



The nexus inequality-finance, a new Gordian knot for economic policy?

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Abstract: Based on the existing literature, this paper aims at examining the two-way relationship between inequality and finance. Concerning the nexus inequality – finance, we find evidence supporting a direct causal link (a rise of credit demand as a result of high inequalities), and an indirect one (accommodative monetary policy and financial deregulation increasing credit supply, as a result of high inequalities); coincident factors (financial deregulation increasing simultaneously both inequalities and leverage) are not to be excluded either. We also report evidence showing the impact of financial development, financial deregulation and financial crises on income distribution. Despite the complexity of these various, intertwined linkages, the presumptions of a circular, self-reinforcing relationship between finance and inequalities are very strong, requiring urgent, adequate policy responses.

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1. Introduction¹

Financial crises are a recurrent phenomenon in both developed and developing economies, with an increasing frequency since the mid-1970s. Financial instability became a part of the macroeconomic picture since then, with regular episodes of more or less massive asset depreciation. However, the most recent one in 2007-2008, often referred to as the “subprime crisis”, is distinguishable from the other in two important aspects. Firstly, the consequences on the real sector have been brutal, massive and long-lasting, with decreases of GDP standing between 3 and 5% and skyrocketing unemployment in most developed countries. It appeared very quickly that the “Great Recession”, could only be compared to the 1930s’ Great Depression. Secondly, the very name of this crisis (*subprimes*) pointed to a specific origin: excess mortgage credit to low-income/low-asset/low credit score households.

As a consequence, this created a breach in the academic, conventional wisdom that an always bigger financial sphere was always profitable for economic growth. For example, Arcand et al. (2012) provide evidence that financial development enhances economic growth until a certain threshold, after which the financial sphere gets too big and starts hurting the real sphere. This gave formal, empirical evidence to intuitions developed earlier by Stiglitz (2009,2010). Less expected was the direct, causal relationship made between inequality, excess leverage and financial crisis which became increasingly advocated by academic economists at the beginning of the year 2010s. The debate entered the public sphere with the book by Rajan (2010), ‘Fault Lines’, where the author argues that rising income inequality constrained low and middle-income households to increase their indebtedness in order to maintain their consumption level, buffering temporarily the impact on GDP growth.

Recently, Van Treeck (2014) provided an extensive review of the various arguments supporting that rising inequalities did cause the 2007-2008 financial crisis. Building on this contribution, **our goal in this paper is to go one step further, by examining the two-way relationship between inequality and finance.** In other words, we want to review not only through which channels inequality may trigger excess leverage leading *in fine* to a financial crisis, but also how financial dynamics may in turn influence the evolution of inequality. A major challenge here is that these reciprocal influences are likely to act through several factors, some co-founding. Beyond a simple inventory of the papers supporting one causal relationship (from inequality to finance) or the other (from finance to inequality), our purpose here is to disentangle the various influences underlying this two-way-causality. Besides, **an additional contribution is to enlarge the scope of the paper beyond the US case to other financial crises and other countries with different institutions.**

A first step is to identify what we could call the main link in the causality chain: inequality,² the volume of credit in the economy, and financial crises. Then, when thinking of the first linkage between inequality and credit, one has to distinguish how inequality may influence credit demand (for the above mentioned reasons) and credit supply. A main line of argument is that financial

¹ The authors would like to thank FEPS and members of the FEPS ‘Finance and Inequality’ project (in particular presentation of papers, which are available at:

https://sites.google.com/site/jpcdhericourt/Online%20Appendix_Bazillier_Hericourt.pdf?attredirects=0&d=1)

² When referring to inequality, most authors refer to income inequality. We will see however, that useful distinctions can be made between different sort of inequalities: income inequality, consumption inequality, functional inequality.



institutions have been actually pushed to raise loans to riskier individuals (Rajan, 2010; Atkinson and Morelli, 2010) by the institutional environment. The latter can be roughly subdivided in two parts. On the one hand, financial liberalization, by giving to banks the possibility to securitize and trade loans (Shleifer and Vishny, 2010) *structurally* triggered credit supply to riskier borrowers. On the other hand, recurring expansionist monetary policy may have provided *cyclical* support to “generous” credit distribution. Here one can see that establishing causality simply from inequality to credit volume is already a challenge. Taking the supply-side arguments we just mentioned, it is very possible to imagine that increased inequality and financial deregulation (leading to more credit to riskier borrowers) are the joint product of the general rightwing shift of economic policies over the past 30 years: this would imply a positive correlation between inequality and credit supply, but not a causal relationship from the former to the latter. Besides, if not at the core of our argumentation, the links between inequality and public leverage are not to be neglected either: by reducing the tax basis, increased inequality may have increased fiscal deficits and public debts.

The second major step is to explore carefully the mechanisms through which the increased volume of credit can bring out some excess leverage, the latter eventually degenerating in a financial crisis. Starting from the beginning, it is once again difficult to discriminate between a real causal relationship going from inequality to leverage, from a simple correlation due to a cofounding factor. Evidence in the academic literature is quite contradictory, some finding no impact (Bordo and Meissner, 2012), others finding mixed evidence (Atkinson and Morelli, 2010), others supporting the causal relationship we suspect (e.g., Perguni et al., 2013) – in any case, this will undeniably need additional investigation in the future. Besides, a direct corollary of increased aggregate debt is a current account imbalance, providing an alternative test of the role of inequality in creating macroeconomic disequilibrium. Evidence here is more conclusive, both theoretically and empirically (Behringer and Van Treeck, 2013, Belabed et al., 2013, Kumhof et al., 2012): if underlying mechanisms may differ, higher inequalities seem to be associated with lower household net lending and therefore, a decrease in the current account. As for the link between (excessive) leverage and financial crises, this is certainly one of the most consensual points in the literature. Recent contributions highlight that financial crises triggered by credit bubbles are not the prerogative of developing/emerging countries anymore, and that household leverage is a key driver of both the boom and the bust dynamics in developed countries. Òscar Jordà, Moritz Schularick and Alan Taylor have recently provided in several papers new empirical evidence on that ground, based on long-time historical data; Martin and Philippon, 2014, provide a theoretical rationalization of these mechanisms.

The third and final step focuses on the reverse relationship, namely, the distributional consequences of finance. Here one must be very careful about what lies behind the word “finance”, and distinguish explicitly between the behavior of the financial sphere in “normal times” (ie, outside the periods of financial turmoil) and what happens in periods of massive financial instability. The first dimension relates to the expected impact of the dynamics (both quantitative and qualitative) of the financial sector on inequality. Until recently, the conventional academic wisdom (strongly supported, among others, by Ross Levine) was that the quantitative enlargement of the financial sphere (involving more credit and financial services) would systematically reduces income inequality, by allowing the more constrained individuals (mostly, the poor) to access external finance. This belief is nevertheless challenged by studies (e.g., Greenwood and Jovanovic, 1990) pointing a not-so-linear relationship: depending on the level of overall economic development and the existence of rent-capturing behavior of elites, quantitative financial development may as well increase overall inequality. As for

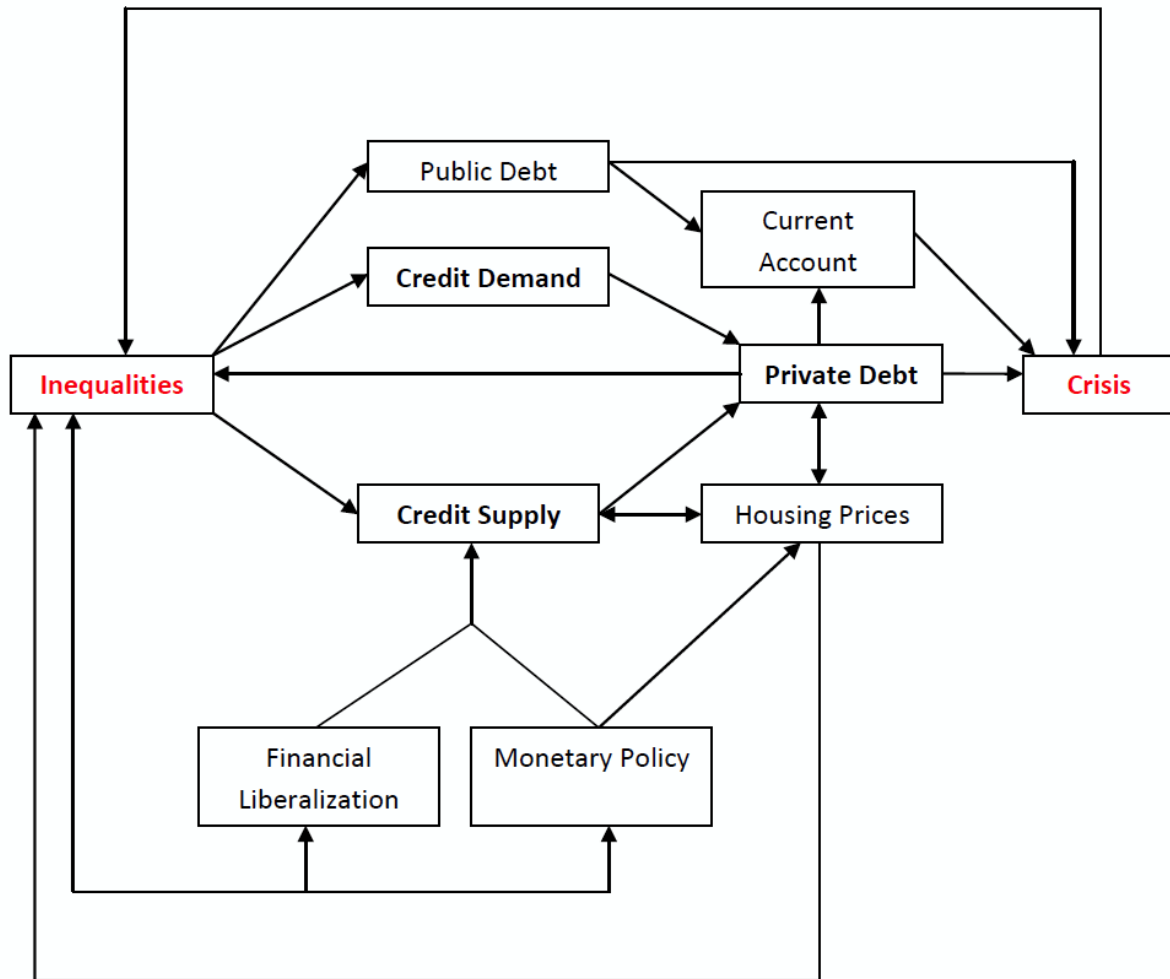
the qualitative aspect of financial dynamics, conclusions are much firmer: most studies find that financial liberalization and deregulation actually increase inequalities. Poor institutions favoring rent-seeking behaviors (Claessens and Perotti, 2007) and the inability of the financial sector to promote transparency and to allocate resources optimally are the main explanations. The second dimension relates to the specific, distributional impact of financial crises. If the overall impact on output seems negative, it is strongly heterogeneous across the type of crisis (banking, currency, or both) and the time and geographical coverage of the studies. As for the distributional impacts themselves, they are quite debated and there is no consensus on the sign of the relationship. It is fair to say however, that a majority of studies conclude to an increase of both income and functional inequality (i.e., a decrease in the labor share) following a financial crisis. In this respect, fiscal consolidation may play a non-negligible role in this negative distributional impact (see, among others, Ball et al., 2013).

All these various and intertwined mechanisms potentially underlying the two-way relationship between finance and inequality are summarized in Figure 1 (arrows point the direction of potential causality).

In a few words, evidence presented in this paper about the inequality-finance nexus may appear more mixed than expected. But overall, even if some links in the causation chain do deserve serious additional investigation, the presumptions for a circular causality between the dynamics of inequality and various aspects of the financial sphere evolution over the past decades are very strong. They call for several, consistent policy response, holding both ends of the “chain”. This would notably imply on the one hand, to regulate strongly the extension and prerogatives of the financial sphere, and on the other hand, to tackle the root of growing inequality.

The remaining of the paper is organized as follows. The following section introduces some descriptive evidence pointing at a positive correlation between income inequality and the growth of financial sphere, before presenting several mechanisms linking inequality and both credit supply and demand. Section III splits the complex linkages between inequality, leverage and financial crises up, starting with the links between inequality and leverage; a complementary channel going through the current account balance is then investigated, before showing evidence concerning the link between leverage and financial crises. Section IV looks into the possible reverse causations between finance and inequality, discriminating between the dynamics of the financial sphere in “normal times” and the periods of financial crises. Section V concludes and draws policy conclusions.

Figure 1: a complex tangle of mechanisms

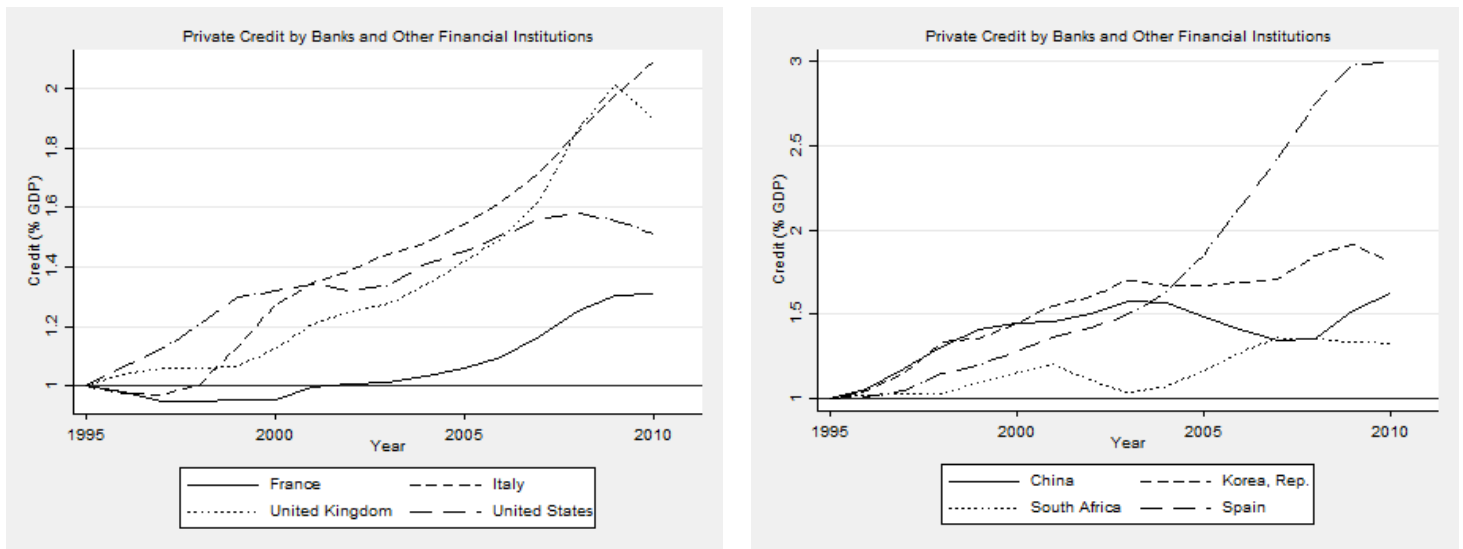


2. Is there an Inequality effect on indebtedness?

2.1. Inequality and Finance: some intriguing anecdotal evidence

Most industrialized countries faced an increase in their public and private indebtedness in the last decades. This increase in leverage raised some concerns both about financial instability and the sustainability of current account imbalances. This process has been well documented for the US (Greenwood and Scharfstein 2013), for which the credit boom has been seen as a major determinant of the financial crisis. This credit boom has also been observed for other countries, including a significant number of European countries, but also some emerging economies such as China or Korea. Figure 2 represents the evolution of private credit by banks and other financial institutions (in percentage of GDP) between 1995 and 2010. In countries like Spain, the share of credit has multiplied by 3 (from 70 % in 1995 to 210 % in 2010).

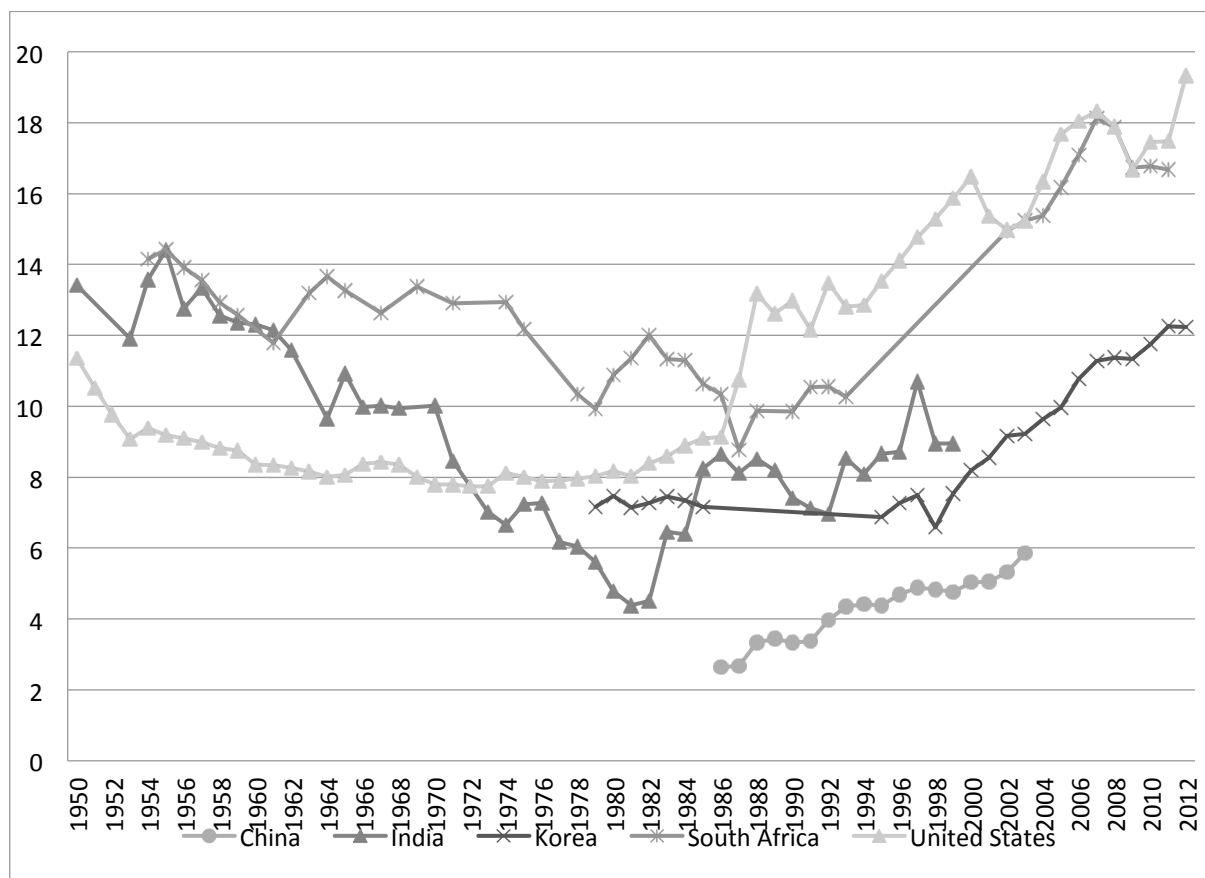
Figure 2: Private Credit (1995-2010) – Selected countries



Source: Financial Development and Structure Dataset (Beck et al., 2000, 2009; Cihak et al., 2012),
Base 1 in 1995

Meanwhile, there has been a renewed interest in the topic of inequalities. *“Bringing income distribution in from the cold”*: in his 1997 presidential address to the Royal Economic Society, Atkinson (1997) calls for new researches related to income distribution. Since then, several studies have focused on the long-run changes in the distribution of income and wealth. Piketty (2003) documented the long-run evolutions of inequalities in France, while Piketty and Saez (2003) did it for the US. They showed that level of inequalities was relatively stable in the long-run while the decrease in inequalities observed during the century was mainly the result of negative shocks due to the first and the second World Wars. Piketty and Saez (2006) and Atkinson, Piketty and Saez (2011) show how top incomes have dramatically increased since the eighties, mostly in developed, English-speaking countries but also in emerging ones like India or China. This increasing share of top incomes has been driven by the rise of top wages, comprising a larger fraction of top incomes than in the past. Figure 3 shows the evolution of the top 1% income shares in selected countries. We clearly see a breakdown in the downward tendency occurring at the end of the seventies – beginning of eighties.

Figure 3: Top 1% Income Shares, 1950-2012

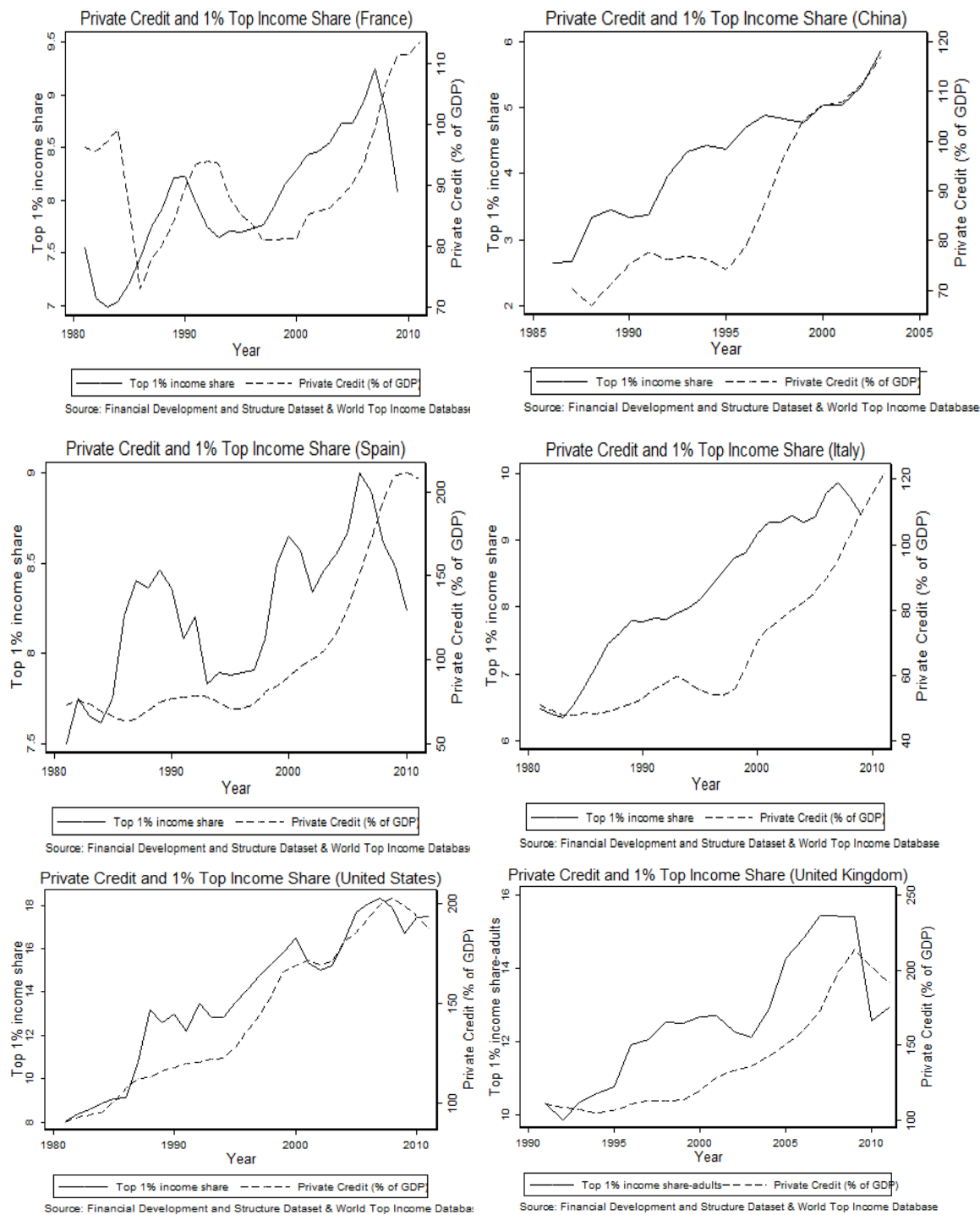


Source: World Top Income Database (Alvaredo et al., 2014)

For illustrative purpose, we describe the evolutions of top income share and private credit based on the World Top Income Database and the Financial Structure Dataset. The strong correlation between the two dynamics is struggling, not only for the US and UK but also for European Continental countries (such as Spain but also France) and China (see Figure 4). This convergence is particularly strong after 1990 and even more after 1995 – 2000. Of course, at this stage, we just present correlations. As emphasized by Atkinson and Morelli (2011), there is a distinction to make between analyzes focusing on a *causal* impact of inequality on debt and crisis, and the ones focusing on possible *common* causes of both phenomena. Also, if there is a causal impact, it is worthwhile to analyze if it comes from an *overall* inequality effect, or from inequality *at the top*, and/or from inequality *at the bottom*.

Before studying the consequences of credit boom on financial instability and financial crises, we propose to review the main explanations of such booms, focusing on the potential causal impact or coincidental role of inequalities. The literature has focused so far on credit demand and credit supply channels to explain such leverage. We will follow this distinction in the next section, focusing firstly on the explanations of private indebtedness.

Figure 4. Top 1% Income Share and Private Credit (% GDP)



2.2. Credit Demand: the role of inequalities

Three possible channels inducing a causal impact of inequalities on private indebtedness are commonly studied. The first two ones relate to consumption behavior, the third one goes back to the level of aggregate demand. A critical point is that the theoretical mechanisms will be very different if the rise of inequality is explained by a higher dispersion of transitory income or by a shift of permanent income between social groups. According to the permanent income theory, leverage may be seen as a rational answer to a higher dispersion of transitory income. But if the shift of income is permanent, alternative theories should be mobilized to explain why households decide to increase their borrowings in response to stagnant incomes. The last two subsections will focus on such theories.

a) A higher dispersion of transitory income?

The first explanation comes from a higher dispersion of the transitory component of income. Using Italian data, Krueger and Perri (2011) show how credit can be used to smooth consumption when facing income shocks. Krueger and Perri (2006) find that the increase in US income inequalities observed in the last 25 years has not been followed by an increase in consumption inequalities. They argue that income distribution may be not a good proxy of allocation of welfare since a significant share of income variations is transitory and does not affect *permanent* income. Then, if the volatility of transitory income is increasing (reflecting higher income inequalities in the short run), the smoothing of consumption through credit may be a rational answer of consumers facing a negative income shock. They develop a theoretical model with endogenous debt constraints to explain such dynamics. Iacoviello (2008) proposes a quantitative dynamic model to replicate the observed simultaneity between evolutions of inequalities and household debt. He explains the rise of debt after 1980 by the increased level of income volatility. The model shows that the permanent increase in income volatility after 1980 has been the main driver of credit boom. By focusing on income volatility, he focuses also on the transitory component of income.

Note that the Krueger and Perri (2006) argument relates only to *within*-groups and not to *between*-groups inequalities. It is very important as within-group inequalities are more likely to be transitory and explained by higher income volatility for individuals.

If expectations of consumers are correct (in particular regarding the transitory feature of the income shocks they are facing) and if income shocks are *indeed transitory*, the relation between inequality and leverage may not be a factor of crisis. Such increase in leverage would be a direct answer to higher risks (volatility) and better risk-sharing among groups. As mentioned by Kopczuk *et al.* (2010), “market economies also generate substantial mobility in earnings over a working lifetime. As a result, annual earnings inequality might substantially exaggerate the extent of true economic disparity among individuals” (p.91). If it is the case, the consequences of such rise of inequalities would be less severe. It is therefore very important to understand the dynamics explaining the evolution of inequalities. Krueger and Peri (2006) main argument is based on the idea that increased inequalities are explained by higher idiosyncratic labor income shocks and reflect higher variations of transitory income.

This assumption is challenged by several authors. First, Van Treeck (2014) argues that the distinction, made by Krueger and Peri (2006), between *within-group* inequalities (assumed to be transitory) and



between-group inequalities (assumed to be permanent) may be conceptually problematic. He argues that the set of individual characteristics used to define between-group inequality may be too limited and therefore the estimation of between-group inequality may be underestimated. Second, Kopczuk *et al.* (2010) show that income mobility has slightly decreased since the 1950s in the US. This result contradicts the hypothesis that the rise of inequalities was explained by a higher income mobility and volatility. Moffitt and Gottschalk (2002, 2008) also find that the variance of transitory income declined or remained constant after 1980, contrary to the variance of permanent income (see also Sablehaus and Song 2009).

As argued by Piketty and Saez (2013), if households perceive the income shock to be *permanent*, they should adjust their consumption accordingly and no changes of liabilities or assets should be observed. But if increased inequalities are explained by a permanent income shock and not by an increase of income mobility, it would mean that the growing gap between income inequality and consumption inequality may have led to unsustainable increases in leverage. We therefore need to understand why households did not adjust their consumption accordingly.

b) "Keeping-up with the Joneses" and the Relative Income Hypothesis

Piketty and Saez (2013) insist on the massive income shift observed in the US since the early 80s : « *the bottom 90 percent has become poorer, the top 10 percent has become richer, with an income transfer over 15 percent of US national income. This was a permanent income transfer.* » If the transfer was really permanent, the only way to explain such increase in leverage, in the permanent consumption theory framework, is that households did not perceive immediately the income shock to be permanent (ie, they made errors of expectations), or tried to resist it. Bertrand and Morse (2013) argue that households may not adjust totally their consumption to their income if the welfare loss induced by such consumption cut is too large in the short run. Piketty and Saez (2013) compute that if the bottom 90 percent cuts its consumption level by the half of the negative income shock they faced (7.5% of GDP instead of 15%), it will be sufficient to explain an increase by the equivalent of 75% of households' debt, which is roughly what was observed.

Since Veblen (1899), it is well-known that the overall level of satisfaction derived from a given level of consumption depends not only on the current consumption level itself, but also on how it compares with some benchmark levels. This is often referred to as "keeping up with the Joneses", and the agent being described as "outward-looking".

Relative income hypothesis use very similar arguments. This theory initially proposed by Duesenberry (1949) suggests that households' consumption is a function of the household position in the income distribution and past levels of consumptions. Van Treeck (2014) argues that it is one of the main explanation of a relatively high consumption path of lower and middle-class households despite the stagnation of their income.

Franck *et al.* (2014) propose a theory of "expenditure cascade" which is in line with the relative income theory hypothesis. Here the rise of the top incomes may have direct impact on the consumption of the poorest households through this "expenditure cascade". More precisely, "*Changes in one group's spending shift the frame of reference that defines consumption standards for others just below them on the income scale, giving rise to expenditure cascades.*" (Franck *et al.* 2014, p. 55). Here, the driving force is the income boom of the richest and its consequences on consumption behaviors of low income.

These arguments closely relate to the so-called "Stiglitz hypothesis" (as referred by Atkinson and Morelly, 2011). According to Stiglitz (2009), increase in leverage is explained by the willingness of



poorest households to maintain their living standards in a context of income stagnation. Here, the driving force is not anymore the income boom of the richest but rather the comparison with past living standards for households facing a relative worsening of their income. A similar argument is used by Kumhof and Ranciere (2010), and is consistent with the structure of household debt by income level. As noted by these authors, the top 5% households had a higher level of debt (by 15%) than the bottom 95% in 1983. In 2007, situation has reversed: the debt-to-income ratio of the bottom 95% was twice as high as the one of the top 5%.

Contrary to the explanation related to the higher level of transitory income, these hypothesis are consistent with empirical studies showing a *permanent* shift of income from the bottom 95% to the top 5%.

c) Under-consumption theories and the level of aggregate demand

The two previous explanations rely on consumer behaviors. Another set of theories focus on the insufficient aggregate level of demand resulting from an increased level of inequalities. Atkinson and Morelli (2011) labeled it the "under-consumption theories". This argument is far to be new, going back to both Marx and Galbraith. The former focusing on the "poverty and restricted consumption of the masses" to explain crises (Marx, *Capital* Vol. III, ch. 30 quoted by Atkinson and Morelli 2011). The latter identified the "distribution of income" as the first "weaknesses" of the US economy before the 1929 Great Depression (Galbraith 1954). When income distribution is very unequal, a high level of demand relies on investment and luxury consumption which may not be enough. This idea is supported by Fitoussi and Saraceno (2009) : "*at the outset there is an increase in inequalities which depressed aggregate demand and prompted monetary policy to react by maintaining a low level of interest rate which itself allowed private debt to increase beyond sustainable levels*"(p. 4).

All these possible theoretical channels imply a causal link from inequalities to leverage. Kumhof and Ranciere (2010) propose a theoretical model including such mechanisms related to the credit demand. Workers borrow to "limit their drop in consumption following their loss of income" (p. 3) which in turns leads to financial fragility. In their model, loans are relaxing the budget constraints and can therefore be used to maximize utility in each period. The consequence is that it also increases the risk of default, leading to a higher level of financial instability. Lastly, we have to notice that the increase in inequalities is explained in this model by an exogenous fall of bargaining power of workers.³

If the recent rise of inequalities is likely to be explained by a permanent income shock, the theory should explain why households did not adjust their consumption accordingly. As we just saw, different theories such as the relative income hypothesis, the expenditure cascade and the need to sustain living standards may explain such apparent paradox. In this respect, all these approaches, regardless of their differences and their sometimes conflicting results, rely on the same hypothesis, namely, that households can freely and always access credit to support their consumption level, whatever their income or risk level. While it is certainly true that financial development, deregulation and abundant liquidity provided by expansionary monetary policies (see below) considerably eased credit access to low-income and/or risky households, it is also true that the latter can be discriminated in their access to basic, retail financial services, and may disproportionately suffer from credit rationing during business downturns. Based on the UK experience French, Leyshon and Meek (2013) provide some interesting descriptive evidence of this kind of phenomenon of "financial exclusion"/"financial precarity", and insist also on the geographic dimension (e.g., the

³ However, in Kumhof, Ranciere and Winant (2015), the rise of inequalities is not explained by this fall of workers bargaining power anymore. It is just assumed to be exogenous and permanent or near-permanent.



disproportionate decrease in the number of bank branches in poorer areas between 1989 and 2012) of this issue. In any case, this is clearly an underinvestigated topic in the literature, deserving additional research.

In any case, there is undeniably a solid theoretical background to explain how inequalities may have increased leverage through an increased demand for credit. We will see in the next section how an increase of credit supply may have played a role and what are the potential relations with growing inequalities.

2.3. Credit Supply: Correlation or (reverse) causality? The ambiguous role of the macroeconomic environment

In his recent survey on the relationship between inequality and the US financial crisis, Van Treeck (2014) highlights that disentangling demand-side and supply-side influences on the total amount of credit distributed in the economy is not an easy task. Besides, the literature focusing on the supply-side is itself both heterogeneous and inconclusive on the key issue of the causal impact of inequality on the distribution of credit.

The first causal link that can be identified is explained by the rise of income for the richest. If it leads to a rise of saving for this group, it will also increase the rise of credit supply. It is exactly the mechanism developed in Kumhof and Ranciere (2010) and Kumhof et al. (2015). Lysandrou (2011) starts from a similar basis: the rise in global savings made possible a huge accumulation of private wealth, which in turn triggered a global excess demand for securities driving credit supply up. This rising supply of capital needs to be invested, even to riskier borrowers, and eventually, this type of investment is made easier by structured credit products.⁴ Here the causal argument seems to be firmly established, at least at first sight.

Coibion, Gorodnichenko, Kudlyak, and Mondragon (2014) also raise explicitly the question of a causal link from inequality to credit, based on an empirical analysis using household level data on debt accumulation during 2001-2012. They reject a demand-sided explanation of the credit bubble, i.e. that low-income households increased their demand for credit to finance higher consumption expenditures in order to “keep up” with higher income households (see section 2.2. above). Coibion, Gorodnichenko, Kudlyak, and Mondragon (2014) are therefore in favor of a supply-side interpretation of debt accumulation patterns during the 2000s. They build a model in which banks use applicants’ incomes, combined with local income inequality, to infer the underlying type of the applicant, so that banks ultimately channel more credit toward lower-income applicants in low-inequality regions than high-inequality regions.

Another common view is that financial institutions have been actually pushed to raise loans to riskier individuals, with the paroxysm reached with the development of subprime loans massively distributed to (sometimes very) low income individuals, with a high risk of default.⁵ Rajan (2010,

⁴ An alternative, political economy approach can be found in Atkinson and Morelli (2010, page 60): the decrease of welfare incomes in general, and pensions from public-funded schemes in particular, implies loss of income for beneficiaries and consequently, a rise in inequality. Households respond by saving more in private pension schemes (and by purchases of housing). In turn, private pension schemes need to invest the additional funds, even with an increased risk.

⁵ Charles R. Morris, in his book “The Two Trillion Dollar Meltdown” (2008), nicknamed the very low quality subprimes *NINJA* loans - No Income, No Job, (and) no Assets loans, because the only thing an applicant had to show was his/her credit rating, which was presumed to reflect willingness and ability to pay.

page 43, quoted by Atkinson and Morelli, 2010), is supporting this kind of political economy analysis when he states that “growing income inequality in the United States (...) led to political pressures for more housing credit. This pressure created a serious fault line that distorted lending in the financial sector”. This argument completes the one by Krugman (2010), Acemoglu (2011) and Atkinson and Morelli (2010) themselves at the beginning of their contribution (p. 3): the empirical association between increasing inequality and the boom of credit does not imply necessarily causality. Both phenomenons may also well be the joint by-products of a general political shift towards a more free-market stance.

At this step emerges what seems to be a crucial point: the role of public/government policies in favoring the supply of loans to low income/low wealth individuals. Indeed, the latter could not have happened without a favorable macroeconomic and regulatory environment, related to the dynamics of financial transformation and monetary policy.

a) Financial development, deregulation and monetary policy

The fact that the empirical association between increasing inequality and the boom of credit does not imply necessarily causality does not need either to contradict the various approaches we mentioned earlier: it just simply means that it is unlikely that a single story explains the joint rise in inequality and credit, a fact already highlighted by Atkinson and Morelli (2010) on their table 7 (page 60) where they list some of the alternative underlying theoretical mechanisms. Here, we adopt a macroeconomic perspective by focusing on two aspects which may have favored a simultaneous increase in inequality and credit supply, without necessarily implying a causal relationship from the first to the second: the various aspects of financial dynamics over the past decades on the one hand, and monetary policy on the other hand.

What is important here is that, if financial development is tightly associated with the rise in credit supply, financial dynamics as a whole over the past two or three decades do not restrict to it theoretically. Deregulation and liberalization have been the other prominent features of the evolution of financial systems. Macro prudential policy gave progressively more freedom for banks to operate on financial markets. It is frequently emphasized that banks progressively externalized their core function of balancing risks and profitability of projects by potential borrowers. Indeed, securitization allows banks removing loans from their balance-sheets by transforming them into securities traded on financial markets. Shleifer and Vishny (2010) formalize explicitly this behavior of banks in a model where banks make, securitize, distribute and trade loans – as an alternative to holding cash. Banks also borrow money, using their security holdings as collateral, and they operate on markets influenced by investor sentiment. Insofar as mortgage and other loans could easily be securitized, and that there was a huge demand for these securities, banks were pushed to take on greater risks: “Banks were intimately involved in both underwriting these securities and holding large inventories on their own books, financing them in large part through short-term borrowing” (Shleifer and Vishny, 2010, p. 316). On the other hand, housing bubble triggered subprime loans (easy to securitize) to low-income households: inequality had therefore a magnifying effect on the risk-taking behavior of banks. Here, the distribution of incomes appears to have had a *causal* impact on credit supply.

That said, and without anticipating the developments in section 4 below, studies establishing causality from financial development to inequality have direct consequences here. For example,



Levine concludes (2005) to a positive impact of financial development on inequality. From that perspective, the *increase* in credit supply and the *decrease* in income inequalities are both *simultaneous* corollaries of financial development, but *with no necessary causal link* from one to the other. Conversely, Jauch and Watzka (2011) conclude to the exact opposite relationship (i.e., financial development increases inequalities), meaning that we may have the opposite correlation to the one implied by Levine 2005). In other terms, financial development may bring a *simultaneous increase* in credit supply and inequality, reflecting a correlation seemingly consistent with our story, but once again, with no real possibility of establishing causality from one variable to the other.

A second key aspect much more often (but not always) neglected by the literature on the finance-inequality nexus relates to the potential part of monetary policy. Here also, the existence and direction of causality is a significant puzzle mainly unsolved. An exception is the work by Fitoussi and Saraceno (2009) who support that “an increase in inequalities (...) depressed aggregate demand and prompted monetary policy to react by maintaining a low level of interest rates which itself allowed private debt to increase beyond sustainable levels”, using an argument similar to Rajan (2010). Here there is a clear causal relationship from inequality towards monetary policy, which in turn triggered the credit bubble – but once more, it is not absolutely clear that the authors are thinking of a supply rather than a demand-side story for credit.

c) Credit supply vs. credit demand: the difficulties for disentangling their respective influences

It is clear from the overview of papers presented above that is irrelevant to try to point a single type of explanation (either demand or supply-sided) for the causal nexus inequality-debt increase. If some of the above-mentioned papers are based on anecdotal or descriptive evidence, most are based on rigorous theoretical or empirical frameworks delivering more systematic evidence in favor of both channels. It seems more than plausible that both were activated simultaneously. This is corroborated by some other studies that present arguments encompassing both types of explanations. Fitoussi and Saraceno (2009) for example also support, in addition to their demand-sided argument, that “*On the other hand the search for high-return investment by those who benefited from the increase in inequalities led to the emergence of bubbles. Net wealth became overvalued, and high asset prices gave the false impression that high levels of debt were sustainable. The crisis revealed itself when the bubbles exploded, and net wealth returned to normal level. So although the crisis may have emerged in the financial sector, its roots are much deeper and lie in a structural change in income distribution that had been going on for twenty-five years.*” (p. 4).⁶ Here, the underlying rationale clearly refers to the credit *supply* channel.: inequalities had a causal impact on monetary policy which in turns leads to higher level of leverage. A similar argument is made more formally by Tridico (2012) who see the finance-led model of growth as a main factor explaining the current crisis. In his view, labor market flexibility and wage moderation have diminished workers' bargaining power which was partly compensated by increased borrowing opportunities due to financial liberalization. According to this view, the policy package which includes both labor market and financial liberalization has two consequences: an increase of the demand for credit due to the fall of workers' bargaining power, and an increase in credit supply explained by financial liberalization.

⁶ And when it comes to the part of monetary policy, Fitoussi and Saraceno (2009) are not more specific on which side of the credit market (supply, demand or both at the same time) bears the responsibility of the increase in debt – see above.



Besides, it is also clear that, if many analysis are consistent with a causal link from inequality to credit, we cannot ignore either that the macroeconomic background may have generated *additional* simultaneous increases in inequality and credit, with no causal link involved here - though the sign of the correlation is really not that clear for monetary policy, essentially because the effect of systematic monetary policy and monetary policy surprises are not the same. Disentangling what really comes from a causal relationship from inequality to finance from what is a simple correlation due to an omitted macro factor is one of the major challenge for research in the future years.

3. Inequality, Leverage and Financial Crises

We will review empirical studies focusing on the linkages between inequality, leverage and financial crises in three steps. First, we will focus on the links between inequality and leverage. Second, we will investigate a complementary channel going through the current account balance. Finally, we will show evidence concerning the link between leverage and financial crises.

3.1. Inequality and Leverage: some empirical evidence with various explanations

Bordo and Meissner (2012) propose to analyze empirically the linkages between the income share of the 1% top income, credit booms and financial crises. They use a panel of 14 mainly advanced countries from 1920 to 2008. They study the determinants of credit growth using macroeconomic variables and the level of inequality measured the 1% top income share. The goal is to see if the positive correlation observed between credit growth and the 1% top income share is still valid once controlling for traditional determinants of credit growth (the business cycle and other macroeconomic aggregates). They firstly analyze the determinants of credit growth using five-years period, and find that the cumulative change in the log of real GDP is the only significant determinant of credit growth. They also do not find any significant relation between inequality and credit growth when using the share of the top 0.01%, the top 5% and the top 10%. Then they use annual data and find that both the growth of GDP and the short-term nominal interest rate are significant determinant of credit growth. They still do not find any impact of income inequality.

We see three major drawbacks in their analysis calling for complementary empirical researches on the topic. First, they completely neglect the potential endogeneity between inequality and credit growth but also between credit growth and other macroeconomic variables. There is a strong literature on the impact of financial development on inequality (see section 4 below). Therefore, a two-way relationship has to be considered. Second, they only focus on the potential influence of top income share. In the theoretical analysis proposed by Kumhof and Rancière (2010), for instance, the causal impact of inequality on credit growth may come from two sources: the income increase of the richest (which increases the supply of credit) and the decrease (or stagnation) of the poorest's income (which increases the demand of credit). They do not test the latter. Finally, and probably the most important, they consider an overall private credit aggregate, without discriminating between household and firms' credit – section 3.3 below will emphasize how crucial this distinction is.

Atkinson and Morelli (2010) study the evolutions of inequality prior to 37 systemic banking crises over the period 1911-2010 (73% in OECD countries). More precisely, they observe the variations in the distributional variables taking a 5-year “window” either side of the crisis date. They find that inequalities have increased before the crisis in 10 cases out of 25 that could be identified. In 8 cases, they do not observe significant change in income distribution before the crisis and in 7 cases, they observe a decrease of inequality prior to the crisis. Evidence is therefore very mixed and it is very

difficult to get a conclusive answer on the possible causality but also on the sign of the relation. Globally, they tend to find that the relatively most predominant scenario is an inverted U-curve (increasing inequality before the crisis, decreasing inequality after the crisis) in 5 out of 25 cases.

Then, they compare the situation of countries where a systemic banking crisis has been identified in 2007-2008 and countries without such crises. When using the GINI coefficient, they find a similar share of countries where inequalities have increased during the ten years preceding 2007 in the two groups. When focusing on the 1% top income share, there is a slightly higher share of inequality-increasing countries in the group that faced a systematic banking crisis in 2007.

In their conclusion, they emphasize the potential heterogeneous role of income distribution changes: *“Different parts of the income distribution react differently, and the conclusions drawn regarding the origins and the impact of the crisis may depend which part of the parade we are watching. The top and the bottom may be the most affected; depending on the theoretical model adopted, either the top or the bottom may be more relevant to understanding the origins of the crisis”*. As already mentioned, it is therefore important to have a closer look to the potential impact of the whole distribution of income and not only the top income share, as in Bordo and Meissner (2012).

Other papers find more decisive evidence supporting the idea of a causal link between inequality, leverage and financial crisis. The first one uses a statistical methodology similar to Atkinson and Morelli (Belletini and Delbono 2013). The second one is closer to Bordo and Meissner but finds opposite results (Perugini and al. 2013). Another one uses time-series on the US (Christel and Morgan 2005).

Belletini and Delbono (2013) checked how many countries, that experienced banking crises, fell above or below the relevant OECD average inequality level, used as a benchmark. As in Atkinson and Morelli (2010), this analysis can be interesting but is not sufficient to claim any causal relationship between inequalities and financial crises as they do not account for the potential impact of confounding factors. However, based on a sample of banking crises over the period 1980-2010, they find opposite conclusions than Atkinson and Morelli (2010): they find that a large majority of banking crises has been preceded by persistently high levels of GINI coefficients. *“9 banking crises out of 14 have been preceded by persistently high levels of high (disposable) income inequality”* (Belletini and Delbono, 2013, p. 8). The main reason is that they focus on the *level* and not on the *evolutions* of inequalities as in Atkinson and Morelli (2010).

Perguni *et al.* (2013) perform an econometric analysis of the determinants of credit growth. Contrary to Bordo and Meissner (2012), they do find a positive link between income inequality and credit growth. They use a panel of 18 OECD countries over the period 1970-2007.⁷ Two main options may explain such differences. First, they consider the problem of endogeneity and reverse causation⁸. Second, they also explicitly take into account institutional drivers such as financial deregulation. They also use an alternative measure of credit (the ratio credit/GDP instead of the log of real bank loans to

⁷ The time-dimension of their analysis is therefore more limited than Bordo and Meissner (2012). They argue that it is not 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 non-monetary 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”* (Perguni and al. 2013, p. 4).

⁸ To address potential endogeneity, they use both internal (lagged values of the endogenous variables) and external instruments. As external instruments, they use institutional indicators related to labor and product markets, to the rule of law and trade openness. The authors assume that such variables are correlated with inequalities, investment and growth but without direct impact on credit.

the private sector) and propose to estimate the model in levels rather than in variations. Concerning this last choice, the authors argue that *“the literature emphasizes 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 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”*. (Perguni and al., 2013, pp. 12-13). All in all, they find a positive impact of inequalities on credit, *“suggesting that higher inequality directly drives higher credit, once its conventional determinants are controlled for”* (Perguni and al. 2013, p. 24). However, the authors do not find that financial deregulation magnifies the effect of income inequality on credit. But as deregulation is found to have a strong and positive impact on credit growth, the authors conclude that *“the two effects acted separately on credit expansion, without self-reinforcing patterns”* (p. 25).

Using time-series data, Christen and Morgan (2005) do find a *“strong positive effect of income inequality on household debt relative to disposable income as well as the components of the household debt (mortgage debt, revolving debt, e.g. credit cards, and non-revolving debts, e.g. car loans”* (p. 148). More precisely, they analyze the determinants of the *total* household debt using quarterly U.S. data covering all years from 1980 to 2003. They find that the income inequality effect is strongest for non-revolving debt and weakest for mortgage debt, but positive in all cases. They argue that this effect is likely to be driven by conspicuous consumption (and therefore a credit demand channel), and has increased over time.

To conclude this section, we can say that existing empirical evidences focusing on the link from inequality to leverage are still scarce and their conclusions are diverse. We identify several dimensions that may explain such divergence, calling for future researches in this area. First, we should identify if it is the *level* or the *evolution* of inequality that matters. The two hypotheses make sense but imply different empirical strategies. Second, the main challenge is to properly address the potential problem of endogeneity: financial development as an obvious effect on inequalities as we will see in section IV. Table 1 summarizes main results concerning the impact of inequalities on credit.

Table 1. The Impact of Inequality on Debt

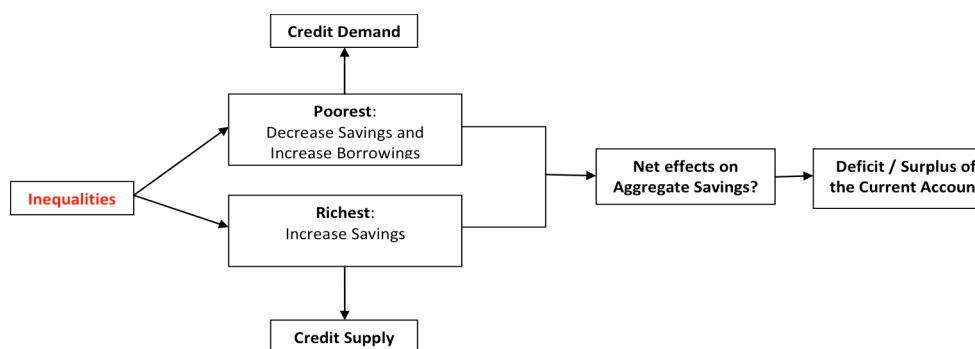
Paper	Dependent Variable	Inequality Measure	Key Results
Bordo and Meissner (2012)	Log(Bank Loans to Private sector)	Top 1 %	0 (no impact)
Perugini et al. (2013)	Priv. Credit/GDP	Top 1 %	[+7%; +11%]
Perugini et al. (2013)	Priv. Credit/GDP	Top 5 %	[+3%; +5%]
Perugini et al. (2013)	Priv. Credit/GDP	Top 10 %	[+2; +4%]
Christen and Morgan (2005)	Household Debt	Gini	+0,36%
Coibion et al. (2014)	Household debt	Ratio 90/10 (local distri.)	[-0,4% ; -1,1%]

Note: For each paper, the table reproduces the key estimates for the relationship between a dependent variable and an inequality measure. For the Gini, the reported impact corresponds to a 1 point positive variation. For all other measures of inequality, the reported elasticities correspond to a 10% variation in the considered measure.

3.2. Inequality and current account imbalances

Wondering about the impact of inequalities on leverage and debt immediately raises a related issue concerning the external equilibrium of the economy. If there is indeed an impact of inequality on overall leverage, one should consequently expect a modification of aggregate net savings, and therefore of current account – this is due to the well-known accounting identity: $X-M = S-I$, i.e., net exports must be equal to net savings. However, even if we take the impact of inequality on credit and leverage as granted, the sense of the consecutive impact on net savings is not straightforward. All papers mentioning a credit demand channel (see section 2.2. above) implicitly or explicitly assume that any increase of inequality should lead to a decrease of savings (and more precisely to an increase of borrowing) from the bottom of the income distribution. But the increase in income of the richest may also increase their level of savings (allowing more credit supply, as in the Kumhof and Ranciere's framework, 2010). The latter may or may not increase sufficiently at the aggregate level to offset the decrease in the net savings (equivalent to an increase of their indebtedness) of the poorest. The *net* effect of inequality on national savings is therefore ambiguous. This ambiguity has been confirmed by Schmidt-Hebbel and Serve (2000) or Leigh and Posso (2009) who found no systematic link between inequality and aggregate savings. Therefore, the impact on the external equilibrium is *a priori* undetermined. In any case, there is an increasing number of papers trying to deal with this issue. Figure 5 summarizes the possible linkages between inequalities and current account balance.

Figure 5: Inequalities and the Current Account



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declining labor income are associated with higher current account balances (through the corporate financial balance).

Some recent dynamics are consistent with this result. As argued in their paper, the United States or the United Kingdom have faced very strong increase of their top income share while the shares of labor and capital have remained constant. These countries have observed a strong decline of household saving and a strong increase in their current account deficit. On contrary, income inequalities in countries such as Germany have not changed fundamentally while their labor share has declined strongly. According to the authors, it is consistent with an increase in their current account surplus.



The key element in their analysis is the impact of both personal and functional inequalities on national saving. Behringer and Van Treeck (2013) find a *negative* impact of income inequality (personal inequality) on aggregate savings. It means the decrease of savings (or an increase in borrowing) from the poorest overcomes the increase in savings of the richest. In their view, this result can be explained by the expenditure cascade theories (Frank, 2014) which explains how consumption increase of the richest has a trickle-down effect on consumption of the poorest (see also Bertrand and Morse, 2012). This idea is supported by the fact they observe a stronger effect for the top income share than for the Gini coefficient. The top of the distribution would shape the consumption behavior of the whole population, explaining a negative impact on aggregate savings. Alvarez-Cadrado and El-Attar Vilalta (2012) also support this idea arguing that *“individual saving rates decrease with reference income while aggregate saving decreases with income inequality, when households care about their consumption relative to others”*.

However, Behringer and Van Treeck (2013) do find an opposite result as far as *functional* (profits vs. wages) inequalities are concerned. More precisely, they find a positive link between the corporate financial balance and the current account. Their main assumption is that household consumption is more sensitive to current income than capital gains. The consequence of that is *“aggregate personal saving declines much more strongly when the corporate sector distributes income to rich households in the form of salaries, bonus or dividends, than when it accumulates net financial assets, even if they are ultimately owned by the same households.”* (p. 8)⁹ Corporate gains are more likely to be saved, thus increasing the current account.

This distinction may explain the heterogeneous effects of inequality on the current account. In countries such as the UK or the US, it is mainly personal inequalities which have increased, explaining a worsening of the current account through decreased levels of savings. On the contrary, in Germany or China, the authors support that the increase of inequalities was mainly functional with a fall of the labor share. Increase in corporate gains has explained a boom of aggregate savings and therefore an improvement of the current account. Following the same idea, Belabed et al. (2013) build a stock-flow macroeconomic model where each country has a household and a non-household (corporate) sector. The household sector is divided into deciles and characterized by upward-status looking comparisons (in line with the relative income hypothesis and the expenditure cascade described by Frank, 2014). Country-specific institutions explain the dynamics of consumption. The model is then calibrated for the US, Germany and China and explains the dynamics of the current account by the worsening of personal inequality in the US and by a transfer from household to the corporate sector in Germany and China.

This distinction between personal and functional inequality is not possible in the Kumhof et al. (2012) theoretical framework as they assimilates the rich to the investors and the poor to the workers. Income and functional distribution are therefore equivalent in their framework, following the tradition of Kalecki (1954). They build a model where investors' income share increases at the expense of workers. Workers borrow to national and foreign investors to offset the drop of their income share. It supports aggregate demand but has a negative impact on the current account. They have a different interpretation than Behringer and Van Treeck (2013) concerning the heterogeneous

⁹ We should note that the distinction between different types of functional income is ambiguous. The first definition given by the authors is based on a distinction between profits and wages, which is equivalent to a distinction between labor income and capital income. However, they aggregate dividends with other wage income and quote several papers on the corporate veil, studying how investor consumption is affected by the type of returns. For instance, Baker et al. (2007) show that dividends have much stronger effects on consumptions than capital gains.

effects on inequality on the current account. In their view, it is financial market imperfections and the incapacity for workers to borrow from investors that explain why increased inequalities lead to an improvement of the current account in emerging economies. In these countries, only investors have an access to financial markets. They deploy their capital abroad (as national workers cannot borrow), leading to a surplus of current account. If this framework is convincing to rationalize current account evolutions in the UK, the US and in emerging economies, it is difficult to explain the case of Germany¹⁰. Using a panel of 18 OECD countries over the period 1960-2006, and when controlling for the traditional determinants of the current account, Kumhof et al. (2012) find a negative correlation of -0.1 with the 5% top income share and -0.3 with the 1% top income share. Taking into account the medium-term dynamics, the effect of a 1% increase of the top 5% income share is about -0.25%/-0.3% and the effect of a 1% increase of the top 1% income share is about -0.6%. One interesting feature of their empirical analysis is they include the impact of financial development / financial liberalization, echoing the debate on the demand versus supply credit channel developed in the previous section of this paper. Kumhof et al. (2012) find that a 1% increase of the ratio of credit to GDP leads to a 5% deterioration of the current account. That is why they conclude that *“if financial liberalization is an endogenous response to an increase in inequality, as Rajan (2010) claims for the United States, estimated coefficients for top income shares may capture part of the effect of financial liberalization”* (Kumhof et al. 2012, p. 10). However, they do not quantify the relative importance of such indirect channel. Their theoretical model shows that financial liberalization can be a rational answer to a shift of income from workers to investors as it can finance an increase in aggregate demand. But it also makes investors steer a larger share of their additional income to financial rather than real investments. It restrains aggregate supply by slowing down capital accumulation and leads to a higher increase of the rate of return to capital. Because of a higher increase of aggregate demand and a lower increase of aggregate supply, financial liberalization magnifies the effect of inequalities on the current account. “

Table 2 summarizes main results concerning the impact of inequality on current account.

Table 2. The impact of Inequality on Current Account

Paper	Dependent Variable	Inequality Measure	Key Results
Kumhof et al. (2012)	Net Current Account Balance	Top 1%	Short-term: -3 %; Medium-term: - 6%
Kumhof et al. (2012)	Net Current Account Balance	Top 5%	Short-term: -1% / Medium-term:-2.5%
Berhinger and van Treeck (2013)	Net Current Account Balance	Top 1%	[-4% ; -5.5%]
Berhinger and van Treeck (2013)	Net Current Account Balance	Top 5%	[-4% ; -5.5%]
Berhinger and van Treeck (2013)	Net Current Account Balance	Top 10%	[-3.4% ; -4.2%]
Berhinger and van Treeck (2013)	Net Current Account Balance	Gini	[- 0.32% ; -0.45%]

¹⁰ But as they only consider the case of income inequalities and more precisely top income shares, they do not take into account or try to explain the fall of the labour share observed in Germany. In their view, Germany is part of a group of countries where *“no or small increase [of inequalities]”* were observed.

Note: For each paper, the table reproduces the key estimates for the relationship between the net current account balance and an inequality measure. For the Gini, the reported impact corresponds to a 1 point positive variation. For all other measures of inequality, the reported elasticities correspond to a 10% variation in the considered measure.

3.3. From leverage to financial crisis.

The causal role of excessive private debt in triggering the global financial crisis is a quite ancient idea in the literature, going back to Fisher (1932, 1933) or Minsky (1977), and certainly one of the most consensual points in the literature. Even if some authors point out alternatively the role of sovereign debt (see Reinhart and Rogoff, 2010; Reinhart, Reinhart and Rogoff, 2012), almost all of recent academic researches emphasize the unstable dynamics resulting from private debt accumulation as the first trigger of the financial crisis.¹¹ In a very recent paper focusing on the euro area, Martin and Philippon (2014) develop a model of open economies within a monetary union where macroeconomic dynamics are driven by private leverage, fiscal policy, interest rate spreads and foreign demand. Their analysis confirms that household leverage is a key driver of both the boom and the bust dynamics. Excess sovereign leverage seems to be the smoking gun only in the case of Greece, where fiscal policy appears as the main driver of macroeconomic dynamics.

As emphasized by Schularick and Taylor (2012) however, systematic statistical evidence is not overabundant, and seems mainly focused on emerging countries (McKinnon and Pill, 1997; Kaminsky and Reinhart, 1999; Gourinchas, Valdes, and Landerretche, 2001). Regarding developed countries, the idea that systemic financial crises tend to be preceded by rapid expansions of credit has of course been pointed out for the 2007/08 crisis (Hume and Sentance 2009; Reinhart and Rogoff 2009), as well as in the Great Depression (Eichengreen and Mitchener, 2003). But until very recently, systematic evidence allowing a fine identification of crisis episodes was missing for developed countries. This gap is currently been filled by researchers who assembled long-run data for some industrialized countries.

Using a dataset very close to the one used in the works by Jordà, Schularick and Taylor (see below), Bordo and Meissner (2012) find a strong positive relationship between the probability of having a banking crisis and real credit growth, with a somewhat lagged effect: their downward benchmark implies that a regular 10% rise in real bank loans over a five year period leads to an increase by 5% of the probability of a banking crisis. Perugini, Holscher and Collie (2013) find very similar result on a dataset of 18 OECD countries, over the period 1970-2007: depending on specifications, a 10% increase in the ratio of credit to GDP raises the probability of a banking crisis by 3.5/4.5%.

Those results are consistent with those of Schularick and Taylor (2012), to whom Bordo and Meissner (2012) borrow their data on credit growth. In this paper, Schularick and Taylor (2012) highlight the divergence between monetary aggregates and credit dynamics in the 2nd part of the 20th century, and that the recurrent episodes of financial instability have more often been the consequences of credit booms gone bust.

Based on an original, very long (1870-2008) dataset for 14 developed countries, Moritz Schularick and Alan Taylor originated a consistent research program, together with Oscar Jordà, on the relationship between credit booms and financial crises. In Jordà, Schularick and Taylor (2011), crisis

¹¹ This does not mean of course that public debt is exempt from all responsibility: Jordà, Schularick and Taylor (2013b) show that the level of sovereign debt magnifies the negative impact coming from massive private deleveraging following financial crisis.



of 2007/08 is identified as one of the five big synchronized global financial crises over the considered sample, together with two crises in the 19th century (1873, and the early 1890s), 1907 and the one of the Great Depression, 1930/31. They show that the global crises are typically characterized by booms and bust dynamics (as measured by growth and investment) strongest than in the case of national crises, low short-term rates compared to real growth rates, and deeper recessions¹² than in normal times. More importantly in our case, they find that credit trends are a strong predictor of financial crises, in any case stronger than external imbalances.

Additional (but somewhat more moderate) evidence is displayed in Jordà, Schularick and Taylor (2014), which we already mentioned before (see box 2 above). Over 5-year windows (short business cycles), rises in mortgage lending and house prices clearly delivers information on the likelihood of financial crises, but cannot predict them perfectly. However, this predictive power becomes stronger in the post-World War II period, with the rapid rise of real estate lending. This raises indirectly another key issue, related to the distinction between household and firm credit. Based on a panel of 37 developed and emerging countries over the 1990 to 2007 period, Büyükkarabacak and Valev (2010) do find that a boom of the credit to the private sector as a whole is associated with subsequent banking crises. However, they also provide evidence that the household component have been the driving factor of that effect, whereas firm credit growth do not display such a robust and significant impact. The underlying intuition is that firm debt has a much more significant impact on long-term income than household debt. The result is that the growth in household credit is much more likely to raise the debt to GDP ratio over the long-run and therefore, the probability of a banking crisis.

When pointing the specific importance of household debt, one may be tempted to focus mainly on real estate bubbles: after all, the 2007-2008 subprime crisis was directly rooted in the huge amount of bad loans to people with low or moderate credit scores to buy homes. Relying on data for US counties over 2002-2009, Mian and Sufi (2010) emphasize that short-term finance also played a major role in the deepening and the persistence of the 2007-2009 recession.¹³ The first step of the analysis focuses on the *timing* of the recession, and shows that counties experiencing the largest increase in household leverage from 2002 to 2006 exhibited the sharpest relative decline in durable consumption as soon as the end 2006 (almost a year before the official start of the recession). The second step shows that counties with households more exposed to short-term credit (as proxied by credit card utilization rate as of 2006) experienced an acceleration of the recession from the fourth quarter of 2008 through the second quarter of 2009. Household leverage *as a whole* (i.e. including housing credit *and* short-term finance) appears as a powerful predictor of both the occurrence and the severity of the 2007-2009 recession across US. counties.

Mendoza and Terrones (2008) complete the previous analyses by distinguishing between credit booms in advanced and emerging countries, and by relating macro developments to micro, firm-level measures of leverage and financial constraints. Based on data spanning the 1960-2006 period, they

¹² This point is developed in Jordà, Schularick and Taylor (2013a), who find that financial crisis recessions are costlier than typical recession, and that more credit-intensive expansions tend to be followed by deeper recessions and slower recoveries. Mian and Sufi (2010) find similar evidence in the case of the USA for the most recent crisis: based on cross-sectional data for household credit reliance at the county level, they show that household leverage as of 2006 is a powerful statistical predictor of the severity of the 2007–09 recession

¹³ Note that Mian and Sufi (2010)'s approach is slightly different from the one consisting in assessing the impact of the dynamics of credit on the probability of banking crisis. Their focus is more general, in the sense that they study the impact of household leverage on several outcomes, including house prices, new housing building permits, default rates, unemployment and auto sales.



use event study methods to identify 27 credit booms in industrial countries, and 22 in emerging economies. Therefore, they do not discriminate, as Jordà, Schularick and Taylor (2011) do, between credit booms simultaneous to big global financial crises and the others. Mendoza and Terrones (2008) find that credit booms are associated with periods of economic expansion, rising asset prices, real appreciation and widening external deficits, followed by the opposite dynamics when the credit boom goes bust. Credit booms are also associated with procyclical movements in firm-level indicators of leverage, firm values, and use of external financing. When distinguishing between industrialized and emerging economies, they find that these movements are exacerbated for emerging countries. In particular, they find that credit booms are more likely to end in a financial crisis in emerging countries.

To make a long story short, a major insight from section III is that the type of leverage considered is probably crucial: it seems to be the major predictor of financial crises, and more widely recessions. Failure to examine the specificity of household leverage (compared to total leverage, or firm leverage) may also explain the mixed evidence found regarding the relationship between inequality and leverage. The clarification of this relationship, based on different measures of credit aggregates, seems therefore a priority avenue for future research. Table 3 summarizes main results.

Table 3. The Impact of Leverage on the Probability of Occurrence of Financial Crises

Paper	Dependent Variable	Leverage Measure	Key Results
Büyükkarabacak and Valev (2010)	1 when a systemic banking crisis occurred, 0 otherwise	Household credit/GDP	+7.6%
Büyükkarabacak and Valev (2010)	1 when a systemic banking crisis occurred, 0 otherwise	Business credit/GDP	0 (no effect)
Bordo and Meissner (2012)	1 when a systemic banking crisis occurred, 0 otherwise	Real bank loans	[+5% ; +15%]
Jordà, Schularick and Taylor (2011)	Log level effect, four years before crises vs. non-crisis trend	Loans/GDP	[+0.9% ; +1.1%]
Jordà, Schularick and Taylor (2014)	1 when a financial crisis occurred, 0 otherwise	Mortgage loans/GDP	+1.7%
Jordà, Schularick and Taylor (2014)	1 when a financial crisis occurred, 0 otherwise	House prices/income	+0.7%
Mian and Sufi (2010)	Changes in mortgage default rates	Household debt/income	[+0.5% ; +0.9%]
Perugini, Holscher and Collie (2013)	1 when a banking crisis occurred, 0 otherwise	Private credit / GDP	[+3.5 ; +4.5%]
Schularick and Taylor (2012)	1 when a financial crisis occurred, 0 otherwise	Real bank loans	[+3%; +4%]

Note: For each paper, the table reproduces the key estimates for the relationship between a dependent variable and a leverage measure. For all measures of leverage, the reported elasticities correspond to a 10% variation in the considered measure.

4. The impact of Finance on Inequalities

The identification of a causal link from inequality to financial crises is a difficult task. As we saw in section II, the theoretical mechanisms are numerous but the main challenge is to disentangle direct causal impact, indirect causal impact and coincident factors. In section III, we saw that there was no consensus in the empirical literature and we identified several challenges that must be addressed in future researches. One obvious dimension is the reverse causality. As we will see in this section, both financial development and financial crises have strong effects on the distribution of income. Here also, we have to be sharp on what we need to identify as the effect of financial development (the growth of credit), financial deregulation and financial crises may have contradictory effects



4.1. The impact of financial development in “normal times” on inequality

Why should capital markets imperfections have a persistent significant impact on income distribution in the economy? When informational asymmetries and transaction costs are strong, credit constraints are likely to be disproportionately more binding for those like the poor and small businesses who do not have collateral and/or long run relationships with credit suppliers. The development (both quantitative and qualitative) of the financial sector relaxes these credit constraints, and allows more constrained individuals accessing external finance. This, in turn, should improve the allocation of capital and alleviate income inequality.

At the beginning of the nineties, however, the theoretical relationship between finance and inequalities does not appear to be straightforward. The approach by Greenwood and Jovanovic (1990) predicts a Kuznets curve (an inverted U, i.e. a hump-shaped relationship) between financial development and inequality. In the early stages of development, when the financial sector is underdeveloped and the economy grows slowly, financial superstructure (i.e., all institutions designed to pool risks and increase the efficiency of capital allocation) begins to emerge as the economy approaches the intermediate stage of the growth cycle. Here the economy's growth and savings rates both increase, but poor individuals save less and thus accumulate wealth more slowly: income differences between high-income individuals and low-income ones will widen, resulting in an increase in income inequality. By maturity, the economy has developed an extensive structure for financial intermediation, and more agents see their income increase as they gain access to the financial intermediary sector. In the final stage of development the distribution of income across agents stabilizes, the savings rate falls, and income inequality will shrink.

However, the idea of a *linear, positive* relationship between financial development and inequality became quickly widespread in economic research. It is underlying, for example, Banerjee and Newman (1993)'s approach of the interactions between occupational choices and development. In their framework, financial market imperfections are mainly binding on the poor, who cannot support the high levels of investment required by entrepreneurial activities, and choose instead to work for other, wealthier, employers. The main conclusion of the model is to show that the initial distribution of wealth is crucial for determining the ultimate path of the economy – if initial inequality (the ratio of poor to wealthy people) is too high, the economy will get trapped in a low employment and wages equilibrium. One can see immediately, however, that a reduction in financial imperfections (that is, an increase in financial development), allowing more poor people to become entrepreneurs, will make this outcome less likely. A very similar argument is made in Galor and Zeira (1993), who also conclude to an impact of the initial distribution of wealth on aggregate output and investment both in the short and in the long run. One of the key underlying hypotheses is once again the presence of capital market imperfections, which hampers (indivisible) investment in human capital for those who do not inherit an initial large enough wealth – in other words, the poor. A better access to well-functioning credit markets should therefore reduce inequalities in individual investments in human capital, and therefore the impact of initial inequality on aggregate outcomes, if not inequality itself.

This now long-standing conventional wisdom about financial development and inequality (also shared, among many other, by Aghion and Bolton, 1992, 1997, or Piketty, 1997) was summarized in Levine's Chapter of the *Handbook of Economic Growth* (2005). He was unambiguous on the subject:



quoting the study by Beck, Demirgüç-Kunt and Levine (2004)¹⁴, Levine (2005, page 920) concludes: “the results indicate that finance exerts a disproportionately large, positive impact on the poor and hence reduces income inequality.” Levine acknowledges “the methodological weaknesses associated with cross-country regression”, but even in his most recent works (see e.g. Beck, Levine and Levkov, 2010), Levine never doubts the positive impact of financial development on inequalities.¹⁵

However, reviewing the literature over very recent years tends to question this consensual view. As we already pointed out, a crucial point is what one puts behind the idea of financial development. If the focus is on a restricted view of financial development, mainly based on the size of available credit and liquidity, most (but not all) papers conclude to a positive impact on inequality. When the perspective is widened to qualitative dimensions of financial dynamics of the past decades, like deregulation and liberalization, the sign of the relationship becomes much less clear, to say the least. Besides, a crucial methodological point relates to the way endogeneity issues are handled, insofar as reverse causality between financial development and inequality may arise for a number of reasons – in addition to the ones listed in section 2, Kim and Lin (2011) detail some other possible channels, many of them related to weak institutions (e. g., inequality affects *de facto* political power, which determines strongly the ability of the financial sector to develop and play efficiently its part, see Acemoglu et al., 2005).

In theory, financial liberalization and deregulation are implemented to simultaneously increase the volume of available capital, efficiency in its allocation and improve access to external finance for credit constrained individuals. However, Claessens and Perotti (2007) review evidence supporting the idea that the quality of institutions play a decisive part in determining the way financial reforms designed to increase access to external finance will effectively allow reducing inequality. A key condition is to prevent insiders to capture financial regulation to preserve their own, established interests. Claessens and Perotti (2007) provide evidence that captured reforms in developing countries deepen rather than broaden access to credit, and produce concentrated benefits while risks become socialized. Therefore, financial liberalization motivated to increase access may in practice increase fragility and inequality. In addition to a buildup in oversight institutions, Claessens and Perotti (2007) suggest that liberalization reforms should be gradual, aimed explicitly at reducing inequality of access and maintaining support competition.

Focusing on the case of India, Ang (2010) seeks to discriminate explicitly between the impact of financial development and the one of financial liberalization on the evolution of income inequality. The main results indicate that, while financial development can help reduce income inequality, financial liberalization seems to exacerbate it. Regarding financial development, Ang (2010) support the idea of a linear impact on income inequality, rejecting all the arguments in favor of non-linearity we presented before (Greenwood and Jovanovic, 1990; Tan and Law, 2012). Besides, Ang (2010) attribute the magnifying impact of financial liberalization on inequality to a rent-capturing attitude form well-connected elite, in line with the analysis by Claessens and Perotti (2007).

¹⁴ In a revised version of the paper, Beck, Demirgüç-Kunt and Levine (2007) confirm those conclusions: “Financial development disproportionately boosts incomes of the poorest quintile and reduces income inequality. About 40% of the long-run impact of financial development on the income growth of the poorest quintile is the result of reductions in income inequality, while 60% is due to the impact of financial development on aggregate economic growth. Furthermore, financial development is associated with a drop in the fraction of the population living on less than \$ 1 a day, a result which holds conditioning on average growth.”

¹⁵ In his Vox-Eu column published the 25th of October 2011, Levine states: “Research also shows that bank development disproportionately helps the poor.”



Gimet and Lagoarde-Segot (2011) provide a detailed analysis of the impact of financial development on income inequality, focusing on the characteristics of the financial sector (banking and capital market size, robustness, efficiency and international integration). Main results indicate that increased banking credit and credit market imperfections tend to raise inequalities, while bigger and more efficient capital markets tend to reduce inequalities. Quantitatively, the empirical analysis support that the banking sector exerts a stronger distributional impact on inequality than capital markets, and that the relationship depends on the characteristics (transparency and ability to allocate resources optimally) of the financial sector, more than its size. The analysis by Gimet and Lagoarde-Segot (2011) have therefore some interesting implications for reforms aiming at financial liberalization. More precisely, the authors propose a two-step process: *“In the first stage of reforms, prudential supervision and anti-monopolistic policies could be implemented in the banking sector, while corporate governance and information disclosure is improved in capital markets. Financial sector expansion policy programs (IPO, privatization etc.) could then take implemented as a second step.”* Liberalization and deregulation should be therefore gradual, partial and ordered.

Two main lessons can be drawn from this section. First, effects of financial development (and more specifically, of the growth of credit) and financial deregulation on income distribution are very likely to differ. Financial development *itself* has ambiguous effects on inequality and many studies find non-linear (sometimes deeply diverging) relationships between the two. The level of development and other factors such as the quality of institutions preventing rent-capturing behaviors should also matter. On the contrary, financial deregulation *itself* is more likely to increase inequalities. The second conclusion is that finance has an obvious impact on income distribution, whatever dimension we are focusing on. It is therefore absolutely necessary to keep this two-way causality in mind when trying to identify empirically the causal impact of inequality on leverage. Table 4 summarizes these results.

Table 4. The Impact of Finance on Inequality

Paper	Dependent Variable	FD measure	Key Results
Ang (2010)	Gini	Private credit/GDP, (M3-M1)/GDP	[-3% ; -0.4%]
Ang (2010)	Gini	Financial Liberalization	[+0.2% ; +0.7%]
Arora (2012)	Gini	Private credit or personal loans/SDP, population per bank branch	FD decreases (increases) inequality in the urban (rural) areas
Beck, Demirgüç-Kunt and Levine (2007)	Growth of Gini	Private Credit/GDP	[-0.05 ; -0.1] pp
Beck, Demirgüç-Kunt and Levine (2007)	Growth of the %age of the pop. living on < \$ 1 dollar / day.	Private Credit/GDP	- 0.5 pp
Beck, Levine and Levkov (2010)	Gini or Theil	1 when state permits in-state branching, 0 otherwise.	[-3% ; -2%]
Beck, Levine and Levkov (2010)	90th/10th or 75th/25th ratio	1 when state permits in-state branching, 0 otherwise.	[-10% ; -7%]
Clarke, Xu and Zou (2006)	Gini	Private Credit/GDP	-3%
Enowbi Batuo, Guidi and Mlambo (2010)	Gini	liquid liabilities/GDP, M2/GDP, Priv. Credit/GDP	[-0.5%;-0.2%]
Jauch and Watzka (2011)	Gini	Priv. Credit/GDP, Bank deposits/GDP	+0.23 pp
Kapell (2010)	Gini coefficient	Priv. Credit/GDP, stock market cap./GDP	[-2;-1] pp
Kim and Lin (2011)	Growth of the Gini	Priv. Credit/GDP, Liquid Liabilities/GDP, Bank Assets/GDP	[+ 2% ; +3%] when fin. Intermediation low
			[-12% ; -7%] when fin. Intermediation high
Law and Tan (2009)	Gini	Priv/ Credit/GDP, stock market cap./GDP	0 (no effect)
Law and Tan (2012)	Gini	Priv. Credit/GDP, Liquid Liabilities/GDP	Evidence of non-linear effects, quantitatively very small
Mookerjee and Kalpioni (2010)	Gini	Number of bank branches per 100,000 populations	Negative impact on inequality, but quantitatively hard to interpret
Roine et al. (2009)	Top 1%	(banking and stock market sectors)/GDP	+1 sd (50% of GDP) leads to +0.5 pp for top 1%

Note: *pp*: percentage points. For each paper, the table reproduces the key estimates for the relationship between a dependent variable and a Financial Development (FD) measure. Unless otherwise mentioned, the reported figures correspond to a 10% variation in the considered continuous FD variable, or a switch from 0 to 1 when it is binary. When estimates are multiple, and/or when quantitative assessment is not possible, qualitative interpretations are reported.

4.2. The impact of financial crises on inequality

If financial development and leverage have an impact on income distribution, financial crises also have additional and specific effects. The link with the development of the financial sector is obvious. The larger is the financial sector, the higher will be the probability of a financial crisis, all other things being equal. Besides, the consequences of financial crises will also depend on the size of the financial sector, as the larger the financial sector, the more severe the crisis.

The distributional impact of financial crises

Surprisingly, there seem to be only very few papers focusing specifically on the distributional impact of financial crises.

In addition to the impact of a slowdown in economic activity, Baldacci et al. (2002) identify three other channels through which financial crises affect poverty and income distribution: relative price changes, fiscal retrenchment and changes in assets. Devaluation leads to a fall in earnings of those employed in the non-tradable sector while it increases the demand for exports and therefore leads to an increase in employment and earnings in this sector. The poor may also be affected by the price increase of imported goods, especially food prices. As a crisis is generally followed by fiscal retrenchment and public spending cuts, it may affect social assistance outlays, which amplifies the consequences of the crisis on the poor. Lastly, changes in the value of assets have an impact on income distribution as changes in interest rates, asset and real estate prices are more likely to affect the wealth of the better off.

Baldacci et al. (2002) propose to analyze empirically the impact of financial crises on poverty and inequality using two types of data. First, they use cross-country macroeconomic data in a quasi-experiment setting, with a special interest in currency crashes. Then, they focus on the Mexican case and the impact of the 1994-1995 crisis using micro-data. As we have seen above, contrary to banking crises, they have ambiguous effect on the output, making the study of their distributional impact more difficult. On the whole, they find a positive impact on poverty headcount ratios and on Gini coefficients. However, the poor in the lowest income quintile are not the most affected. The most affected are those in the second lowest income quintile. The paper argues that it is explained by the capacity of the poorest to find income opportunities in the informal sector. Another result is that the association between income distribution and poverty is stronger when crises are associated with a fall of income. This fall of income accounts for 15 to 30 % of the variations in the poverty and inequality indicators. They do not find significant impact on formal unemployment. The rise in inflation is associated with an increase in the income share of the middle-income quintile while fiscal retrenchment is associated with a worsening in the distribution of income.

The micro-analysis shows a very different picture. If they also found a positive impact on poverty, they observe a decrease in inequality, explained by a stronger fall of income of the richest. They find that households that were already poor before the crisis were not necessarily the hardest hit. It shows that crises are indeed likely to have massive distributional impact, which can be hidden when looking at macroeconomic aggregates.

Galbraith and Jiaqing (1999) also propose to study the impact of financial crises on inequality. They also focus on currency crises, using the data set of Kaminsky and Reinhart (1996) in which financial crisis are defined as a weighted average of exchange rate changes and reserve changes. They find that the mean increase in inequality in the two-year period immediately following a crisis is 16.2%, against 3.2% in years without crises, the difference between the two being statistically significant. However, this analysis does not take into account possible confounding factors that may affect both the probability of a crisis and inequality. They also note that crises raise inequalities *“more in the most deregulated labor markets and less in more highly regulated ones”* (p. 7). They note that financial crises have had *“worse effects on Latin America workforces than on Asians, and worse on Asians than on the organized and politically powerful workers of the North”* (p. 7). If this possible interaction between labor market institutions and the effect of a crisis is interesting, this should be confirmed by more detailed empirical studies, focusing on the identification of a causal link.

Another paper is about the impact of systemic banking crises on the top income share in the US. Morelli (2014) shows that these crises had only little impact on the income of the top decile. He identifies three possible theoretical channels and takes into account the possible reverse causality



(e.g. the impact of inequality on financial crises). The three channels are (1) Stock and Real Estate markets dynamics, (2) the Economic Recession and unemployment and (3) the effect of government interventions and fiscal policies. He estimates a *total* short-run effect, taking into all these possible channels. He uses *gross* income distribution data, in order to exclude the direct effect of changes in fiscal policy. Marginal tax rates at the top are added as additional control variables in order to account for indirect effects of fiscal policy (e.g. the effect of changes in tax rate on *pre-tax* income). The author focuses only on *systemic* banking crises, based on three databases: Bordo et al. (2001), Reinhart and Rogoff (2008), Laeven and Valencia (2008). During the last century, the Great Depression of the 1930s, the Savings & Loan crisis of the 1980-1990s, and the Great Recession of 2008-2009 are characterized as systemic banking crises. Morelli (2014) uses data of the US top-income shares built by Piketty & Saez (2003). A first look to the data shows that the impact on top income share (top 0.01% and top 10%) was low. The stronger negative impact has been observed for the Great Depression. Morelli (2014) then conducts several counterfactual analysis that confirm the small impact of such crises on the top income share. Only post-crisis growth rate for the top 0.01% tends to be lower than what was predicted based on the pre-crisis trend. The author observes that *“the impact of US banking crises so far has been negative at the very top, positive at the bottom of the decile and, as a consequence, neutral for the entire top decile share”* (p. 23). These differences can be explained by the composition of income for the different percentiles. It seems that capital income is the main driver of the growth of total income for the richest while wage income appears to be more important for the 90-95% group. Morelli (2014) argues that these households benefit from a relative higher protection against unemployment and wage cuts compared to the bottom of the distribution. He observes that the rise of this group was observed when change in unemployment was more pronounced. Concerning the evolution of capital income, he suggests that it may be driven by *“endogenous behavioral response of investors to market conditions. (...) Investors might liquidate their risky assets during downsizing and re-purchase assets once the market prospects are improving.”* (p. 39). Also the high cyclical of top wage income may explain part of the effect for the top 0.01%. It is consistent with Frydman and Saks (2010) who show the strong correlation between the stock market index and the pay of firms' executives.

Impact on functional inequalities

A second trend of the literature focuses on the impact of financial crises on the labor share. As we just saw, it is likely that the dynamics of income around crises is likely to be different for capital and labor incomes. As noticed by Rodrik (1998), one feature of the globalization is that capital is much more mobile than labor. Because of that, labor is more likely to bear the largest burden in case of negative shocks, since capital can always threaten to flee. Using a large panel of countries, Diwan (2001) shows that currency crises are associated with strong fall of the labor share. This fall is only partially compensated in the following years. He thus argues that the long-term trend of declining labor share is mainly explained by financial crises.

Maarek and Orgiazzi (2013) find similar results using a panel of manufacturing sectors in 20 advanced economies. The interesting feature of the research is the exploration of within and across sector effects. By eroding the bargaining power of workers, financial crises may reduce the labor share within sectors. But it can also lead to structural changes with various effects among sectors. On average, they find that currency crises reduce the labor share by 2 percentage points and this effect comes mainly from within manufacturing sectors changes.

This last result is consistent with Bazillier and Najman (2012) who use a panel of developing and developed countries and aggregate data. This paper also extends the analysis to banking crises and finds very different results. If currency crises are also found to reduce the labor share by 2



percentage points in the three years following a crisis, banking crises affect primarily capital returns, at least the year of the crisis. They also find a stronger effect of banking crises in OECD countries with a more positive impact the year of the crisis but also with significant fall of the labor share in the following years. These results confirm the potential heterogeneity of financial crises' impact, depending on the type of crisis.

The distributional impact of the Great Recession

As it is done in Morelli (2014), it is possible to characterize the current crisis as a systemic banking crisis. Therefore, all previous studies focusing on currency crises have little predictive power on the potential impact of such crisis, as we saw that the distributional impact is more likely to differ for banking crises. At the cross-country level, the most comprehensive study on the distributional impact of the Great Recession (GR) is certainly the one of Jenkins et al. (2013) focusing on 21 OECD countries. Globally, they found little change in household distribution of income in the two years following the crisis (2007-2009). Over the first years of the crisis, it seems that social protection plays a role in supporting income households. As a result of this, the gross household disposable income rose in 12 countries. The most notable case is Ireland where the GDP falls by 11 per cent while the total household income rise by over 3.5 percent. As stated by the authors, *“In general, the household sector appears to have been well protected over 2007–9 from the impact of the downturn – in aggregate. The data cannot tell us about differences within that aggregate, but warn us that it would be misleading to infer the short-term impact of the GR on living conditions from looking only at GDP change”* (p.49). Building a counterfactual based on social spending prior to the crisis, they found that total household sector incomes would have fallen without the support of governments through the tax and benefit system in most countries. However, they think that consolidation policies, implemented after 2010 are likely to have a greater effect on income distribution.

Meyer and Sullivan (2013) analyze the evolution of income and consumption inequality in the US over the period 2000-2011. Using the 90/10 ratio as a proxy of inequality, they found that income inequalities have risen by 11% between 2007 and 2011, while consumption inequalities have decreased after 2005. During the Great Recession, one explanation is that the fall in asset prices had a strong effect on those with higher consumption levels. It leads to a negative wealth effect that could have a stronger impact on the richest households (De Nardi et al. 2012).

Cho and Newhouse (2012) do not directly study the impact of the crisis on income inequality but indirectly address the issue by studying the impact on different categories of workers using a sample of 17 middle-income countries. Female workers or low-skilled workers are not necessarily the most affected by the crisis. On the contrary, *“better educated and urban residents, to a lesser extent, also suffered disproportionate employment losses. The decline in wage employment was also slightly larger for more educated workers”* (p. 37). These results suggest little impact of the crisis on inequality, at least in middle-income countries.

This last result contradicts other studies in emerging countries such as South Africa (Leung et al. 2009) or China (Park et al. 2012) where low-skilled workers are found to be more affected by the crisis. In the US, Elsby et al. (2010) find that vulnerable groups, including low-skilled workers, were more affected by the crisis, suggesting a possible positive impact on inequalities. Hoyes et al. (2012) find similar results: *“the impacts of the Great Recession have been felt most strongly for men, black and Hispanic workers, youth, and low-education workers”*. They also note that the cyclicity across demographic groups is very similar than in previous recessionary periods.

The distributional impact of fiscal consolidation



Financial crises may have direct effect on the distribution of income but also indirect effect through the effect related to policy responses. In most countries, the crisis has been followed by fiscal consolidation which may also have strong distributional impact. Using a panel of 17 OECD countries over the period 1978-2009, Ball et al. (2013) show that fiscal consolidation are usually associated with a rise of inequalities, a fall of the labor share and a rise of long-term unemployment. This result is confirmed by Woo et al. (2013). Using a panel of emerging and advanced economies over the last three decades, they find that, on average, a fiscal consolidation of 1 percentage point of GDP is associated with an increase in the Gini coefficient of around 0.4-0.7 percent over the first two years. As unemployment is found to increase inequalities and that fiscal consolidation increases unemployment, they show that 15-20% of the inequality increase following a fiscal consolidation is explained by the rise of unemployment.

Fiscal consolidation may also have adverse effects on inequality if governments decide to cut social spending. Woo et al. (2013) show that a 1 percent decrease in social spending is associated with a rise of 0.2 to 0.7% in inequality. Lewis & Verhoeven (2010) show that crises have strong effect on social spending. If most governments try to protect investment in education, lowest income countries are more likely to cut social spending during crises. Bonnet et al. (2010) confirms that the Great Recession has been followed by cuts in social security spending. Concerning the pension system, they note that *“the current crisis has produced financial constraints leading to cuts or restrictions in benefit levels— specifically for pre-funded defined-contribution pensions — and negative rates of return on pension fund investments, undermining the benefit levels of those already retired, those close to retirement and those who will retire in future”* (p. 48).

Overall, the distributional impacts of crises are debated and there is no consensus on the sign of the relationship. However, it is clear that there *is* an impact. It highlights the need to address seriously the problem of reverse causality when dealing about the causal impact of inequality on leverage and financial crises. One additional remark is that the impact of financial crises on inequalities will also depends on the *size* of the financial sector. In other words, financial crises will have a stronger impact on the output and therefore on the distribution of income if financial development is strong. Therefore, in addition to the direct impact of financial development on inequalities (section 4.1.), the size of the financial sector has an impact on (i) the probability of a crisis (section 3.3), but also on (ii) the magnitude of the financial crises' impact. This last dimension should be considered when analyzing the two-way relationship between financial crises and inequality. Table 5 summarizes the results.

Table 5. The Impact of Financial Crises on Inequality

Paper	Dependent variable	Crisis Measure	Key Results
Baldacci et al. 2002	Gini, Income by quintile, Poverty headcount	Currency crises	Positive impact on poverty headcount and Gini. The second lowest income quintile are the most affected.
Baldacci et al. 2002	Poverty, Income by level	Mexican crisis	Increase in poverty and poverty gap but significant reduction on inequality
Bazillier and Najman (2012)	Labor Share	Currency and Banking Crises	[-2%; -2.5%] for the three years after a currency crisis, 0 for a banking crisis
Cho and Newhouse (2013)	Income by category of workers	Financial Crisis 2007-2008	Female workers and low-skilled are not the most affected. Better educated workers more affected.
Diwan (2001)	Labor Share	Currency crises	-5% during the crisis, -2.6% for the 3 following years
Elsby et al. (2010)	Income by category of workers	Financial Crisis 2007-2008	Low-skilled workers are the most affected
Galbraith and Jiaqing (1999)	Theil indices for income	Currency crises	+16,2 % in the two-year period after a crisis.
Hoynes et al. (2012)	Income by category of workers	Financial Crisis 2007-2008	Low-skilled workers are the most affected
Jenkins et al. (2013)	Gross household disposable income	Financial Crisis 2007-2008	0 (no effect)
Leung et al. (2009)	Income by category of workers	Financial Crisis 2007-2008	Low-skilled workers are the most affected
Maarek and Orgiazzi (2013)	Labor Share	Currency crises	-2%
Meyer and Sullivan (2013)	90/10 income ratio	Financial Crisis 2007-2008	+1 pp 3 years after crisis
Meyer and Sullivan (2013)	90/10 consumption ratio	Financial Crisis 2007-2008	-0.2 pp 3 years after crisis
Morelli (2014)	Top income 0.01%	Systemic Banking Crises	[-1%; -0.5%]
Morelli (2014)	Top inc. 10% - Top inc. 5%	Systemic Banking Crises	[+0.7%; +0.9%]
Morelli (2014)	Top income 10%	Systemic Banking Crises	0 (no effect)
Park et al. (2012)	Income by category of workers	Financial Crisis 2007-2008	Low-skilled workers are the most affected
Roine et al. (2009)	Top income 1%	Banking Crises – Currency Crises (binary indicators)	Banking crises: -1.1% for top 1% ; Currency crises: 0
Roine et al. (2009)	Top inc. 10% - Top inc. 1%	Banking Crises – Currency Crises (binary indicators)	Banking crises: 0 ; Currency crises: 0 for all.

Note: For each paper, the table reproduces the key estimates for the relationship between a dependent variable and a Financial Crisis measure. When the estimates are multiple, and/or when the quantitative assessment is not possible, qualitative interpretations are reported.

5. Conclusion

Linkages between inequalities, leverage and financial crises are numerous and complex. Based on the existing literature, we present in this paper an overview as exhaustive as possible of the potential, intertwined relations, surveying both theoretical and empirical evidence related to the various aspects of that subject.

The first conclusion we can draw is that there is strong evidence already supporting the idea that inequalities do play a role in the dynamics of credit, finance and possibly financial crises. The causal relation is much more difficult to establish *per se*, mainly because of the obvious, reverse impact of both finance and financial crises on the distribution of income. But overall, even if some links in the causation chain do deserve serious additional investigation, the presumptions for a circular causality



between the dynamics of inequality and various aspects of the financial sphere evolution over the past decades are very strong.

Overall, we emphasize that inequalities are likely to affect both credit demand and credit supply directly and indirectly. If it is quite challenging to disentangle both channels, we have presented a wide range of theoretical studies explaining why households may increase their borrowings in response to rising inequalities. It is consistent with the dynamics observed in developed countries prior to the Great Recession. An alternative explanation relies on an increase of credit supply because of both an accommodative monetary policy and financial liberalization. Even in that case, it is very likely that inequalities have played a role. Stagnant income of the poorest households (but also more generally the middle-class) may have pushed Central Banks and Governments to implement policies aiming at supporting aggregate demand through increased borrowings for these households. As credit boom appears to be the main determinant of financial crises, the possible direct and indirect impact of inequalities on such boom is a fundamental dimension to be taken into account by policymakers.

One cannot completely exclude that the relation between inequalities and credit boom has been more *coincident* than causal, as financial deregulation tends to increase inequalities and aims also at increasing credit. Banking deregulation and policies promoting the development of finance have been a common trend of economic policies in most countries since three decades. We argue that inequalities may be both a cause and a consequence of such deregulation. As already mentioned, it is likely that policymakers were pushed to increase the access to credit in response to stagnant income. But it is also clear that deregulation has played a role in the huge rise in inequalities observed in the 1990s and 2000s. To summarize our view, we think that the links between inequalities and leverage are likely to be a mixture of direct and indirect causal relations, as well as coincident factors. The remaining challenge is to empirically measure the relative weight of each channel.

All channels call for several, consistent policy responses, holding both ends of the “chain”. This would notably imply on the one hand, to regulate strongly the extension and prerogatives of the financial sphere, and on the other hand, to tackle the root of growing inequality. Our view is that these two dimensions should be seen as the two sides of the same coin as the development of an unregulated financial sector and the rise of inequalities are likely to be two dynamics feeding each other and creating financial instability and possibly financial crises. After all, policy implications would be similar if inequality did cause excess leverage, or if the two were the joint by-products of the same shift to the right of economic policy. The main challenge remains to tackle both simultaneously.

Financial reforms to be implemented are already known. For instance, Gimet and Lagoarde-Segot (2011) argue that *“in the first stage of reforms, prudential supervision and anti-monopolistic policies could be implemented in the banking sector, while corporate governance and information disclosure is improved in capital markets.”* Such a program seems primarily directed towards developing countries but even in developed countries, it appears that governments simply forgot the “prudential supervision” and “improved corporate governance” that should have been the minimal counterparts to financial deregulation. This should be a starting point of all financial reforms. More generally, if we combine conclusions of papers showing that a too big financial sector may hurt economic growth (Arcand et al. 2012) with others showing that credit booms are the main driver of financial crises (Schularick and Taylor 2012), it seems reasonable to think that the size of financial sectors and banks should be reduced in many countries.

The second dimension is inequality. The link with financial crises is obviously not the only reason why inequalities should matter for economic policy¹⁶ but it is an important one. There are a multitude of policy tools available and governments should use several of them. Such equalitarian policy should be based both on *predistributive* and *redistributive* policies. Concerning the latter, Diamond and Saez (2011) suggest different policy recommendations using progressive taxes. They calculate that the *optimal top income tax rate* should be much higher than what it is in most countries. Concerning *predistributive* policies, the goal is to improve *pre-tax* income distribution. If there is no consensus on the detailed content of such policies, the literature on *social investment* (Morel et al. 2010) gives some insights on what should be done. The role of labor market institutions is also crucial (Checci & Penalosa 2008, Bazillier 2013b). Overall, the important point is to consider these two types of policies as complementary and not substitutable.

This article opens new perspectives for research as well as strong background for new policy proposals. Our main message is that equalitarian policies and regulation of finance should be seen as two top priorities to be tackled simultaneously.

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