which are consistent with the previous literature, allow and encourage us to focus the attention on the fundamentals drivers of credit expansion, particularly to those of our interest.

Table 1. Banking crisis and credit expansion, 1970-2010

Dep. Var.: Banking crisis	Logit; RE (1)	Logit; RE (2)	Logit; FE (3)	Logit; FE (4)
cred_GDP	0.034*** (7.12)	0.036*** (3.80)	0.040*** (7.42)	0.046*** (3.86)
Const	-6.489*** (-8.80)	-28.714 (-0.00)	- -	- -
Time dummies	No	Yes	No	Yes
Wald test (RE) / LR test (FE)	50.73***	-	103.80***	213.27***
N observations	719	719	599	599

Notes:
The dependent variable is a dummy equal to 1 when a banking crisis occurred according to Laeven and Valencia (2013)
*Z statistics are based on robust standard errors and reported in brackets. * p<0.10, ** p<0.5, *** p<0.01*

Table 2 provides basic descriptive statistics on them showing that the key variables provide acceptable levels of variability both over the cross-section and the time dimension. The data availability on inequality poses the most severe constrain to the analysis

Table 2. Descriptive statistics

Variable		Mean	Std. Dev	Min	Max	Observations	
cred_GDP	overall	87.266	44.339	12.767	227.753	N	670
	between		31.871	51.708	166.951	n	18
	within		32.387	23.008	222.120	T-bar	37.222
Ineq (top 1%)	overall	7.576	2.466	3.490	18.290	N	531
	between		1.826	4.263	11.706	n	18
	within		1.721	3.611	17.671	T	29.5
Dereg (cred mkt)	overall	8.231	1.151	4.422	10.000	N	684
	between		0.883	6.076	9.280	n	18
	within		0.766	5.821	10.328	T	38
cap_form_gdp	overall	22.567	3.963	15.312	36.372	N	673
	between		2.799	18.174	28.753	n	18
	within		2.867	15.263	34.185	T-bar	37.389
portf_inv_gdp	overall	-4.335	67.516	-575.262	598.181	N	661
	between		25.388	-82.258	22.129	n	18
	within		62.805	-538.388	609.322	T-bar	36.722
M2_gdp	overall	85.751	43.493	18.589	238.975	N	683
	between		37.389	51.092	169.731	n	18
	within		23.857	-1.560	242.541	T-bar	37.944
real_int_rate	overall	4.240	3.963	-13.184	19.259	N	676
	between		1.883	1.781	7.629	n	18
	within		3.514	-11.065	16.257	T-bar	37.556
Real_gdp_growth	overall	0.027	0.022	-0.076	0.110	N	684
	between		0.008	0.015	0.049	n	18
	within		0.021	-0.067	0.103	T	38
pc_gdp (ln)	overall	9.664	0.311	8.608	10.353	N	684
	between		0.200	9.184	9.999	n	18
	within		0.243	9.001	10.547	T	38

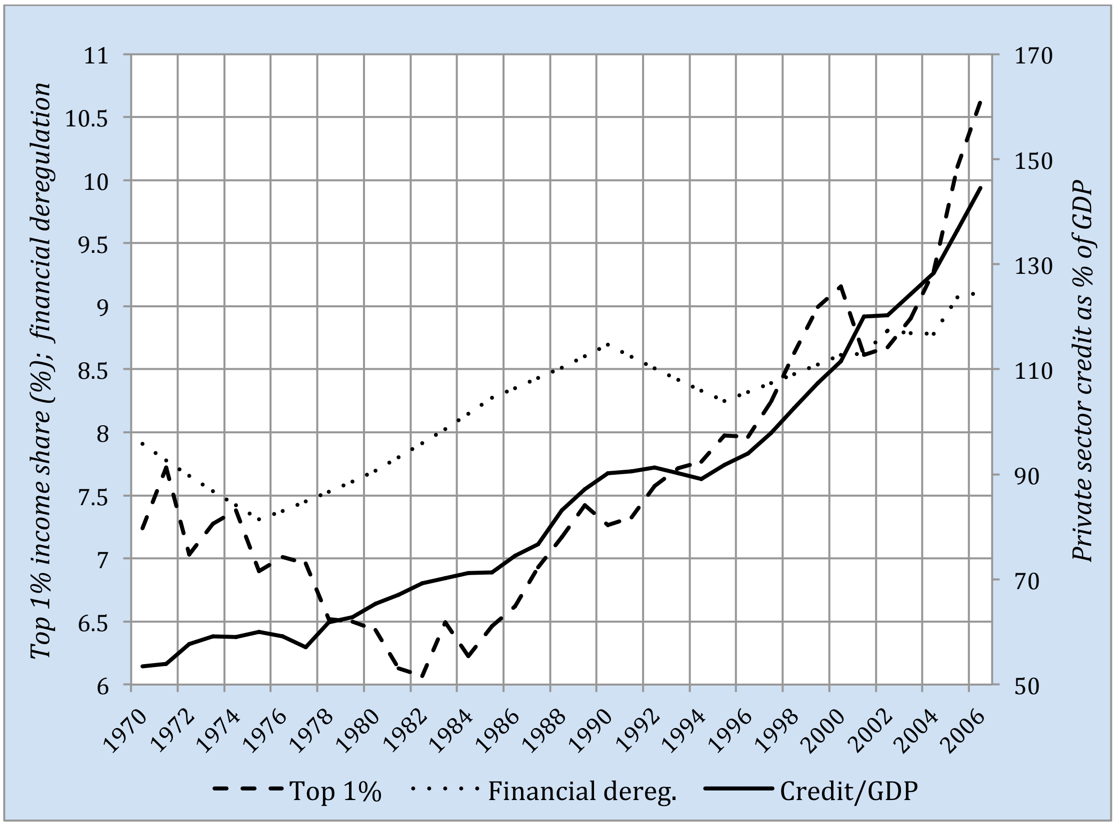
Table 3. Correlation analysis

	cred_GDP	Ineq (top1)	Dereg (cred mkt)	cap_form_gdp	portf_inv_gdp	M2_gdp	real_int_rate	pc_gdp (ln)	Real_gdp_growth
cred_GDP	1								
Ineq (top1)	0.5030*	1							
Dereg (cred mkt)	0.2696*	0.1828*	1						
cap_form_gdp	-0.1109*	-0.2299*	-0.3692*	1					
portf_inv_gdp	-0.0788*	0.0603	-0.0465	0.0910*	1				
M2_gdp	0.6931*	0.2903*	-0.0795*	0.0734*	-0.0327	1			
real_int_rate	-0.1625*	-0.1096*	0.2265*	-0.1976*	0.1396*	-0.1873*	1		
pc_gdp (ln)	0.5832*	0.3745*	0.5783*	-0.3839*	-0.1756*	0.1795*	0.1706*	1	
real_gdp_growth	-0.1616*	0.0345	-0.0125	0.0357	-0.0097	-0.0895*	-0.1500*	-0.2150*	1

**: significant at the 1% level or better*

The simple correlation analysis shows preliminary support to the existence of a remarkably strong link between credit expansion and the remaining variables, especially inequality, monetary policy and per capita GDP. The relationship between broad money supply and the interest rate, although negative and statistically significant, is low, providing evidence of the inability of the interest rate alone to properly depict the monetary policy environment (Elekdag and Wu, 2011). Figure 2, plotting the yearly average of credit, inequality and financial deregulation, for the 18 countries of our sample, also provides preliminary support to the idea of a co-movements between the variables of key interest here.

Figure 2. Private credit as % of GDP, Top 1% income share, and financial deregulation: mean of 18 OECD countries 1970-2007



This particularly holds after the turbulent 70s, when all three indicators rose sharply until a period of stabilization in the first half of the 90s and then kept rising again. The positive inequality/credit relationship is particularly straightforward for some countries, as illustrated in the scatterplots in Figure 3.

The correlation among the remaining variables, then used in our empirical model as drivers of credit growth, is instead relatively low. In particular, the weak links between inequality, credit market deregulation and monetary policy variables are of interest here, since they are related to interpretative frameworks emphasising the role of political economy meachanisms. Results of Granger causality analysis (Granger, 1969), although not conclusive due to their own nature and limitations, tend to confirm that the empirical evidence is far from being supportive to these interpretations (Table 4). Outcomes reveal that the hypothesis of deregulation Granger-causing inequality (as per Acemoglu's conjecture), or of inequality Granger-causing deregulation (Rajan) and monetary expansion (Stiglitz), cannot be accepted.

Figure 3. Private credit/GDP and inequality: select countries, 1970-2010

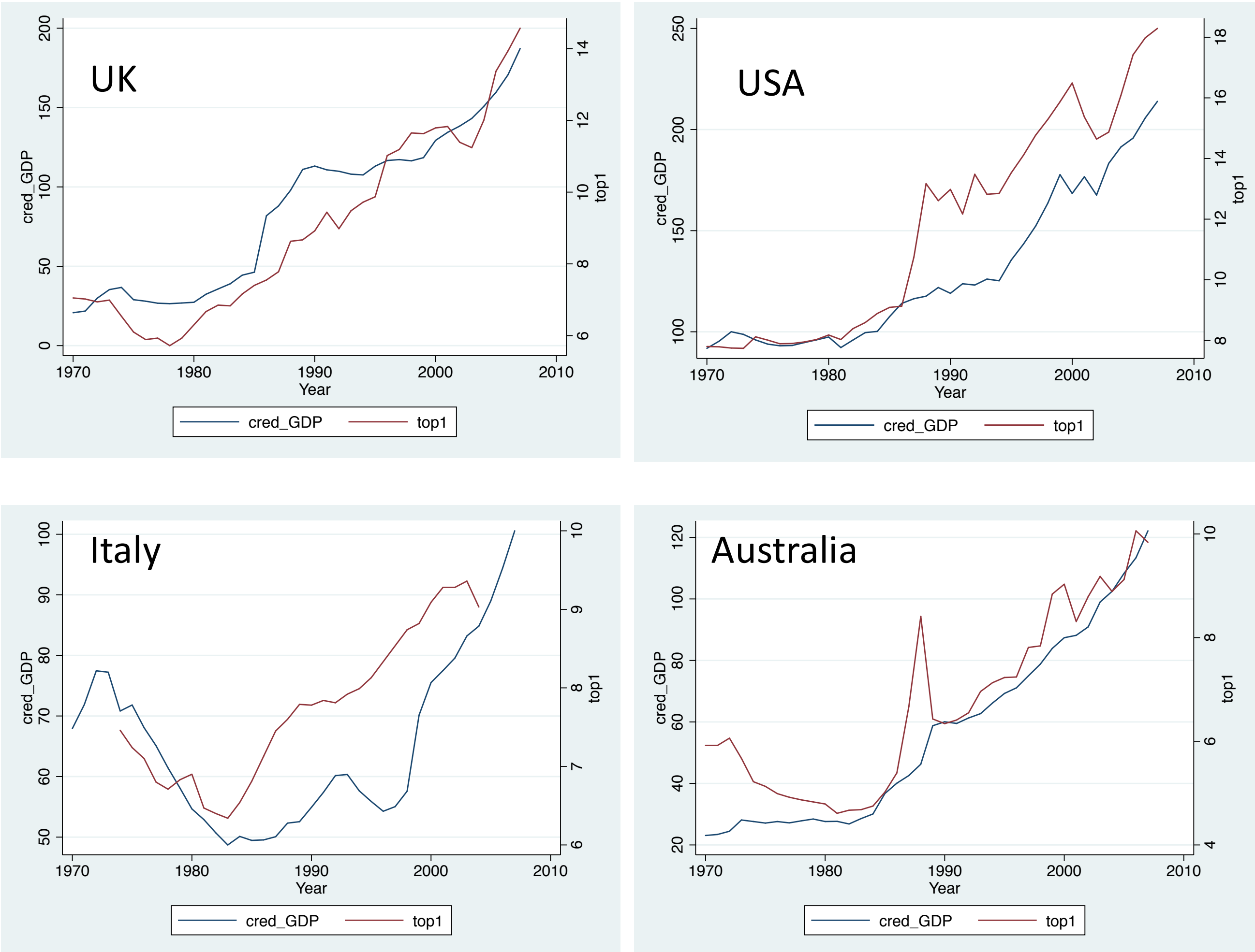


Table 4. Granger causality tests (deregulation / inequality and monetary policy/inequality)

	H0: Dereg_cred_mkt does not Granger cause Ineq (top1%)	*H0: Ineq (top1%) does not Granger cause Dereg_cred_mkt*		*H0: Ineq (top1%) does not Granger cause M2_gdp*	
	F-stat [p_values]	F-stat [p_values]	N. obs	F-stat [p_values]	N. obs
1 lag	1.467 [0.226]	0.981 [0.322]	487	1.387 [0.238]	489
2 lags	2.794 [0.062]	1.570 [0.209]	468	0.487 [0.615]	470
3 lags	1.977 [0.116]	1.514 [0.210]	449	0.407 [0.748]	451
4 lags	1.346 [0.251]	1.194 [0.313]	430	0.341 [0.850]	432
5 lags	1.591 [0.161]	1.802 [0.111]	411	0.230 [0.949]	413

Notes: P-values reported in brackets.

3.2. Econometric Methods and the Empirical Model

In order to assess the impact of inequality and deregulation on credit expansion we consider the following dynamic model:

$$
\begin{aligned}
Cred_{i,t} = {} & \alpha_i + \tau_t + \gamma Cred_{i,t-1} + \beta_1 Ineq_{i,t} + \beta_2 Fin\_Dereg_{i,t} + \beta_3 Inv_{i,t} + \beta_4 Port\_Inv_{i,t} + \\
& + \beta_5 M2_{i,t} + \beta_6 Int\_rate_{i,t} + \beta_7 GDP\_gr_{i,t} + \beta_8 PCgdp_{i,t} + \varepsilon_{i,t}
\end{aligned}
\qquad [1]
$$

where subscripts *i* and *t* refer to countries and years, respectively (*i = 1, ..., 18; t = 1970, ..., 2007)*; α_i and τ_t are the country and the time specific effects and ε_{it} the error terms. The acronyms indicate the variables as described in the previous sections. Obviously, the inclusion of country and time specific effects is a major advantage of the panel approach, providing controls for unobservable or not properly measured drivers of credit, such as global liquidity conditions or foreign capital inflows. Specification [1] is then further extended by introducing an interaction variable obtained multiplying the indicators of inequality and credit market deregulation, in order to test the possibility that the effect of inequality on credit growth might be enhanced under laxer regulatory frameworks.

The dynamic specification of [1] allows accounting for the fact that within-country credit growth is characterized by inertia and can be viewed as a time-persistent phenomenon; neglecting this term, if significant, would affect the estimation outcomes due to omitted variable bias. In addition, as previously mentioned, specification [1] can be characterized by the presence of other endogenous regressors and reverse causality issues. A large body of literature has for example analysed the effect of credit on inequality. Theoretical and empirical evidence has spotlighted how the possibility of undertaking investments in physical and especially human capital, which is among the main drivers of labour income and socio-economic mobility (Aristei and Perugini, 2012), may be hampered by the existence of credit constraints (Li et al., 1998), credit market imperfections (Galor and Zeira, 1993), and poorly developed financial markets (Greenwood and Jovanovic, 1990). Also, as pointed out by Borio and White (2003), workers and consumers have balance sheets that improve during credit expansion, so being able to sustain consumption growth with credit rather than demanding wage increases. At the same time, creditors, capitalists and rentiers enjoy growing capital gains driven by the economic and financial expansion; the combined effects could lead to a surge in both functional and personal income inequality. Similar concerns of inverse direction of causality may arise between the size of credit in the economy and the extent of investments, consumption and ultimately aggregate demand (e.g., Elekdag and Wu, 2011) and with reference to growth and development driven by financial deepening and increased levels of intermediation (Rajan and Zingales, 1998; Decressin and Terrones, 2011).

Dealing with all these issue simultaneously is a challenging task, also in view of the characteristics of the data available. Our choice here is to use different approaches able to address the various problems mentioned and to compare the outcomes, particularly the relationships of interest, step-by-step. As a first pass, we estimate a standard fixed effects model, as in Bordo and Meissner (2012), but employing a Panel Corrected Standard Error model, which allows correcting for autocorrelation and heteroskedasticity of errors and obtaining more reliable standard errors (Beck and Katz, 1995). However, the presence of the lagged dependent variable, due to its potential correlation with the composite error $(\alpha_i + \varepsilon_{i,t})$, may lead to inconsistent parameter estimates also when country heterogeneity is accounted for by means of conventional fixed- or random-effects estimators (Baltagi, 2001). This is due to the so-called dynamic panel bias which however, if T (the time dimension) is large enough, becomes insignificant; under such circumstances, a standard straightforward fixed-effects estimator can be employed (Roodman, 2009). Yet, such approach would not allow addressing the problems of endogeneity due to potential reverse causality. To deal with this issue, we adopt here two different approaches. The first relies on a fixed effect instrumental variable estimator based on the Hansen (1982) original Generalized Methods of Moments which allows, besides instrumenting the variables at risk of endogeneity, to have standard errors robust to heteroskedasticity and autocorrelation. As instruments, the validly of which is tested by means of the standard methods, we use a mix of internal instruments (up to 2 lags) and external variables derived from the literature and expected to impact on inequality, investments and growth and to be uncorrelated with the dependent variable (credit as a % of GDP). They include institutional indicators related to labour and product markets, to the rule of law and trade openness. As a check for the robustness of the results obtained, we also approach the endogeneity issue from another side.

A possible alternative approach would be using the first-difference GMM estimator proposed by Arellano and Bond (1991), which is based on first-differencing the regression equation to eliminate the country-specific effect and using lagged independent variables as instruments. However, for the aim of the present analysis, the main issue of using this estimator is related to the possible persistency over time of the dependent variable: the cross-sectional variation embodies a large part of the information since within-country credit size (on GDP) can be, for the reasons previously explained, remarkably persistent. In this respect, although the first-difference GMM estimator allows controlling for possible measurement errors, country-specific heterogeneity and endogeneity bias, ignoring cross-sectional variation may fatally affect the precision of the estimates. Moreover, as pointed out by Blundell and Bond (1998), the lagged levels of the explanatory variables are weak instruments for the variables in differences when explanatory variables are persistent[6]. The system GMM estimator (Arellano and Bover, 1995; Blundell and Bond, 1998) allows addressing these shortcomings, exploiting also the cross-country variation in the data. In the system-GMM approach specifications in first-differences and in levels are combined; namely, the GMM-sys augments the GMM-diff by including and equation in levels (so keeping the cross-country dimension) and estimates simultaneously in differences and levels. The two equations are separately instrumented.

The system GMM estimator uses as instruments the lagged values of the endogenous explanatory variables, thus requiring a more stringent set of restrictions compared to the GMM-diff. Variables in levels are instrumented with lagged first differences; in order to consider these additional moments as valid

[6] Bond *et al*. (2001) show that in small samples such weak instruments issue further translate into a large finite sample bias.

instruments for levels, the identifying assumption that past changes of the explanatory variables are uncorrelated with current errors in levels, which include fixed effects, is required (Roodman, 2009). If the moment conditions are valid, Blundell and Bond (1998) show that the system GMM estimator performs significantly better than the first difference GMM estimator. The validity of the moment conditions can be tested by means of the test of overidentifying restrictions proposed by Sargan (1958) and Hansen (1982) and by testing the null hypothesis of no second order serial correlation in the error term.

As already explained, the GMM-Sys estimator has the advantage of allowing instrumentation of endogenous variables with internal lags; however, it is designed for large N small T panels and to deal efficiently with dynamic panel bias. Its employment to dataset like ours (large T, small N) may cause a natural proliferation of the number of instruments. Although this does not compromise consistency, a small N may lead to unreliable cluster-robust standard error and AB autocorrelation test (Roodman, 2009), and weaken the Hansen test to the point where it generates implausibly good p-values of 1.000 (Anderson and Sorenson 1996; Bowsher 2002). However, system GMM estimation allows some flexibility by means of several specification choices. In particular, given the structure of our panel we use the one-step estimator and correct the standard errors to take account for small-sample bias and heteroschedasticity, by applying the Huber and White robust variance estimator. Furthermore, to address the problem of the overfitting bias caused by instrument proliferation in dynamic panels, we use a combined strategy obtained by collapsing instruments (i.e., creating one instrument for each variable and lag distance only, with 0 substituted for any missing values) and restricting the number of lags used as instruments. We of course keep the GMM-Sys shortcomings in mind when interpreting results, which are however mainly used as a robustness check for those obtained with previous approaches.

As in most empirical studies on inequality, the estimation of the empirical model relies on an unbalanced and unequally spaced panel dataset. The use of a panel of unequally spaced spells, while allowing to keep the sample size reasonable high, could lead to an over representation of countries with a large number of observation and to inconsistent estimates if one period in the theoretical model has to perfectly correspond to a certain time span in empirical data (Tamm *et al*., 2007). This is kept on mind in the interpretation of results.

3.3. Results

Table 5 reports the estimates of our empirical model, which first of all highlight an overall stability to alternative econometric approaches. The coefficient of the lagged dependent variable is always positive and significant, confirming the validity and the necessity of a dynamic specification. Conceptually, this evidence can be explained in terms of herd behaviour, as explained by Rajan (1994). The fact that others are lending may be considered as invaluable information concerning the creditworthiness of a potential borrower; more importantly, being performance generally assessed relative to some market benchmark, managers from financial institutions have a strong incentive to behave as their peers, reinforcing credit expansion inertially over time (Gosh, 2010).

Table 5. Credit, inequality and financial deregulation (18 countries, 1970-2007)

Dep. Var.: Cred_GDP	PCSE	IV (1)	IV (2)	IV (3)	IV (4)	GMM sys (1)	GMM sys (2)	GMM sys (3)	GMM sys (4)
L (1) cred_GDP	0.829***	0.852***	0.845***	0.870***	0.870***	0.831***	0.832***	0.836***	0.841***
	(0.016)	(0.049)	(0.049)	(0.054)	(0.054)	(0.021)	(0.021)	(0.020)	(0.017)
Ineq (top 1%) [(a)]	0.417**	0.818**	0.805**	0.811*	0.810*	1.131*	1.042*	0.897*	0.734***
	(0.199)	(0.310)	(0.387)	(0.456)	(0.456)	(0.596)	(0.611)	(0.505)	(0.210)
Dereg (cred mkt)	2.105***	1.644***	1.693***	1.951***	1.952***	1.416***	1.560***	1.791***	1.478***
	(0.408)	(0.537)	(0.512)	(0.564)	(0.565)	(0.429)	(0.534)	(0.492)	(0.471)
cap_form_gdp [(b)]	0.999***	0.868***	1.000***	0.775***	0.774***	0.514***	0.679*	1.176***	0.482*
	(0.119)	(0.203)	(0.249)	(0.239)	(0.239)	(0.138)	(0.412)	(0.398)	(0.273)
portf_inv_gdp	-0.018***	-0.000	-0.001	-0.003	-0.003	-0.028***	-0.028***	-0.030***	-0.024**
	(0.005)	(0.014)	(0.012)	(0.014)	(0.014)	(0.011)	(0.011)	(0.011)	(0.010)
M2_gdp	0.076***	0.063*	0.078*	0.053	0.052	0.084***	0.080***	0.068***	0.083***
	(0.013)	(0.037)	(0.042)	(0.039)	(0.039)	(0.015)	(0.018)	(0.017)	(0.016)
real_int_rate	0.081	0.006	-0.030	0.003	0.003	0.049	0.043	0.087	0.076
	(0.071)	(0.156)	(0.152)	(0.172)	(0.173)	(0.158)	(0.158)	(0.158)	(0.158)
Real_gdp_growth [(c)]	0.938	4.779	5.144	-63.917	-63.900	-6.933	-4.961	54.577	-25.534
	(14.758)	(17.852)	(16.891)	(63.676)	(63.669)	(24.919)	(25.152)	(41.006)	(22.558)
pc_gdp (ln) [(d)]	14.472***	15.516***	18.327***	13.664*	13.724*	7.867***	8.217***	10.129***	7.557***
	(3.714)	(5.029)	(5.670)	(7.077)	(7.087)	(2.418)	(2.491)	(2.673)	(2.405)
Instrumented Variables	-	*a*	*a, b*	*a, b, c*	*a, b, c, d*	*a*	*a, b*	*a, b, c*	*a, b, c, d*
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
[Joint significance]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.012]	[0.002]	[0.001]
Wald Test	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
N	505	469	445	469	469	505	505	505	505
R2	0.991	-	-	-		-	-	-	-
Centered R2	-	0.922	0.917	0.921	0.921	-	-	-	-
Underid. Test	-	19.850 (3) [0.000]	19.639 (5) [0.002]	26.693 (4) [0.000]	26.668 (4) [0.000]	-	-	-	-
Weak Instrum. Cragg-Donald F stat	-	318.320	137.013	14.941	12.802	-	-	-	-
Hansen J (overid) staistics.	-	4.021 (2) [0.134]	4.728 (4) [0.316]	5.302 (3) [0.151]	5.305 (3) [0.150]	-	-	-	-
Endog. Test	-	1.938 (1) [0.1639]	5.317 (2) [0.070]	2.680 (3) [0.443]	2.755 (4) [0.599]	-	-	--	-
A-B AR(1) test	-	-	-	-	-	-10.45 [0.000]	-10.42 [0.000]	-10.39 [0.000]	-10.55 [0.000]
A-B AR(2) test	-	-	-	-	-	1.43 [0.152]	1.41 [0.158]	1.42 [0.155]	1.42 [0.154]
Sargan overid. test	-	-	-	-	-	1.65 (3) [0.648]	1.40 (4) [0.844]	6.66 (7) [0.465]	12.85(10) [0.232]

Notes:
T statistics are based on robust standard errors and reported in brackets; A-B AR(1) and A-B AR(2) are tests for first- and second-order serial correlation in the first-differenced residuals (Arellano and Bond, 1991)
$* p<0.10$, $** p<0.5$, $*** p<0.01$

As for the other control variables, capital formation emerges as positively related to credit, confirming the long and extensive literature arguing that investments (especially those driven by technological breakthroughs and displacement) need to be financed with credit (Fisher, 1933; Minsky, 1986). Also as expected (Elekdag and Han, 2012; Eleldag and Wu, 2011), larger money availability (M2/GDP) drives credit expansion; similarly, more advanced levels of development (per capita GDP) are positively associated to higher credit. The size of portfolio investments, which also include external liabilities and should control for capital inflows, is either not significant or negative, indicating, as per Mendoza and

Terrones (2008) that credit expansion is lower in the presence of large portfolio investments. The remaining control variables (real interest rate and GDP growth) only turn up significant in a few sporadic cases. All results are robust to the adoption of IV approaches, which allows for potential endogeneity of explanatory variables.

In all specifications the proxy for financial liberalisation is found to have a positive, statistically significant (at more than a 99% confidence level) effect on private sector credit, firmly justifying its inclusion in the model and indicating that its exclusion would certainly lead to omitted variable bias. This evidence is consistent with the conceptual explanations provided by the literature (e.g., Gosh, 2010; Dell'Ariccia and Marquez, 2006; Gourinchas at al., 2001) and well-established empirical findings (e.g., Mendoza and Terrones, 2008).

As far as the focus of our analysis is concerned, the coefficient of the inequality variable (top1) is positive and significant in all estimates, suggesting that higher inequality *directly* drives higher credit, once its conventional determinants are controlled for. This is consistent with the line of reasoning illustrated by Kumhof and Rancière (2011) and Kumhof et al. (2012), that we developed in terms of relative income (Duensberry, 1949; Barba and Pivetti, 2009) and expenditure cascades (Frank et al., 2010) hypotheses. Therefore we found clear cross-country evidence that inequality impacts on credit.

Generally speaking, a major concern when studying the effects of inequality is the dependence of outcomes on specific measure of inequality employed (used, since they can produce different outcomes (Litchfield, 1999). In order to assess the robustness of the effects of inequality on credit growth, we carried out additional estimations using the share income hold by the top 5 and top 10% of the distribution. Results are reported in table 6 and confirm that higher inequality triggers higher credit; similarly, the signs and significance of the remaining variables remain virtually unchanged.

Although the literature explored here envisages direct and independent relationships between credit expansion and inequality on the one side and financial deregulation on the other, other recent contributions tend to hypothesize a joint, self-reinforcing interaction effect. Tridico (2012), for example, conjectures that the rise of inequality generated on the labour market led to an increased demand of credit, which translated into a credit expansion due to the increase of supply fed by lax monetary policies and financial deregulation. This would suggest that the effect of inequality on credit expansion should be magnified by deregulation; translated into empirical terms, the interaction term between metrics of inequality and deregulation should turn out positive and significant in the estimates. The estimation of the models with the inclusion of this interaction effect (columns 1-3 of table A1 in the appendix) does not provide support to the possibility that inequality may further foster credit expansion in presence of less regulated institutional settings. On the contrary, the interaction terms render the main effects insignificant: so they do not add any additional information to the model, while producing only disturbance due to redundant information. In columns 4-6 (Table A1) we test the possibility that this effect might be confined only to countries with very high levels of deregulation; to this aim, the interaction term is generated by multiplying the inequality indicator and a dummy variable that is one if the corresponding level of deregulation is in the top decile of its distribution, and zero otherwise. This sorts out the issues of multicollinearity (the main effects re-gain significance) but the coefficient of the interaction term is again not statistically different from zero. This means that while there is a direct effect of inequality on credit (and of deregulation on credit), it is not magnified by deregulation. Therefore, the two effects acted separately on credit expansion, without self-reinforcing patterns.

Table 6. Robustness of the outcomes to different measures of inequality: top 5 and top 10%

	Top 5 %			Top 10%		
Dep. Var.: Cred_GDP	PCSE (1)	IV (2)	GMM sys (3)	PCSE (4)	IV (5)	GMM sys (6)
L (1) cred_GDP	0.829*** (0.015)	0.882*** (0.040)	0.843*** (0.019)	0.839*** (0.014)	0.882*** (0.040)	0.825*** (0.022)
Ineq[(a)]	0.256*** (0.092)	0.454* (0.251)	0.495*** (0.151)	0.209** (0.087)	0.306* (0.184)	0.447*** (0.118)
Dereg (cred mkt)	2.184*** (0.406)	1.843*** (0.627)	1.201*** (0.488)	1.773*** (0.414)	1.639** (0.677)	1.568*** (0.509)
cap_form_gdp[(b)]	0.954*** (0.061)	0.718*** (0.212)	0.502* (0.280)	0.838*** (0.129)	0.766*** (0.275)	0.666** (0.308)
portf_inv_gdp	-0.016*** (0.004)	0.002 (0.012)	-0.023** (0.010)	-0.016*** (0.005)	-0.002 (0.014)	-0.023** (0.010)
M2_gdp	0.069*** (0.011)	0.043 (0.036)	0.079*** (0.017)	0.085*** (0.014)	0.056 (0.040)	0.059*** (0.018)
real_int_rate	0.086 (0.059)	-0.030 (0.150)	0.103 (0.170)	0.111 (0.079)	0.004 (0.166)	0.039 (0.175)
Real_gdp_growth[(c)]	-5.724 (10.510)	-81.300 (52.604)	-32.391 (25.410)	-1.746 (17.433)	-110.000 (70.373)	-29.164 (26.712)
pc_gdp (ln) [(d)]	11.439*** (1.837)	9.009** (4.067)	10.538*** (3.044)	5.961*** (1.816)	10.478** (4.468)	9.374*** (2.678)
Instrumented Variables	-	*a, b, c, d*	*a, b, c, d*	-	*a, b, c, d*	*a, b, c, d*
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
[Joint significance]	[0.000]	[0.029]	[0.002]	[0.000]	[0.005]	[0.000]
Wald Test	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
N	484	441	484	459	383	459
R2	0.991	-	-	0.989	-	-
Centered R2	-	0.919	-	-	0.912	-
Underid. Test	-	28.357 (6) [0.000]	-	-	17.623 (5) [0.000]	-
Weak Instrum. Cragg-Donald F stat	-	8.382	-	-	3.476	-
Hansen J (overid) staistics.	-	7.933 (5) [0.160]	-	-	4.806 (4) [0.308]	-
Endog. Test	-	3.442 (4) [0.487]	-	-	4.364 (4) [0.359]	--
A-B AR(1) test	-	-	-9.92 [0.000]	-	-	-9.77 [0.000]
A-B AR(2) test	-	-	1.33 [0.184]	-	-	1.42 [0.155]
Sargan overid. test	-	-	29.25 (42) [0.932]	-	-	34.46 (52) [0.971]

Notes:
T statistics are based on robust standard errors and reported in brackets.; A-B AR(1) and A-B AR(2) are tests for first- and second-order serial correlation in the first-differenced residuals (Arellano and Bond, 1991)
** p<0.10, ** p<0.5, *** p<0.01*

4. Discussion and final remarks

The principle aim of this study was to show that that higher inequality leads to increased credit demand and indebtedness, which then results in higher financial fragility eventually resulting in a crisis. This turns conventional wisdom from the head on its feet. The causality chain from higher inequality to crisis rather than the other way round finds strong support by empirical evidence. With respect to the various measure of inequality used here, this underpins the Kumhof and Rancière view, augmented with the relative income hypothesis of consumption and the imitative consumption cascade conjecture. These *results* are however not consistent with B&M, who used a smaller set of countries, a longer time period, and an empirical approach from which we depart remarkably in terms of: the measure of credit employed, control for potential endogeneity biases, the effects of changing financial markets institutional arrangement, and econometric techniques. Therefore our study provides a contribution to scanty and sparse empirical literature on the topic, suggesting that more research effort should be devoted to the impact of inequality on the stability of modern capitalistic models of society. One option for future research is to focus on microeconomic analysis of the relationship between the individual's relative position in the income ladder, consumption and recourse to credit.

The particular contribution of this study lies in providing evidence on a cross-country basis not limited to US, as most other related studies do and to which all conjectures on the link between inequality and crisis refer to. The analysis adds to the existing knowledge on other episodes of credit booms / financial crisis (see B&M, p.2160, last paragraph of section 5) in which inequality was not considered as a possible driver at all. The view that inequality is not significant, as B&M maintain, is rejected on the basis of rigorous methodological grounds. The findings of this study are interpreted as a stimulus to consider the role of inequality on credit expansion and financial instability in a wider context than the US in view of alternative country-specific possible explanations and varieties of capitalism. All previous explanations associated with credit booms and banking crises to factors are controlled for in the model presented, i.e., deregulation, monetary policy, investment inflows, expansionary monetary policy. Even after these controls it is shown that rising inequality drives rising credit conducive to crisis.

A further contribution of this study lies in the explicit consideration of the impact of deregulation. This has not been done before, but omitting it might result in remarkable biases on the measured effects of the other explanatory variables. Policy implications of our analysis are that various factors concur to shape the risk of excessive credit growth and financial instability and most of them seem related to policy tools that have traditionally represented the role of the state in the economy, i. e. monetary policy, redistributive policies and regulation. These have been gradually given up or sacrificed to monetary or fiscal rules or to the ideologies of deregulation of markets. The findings of this study call for a return of income inequality as an important factor relevant for macroeconomic aggregates and stability.

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Appendix

Table A1. Credit, inequality and deregulation (18 countries, 1970-2007) – with interaction (ineq*dereg)

Dep. Var.: Cred_GDP	PCSE (1)	IV (2)	GMM sys (3)	PCSE (4)	IV (5)	GMM sys (6)
L (1) cred_GDP	0.829*** (0.016)	0.851*** (0.057)	0.829*** (0.023)	0.830*** (0.016)	0.867*** (0.054)	0.840*** (0.017)
Ineq (top 1%))[(a)]	-0.298 (1.979)	-2.067 (4.123)	0.225 (5.937)	0.416** (0.195)	0.786* (0.476)	0.704*** (0.212)
Dereg (cred mkt)	1.615 (1.419)	0.094 (2.697)	0.851 (4.575)	2.106*** (0.397)	1.870*** (0.568)	1.190** (0.529)
Ineq * Dereg [(b)]	0.081 (0.224)	0.335 (0.484)	-0.003 (0.669)	-	-	-
Top der_d * Ineq[(b)]	-	-	-	-0.001 (0.088)	-0.225 (0.184)	0.337 (0.277)
cap_form_gdp[(c)]	0.989*** (0.132)	0.758*** (0.232)	0.411 (0.304)	0.999*** (0.121)	0.790*** (0.226)	0.471* (0.271)
portf_inv_gdp	-0.018*** (0.005)	-0.004 (0.013)	-0.019* (0.011)	-0.018*** (0.005)	0.002 (0.013)	-0.024** (0.010)
M2_gdp	0.075*** (0.014)	0.055 (0.034)	0.100*** (0.025)	0.076*** (0.013)	0.065* (0.039)	0.085*** (0.016)
real_int_rate	0.083 (0.072)	-0.019 (0.168)	-0.009 (0.166)	0.081 (0.071)	-0.015 (0.178)	0.061 (0.158)
Real_gdp_growth[(d)]	0.463 (14.547)	-87.326 (68.970)	-6.732 (26.433)	0.943 (14.839)	-43.201 (58.716)	-25.141 (22.497)
pc_gdp (ln) [(e)]	14.662*** (3.835)	13.991** (6.992)	16.767** (7.103)	14.488*** (3.710)	17.543*** (6.241)	7.741*** (2.397)
Instrumented Variables	-	*a, b, c, d, e*	*a, b, c, d, e*	-	*a, b, c, d, e*	*a, b, c, d, e*
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
[Joint significance]	[0.000]	[0.001]	[0.191]	[0.000]	[0.000]	[0.002]
Wald Test	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
N	505	469	505	505	453	505
R2	0.991	-	-	0.957	-	-
Centered R2	-	0.920	-	-	0.918	-
Underid. Test	-	28.362 (4) [0.000]	-	-	25.823 (6) [0.000]	-
Weak Instrum. Cragg-Donald F stat	-	9.694	-	-	9.226	-
Hansen J (overid) staistics.	-	5.048 (3) [0.168]	-	-	8.005 (5) [0.156]	-
Endog. Test	-	3.877 (5) [0.168]	-	-	2.631 (5) [0.757]	--
A-B AR(1) test	-	-	-10.48 [0.000]	-	-	-10.51 [0.000]
A-B AR(2) test	-	-	1.48 [0.138]	-	-	1.44 [0.151]
Sargan overid. test	-	-	13.78 (11) [0.245]	-	-	14.61 (13) [0.332]

Notes:
T statistics are based on robust standard errors and reported in brackets; A-B AR(1) and A-B AR(2) are tests for first- and second-order serial correlation in the first-differenced residuals (Arellano and Bond, 1991)
** p<0.10, ** p<0.5, *** p<0.01*