



LUND UNIVERSITY

School of Economics and Management

Master in Economic Development and Growth

Inequality as a Cause of Systemic Banking Crises

– Some New Theory and Evidence

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Abstract: This thesis argues that systemic banking crises and inequality go hand in hand, with inequality in front. Through showing how factors commonly found to influence banking crises, such as household and business debt levels, asset prices, default ratios, and credit growth (all important in Minsky-type bubbles) theoretically can be attributed to decreasing relative wages of households, and/or concentration of wealth among "hoarders" it sheds light on a potentially strong link between inequality and banking crises. It proposes a simple theoretical model formalizing this link through a capitalist spirit utility function and Minsky-type asset inflation, and tests the effect of inequality on the probability of suffering a systemic banking crisis through a multivariate logit approach. The results are conclusive in favor of growing inequality being a significant factor increasing the probability of suffering a systemic banking crisis in the future. Reducing income inequality is argued to be a first-best policy option for reducing financial fragility.

Key words: Banking Crises, Inequality, Asset Bubbles

EKHR92

Master thesis, second year (15 credits ECTS)

June 2013

Supervisor: Håkan Lobell

Examiner: Anders Ögren

1. Introduction

The last five years has for obvious reasons seen an upswing in the literature examining the causes of financial crises. During the period we have once again been reminded of how the failure of banks and financial systems can have devastating effects on lives throughout the global economy. Finding out why crises such as these happen, and how to prevent them, is one of many important questions waiting to be answered in the field of economics. This thesis humbly seeks to make a contribution towards performing this paramount task.

The literature has come to agree on a narrative as to *what* happened in the financial melt-down of 2007-2008, but there is no clear consensus as to *why* it happened. This thesis will propose one possible explanation by combining two theoretical approaches; that of Minsky-bubbles and that of wealth hoarding. Through doing this it will argue that many of the mechanisms commonly assumed to influence banking crises are linked to inequality, or more specifically to the concentration of financial wealth, and that wealth concentration thus might be one of the main drivers of systemic banking crises in a "very macro perspective". It will begin by going through some selected theoretical works, before formulating the Minsky-hoarder nexus. It will continue to give a simple theoretical model showing how the Minsky-hoarder nexus can cause a banking crisis, before it goes on to an empirical analysis to shed light on the relationship between inequality and banking crises during the last 20-40 years. Through applying a multivariate logit approach, it shows that income inequality seems to be one of the factors positively influencing the probability of suffering a systemic banking crisis. The result is robust and consistent through a number of specifications, even as most other explanatory variables loose significance. We thereby conclude that reducing inequality is not only an obvious goal for moral reasons, but also an important policy measure towards increasing financial stability.

2. Theoretical Causes of Systemic Banking Crises

In this section of the thesis we will summarize the theoretical causes of systemic banking crises (hereafter SBCs) most commonly referred to in the literature. Extra weight will be given on the topic of financial bubbles, as this is where the thesis

seeks to make a contribution. The US “sub-prime” bubble which busted in 2007 will be referred to frequently. Even though the particulars of this bubble are not described here, we assume them to be well known to the reader. The frequent use of this crisis as an example is motivated by having a common reference upon to base arguments, and should not be taken as implying that this crisis is the main topic of concern; crises and bubbles in general are the main concerns of the text.

Before we start, some justification might however be in order. What will be summarized below are not detailed descriptions of the regulation of financial markets; we will not dig into legal frameworks, nor the corporate structure or cultural attributes of financial systems. Explanations disregarding such factors could very well be futile. As argued by Llewellyn (2002), the main causes of banking crises *might* be found within the banking sector itself, rather than in factors easily quantifiable and comparable between countries. However, as economists we seek to shed light on the quantifiable factors influencing economic activity, we search for explanations in the aggregate rather than the specific. To borrow a quote from the late Charles Kindleberger: "*For historians each event is unique. Economics, however, maintains that forces in society and nature behave in repetitive ways. History is particular; economics is general.*" (Kindleberger & Aliber, 2011, p.24, also quoted in Kaminsky and Reinhart 1999). I agree with Kindleberger, and would be so bold as to say that what is interesting is not the specific, but the general. This is why we in the following will focus on just that.

2.1. Banks and Fragility

Banks supply credit, which in a sense means that they buy and sell money. However credit is not a good like all others, it is a very special type of good, and the normal supply and demand framework does not suit well for understanding it. The main difference is that the price for credit is not paid at the same time as it is transferred, it is paid over a time-span following the transfer - uncertainty and risk thus become important factors in the functioning of banks. Furthermore banks serve a very important role in the economy, diverting means to those who want it in the present from those who want it at a later point in time. Without this type of intermediating, the economy would function very differently. In other words; banks are a paramount part of the economy, all the while they are disproportionately

exposed to risk. This of course makes for a very interesting topic for economists, and the literature on the topic is as voluminous as can be expected. When the main factors believed to influence bank fragility are summarized below, some contributions are thereby necessarily left out, while others are extremely simplified. The selection is based on what variables will be included in the empirical part of this thesis, and on what will be important for the following theoretical discussion. I am however confident that the reader is well familiar with the workings of the banking system, and does not need to be explained the entire workings of the banking sector, if not I can recommend the concise survey from Allen & Carletti (2008).

Banks lend the money of depositors to borrowers (debtors). They promise to pay back the deposits plus an interest to the depositors, and they finance this by charging an interest rate on the debtor. However, banks can never know if the debtor is willing or able to repay the debt. In fact this is one of the reasons why banks arose; there are economies of scale in evaluating the risk of debtors. The depositors accept a lower rate of return on their money than what the debtors pay, in return for the banks taking the risk. The system works as long as this *spread* covers all losses from defaults. Problems arise if the banks misjudge the risk and are unable to repay their depositors based on what they receive by the debtors. Problems can also arise due to the temporal imbalance between the bank's liabilities (deposits) and assets (money owed by debtors). Where as depositors usually are free to withdraw their money at any point, debtors usually have a fixed down-payment plan. Thus, if a large portion depositors want to withdraw their money at the same time, the bank does not have the means to repay them. They are in other words reliant on withdrawals roughly equaling new deposits and down payments at any point in time. Thus the banking system is inherently unstable, and relies on everyone trusting the system for the system to work. (Levine, 1997)

This makes banks vulnerable to *bank runs*, either based on fundamentals, or simply self-fulfilling prophecies. The less money they keep liquid the more vulnerable they are. Banks are therefore often obliged by regulations to keep a percentage of total assets in government bonds. In addition governments often supply deposit insurance to depositors, meaning that they ensure the repayment of deposits below a certain limit to all depositors in the event of a bank failure. This on one hand lowers the risk of a bank-run, but on the other hand can increase the moral hazard of banks as depositors choosing banks based on their

soundness often is believed to be an important factor reducing bank risk (see Diamond & Dybvig, 1983).

As Stiglitz (1972) argues, moral hazard will however always be a problem in the banking sector, as potential gains for bank owners (if separate from depositors) always will be bigger than potential losses, which are limited to bankruptcy. This is especially true for high leverage levels, where potential gains can be astronomical. Accordingly, regulatory frameworks on leverage, on risk-taking, and on criminal accountability, becomes important for stability.

Interest rates can also affect bank stability, especially if banks are dependent on interbank loans or loans from foreign sources, both of which often have interest rates that can vary independently of the rate they receive from debtors. Thus higher interest rates on money borrowed in the market entail higher losses for banks. This again is related to financial openness, as more financial openness increases the probability of foreign loans by banks and of high interest rates to fend off speculative attacks on the currency (especially with fixed exchange rates) (Demirguc-Kunt & Detragiache, 2002). Financial openness also affects stability through larger probabilities of sudden out-flows of capital, especially if not backed by sufficient holdings of foreign currency by the central bank (see Obstfeld, Shambaugh & Taylor, 2008).

High interest rates are also potentially less stable than low interest rates even when considering equal rates on assets and liabilities. Very profitable projects are often risky, thus the debtors willing to pay a high interest rate are on average more risky than those willing to pay a low one. Under asymmetric information, this entails higher risk for the banks. (Mishkin, 1991)

Bank concentration, meaning the number of banks in an economy can also have effects on stability. Large banks (relative to the economy) might be more inefficient than smaller banks due to dis-economies to scale, and more prone to risk than smaller banks due to being more sure of a bail-out in the case of insolvency. On the flip-side, there might be economies of scale in banking, rather than dis-economies, and large banks might have larger profits and thus be less prone to risk (at the fear of losing future profit). (Demirgüç-Kunt & Levine, 2000)

These are some of the basic attributes of banks that influence their fragility. Later we will see how variations in these attributes are found to influence the probability of suffering an SBC in the empirical literature, but first we need to see just what constitutes an SBC.

The failure of one bank can, and often does, cause other banks to fail, especially if the banks in the system are fragile at the point. This can happen through decreased trust in the system causing either withdrawals, higher prices for interbank credit, higher default rates due to lower profitability in the economy, all at once, or general liquidity shortages (Diamond & Rajan, 2005). When many banks fail at the same time, we conclude that something was wrong with the system, not just the banks, and that's why we call such incidents *systemic* banking crises¹ (SBCs).

The factors mentioned above all influence the stability of the banking sector. The primary channel through which they do this is by influencing to what degree the banking sector can handle unpredicted losses, or shocks. Shocks in this context can be sudden decreases in investment and/or consumption, causing higher default ratios and thereby losses. Such shocks can come from decreased foreign demand, from a down-turn in the business cycle, from natural disasters, or from a sudden drop in asset prices. We will in the following focus on the latter, namely a sudden drop in asset prices as caused by a bursting asset bubble.

2.2. Bubbles

A potential contributor to the inherent bank instability created by the factors mentioned above are asset bubbles. Asset bubbles have many definitions, and different definitions give different economic significance. The definition is thus not arbitrary. We will in the following employ a definition of bubble as *price increases of assets, that do not correspond to their future returns (or “fundamentals”), but rather to expected future price growth*. With backward looking expectations, this means that decelerating prices will cause investors to demand less assets, potentially causing a crash in the prices. This crash potentially transmits to banks through defaults. This can, but does not always, lead to a systemic banking crisis. Since bubbles are essential to our theoretical approach, we will in the following give more in-depth explanations of bubbles from different theoretical approaches, but first we will dwell a bit more on the definition.

Notice that our definition goes against that of Blanchard (1979) and Blanchard & Watson (1983). They argue that bubbles can grow and burst even with rational expectations and full and equal information. If the risk of collapse is known by everyone, but returns with no collapse are high, prices will be above their

¹ For a more formal definition see section 4.4.

fundamental value when the collapse happens. They show how this can have real effects on the economy, even when rationally expected real returns are based on fundamentals and equal among assets². This definition is the basis of the non-interventionist bubble-policy sketched by Bernanke in (2002), which can be seen from his modeling in Bernanke and Gertler (2000). Here the authors include an exogenous asset bubble in a standard new Keynesian macro economic model, but assume that the bubble is either based solely on fundamentals, or detached from fundamentals while assuming that future investments are made solely based on fundamentals (ibid., p. 6). This approach to bubbles will not be applied here, due to the clear evidence from the recent crisis supporting asset prices deviating significantly from fundamentals and causing busts much more prominent than what is predicted by these models. The evidence is excellently summarized by Bernanke himself in his 2010 speech for the American Economic Association (Bernanke, 2010), though combined with little or no regret with regard to policy.

The monetarist approach on the other hand, view bursting bubbles as the result of failed monetary policy rather than market failures, meaning that as long as central banks keep to their jobs of supplying enough liquidity to obtain stable inflation, the market will take care of the rest (Friedman & Schwartz, 1982, 1986). This is basically a rejection of the entire concept of bubbles, and arguably disregards the possibility of differing inflation rates between sectors, most notably between assets and consumption (see O'Driscoll Jr 2009, Kindleberger and Aliber 2011, p15). The monetarist approach builds heavily upon Irving Fisher (1933), who was the first to show how excessively high debt-levels could cause substantial crashes when repayment causes deflation. The basic argument is that repayment causes asset selling which lowers the circulation of money, which causes lower prices and activity, which in turn increases the *real* debt - meaning that debt liquidation can actually cause *higher* real debt levels, inducing a negative spiral. In Fisher's view, over investment is not a problem without over indebtedness, and over indebtedness need not cause problems as long as deflation is avoided (if needed through government interference) in the process of liquidation.

Friedrich Hayek and the Austrian school on the other hand view bubbles much like our definition, but sees investment behavior as secondary to monetary policy as the culprit. While being proponents of the neutrality of money in

2 The effects of such bubbles are however much lower than those of the Ponzi-type bubbles described below.

the long run, “the Austrians” see bubbles as a result of monetary policy (both from private and central banks) prior to the crisis. They see too low interest rates as an accelerator of asset prices over consumption prices, due to interest rates working firstly through investments, while affecting consumption only secondary (Butos, 1985; Hayek, 1939; Leijonhufvud, 2009). Their policy suggestions are of the “hands-off variety” post crisis, meaning that expansionary monetary policy would be futile, or even contra-productive due to inflating asset prices even more. This argument is, as we will see, very similar to that of the Ponzi-style bubbles of Minsky, and in fact the kinship between Hayek and Minsky might be stronger than what one would think. The ongoing debate on whether central banks should focus only on keeping inflation stable (as measured by CPI), or also should be watchful of asset prices to avoid bubbles from occurring, is thus in many ways a debate between the monetarist approach and the Austrian/Minsky approach. Many argue for a middle ground, where the central bank keeps an eye on both general inflation and asset prices (see Cecchetti, Genberg & Wadhvani, 2002; Gruen, Plumb & Stone, 2005).

Before we move on to Minsky, we should however mention that the short survey above clearly is superficial, and in some ways outdated. But we believe that the basic ideas depicted here and below are exhaustive with regard to the basic mechanisms behind bubbles. Clearly many of the newer contributions to the field are overlooked here, and much progress has been made on modeling bubble behavior in different markets. However, we see these contributions as building, more or less explicitly, on the basic underlying explanations of bubble behavior mentioned above and below. For an excellent review of the more recent contributions we can however recommend Brunnermeier (2003).

2.2.1. Minsky

Hyman P. Minsky warned of the inherent instability of financial systems during the relative tranquil period of the late 1960's. Though not given much heed at the time his theories have recently reemerged and been given mainstream attention (Cassidy, 2008).

As mentioned above, the Minsky approach to bubbles is related to that of the Austrian school. Common to them both is the emphasis on the price distorting

effects of investments. Minsky separates between *hedge*, *speculative*, and *Ponzi* financial units. Hedge units are able to repay their loans and interest based on their cash flow, speculative units can only pay interest from their cash flow and are dependent on selling assets or rolling over debt (taking on new loans) in order to repay maturing debt. Ponzi units cannot pay either interest, nor repay their loans based on cash flow, and are dependent on selling assets or rolling over debt in order to pay their creditors anything at all. Speculative and Ponzi units are dependent on the increase of asset prices being higher than the level of their interest rates in order to survive. Thus they are dependent on an uninterrupted flow of capital to their sector keeping asset inflation high (Minsky, 1977, 1992). Thereby any slight increase in the interest rate or exogenous decrease in investments could cause all Ponzi units to get out of their positions simultaneously, potentially causing a complete collapse of asset prices, which in turn will cause a Fisher-style crash in the economy³. The novelty of Minsky's approach is that the increasing relative prices of the bubble-asset is both the cause and effect of speculation, and thus bubbles are sectoral, reinforcing, and can appear at any time and point. Furthermore, in Minsky-bubbles the monetarist/Fisher solution of boosting inflation will not be sufficient, as it does not solve the fundamental problem of over-priced assets. Once a bubble is inflated, it inevitably needs to burst. The government in Minsky's view has to keep the bubble from building, or keep the burst from spreading through the economy, but never keep an already existing bubble from bursting (ibid).

Following Kalecki (1942), Minsky sees the rate of profit as depending on (and actually equaling) the level of new investments and government deficits for all periods (Minsky, 1982, 1992). This is based on Kalecki's steady state assumptions, and is arguably one of the more dubious sides of Minsky's theory. Using this simplification he argues that big government can reverse crashes through increasing deficits, and thereby boosting investments and profits once again. His policy suggestion is big government active in "resource creation and development" rather than consumption creation (Minsky, 1982, p.13). He however becomes less bombastic later in his career, emphasizing the innovative tendencies of finance, and the role of government in being one step ahead in the regulatory framework in order to hinder the emergence of new Ponzi-type financial instruments. He argues

3 Fisher himself actually points to a Minsky-type effect exacerbating bubbles some 30 years before Minsky
Fisher, Irving. (1933). "The debt-deflation theory of great depressions." *Econometrica: Journal of the Econometric Society*:337-357.(see Fisher 1933, article 47b)

forcefully towards the adaptation of policy to changing environments, and that there exists no one-size-fits-all policy (Minsky, 1993).

The bubbles of Minsky seemingly necessitates Galbraith-type irrationality (see Galbraith, 1994) or limited information, as the Ponzi-units rely on ever-increasing asset prices and razor thin margins. The investors holding assets when a bubble bursts are doomed to loose, and rational investors would shun the Ponzi scheme all together. The literature however has several explanations as to why investors would chose such a tactic. Managers not knowing their own relative ability to value risk and profitability can cause herd behavior (Scharfstein & Stein, 1990), habits, heterogeneous investors with regards to risk and/or information and different forms of irrational expectations can do the same (see Campbell, 1999, for a survey) and, as we will see below, so can inequality. Minsky himself wasn't too preoccupied with the formal modeling of investment behavior, he found it sufficient to state that prolonged periods of stability would increase the probability of Ponzi-behavior, basically assuming backward looking expectations on volatility.

Charles Kindleberger contributed to the understanding of bubble behavior from a different angle. He applied the Minsky hypothesis in his monumental *Manias, Panics and Crises – A History of Financial Crises* (5th edition, post mortem, co-written by Aliber, 2011), and through a rigorous historical review arguably contributed more to the Minsky hypothesis than Minsky himself. The historical perspective allows for a case-by-case walk through of bubbles using Minsky's arguments while disregarding some of the theory's shaky foundations. He argues that the government, functioning as a lender of last resort, if possible should stop the bubble from growing, but if too late should “*wait long enough for the insolvent firms to fail, but not so long as to let the crisis spread to the solvent firms that need liquidity—‘delaying the death of the strong swimmers,’ as Clapham⁴ put it*” (ibid. p. 241). His major contribution is however on showing how speculative behavior seems to arise in periods of high investments and general optimism, due to inflation in asset prices making it profitable.

The Minsky-Kindleberger hypothesis summarized above is the basis of the argument introduced later in the thesis, where we will argue that wealth hoarding is one of the fundamental causes of Minsky-bubbles, and thus that

4 Kindleberger here quotes Sir John Clapham (1945). *The bank of England: a history* (Vol. 2). The University Press.

inequality increases both the probability of and the severity of a bubble burst. First we will however look at some other bubble-theories.

2.2.2. Schumpeter

Schumpeter, following Kondratiev, introduced technology-driven long waves of growth (Schumpeter, 1934; Schumpeter & Fels, 1939). In his view new technology developed by innovators creates large profits which induces investments by imitators. The large influx of investments is followed by many failures, and in a period of unrest the strong firms survive while the weak ones go under (ibid). These periods of “creative destruction” are arguably analogue to the bursting of a technology driven bubble. Elliot (1980) actually argues that Schumpeter's analysis is similar to Marx's, but where Marx saw ever growing wealth and decreasing profit rates as forcing a revolution and bursting the bubble of capitalism, Schumpeter saw limits to investments as causing declines in profit rates, bursting the growth cycle, and spurring new innovation. So, arguably both Schumpeter and Marx wrote about the bubble-concept, but we will in the following focus on the Schumpeterian approach.

We will not dwell to much on the literature here, but we will give a short account on some contributions, in order to compare his legacy to that of Minsky. Kleinknecht (1981) shows how the emergence of new technologies are likely to come during a crisis, due to investors' need to try something new. He thus anticipates the separation between the transformation and rationalization periods defined by Lennart Schön (1991, 2009), basically equivalent to the installation and deployment periods defined by Carlota Perez (Freeman & Perez, 1988; Perez, 2006). In these theories new technology finds its way into production during the aftermaths of a declining growth cycle, the new technology after a while gives large profits to the firms who master it, this gives incentives for more investments in the technology and spurs rapid growth. As the technology spreads, the profit rates decline, technological innovation gives way to cost-cutting, and the high investment rates become unsustainable, a recession follows, and investors (having exhausted the possibilities of the old technology) start looking for a new technology. Thus the cycle repeats itself, making SBCs regular events.

Minsky himself (being a former student of Schumpeter) became an outspoken critic of Schumpeter's work towards the end of his career, saying that he

was too influenced by Walrasian economics, and thus blinded by the *axiom of real* (Minsky, 1986). By this he meant that Schumpeter underestimated the influence of monetary values, and disregarded the lessons from Keynes on the relative demand for liquidity. He meant that the asset pricing effects in the last stages of Schumpeter's growth cycles were understated, and that the monetary influence thus was underestimated. As argued by Knell (2012) Minsky relied more on the animal spirits of Keynes than the innovations on Schumpeter, and the two can thus be seen as opposing theories on what is basically the same issue.

Carlota Perez (2009) however argues that the two approaches are relevant for different cases. She differentiates between Major Technology Bubbles (MTBs), which are basically Schumpeterian boom-bust cycles, and Easy Liquidity Bubbles (ELBs) which are basically Minsky Bubbles. She argues that the last stages of MTBs are driven by inflating asset prices, but that since they are started by technological opportunities, they are driven by a *technological pull* rather than an *easy credit push*, and that this makes them fundamentally different from ELBs (ibid). In her eyes, the reaction to the dot-com bubble was what created the sub-prime bubble. Low interest rates, inflow of liquidity from emerging markets, and purchasing power being kept up by cheap imports, allowed the financial industry of the US to keep on unchecked. This combined with the computerized trade developed in the proceeding years let speculation be institutionalized in the financial system, and allowed for a devastating Minsky bubble to grow in real estate (ibid). This argument supports Minsky's assertion of the financial industry being the industry most prone to Schumpeterian innovation, and in need of vigorous regulation (Minsky, 1993). Thus the policy advice becomes similar to that of Kindleberger (2011); making sure that the bubble really burst - all the while keeping the burst from spreading to the rest of the economy. The difference in their initial conditions become secondary, as they are driven to the last stage by the same mechanisms.

2.2.3. Inequality and Credit Demand

When bubbles appear in real estate, the Schumpeterian explanations clearly do not apply, and as argued by Perez (see above), the bubble is likely to be driven by Minsky-type asset price inflation and easy access to credit. However, in Perez' framework part of the reason could be found in the preceding period of introducing

new technology. This period increases inequality and induces heavy investments, if the following bubble does not burst sufficiently credit will continue to flow to new sources (Perez, 2006). Keen (2009) argues along the same lines, creating a model where speculation becomes profitable, and where federal policy pushes financial capital in to new bubbles when one bubble bursts. Thus the relative small recessions in the US economy following apparent bubbles in the 90's and early 2000's were due to high financial fortunes, and the FED pushing their problems ahead. He followingly argues that the sub-prime bubble will be the last, due to no new possible takers of credit. The same basic argument is found throughout Rajan's (2011) influential book *Fault Lines*.

However, households are arguably different from other financial units, and as shown by Barbra and Pivetti (2009), the debt levels of US households cannot be explained by cheap credit and increasing housing prices alone. They implicitly argue that households are less prone to Ponzi-type speculation, and that their increased indebtedness has to be due to other factors. Their basic argument is that the decreasing or stagnating real wages of working class US citizens, coupled with the entry of new expensive technology on the market and the large relative increase of the purchasing power of their neighbors, cause them to want higher consumption than what they can currently afford. Thus they argue against the standard life-cycle model, where borrowing is used by rational individuals to smooth consumption over a life-time. This also contradicts the theories saying that increased debt levels were rational given the low interest rates and ever-increasing housing prices (be it due to Ponzi bubbles, or other causes). In their eyes the extraction of property wealth for consumption is better explained by the wish to uphold relative contemporary consumption levels (when faced with decreasing relative wages) even at the expense of future consumption levels. When large segments of society does this, aggregate consumption is boosted in the present at the expense of aggregate future demand. Thus causing a boom and bust cycle. In their eyes, this was made possible partly through policies aimed at boosting consumer credit (low interest rates and relaxed regulation on credit-worthiness), but most of all due to rising inequality (ibid). Their theory is motivated by the evolution of wages in the US, where income inequality has increased substantially the last 30 years, while consumption inequality has remained stable (see David, Katz & Kearney, 2006; Iacoviello, 2008; Krueger & Perri, 2006).

The theory of Barbra and Pivetti (2009) is based on a “keeping up with the Joneses” assumption on utility, where utility from consumption depends on the consumption of others in the economy (see Abel, 1990), with roots in the emulation motive of Veblen (2005 [1899]). Kapeller & Schütz (2012) use the same assumption in their model of Veblen-Minsky cycles. They build a model where increased inequality builds demand for credit. Their model predicts increasing interest rates with aggregate credit (as assumed by Minsky), but like Minsky they assume that the banking sector's margin of safety decreases with time of financial stability, meaning that the increased demand for credit will be supplied at rather stable interest rates. This is not sustainable, and busts will inevitably happen. They simulate their model, and find regular bubbles when inequality is present (*ibid*).

Matteo Iacoviello (2008) has another approach to inequality and banking crises. His model operates with homogeneous households making decisions based on expected future income, increased inequality within this framework entails increased magnitude of the modeled idiosyncratic shocks to income. Households want to save in good times and borrow in bad times to smooth consumption, and since all deviations from mean income are viewed as temporary, household debt levels increase with inequality. This explains how income inequality can rise while inequality of consumption remains more or less constant. However, it assumes complete income mobility, and the debt levels that arise in the model are completely sustainable (*ibid.*) – thus it has little or no explanatory power for crises. The same goes for Krueger and Perri's (2006) modeling, which is basically identical for all practical purposes.

Cynamon & Farazzi (2008) explain rising household debt ratios with cultural effects. They view the life-cycle model as overly simplified, and reject the rational choice paradigm. They chose to view consumption propensity as exogenous and evolving over time, they thus explain the recent crisis with increased consumerism in the US society, and Minsky-type bubble formation (*ibid*).

Abel (1990), who developed the relative consumption utility function used in much of the above-mentioned literature, used it to show how the equity premium puzzle can be explained (safer savings are preferred when relative consumption levels matter). Gómez, Priestley and Zapatero (2002, 2009) use a similar argument, but for wealth. They show how agents caring about their relative wealth evolution when making investment decisions makes for a negative risk premium for domestic assets, thus allowing for economy-wide asset bubbles

occurring in an open-economy environment. DeMarzo Kaniel and Kremer (2008) use relative wealth to explain herding behavior. Through using a finite horizon, multi-generational savings decision model they show how changes in wealth affects relative asset prices due to actors seeking the same risk profile as their peers in order to avoid being poor when the others are rich. Thus relatively wealthy generations can theoretically herd towards risky investments, inducing negative risk premia and possible bubbles.

It should be mentioned that the literature reviewed in this sub-section is rather selective. The increased household debt levels in the US were for a long time viewed as a positive, and even stabilizing, factor (see for example Campbell & Hercowitz, 2005). After all, the period from the mid-80's running up to the 2007 meltdown was famously dubbed “the Great Moderation” due to the unprecedented financial stability of developed economies in the period. The policies running up to the crisis were, and still are by some, praised for their stabilizing effects (see Bernanke, 2004; Stock & Watson, 2003). In other words, where Perez (2009) and Keen (2009) saw the FED as pushing problems ahead when directing credit towards households, others saw them as doing a good job. The focus above has however been on household debts in a bubble context. Given the evidence, I believe this focus is justified.

2.2.4. Inequality and Credit Supply

Above we have seen different theoretical explanations as to how bubbles can grow. We have seen how businesses and households potentially can demand credit to such a high degree that the debt to income ratios become unsustainable under any other circumstances than increasing asset prices. We have also seen how investors can herd toward one type of asset, making for a sectoral asset bubble. However, this might not be the whole story.

Most of the above mentioned literature focuses on the demand side of credit, showing how increasing asset prices, cultural factors, new technology and/or inequality, coupled with eased access to credit can make financial units increase their debt to income ratios. However, as argued by Bernanke (2010), the high credit-supply growth of the US in the pre-2007 era cannot be explained by monetary policy alone. And even though some of the theories above can explain how investors tend to herd towards the same type of assets, they cannot explain excessively high

aggregate credit rates. Leaving aside foreign capital for a moment (we'll get to that) increased borrowing by some has to be accompanied by increased saving by others. Indeed the last 40 years in the US has seen a large increase in the net wealth of the richest percentages of the US economy, and they have been major suppliers of credit (see Budria, Diaz-Giménez, Quadrini & Rios-Rull 2002; Kennickell, 2003; Piketty & Saez, 2003). Furthermore, this increase in wealth inequality has been much higher than the corresponding income disparities. As argued by Cagetti & De Nardi (2008) the high savings rates of the very rich can not be explained by regular life cycle models, even with very high returns to capital⁵. This means that asset bubbles cannot explain the supply of credit, only the demand for it. The models that have been successful in predicting the high savings rates and concentration of wealth among the very rich, and thereby the increase in credit supply, are models in which the standard life cycle assumption is modified to either allow for inter generational bequests (agents receiving utility by leaving inheritance to their children), or having dynasties (generations of families) be agents either having low inter temporal discount factors or receiving utility directly from holding wealth (ibid). In either case, “initial” inequality increases through time as well-off agents have higher saving propensities than less well off agents. Carroll (1998) justifies letting wealth be a contributor of utility in itself by the fact that very rich people seldom chose to consume of their wealth during their lifetime. Whether this is due to wanting to leave wealth for their children, or simply due to having a level of consumption that is saturated and not wanting to waste wealth, is rather irrelevant. His argument is that since accumulation of wealth up to the point of death is desired by the very rich, any utility function that prefers holding wealth at death over not holding it (for agents with very high consumption levels) is an improvement over utility functions that do not (ibid). We will later incorporate such a utility function in a standard growth model, showing how unsustainable debt accumulation will be the outcome.

This kind of utility functions makes net savings less dependent on demand for capital. Rather than being a way of putting financial wealth to work for future consumption, saving becomes a way of keeping and increasing wealth as a goal in itself. Kumhof & Rancièrè (2010) uses this kind of utility function to show how increased bargaining power of the 5% richest (giving increased income inequality) will increase their supply of credit to the rest of the economy. Thus household credit ratios (dubbed leverage) will increase. Through a calibrated simulation they show

5 The return to capital was in fact quite low in the period in question.

how high leverage levels will entail a crises unless the bargaining power of the 95% increases, and that the crises itself will not do much to decrease leverage levels, thus more crises will follow unless inequality is reduced. This is the only formal modeling on the link between wealth hoarding and inequality that I know of.

The link has however been treated in several less formal work (often relying on anecdotal evidence). One such example is Stiglitz (2013), who recently has argued that reallocation of means from rich savers to poorer consumers is pivotal to recovery. Branco Milanovic (2009) argues that the immense holdings in the hands of the very rich paved the way for increased risk-taking, while Thaker & Williamson (2012) view the increased wealth of the few as causing instability through the purchase of political power, which has caused financial deregulation, not unlike the argument put forward by Paul Krugman & Robert Wells (2012) who in addition see the aversion towards contra cyclical financial policies as a result of the political influence of the very rich. Hockett & Dillon (2013) see the wealth concentration as causing instability through an argument combining increasing saving propensities with wealth and a Kaleckian view on effective demand - saying that savings will not stimulate the economy (consumption) directly, only investments.

Much of the above mentioned literature applies a logic which was first put forward by Marx, though seldom, if ever, acknowledging it. Marx therefore deserves some treatment when reviewing their arguments. In the his approach, the propensity to save among the capitalists makes for accelerating savings, but seeing how savings must equal investments and investments are decided by demand for production, a very unequal society is likely to suffer from too little demand to absorb the level of desired investments, and as the savings of the capitalist class accelerates, capitalism either has to move to new markets or implode (Robinson, 1942, p. 50-60). The Marxist approach is intuitively appealing, but relies on equilibrium of demand and supply being the exception rather than the rule, and is incompatible with most economic modeling. Kalecki applied a similar approach, and showed how savings stimulate investments more than consumption, and thus cyclicality is an inevitability (Sebastiani, 1989). The Keynesian approach relates to this, and the concept of effective demand deals with similar issues, but as argued by Shaikh & Semmler (1989) does not take into account the independent movements of supply and demand. Arguably the Marxist approach is a different paradigm all together, and incompatible with standard economic modeling. This could be one

explanation as to why prominent economists such as Stiglitz formulate similar arguments in editorials, while leaving them out of formal modeling. None the less, the arguments could bring aspects on the table that are easily overlooked in formal modeling, that is why I chose to include it here.

2.2.5. Foreign Influence

Most of the above mentioned literature focuses on closed economies. If bubbles arise from increased demand for credit, the openness of the economy has little influence (except of course for the possibility of rapid capital flight at the burst of a bubble). However, when looking at the supply side of credit, openness clearly is very relevant. Increases in credit supply can come from foreign sources, and can thus come much faster than within a domestic context. Furthermore; hoarding of the rich need not increase investment domestically, as they have the opportunity to invest abroad. Nonetheless, there is much evidence towards a “domestic bias” when it comes to investments and general placement of financial capital (see Abel, 1990), so the credit supply argument need not vanish with an open economy.

A lot has been written on the relationship between financial openness and fragility, and we have too limited space to go through it all. With regard to asset bubbles Wolfson (2002) argues that financial globalization functions as an accelerator of financial innovation, making regulations harder. In his view this facilitates a form of global Ponzi-scheme. On the less extreme side of the discussion, there is a growing body on literature seeing the hoarding of foreign reserves by governments in developing countries as an insurance against rapid outflows of capital. In this view the increasing levels of expensive short term debt from foreign sources of the private sector necessitates the government to hoard low-yielding foreign reserves such as US securities. This constitutes a net loss for the economy, but is arguably necessary in a global context where capital controls are frowned upon. (see Aizenman & Lee, 2007; Jeanne & Rancière, 2008; Mendoza, 2004; and Rodrik, 2006)

Allen & Gale (2000) propose a model where globalization of financial flows increase the propensity of asset prices to grow out of proportion by simple herding behavior of international investors. Thus the onset of a bubble from any of the causes mentioned above could be intensified by a internationally open financial market.

Others again argue that financial liberalization hinder harmful repression by governments, increasing efficiency and stability in the workings of the financial market. Stulz (2005) in this regards views a Miskin-style financial lobby-class as a road block to the full potential gains of liberalization, but argues that liberalization lowers their power, and thus can increase stability.

With regard to the current crises, the argument has often involved international financial flows. Some argue that the under-evaluation of the renmibi and the high savings of China are causing a “savings-glut”, where unsustainable debt in the US is the result (Bernanke, 2005; Funke & Rahn, 2005; Lemmen, 2006), this is basically a global supply-of.credit argument in reverse, where the rich are blaming the poor for supplying them with too much credit. If we follow the self-insurance arguments mentioned above, and see the total gains of the US economy from the spread between debt and foreign holdings, the picture however changes drastically, and as argued by Krugman (2007) this could be a reason for the dollar collapse.

Clearly the picture gets a lot more complicated when the openness of an economy is taken in to consideration. The theoretical effects from inequality become less prominent, regardless; both the supply and demand side effects of income inequality will (if present) function also in an open economy. If the effects will be stronger or weaker is hard to say. Surely the demand for credit is easier met in a global credit market (especially if the domestic market is considered safe). Regarding the supply side of bubbles, one could argue that a bubble driven by supply from the very rich could potentially be double harmful in an open environment, as the very rich are likely to be more prone to capital flight in the event of a collapse.

2.3. Bubbles in Banking Crises

Above we have seen how different theories explain asset bubbles. The relation to banking crises is obvious, however it is hard to evaluate the relative importance of asset bubbles in historical banking crises without going through them case by case. Fortunately we do not have the space available to do so in this thesis, and even more fortunate; others have done it for us. In the following we will give some examples of authors seeing asset bubbles as driving forces in different SBCs. There is in general

seldom agreement on the causes of SBCs (if there were, this thesis would be unnecessary), and the referred works should not be taken to represent the general opinions of the academic field on the issue.

The highly influential works of Reinhart & Rogoff (2008) gives an holistic view on crises and asset bubbles, they show how investors and policy makers repeatedly fool themselves into thinking that “this time it's different”. Exuberant investments, ever-increasing asset prices, increasing debt levels (often from foreign sources) are found to be essential ingredients in episodes of financial crises over the last centuries, and every time the bubble is disregarded until the moment it bursts. Schularick & Taylor (2009) use a similar approach of comparing long run historical data, when they find that credit booms gone wrong constitute the basic explanations for most major crises. Kindleberger & Aliber (2011) show how major historic crises are driven by credit booms creating asset bubbles, through rigorous qualitative analyses.

On a less macro-level Borio, Kennedy & Prowse (1994) give evidence towards increased credit access creating asset booms in among other places Scandinavia, Japan, the UK and Netherlands in the late 80's. All the while Hunter (2005) gives an account of asset bubbles causing crises in Latin America and Asia in the late 20th century, as well as the stock-market crash dubbed the dot-com-bubble (all though the latter did not transfer to become a full-blown SBC, it was close). Hoffmann (2010) argues that the same basic mechanisms were driving the string of crises in central and eastern Europe in the transition from the Soviet era, showing how fast and furious liberalization led to a bonanza inflating assets to unsustainable levels.

All-in-all, one can say a lot about the role of asset bubbles in banking crises, most of which others have already said. Safe to say, they are viewed as important drivers of financial and banking crises by policy makers and academics alike.

3. Theoretical Modeling

In this section of the thesis we will incorporate both Minsky bubbles and wealth hoarding in a simple theoretical model, showing how income inequality increases

the probability of growing and bursting a Minsky-type bubble in a closed economy. Thus giving a theoretical framework to the role of credit supply in asset bubbles.

3.1. The Model

We modify a simple Ramsey–Cass–Koopmans model to include wealth hoarding and Minsky price behavior on assets. By doing this we show how inequality can produce negative real interest rates in steady state, rendering steady state improbable and paving the way for bursting asset bubbles.

Producers

We introduce an aggregate production function with decreasing returns to labor and constant returns to capital.

$$f(K_t, L_t) = \alpha L_t^\sigma K_t$$

Where L is labor, and K is capital. The finished good can be used for consumption or capital.

$$f(K_t, L_t) = C_t + \delta_{k,t} K_t + (K_{t+1} - K_t)$$

However, we will later show how the depreciation rate increases with capital levels, making the return to stable capital levels decrease with the size of the capital stock (thus indirectly giving decreasing returns to capital).

Consumers

We split the consumers in to two groups, where one owns the banks and thus decides on investment positions, while the other can chose to save or borrow from the banks. The bank owners (group 2) receive interest on capital and loans, and bear losses directly. The means available to consumption for the two groups, will thus be given by:

$$R_1 = w_1 + S_{z,t+1} - (1 + r_{z,t} - \delta_{z,t}) S_{z,t}$$

$$R_2 = w_2 + \sum (1 + r_{i,t} - \delta_{i,t}) S_{i,t} - S_{i,t+1}$$

The net savings (S_i) of group two can come in two forms, where $S_{z,t}$ is net lending from group two to group one and $S_{k,t} \equiv K_t$ is invested capital. The return to capital (r_k) and interest rate on loans (r_z) are determined in the market.

The depreciation of the different types of savings are assumed to be given by the following:

$$\delta_{i,t} = \rho_i S_{i,t} - m_i \left(\frac{S_{i,t} - S_{i,t-1}}{S_{i,t-1}} \right)$$

ρ_i is here a parameter that describes how the depreciation rate of a form of saving relates to the stock of savings, the idea is that higher levels of investments or lending necessitate more risk. m_i is what we decide to call the *Minsky factor*. It is included to account for a rise in investments increasing the price of assets, thus reducing the depreciation rate, or even making it negative. It is incorporated in the depreciation in order to be able to operate with one stable price for the joint capital/consumption good.

The consumers in group one gain utility only from consumption, while the bank owners (group two) are assumed to have capitalist spirits, and thus receive utility from holding wealth (following Carroll, 1998).

$$U_1 = \sum \beta^t \gamma \ln(c_{1,t})$$

$$U_2 = \sum \beta^t [\gamma \ln(c_{2,t}) + \omega S_t]$$

Profit maximization:

We set the price of the good as the numéraire. Profit maximization thus gives us:

$$\sigma \alpha L_t^{\sigma-1} K_t = w_t$$

The entire profit goes to the capital owners (group 2), who make future investment decisions based on the return of capital observed in the current period:

$$\pi_t = \alpha L_t^\sigma K_t (1 - \sigma) \equiv r_{k,t} K_t \Rightarrow r_{k,t} = \alpha L_t^\sigma (1 - \sigma)$$

The labor force is given by $L_1 + L_2 = 2$, $L_1 = L_2$, seeing how there is no disutility to labor, the use of labor is thus predetermined at 2. Even though the two groups of labor are homogeneous in regard to productivity, we let them differ in wages based on negotiating power, making for:

$$w_{1,t} = (1 - t) w_t \quad \text{and} \quad w_{2,t} = (1 + t) w_t$$

Utility maximization:

We assume steady state (using the transversality condition) and maximize the infinite horizon utility.

$$\frac{dU_1}{dl_{t+1}} = 0 \Rightarrow 1 + r_t = \beta^{-1}$$

The consumers in group one will demand limitless borrowing at interest rates lower than this, and supply limitless savings at higher interest rates.

$$\frac{d U_2}{d l_{t+1}}=0 \Rightarrow \frac{l}{K}=\frac{\rho_k}{\rho_l}$$

The ratio between physical capital stock and total credit to consumers (group 1) will be given by the inverse ratio of the depreciation factors.

$$\frac{d U_2}{d K_{t+1}}=0 \Rightarrow r_i-\delta_i=\frac{\gamma \rho_l \frac{l}{K}-\omega \frac{w_2}{K}}{\omega\left(1+\frac{l}{k}\right)}$$

While the real return to both capital and credit rate will be given by the expression above. Note that the Minsky factor does not affect the steady state, as the net growth in any asset always will be zero.

Equilibrium

Combining the steady state optimization conditions lets us find the equilibrium real return to savings:

$$r_i-\delta_i=\frac{\gamma \rho_k-\omega(1+t) \alpha \sigma 2^{\sigma-1}}{\omega\left(1+\frac{\rho_l}{\rho_k}\right)}$$

We will not model any of the dynamics here, but rather do an interpretation of this steady state.

It can easily be proven that the *steady rate* of real return to savings expressed above always will be negative for all positive values of ω and t , and become more negative for higher values of these parameters. Meaning that if one group receives utility from holding wealth, and that group has higher income than the rest, they will hoard wealth until the real return to savings become negative. The higher income they have, and the more they value wealth, the more negative returns to savings have to be for them to stop saving.

If they did not receive any utility from wealth, the steady state return to capital would of course be zero as in the standard model. Even zero returns are potentially unstable with the minsky factor, as any negative shock to investments would cause an increase in depreciation, causing lower real returns and even lower new investments the next period. However, if a central bank were to react by lending out money at negative real interest rates this would not only boost investments, but also give new liquidity to the system, ensuring stable inflation. When a movement back towards steady state occurs, investors having borrowed

from the central bank at negative rates, have to repay less than they borrowed, thus money supply is increased also in the long run. The same will in fact be the case for contractive policies, where the central bank borrows money (sells bonds) at positive rates. Thus the central bank can rather easily secure a stable variation around the steady state.

However, when steady rates are negative, investors will constantly push real returns towards negative values. If the negative rates start exceeding inflation, holding cash could become the optimum. The central bank thus needs to keep inflation higher than the absolute value of the steady rate. The problem arises as central banks giving rates on bonds higher than the (negative) steady rate will have to keep supplying ever increasing amount of bonds to supply the appetite of savings. This of course will cause *deflation*.

If deinvestments sets in, quick stimulation is needed in a Minsky-world like this. Imagine a situation of zero returns on government bonds: as savings in capital and credit to consumers increases, the riskiness of projects will increase and so will the returns, eventually reaching zero. At which point investors switch to buying bonds, and deflation sets in. Stimulating the system with negative rates on bonds in this situation will only cause investors to sell (or buy less) bonds (as they will demand an ever-increasing amount of bonds at rates higher than the steady rate). If investors sell bonds, this will stimulate investments and increase money supply temporarily, but a return to pre-stimuli levels will necessitate taking liquidity out of the system. Thus the central bank needs to keep lowering rates until it reaches the steady rate expressed above. At negative rates on bonds, securing stable and positive inflation becomes a real challenge without any form of redistribution (from the hoarders to the consumers). Without inflation, negative returns to savings will cause a movement towards holding cash. That; ladies and gentlemen, is when the shit hits the fan.

Clearly this is all very abstract, and may be hard to relate to the much more complicated real world, but the basic principle is very easy. If wealth is wanted for its own sake, it will be demanded even at a price. When holding wealth has a price, real returns to savings are negative. When real returns to savings are negative, this is because investments are unprofitable. Stimulating the economy through credit will thus be impossible, or at least unsustainable. Redistribution will be needed.

Redistribution is here meant in the wide sense, government spending through printing money can be viewed as redistribution. Anything that increases the purchasing power of consumers over hoarders, as long as it is lasting, is helpful. Credit is not, if it has to be repayed. The effects of increased credit to consumers in the model is a higher depreciation rate, which is basically higher default levels. Which brings us to the point. This is related to banking crises in the following way: Credit will keep flowing to consumers and firms until the real return reaches the negative steady rate, and there is little the central bank can do about it. When returns reach the steady state, any shock will cause the minsky factor to set in, and asset prices will plummet (if they haven't already do to cash becoming higher yielding than investments even based on fundamentals). At which point we will have a bank run, and most likely a systemic banking crisis.

The only way of preventing this in the model is low enough levels of inequality. If we assume that the “capitalist spirit” parameter, ω , is a function of relative income, the problem can be avoided all together with low enough levels of inequality. Even though motivated differently, the consequences are basically Kalckian – stimulating investments is not enough, consumption needs to be stimulated directly, and this can only be done through redistribution.

Is this model relevant? Arguably it fits well with the US reality, where government bonds face high demand even at negative returns⁶. Whether or not this is due to inequality is of course a major question. In the following we will see some empirical evidence linking systemic banking crises to income inequality, which fits well with the theory.

4. Empirical Analysis

6 Informed and alert readers might note that the low real return to financial assets in this model contradicts the literature on the equity premium puzzle, which shows that returns on equity in general is high, and much higher than on government bonds. To that, a couple of things can be said. Firstly, the recent experiences has lowered the calculated premium significantly. Secondly, the return in the literature is calculated based on stock indexes, and only a minuscule part of the calculated returns are actual dividends, the rest is increasing prices of stocks. Meaning that the fundamental return (that not based on asset inflation), arguably has been historically low, or even negative. This model however predicts that the real return to equity (including price increases) will be equal to that of government bonds, which clearly has not been the case historically. In fact the distorting taxes often blamed for the equity premium puzzle are likely to be beneficial in this model, reducing the risk of asset inflation. This is however an extension we will not embark on in this thesis.

Above we have seen how inequality in theory can have an effect on the growth and burst of asset bubbles, which in turn can lead to banking crises. We will in the following seek to measure the influence of inequality on the probability of suffering a systemic banking crisis. First we will give a short survey of previous empirical literature measuring the causes of systemic banking crises, before we apply a multivariate logit approach giving evidence pointing towards a causal link leading from inequality to systemic banking crises.

4.1. Data

We have an unbalanced panel of 104 countries, for the years 1970-2008. We have observations of 15 variables. Our dependent variable will be a binary variable indicating if an SBC is happening or not for each country-year, for this we have two alternative sources.

4.1.2. The Crisis Dummies

CRISIS1 defines a crisis as an SBC if one of the following criteria are met: non-performing assets reached at least 10 percent of total assets at the peak of the crisis, the cost of the rescue operations was at least 2 percent of GDP, emergency measures such as bank holidays, deposit freezes, blanket guarantees to depositors or other bank creditors, were taken to assist the banking system, or large-scale nationalizations took place. The variable is constructed by Demirgüç-Kunt & Detragiache (2005) (DDK hereafter) based on Caprio & Klingebiel (2002) and IMF country reports. The authors use judgment calls to find a correct ending date of the crisis based on quantitative and qualitative information, but where they cannot make an informed decision, a duration of three years is assumed. It covers the period 1980-2002, and identifies 77 SBCs.

CRISIS2 uses a slightly wider definition for SBCs. The variable is constructed by Laeven & Valencia (2012), and defines a crisis as happening if there are significant signs of financial distress in the banking system, or if significant policy interventions are taken in response to bank sector losses. Significant signs of distress are defined as the share of nonperforming loans rising above 20 percent, or fiscal restructuring costs exceeding 5 percent of GDP. Significant policy

interventions are defined as at least three of the following measures being taken: extensive liquidity support, bank restructuring gross costs of at least 3 percent of GDP, significant bank nationalizations, significant (new) guarantees put in place, significant asset purchases (at least 5 percent of GDP), and deposit freezes and/or bank holidays. For more detailed definitions of the policy measures, see the original paper (ibid.). The ending date of crises is defined as the year before both GDP and real credit exhibits positive growth for at least two consecutive years, so crises are in general shorter using this variable than *CRISIS1*. The variable runs from 1970-2011, and identifies 147 SBCs.

4.1.3. The Control Variables

Followingly we will describe the control variables. They are all widely used in the literature (see part 4.2), and all have observations spanning from 1970-2011 for a varying sample of countries. For a detailed description of coverage and distributions see table 7 and 8 of the Appendix. The expected signs and their justifications described below correspond to part 2 of this thesis, for more details and references on this see that part.

M2RES is the ratio between money supply (*M2*) and foreign reserve holdings. The data corresponds to the variable *FM.LBL.MQMY.IR.ZS* from the World Bank (2013) (hereafter *WB*)(2013), except for the Euro-countries, where the variable is missing for years preceding the Euro. We have recomputed the variable for these country-years using $M2 * EXC / RES$. Where *M2* is “Money plus Quasi Money” (current local currency) from IMF's International Financial Statistics Database (hereafter *IFS*) (IMF, 2013), *EXC* is the exchange rate (see below) and *RES* is “Total Reserve Assets” (current US dollars) from the *IFS*. This computation is equal to the one used for the World Bank series. The missing variables in the *WB* data bank is probably due to comparability issues of the domestic money supply before and after introducing the Euro (this is discussed part 4.1.5). The variable is included to account for vulnerability to sudden capital outflows.

PCRATIO is the ratio between total credit to the private sector and GDP, it corresponds to the variable *FS.AST.PRVT.GD.ZS* from the *WB*, where it is computed using *IFS* data on private credit, and *WB* data on GDP. For some countries this variable also captures credit to public enterprises. It's use is in the literature usually justified by being a proxy for financial liberalization and loose

regulation. In general it is thought that the higher the ratio, the higher chance for risky borrowers, and thereby higher fragility.

PCGROWTH is the real growth rate of private credit, calculated as $(PCREDIT-L1.PCREDIT) / L1.PCREDIT$. Where *PCREDIT* is calculated taking *PCRATIO* multiplied by real GDP in constant 2005 US dollars (variable NY.GDP.MKTP.KD from WB). Growth in private credit is expected to positively influence the probability of an SBC through bubble formation or increased risk with scale of new loans.

GDP is GDP per capita in constant 2005 US dollars. It corresponds to the variable NY.GDP.PCAP.KD in WB. In the literature the variable is usually thought to negatively influence the probability of suffering an SBC. The effect is either assumed to come through proxying institutional quality, or general stability.

GROWTH is the annual growth rate of real GDP. It corresponds to the variable NY.GDP.MKTP.KD.ZG in WB. It is generally assumed that growth has a negative impact on the probability of suffering a SBC, this is because growth in GDP is associated with increased earnings for borrowers, and therefore lower default rates.

DEPINS is a dummy taking the variable 1 if the country has an explicit deposit insurance scheme in place. The variable is taken from Demirgüç-Kunt, Karacaovali & Laeven (2005). It only runs until 2003, for the few regressions where it is included for a longer time span, insurance schemes are assumed to be unchanged since 2003. Deposit insurance can theoretically have both a positive and negative effect on the probability of suffering an SBC.

INFLATION is the annual rate of change in the GDP deflator. The GDP deflator corresponds to the variable NY.GDP.DEFL.ZS in WB. Inflation is expected to have a positive influence effect on the probability of suffering an SBC due to often being associated with high nominal interest rates and macroeconomic mismanagement.

H is a dummy taking the value one if there is hyperinflation. Following Cagan (1956) we define hyperinflation as inflation exceeding 50%. We use this to deal with outliers driving the results for real interest rates and inflation.

NH is a dummy taking the value one if there is no hyperinflation. (see above)

REALINT is real interest rate. Following DDK (2005) it is calculated as the treasury bill rate minus *INFLATION*, if not available the discount/bank rate is

used, if neither that is available the deposit rate is used. All interest rates are from the IFS. Treasury bill rate is line 60c, the discount/bank rate is line 60 and the deposit rate is line 60l. This is preferred to the World Bank indicator due to the between country incomparability of the WB series, and to clearer see the effect of monetary policy.

DEPRECIATION is the depreciation of the currency. It is calculated as the growth ratio of *EXCH*. It is included to account for trade shocks. Note that since it is calculated using the exchange rate to US dollars, it the variable is zero for all countries using the dollar as their currency. It is none the less used given the unavailability of a trade-weighted effective exchange rate for a big enough sample to be useful. High depreciation is expected to be positively correlated with SBCs do to capital flight and adverse shocks on profitability.

EXCH is the local currency to US dollar exchange rate. It corresponds to the variable PA.NUS.FCRF of the WB, supplemented with the Euro/dollar exchange rate for euro-countries after entering the Euro (missing in WB).

POLITY is an index on the strength of democratic institutions designed by the Polity IV Project (Marshall, Jaggers & Gurr, 2010). The index is discrete and ranges from -10 to +10 and is increasing in the strength/quality of democratic institutions. The index is assumed unchanged from 2010 to 2012. Quality of institutions is expected to decrease the probability of SBCs through strengthening regulation necessary for keeping the financial system stable.

KAOPEN is an index on the financial openness of the economy. It is constructed by Chinn & Ito (2008) based on the IMF's Annual Report on Exchange Arrangements and Exchange Restrictions. It is continuous and increasing in the openness of the capital account transactions. However, due to being based on several binary variables, it is not very dynamic. The direction of the effect of financial openness is not clear from the literature, it can be negative due to higher exposure to capital flight and international volatility, and it can be positive due to increased competition and efficiency in the financial sector.

LIBERALIZATION is constructed to signal a significant liberalization in capital markets taken place in the last five years. It is constructed as the difference in the *KAOPEN* variable from 5 years prior to the observation. Liberalization is generally thought to have a negative effect on financial stability, following the "short run pain, long run gain" (see Kaminsky & Schmukler, 2003).

FINREFORM is an index constructed by Abiad, Detragiache & Tressel (2009) based on IMF country reports. The index runs from 0 to 1, where a higher value basically entails less government involvement in seven different aspects of financial markets (except for in their “prudential regulation” dimension). Note that the index is heavily correlated with *KAOPEN*, but to a lesser degree with *POLITY*.

Regional Dummies are dummies corresponding to the WB region classification. One dummy for each of the seven regions, taking the value 0 or 1, depending on if the country is in the region or not (see Appendix for list of regions).

Note that there is one variable not included in our regressions, which is commonly included in the literature, and that is changes in terms of trade. This is done for two reasons. Firstly I could not find the variable for a sufficiently large sample. It seems to have been dropped from both the WB and the IFS databases for a large number of countries since the works of Demirgüç-Kunt and Detragiache (see below). Secondly, it is hardly ever significant in the literature, and when included for the 35 countries where we have observations, it does not significantly change the results.

4.1.4. The Gini-Coefficient

We use a gini-coefficient to see the effect income inequality has on the probability of suffering an SBC. In general gini-coefficients are not comparable between countries, and often not even over time within the same countries. The ways of measuring income varies greatly, as does the representativity of the samples. For a long time the data set created by Deininger & Squire (1996) was preferred for cross country studies, but as Atkinson & Brandolini (2001) thoroughly documented, these series were riddled with problems that made them virtually useless. Since then the University of Texas Inequality Project (UTIP) (Galbraith & Kum, 2003) has been rising in popularity. This study uses the Deininger & Squire dataset, but uses data on the between-group inequality of manufacturing wages calculated by the United Nations Industrial Development Organization's (UNIDO) Industrial Statistics database to make them more comparable. More precisely they regress the Deininger & Squire gini on the data from UNIDO and a number of controls, and reports

predicted values of the gini (ibid). This approach has its advantages, but has several caveats, the most serious of which is basing everything on data from one sector.

Another widely used database is that of the Luxembourg Income Study (LIS) (Smeeding & Gottschalk, 1995), and that of the UNU-WIDER World Income Inequality Database (WIID), however both these databases have rather sporadic observations, meaning that a panel is virtually impossible to construct for a sufficient amount of years and countries (for more on the problems of this data, see Galbraith, 2009).

However Frederick Solt has taken it upon him self to create a Standardized World Income Inequality Database (SWIID) (Solt, 2009). Using the WIID and the LIS, he separates the different gini's based on 21 classifications of how and from what sources they were computed, he then continues to estimate the value of the ratio between the different categories for each country-year (assumed to vary with both country and year) based on all available information. Using this estimated value, he estimates the panel for all 21 classifications. Since he believes rapid movements in the gini to be unlikely, he contributes this to measurement errors, therefore he applies a 5-year moving average of all classifications - including interpolation for missing variables (except for the LIS which he considers to be superior and have less measurement errors). The moving average is re-generated one thousand times using Monte-carlo simulations, and this average is used as the gini for each of the 21 classifications for each country-year, standard errors are also computed using the same technique. He then uses the estimates with the lowest standard errors to estimate the average of all classifications, which gives the gini for the country-year in the panel. (ibid)

The description above is for version 1.0 of the SWIID, in the latest version (3.1), he also incorporates the UTIP-data plus an array of other sources for ginis, adding up to a total of 3600 extra observations. Though no new published documentation is given for this database, his entire computation is available online.⁷ In other words, this version of the SWIID incorporates all the other sources described above. Thus the SWIID is arguably *the* best international comparable database on income inequality. Not only does it give a larger panel than all other sources, it also incorporates all major sources in its computation. Furthermore it also gives the standard error for each point estimate, which renders the data much more

7 See <http://myweb.uiowa.edu/fsolt/swiid/swiid/updates.html>

transparent for researchers to evaluate than most other sources. For these reasons, I apply the SWIID-data as the *GINI* variable in the panel.

GINI is the gini coefficient on net income inequality. Net income is defined as income after redistribution. It corresponds to the variable “gini_net” in Solt (2009, SWIID Version 3.1, December 2011) It is a standard gini-coefficient, where a value of zero means every percentile has an equal income, while a value of one means that the top percentile has all the income and all other percentiles no income. According to the discussion in part 2 and 3 we expect *GINI* to have a positive impact on the probability of suffering an SBC.

4.2. Previous Research

The empirical research on causes of systemic banking crises is quite extensive, and any attempt to create an exhaustive review will necessarily be futile. Below we will however give a summary of the most influential works the last 20-so years, focusing on similarities and differences in the obtained results. We believe the works covered represent the scope of the (academically acknowledged) literature on the topic quite well, if any important works happen to be left out, rest assured: it is due to ignorance, not selectiveness.

4.2.1. The Multivariate Logit Approach

Demirgüç-Kunt and Detragiache (hereafter DKD) have produced a vast body of literature on the empirical causes of systemic banking crises, and have since their first paper (DKD, 1998) been two of the most heavily cited authors on the topic. Their preferred method is, like applied below, a multivariate logit model. They construct a binary dependent variable indicating if a systemic banking crisis is happening in a country at a certain point or not, and regress this on an array of variables using a logistic probability distribution (see below). They exclude observations for all crisis years except the first to avoid endogeneity, and cluster errors by country. Robustness checks are stated to include yearly dummies, country dummies, and variables describing previous crisis experience. For more on their method see the next section. Since their first paper their sample and scope of variables has increased a lot, and their result have proven to be quite robust. Money

supply over foreign reserves, private credit over GDP, lagged growth of private credit, inflation, real interest rates and the existence of an explicit deposit insurance scheme are found to positively influence the probability of crisis, while real GDP growth, GDP per capita, growth in terms of trade⁸, and institutional quality are found to have a negative influence (DDK, 1998, 1999, 2000, 2002; 2005). In their 1999 paper they find that financial liberalization (proxied with the abolishment of interest rate controls) has a positive effect on the probability of suffering a systemic banking crisis, but that the effect is reduced (and even becomes negative) with higher levels of institutional quality (DDK, 1999). In their 2000 article they show how the multivariate logit approach can be used to create an Early Warning System (EWS) for banking crises by using forecasted values of the explanatory variables to predict future risks of banking crises (DDK, 2000). In their 2002 article they show how the design of deposit insurance affects its impact on the probability of suffering a systemic banking crisis, the higher the coverage the more positive effect, while the higher institutional quality, the lower positive effect (DDK, 2002). Their 2005 review updates their original analysis for a larger sample, their results from this paper is included in *Table 1*, as a comparison.

Their 2011 article differs from the rest, in that it is not a panel and treats individual banks. Through a cross sectional OLS they regress the z-scores⁹ of banks on each bank's and country's average compliance of basel core principals. They find little or no significance, while macro economic factors such as interest rates and rule of law are significant (Demirguc-Kunt & Detragiache, 2011).

Eichengreen & Rose (1998) use the same basic framework to show how developing countries are subject to changes in fundamentals in the developed world. They use a probit instead of a logit, and shows a positive effect on the probability of suffering an SBC in a developing country from rising interest rates in developed countries, thus indicating vulnerability to capital outflows as a source of instability.

Demirgüç-Kunt & Levine (2000) use a logit to see how bank-concentration affects fragility. The effect is initially positive, but when including

8 Only significant in a few of their studies, see Table x

9 The z-score is defined as $(\text{average return on assets} + \text{equity/assets}) / (\text{standard deviation of the return on assets})$. They interpret it as "the number of standard deviations by which returns would have to fall (starting from the mean) to wipe out all equity in the bank" (Demirguc-Kunt and Detragiache, 2011, p. 182)

relevant controls becomes insignificant. The controls have the same sign and significance as most other studies.

4.2.3. The Signaling Approach

Kaminsky (1999) and Kaminsky & Reinhart (1999) utilize a signaling approach when trying to find a predictor of financial crises for developing economies with fixed exchange rates. Through comparing the movement of several macro economic variables in two years prior to a crisis, with the movements in tranquil periods, they create thresholds of both individual variables and composite indexes, that when crossed, signal that a crisis is imminent. This approach has the clear advantage of lower possibilities of endogeneity, as currency crashes are easily pin-pointed at a date, and it allows for using monthly data. However, the generality of the results are debatable, as the monthly variations of a small set of variables is likely to have very different implications for different countries.

4.2.4 Other Approaches

Klomp (2010) uses a random coefficients logit model, also known as mixed logit, to prove that there exists much heterogeneity in the way variables affect the probability of suffering a SBC. The approach assumes that there is no covariation between the time variant observable variables and the coefficients, and that the coefficients are normally distributed around the mean for all countries. These assumptions are clearly not likely to be met in this case, but through doing this exercise, he shows that there is likely to be much heterogeneity in the coefficients between countries (even the coefficients most significant on average are insignificant in at least 60% of the cases), he thus argues forcefully towards allowing for more heterogeneity through constructing groups of similar countries. This argument is strengthened by Van den Berg, Candelon & Urbain (2008), who uses a Kapetanios-approach to find optimal clusters in a panel of thirteen seemingly arbitrary chosen countries. They show that clustering the countries through a conditional logit improves the within-sample performance greatly.

Caballero (2012) uses a conditional logit model, and a random effects-Mundlak model, meaning that he, as the only author I have seen, allows for country

specific effects. His study shows that a constructed dummy variable indicating if net capital inflows are higher than one standard deviation of the moving average (dubbed *bonanzas*) have a positive effect on the probability of suffering a systemic banking crisis. This seems to be the effect regardless of *booms* which he defines as domestic credit increasing with more than one standard deviation of the moving average (ibid). This indicates that a rapid inflow of foreign capital can cause banking crises, even if the domestic credit does not experience a boom. Interestingly, his macroeconomic control variables are generally insignificant, and except for his boom and bonanza dummies, only a dummy for a contemporaneous currency crisis, and some indexes of institutional quality, including a self-computed *moral hazard* index computed using among others deposit insurance, are significant. This is not implausible, since including country specific effects is likely to leave many previously significant variables insignificant, and since he lags all explanatory variables, which is generally not done in the other literature. The work is still a working paper, he does not give any information on the percentage of crises and non-crises successfully predicted, and there might be some methodological issues as the conditional logit excludes all countries which have not experienced a crisis in the period (21 of 60), while the RE-Mundlak method for non-linear unbalanced panels is rather new (see Wooldridge 2009). More importantly; he does not deal with heavy outliers in his sample¹⁰ Nevertheless his results are interesting. The results imply an asset bubble burst, and that the bubble is driven by foreign credit supply rather than domestic credit demand, as bonanzas are significant even in the absence of booms.

4.2.5 Comparing Approaches

Davis & Karim (2008) has done a comparative study of the (out-of-sample) performance of Early Warning Systems (EWS), thus comparing the validity of different approaches in predicting future crises. Clearly this also is an indicator of how well the approaches work at finding causes of crises. They compare the

¹⁰ He as one of few authors give descriptive statistics of the variables, showing inflation rates of several thousand percent, and equally outrageous levels of depreciation and interest rates. The problem is likely to be present in many of the works mentioned, as they do not specify how they deal with these values. I however suspect that DDK simply delete the observations, but keep the countries in the sample, as using this method on the same sample I get very similar results to them (not reported).

signaling approach to the logit approach through re-estimating models of DDK and Kaminsky & Reinhart. An interesting result from their study is that applying different sources for their crisis variable yields very different results, they therefore use several crises variables to ensure robustness. They unsurprisingly find that the signaling approach works better when using a composite index, than when considering individual variables. They also find that the composite signaling approach outperforms the logit approach for individual countries. However, the logit approach is found to perform much better than the signaling approach when applying the same EWS to many countries. Thus, they conclude, the logit approach is better than the signaling when working on a large number of countries. DDK (2000), all though arguably biased to the issue, also find that the logit approach outperforms the signaling approach on a panel when evaluating individual and country level bank fragility.

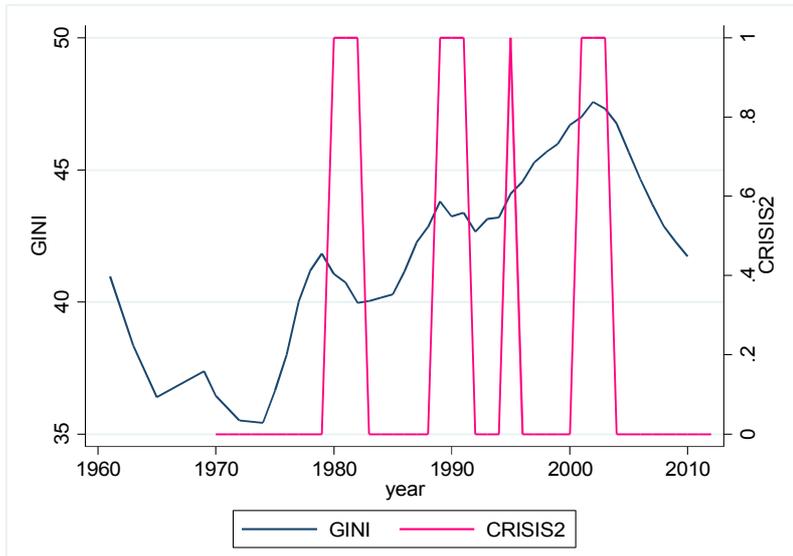
4.2.6 Inequality

When it comes to inequality, very little research is to my knowledge done. The two most serious works I have seen is that of Hockett & Dillon (2013) and Bordo & Meissner (2012). The former try linking income inequality to general market volatility through ARIMA modeling, but no conclusive evidence is found. Bordo & Meissner on the other hand explore the link more underhand. They first do a regression linking credit levels to crises, before they regress changes in credit levels on changes in top one percentile income shares (from Piketty & Saez, 2003). They use changes over five years, because this is where the literature linking inequality to crises “seem to be focused”, and because the series are I(1). They find no significant results. However, their analysis has several problems, firstly their dependent variable is changes *in private credit*, not private credit as a share of anything, thus basically being prone to respond to anything that increases over time. Combining this with the fact that changes in GDP and previous changes in credit are controls, one can see how that crowds out mostly anything. Their third significant control is short term interest rates, which are clearly a mechanism which inequality is expected to work *through* in the theoretical framework. This just proves to show how challenging it is to separate underlying causes from channels of influence in statistics, especially when the series are unusable in levels due to unit roots.

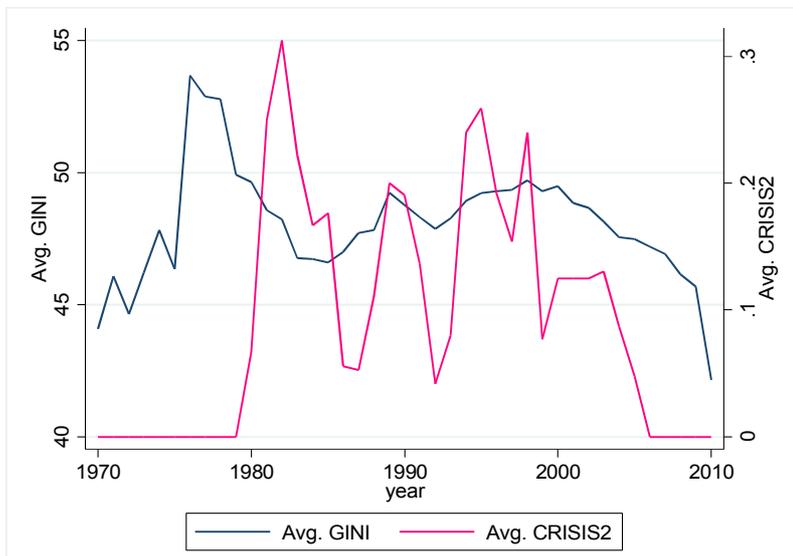
Arguably the best approach would be to do a cointegration analysis, but this is clearly more challenging.

Apart from the two works mentioned above little serious research seems to have been conducted on the issue. Several reports graph various sources of top income shares and periods of crises together to show “the obvious correlation” (see among others Atkinson & Morelli, 2011; Thaker & Williamson, 2012). Often showing graphs like *Graph 1* below, while not showing graphs like *Graph 2*, also below. We will therefore in the following do our best to avoid this kind of anecdotal evidence, and these two graphs will be the last we give.

Graph 1, Crises and Inequality in Argentina:



Graph 2, Average Number of Crises and Average Inequality in Latin America:



4.3. Method

Our method of choice is the multivariate logit approach. We chose this because, as argued above, it is better than the signaling approach when using a panel. We chose it over a conditional logit approach, as the latter necessitates dropping all countries without episodes of crises in the period, which induces loss of vital controls. We chose it over the RE-Mundlak approach because controlling for country specific effects is likely to take away much of the effect of inequality. This is especially true for countries with short time-spans, as inequality varies more between than within countries. We chose it over the probit method, as the thinner tails of the logit allow for more predictions in between 1 and 0. This might seem counter-intuitive, as we are interested in predicting either 0 (no crisis) or 1 (crisis), however, in practice there are very few episodes of crises for a given sample, meaning that the probit will have higher probabilities of predicting a large number of near-zero probabilities than the logit. We assume this is also why it is preferred in the literature.

The method assumes the probability to be given by the following distribution:

$$P(C_{i,t}=1) = F(\beta, X_{i,t}) = \frac{e^{\beta' X_{i,t}}}{1 + e^{\beta' X_{i,t}}}$$

Where $C_{i,t}$ takes the value one if a crisis starts in country i in year t , and zero otherwise (years in crises are dropped). $X_{i,t}$ is a vector of control variables for county i observed in year t . β is a vector of coefficients. The coefficients are estimated using the log-likelihood function, being the values that maximize it. They are computed using maximum likelihood estimation. The log-likelihood function is the following:

$$\log(L) = \sum_{i=1}^n \sum_{t=1}^T [C_{i,t} \log F(\beta, X_{i,t}) + (1 - C_{i,t}) \log F(\beta, X_{i,t})]$$

Where n is the number of countries and T the number of years.

The coefficients thus tell nothing about the *size* of the increase in probability of a crisis for a given change in the variables. This depends entirely on the value of the other parameters. However, the *sign* and *significance* of the parameters have the regular interpretation. Where a negative significant coefficient tells us that an increase in the variable on average gives a significant decrease in the probability of suffering an SBC for the country-year in question. As is common practice in the literature we cluster errors by country, this is particularly important in our case as we use several samples with very unbalanced panels.

A problem with the specification above is that the coefficients are estimated based on the size of the variables in the first year of the crisis, meaning that endogeneity could be a major issue. A crisis starting in year t is likely to affect variable x in year t . Dropping all years of crisis after the first thus does not remove endogeneity completely. The literature in general does not address this possible problem of endogeneity, with the honorable exception of Caballero (2012) who lags all explanatory variables. Since we are interested in the underlying mechanism creating fragility, the contemporaneous levels of macroeconomic variables are arguably noise in our regression, and this approach becomes even more appropriate in our case. In fact, as inequality is expected to have a slow effect, and that effect is expected to happen *through the other controls*, even observations for years leading up to a crisis can be considered noise. Therefore we, after the baseline regressions, usually operate with the third lag of all explanatory variables (or the third lead of *CRISIS* as the dependent variable). When doing this, observations between the time of the controls and the end of the crisis are always dropped. We will discuss this more when presenting our results.

To deal with the issue of poolability we will apply regional dummies to account for country specific time-invariant effects. We in other words do not let the effect from inequality differ between countries. There are several reasons for this, one is as mentioned that inequality varies foremostly between countries not within, therefore country-specific beta's become hard to estimate for short time-spans. Another problem is that country-specific beta's necessitate variation in the dependent variable within countries, which would drop a large percentage of most our samples. An interesting analysis would of course be to find optimal clusters, and see how the effect of inequality differs among clusters, but we have chosen to spend our time differently. One *could* also apply a Mundlak-type variation of the coefficient between countries, but seeing how we are unfamiliar with this type of modeling we have chosen to “keep it simple”. However, we do utilize several different samples (based mostly on data availability), which serves as a robustness check.

Following the recommendations of Davis & Karim (2008) we also control for alternative crises variables for the samples where we have both available. As argued by Harding & Pagan (2011) this should always be done, as constructed binaries always will be sensitive to cut-off levels. We also systematically control for

time-dummies and trends, and the robustness to including them in the specification is reported (if not in tables, in text).

4.4. Results

Below we describe the results from applying the logit approach described above for different specifications on different samples. The general result is that income inequality as measured by the gini-coefficient has a positive effect on the probability of suffering an SBC in the future. The effect however seems to be non-increasing (or even decreasing) with high levels of inequality. The effect of growth in the gini seems to be more important than the general level. The gini loses significance when controlling for macro-economic variables in the first year of crisis, or the first year prior to a crisis. Only results using the second and third lag of explanatory variables are robust. We take this as evidence of inequality working through other macroeconomic variables rather than indicating low institutional quality or other country-specific effect influencing the probability of crisis. Results are presented in tables 1-6. Following DDK (1998, 1999; 2005), cut-off levels for successfully predicting a crisis is set as a probability of 0.05%. This is done due the fact that crises are so rare that the probability distribution necessarily will never yield very high probabilities, and higher cut-off levels will give little or no information on the fit of the model. DDK also argue that due to the costliness of crises, a probability of 0.05 justifies taking measures to avoid them. I see this argument as secondary, as measures are potentially costly and a large number of false alarms will be given by this cut-off.

4.4.1. Baseline regressions

In *Table 1* we see the baseline regressions. The aim is to replicate the standard modeling of DDK using sample one and the controls most commonly used in the literature. Using specification 1 we see that we get similar results to most previous works. However, we are not very pleased with the distribution of *REALINT*, *INFLATION* and *DEPRECIATION*. They all have heavy outliers, and there are observations of over 1000% inflation in country-years not specified as crises. Not only is this most likely a driver of the results, but it also gives a correlation of

0.995 between depreciation and interest rates. One could choose the approach of DDK (1998) and leave them out, or that of Caballero (2012) and most other works, and simply leave them in. Neither are however very satisfactory. Leaving them in has its obvious downsides, and taking them out could cause sample-bias. The approach chosen to deal with this is to include the dummy for hyperinflation (*HYPER*), and interact the three variables driven by hyperinflation with both this dummy, and the dummy for no hyperinflation (*NH*). The reasoning behind this is that when hyperinflation sets in, the level of the inflation does not matter so much (i.e. there's little difference between 50% and 100% inflation). This is supported by the results, as the interactions with the hyper-dummy are insignificant (not reported). This also deals with the problem of multicollinearity as the correlation between the *NH* interactions drops to unworrying levels (see *Table 8*), the correlation is still high between the *HYPER* interactions, so the interaction with depreciation is dropped (as it is basically identical to inflation). As we can see from specification 2 this changes the results quite a bit, and improves upon the general fit of the model.

To see the effect of inequality we include the lagged *GINI* (specification 3). The coefficient is negative, indicating a negative effect on the probability of suffering an SBC from increasing inequality. The coefficient is hugely insignificant, and the sign is opposite of what we would expect. The fit of the model does not increase from leaving the gini out for the same sample (not reported). There are several reasons as to why one should not interpret this result as *GINI* not having an effect. Firstly, the theoretical part of this thesis argues that *GINI* will have an effect through credit levels. Since credit to GDP ratios are included as a control in our regression, there is clearly a risk of this taking away much of the effect. To exemplify: if you want to study the effect of depression on weight gain, thinking that depressed people eat more, you are not likely to capture that effect if you control for calorie-intake. Subsequently the *PCRATIO* variable poses a problem for us. Clearly it is not *as* big a problem as the example above, because credit to GDP ratios do not necessarily give information on unhealthy credit levels. Depending on the country, unstable credit levels could indicate different credit to GDP levels, and thus our *GINI* should have some effect, but nonetheless the *PCRATIO* and *CGROWTH* variables are clearly correlated with unstable credit levels. A second problem with our *GINI*, is that it is expected to have a long run effect. Variations from year to year are secondary to the persistent level of inequality in a country over

time. This brings us to our third problem; that the variation is much higher between countries than within countries, and that having a representative panel thus becomes paramount. 44 countries might simply be too little.

Let's try to deal with the first and second problem first. The problem isn't only that *GINI* works through *PCRATIO*, it also works through interest rates and inflation, and even growth is driven by inequality in our theoretical model. This could lead to the conclusion that our econometric model is all together unsuitable for analyzing the problem at hand. The approach of Bordo & Meissner (2012) is arguably better, namely seeing how inequality affects the variables associated with fragility. However, as argued above, the problems of spurious regressions complicates such an analysis when levels are more important than growth. Anyway, simply proving that inequality increases credit levels does not establish a causal relationship between inequality and crises, as the unsustainability of credit levels depend on several other factors which are also potentially influenced by inequality. Therefore we will in the following seek to separate out inequality as a factor within the logit framework.

An *indicator* on if *GINI* has an effect through credit is simply removing the credit variables from the model. Though crude, it can give some preliminary evidence. When doing this (specification 4 – *Table 1*) the significance of the *GINI* increases some, and the coefficient gets the expected sign. However, it is still largely insignificant.

The second issue of long term versus short term effects, can be a reason for the low significance. We must remember that the other variables are observations for the year in question, meaning that the coefficients on the controls are estimated based on their level in the first year of crisis compared to in years without crisis. As mentioned earlier, this gives a possible problem of endogeneity. We apply the strategy of lagging explanatory variables in specification 5. This is done through changing the explanatory variable to the lead of *CRISIS1* and dropping the first year of crisis from the sample. The *GINI* again gains significance, and is positive. When we drop the credit-variables from the specification, the *GINI* becomes even more significant and positive, though remaining insignificant. Clearly this is in no way conclusive evidence of *GINI* causing *SBCs*. Firstly because of the lack of significance, and secondly due to possible omitted variable bias. With regard to omitted variables the increased significance of *GINI* can come from an array of sources. One obvious culprit is institutional quality (widely thought to be correlated

with inequality) but controlling for this using *POLITY* and *FINREF* does not change the result, neither does controlling for regional dummies (not reported). Again, far from conclusive evidence, but an indicator that there might be more to it than what was evident from the baseline regression.

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Table 1, Sample 1 & 2: Baseline Regressions.

Dependent variable *CRISIS1* for (1)-(4), all crisis years except the first are dropped
Dependent variable first lead of *CRISIS1* for (5) and (6), crisis years are dropped.

	DDK(2005)	(1)	(2)	(3)	(4)	(5)	(6)
<i>GROWTH</i>	negative***	-0.121*** (0.030)	-0.119*** (0.030)	-0.136*** (0.041)	-0.122*** (0.045)	-0.008 (0.036)	0.020 (0.038)
<i>GDPPC</i>	negative***	-0.097*** (0.032)	-0.103*** (0.033)	-0.104** (0.044)	-0.023 (0.022)	-0.084** (0.042)	-0.018 (0.021)
<i>M2RES</i>	positive*	0.0125** (0.005)	0.014** (0.006)	0.014** (0.006)	0.016*** (0.006)	0.019*** (0.005)	0.021*** (0.006)
<i>PCRATIO</i>	positive***	0.0109 (0.007)	0.016** (0.006)	0.023** (0.009)		0.018** (0.008)	
<i>L1.CGROWTH</i>	positive*	0.009*** (0.003)	0.009*** (0.003)	0.010** (0.004)		0.010*** (0.003)	
<i>HYPER</i>			2.101*** (0.485)	2.837*** (0.515)	1.965*** (0.561)	2.367*** (0.809)	1.720** (0.687)
<i>DEPINS</i>	positive**	0.770** (0.335)	0.735** (0.305)	0.941** (0.367)	0.992** (0.388)	0.635* (0.334)	0.619* (0.350)
<i>INFLATION</i>	positive**	-0.000 (0.004)					
<i>NH*INFLATION</i>			0.0424** (0.0173)	0.044* (0.023)	0.023 (0.024)	0.054** (0.027)	0.034 (0.037)
<i>DEPRECIATION</i>	positive	0.005 (0.006)					
<i>NH*DEPRECIATION</i>			-0.001 (0.005)	0.011 (0.008)	0.013 (0.008)	-0.015 (0.018)	-0.014 (0.019)
<i>REALINT</i>	positive***	0.012* (0.006)					
<i>NH*REALINT</i>			0.037** (0.016)	0.029** (0.014)	0.028** (0.011)	0.025*** (0.008)	0.025*** (0.008)
<i>L1.GINI</i>				-0.001 (0.026)	0.009 (0.023)	0.015 (0.026)	0.026 (0.023)
Observations	1612	1,657	1,657	899	899	861	861
No of countries	unknown	82	82	45	45	45	45
Sample number		one	one	two	two	two	two
Pseudo R ²	0.08	0.110	0.142	0.193	0.155	0.102	0.0760
Chi-squared	248.7 ¹¹ **	62.21***	81.80***	77.66***	57.86***	69.12***	69.65***
		*					
% total correct	68.0	82.5	83.7	80.5	82.4	79.3	81.2
% crisis correct	61.0	51.7	60.0	64.9	64.9	63.9	55.6
% no-crisis correct	69.0	83.7	84.5	81.2	83.2	80.0	82.3

Not reported: constant (always negative and significant) and interaction-terms with *HYPER* (never significant).

Credit growth and gini not lagged when lead of crisis is dependent.

Robust standard errors in parentheses (clustered by country) *** p<0.01, ** p<0.05, * p<0.1

¹¹ Note that the Chi-squared reported in DDK (2005) is computed using Likelihood Ratios, while our Chi-squared are computed using a Wald. Wald is simpler, but less accurate for small samples. Since our sample is relatively big, we prefer it. However it renders the values incomparable to those of DDK.

4.4.1. Grouping Countries by Average Gini-Levels

Clearly one can not be content with the kind of indicative “evidence” given above, and another approach is needed. Since we are first and foremost interested in the persistent level of inequality in the country rather than the short-run variations, we can use this to gain observations and deal with possible sample bias. Starting with sample one we drop all countries with 6 or less observations on *GINI* (less than 25% of the time-span). For the remaining 69 countries (sample three), we take the average of the *GINI*. Clearly this average has some problems, as several countries are missing up to 75% of the observations, and even though within-country variation generally is quite low, we risk bias in using the averaged GINIs. To deal with this we group countries corresponding to their average GINI values, as the probability of misclassification with this approach is significantly lower than having a poor estimate of the average GINI. To check if average inequality has an effect we carry on to include dummies for the groups of average GINI-levels in our regression. We check two group-classifications, one classifying terciles, and one quintiles. The dummies are *QUINTILE_i* and *TERCILE_i*, where *i* runs from 1-5 and 1-3 respectively, and a higher *i* means higher mean inequality. The results (*Table 2 & 3*) are uplifting, with positive and significant results of higher inequality even when controlling for credit, and other contemporaneous controls. When controlling for institutional quality (*POLITY*) our tercile dummies lose significance, but our quintile dummies remain highly significant (specification 8). When we control for capital openness (*KAOPEN*) and liberalization in the last 5 years (*LIB*), they are both significant. Same goes for when controlling for financial system regulation (*FINREFORM*). However, there might still be omitted variables driving the results, countries with high inequality might have other characteristics that make them similar which are not captured by our controls. The obvious example of this is the “Latin America effect” reported by Deininger & Squire (1998), where the Kuznet relation between inequality and growth vanishes when including a regional dummy for Latin America. To deal with this possible problem, we also control for regional dummies, but the effect surprisingly becomes more robust (for both groupings).

The results thus give rather robust evidence that countries with higher average gini levels are more prone to crises than the countries with lower inequality. The results are robust to including both time trends and yearly dummies, for all

specifications. They are also robust to lagging all explanatory variables (gaining significance), to including yearly dummies, and to using the alternative crises specification *CRISIS2*. As the results for the quintiles and terciles does not change much with these specifications, the results are not reported here. The results are however clearly not robust to including country dummies (or doing a conditional logit), since the variation over time is zero.

An interesting result from the quintile specification is that the highest group consistently has a lower coefficient than the second highest. This is likely the reason for the tercile specification losing significance in specification 8. The difference is not significant, but highly consistent (also for the unreported specifications). This could indicate that the very unequal countries have a lower probability of crisis than the countries with slightly lower inequality. This could be due to the sensitivity of our group specification, and the fact that the very unequal countries in general have fewer observations of *GINI*, possibly overestimating their mean, but it could also be due to the very unequal countries having more problems with poverty, as poverty is likely to have little or no effect on credit levels (a clear weakness with the GINI-coefficient is that there is no way of telling which of the two are driving inequality). Furthermore, as argued by Ravallion (2001) household surveys, which the GINIs are based upon, have a clear tendency to underestimate the income of the very rich, so to look for indicators of wealth concentration we might have to look elsewhere. Unfortunately we are using one of the few (and arguably the best) internationally comparable datasets on income inequality that exists, and indicators of top income shares, such as those of Piketty & Saez (2006) are still riddled with incomparability-issues.

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Table 2, Sample 3: Terciles of Inequality.

Dependent variable is contemporaneous *CRISIS1*, all crisis years except the first are dropped

	(7)	(8)	(9)	(10)	(11)
<i>NH*INFLATION</i>	0.041*** (0.016)	0.039** (0.016)	0.042** (0.019)	0.038** (0.018)	0.045** (0.021)
<i>NH*DEPRECIATION</i>	-0.004 (0.007)	-0.003 (0.006)	-0.003 (0.007)	-0.004 (0.007)	-0.001 (0.007)
<i>NH*REALINT</i>	0.030** (0.013)	0.034** (0.013)	0.039** (0.016)	0.048*** (0.015)	0.036** (0.018)
<i>GROWTH</i>	-0.144*** (0.032)	-0.145*** (0.033)	-0.163*** (0.036)	-0.176*** (0.039)	-0.214*** (0.044)
<i>GDPPC</i>	-0.058* (0.035)	-0.034 (0.038)	-0.024 (0.035)	0.014 (0.046)	-0.030 (0.040)
<i>M2RES</i>	0.011* (0.006)	0.005 (0.007)	0.008 (0.008)	0.007 (0.008)	0.010 (0.007)
<i>PCRATIO</i>	0.0165*** (0.006)	0.0165*** (0.006)	0.012** (0.005)	0.0133** (0.005)	0.017** (0.007)
<i>L1.CGROWTH</i>	0.010*** (0.003)	0.009*** (0.003)	0.009*** (0.003)	0.001*** (0.003)	0.008** (0.003)
<i>HYPERS</i>	1.726*** (0.471)	1.681*** (0.493)	1.611*** (0.625)	1.495** (0.649)	1.371* (0.740)
<i>DEPINS</i>	0.641** (0.321)	0.708* (0.368)	0.686* (0.386)	0.594* (0.344)	0.680 (0.444)
<i>TERCILE₂</i>	0.764 (0.640)	0.668 (0.681)	0.945 (0.635)	1.127* (0.642)	1.008 (0.677)
<i>TERCILE₃</i>	1.188** (0.604)	1.074 (0.659)	1.463** (0.618)	1.671*** (0.611)	1.622*** (0.603)
<i>KAOPEN</i>				-0.373 (0.237)	
<i>POLITY</i>		-0.048* (0.026)	-0.066** (0.033)	-0.062* (0.032)	-0.047 (0.037)
<i>LIBERALIZATION</i>				0.361 (0.273)	0.124 (0.225)
<i>FINREFORM</i>					-1.387 (1.012)
<i>Regional Dummies</i>			YES	YES	YES
Observations	1,370	1,257	1,257	1,240	898
No of countries	69	64	64	63	47
Pseudo R-squared	0.152	0.151	0.165	0.177	0.213
Chi-squared	110.2***	98.85***	131.3***	175.9***	137.3***
% total correct	78.3	74.70	76.0	77.3	75.4
% crisis correct	68.4	61.4	61.4	64.3	68.7
% no-crisis correct	78.5	75.3	76.7	77.9	75.0

Not reported: constant (always negative and significant) and interaction-terms with *HYPERS* (never significant).

All effects from terciles are robust to including time trends and yearly dummies.

Robust standard errors in parentheses, clustered by country.

*** p<0.01, ** p<0.05, * p<0.1

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Table 3, Sample 3: Quintiles of inequality.

Dependent variable is contemporaneous *CRISIS1*, all crisis years except the first is dropped

	(12)	(13)	(14)	(15)	(16)
<i>NH*INFLATION</i>	0.042*** (0.017)	0.039** (0.016)	0.041** (0.018)	0.038** (0.017)	0.042** (0.019)
<i>NH*DEPRECIATION</i>	-0.004 (0.006)	-0.003 (0.006)	-0.003 (0.007)	-0.005 (0.007)	-0.001 (0.007)
<i>NH*REALINT</i>	0.029** (0.013)	0.034** (0.014)	0.038** (0.016)	0.041*** (0.015)	0.035** (0.017)
<i>GROWTH</i>	-0.148*** (0.032)	-0.149*** (0.033)	-0.166*** (0.036)	-0.181*** (0.039)	-0.221*** (0.045)
<i>GDPPC</i>	-0.048 (0.030)	-0.020 (0.031)	-0.008 (0.036)	0.030 (0.047)	-0.010 (0.041)
<i>M2RES</i>	0.009 (0.007)	0.002 (0.008)	0.006 (0.008)	0.005 (0.008)	0.009 (0.008)
<i>PCRATIO</i>	0.017*** (0.005)	0.018*** (0.005)	0.013** (0.005)	0.015*** (0.005)	0.018** (0.007)
<i>LI.CGROWTH</i>	0.011*** (0.003)	0.010*** (0.004)	0.010*** (0.004)	0.0103*** (0.004)	0.009** (0.003)
<i>HYPER</i>	1.694*** (0.464)	1.587*** (0.485)	1.507** (0.611)	1.364** (0.620)	1.142* (0.655)
<i>DEPINS</i>	0.640** (0.305)	0.738** (0.371)	0.742* (0.405)	0.657* (0.364)	0.803 (0.510)
<i>QUINTILE₂</i>	1.089* (0.617)	0.947 (0.627)	0.874 (0.714)	0.972 (0.676)	1.011 (0.742)
<i>QUINTILE₃</i>	1.446** (0.725)	1.357* (0.720)	1.635** (0.694)	1.834** (0.719)	1.973** (0.769)
<i>QUINTILE₄</i>	2.066*** (0.632)	2.035*** (0.643)	2.335*** (0.653)	2.612*** (0.698)	2.637*** (0.750)
<i>QUINTILE₅</i>	1.571** (0.686)	1.269* (0.747)	1.854** (0.826)	2.045** (0.814)	2.379*** (0.875)
<i>KAOPEN</i>				-0.388 (0.242)	
<i>POLITY</i>		-0.062** (0.032)	-0.071** (0.035)	-0.068** (0.034)	-0.048 (0.039)
<i>LIBERALIZATION</i>				0.405 (0.286)	0.157 (0.239)
<i>FINREFORM</i>					-1.603 (1.062)
<i>Regional Dummies</i>			YES	YES	YES
Observations	1,370	1,257	1,257	1,240	898
No of countries	69	64	64	63	47
Pseudo R-squared	0.161	0.164	0.174	0.188	0.223
Chi-squared	109.2***	88.03***	138.9***	200.4***	138.6***
% total correct	77.1	75.7	75.8	77.8	75.9
% crisis correct	68.4	66.7	66.7	67.8	75.0
% no-crisis correct	77.5	76.2	76.2	78.3	75.9

Not reported: constant (always negative and significant)

All effects from terciles are robust to including timetrends and yearly dummies.

Robust standard errors in parentheses, clustered by country.

*** p<0.01, ** p<0.05, * p<0.1

4.4.3. Bigger Sample

CRISIS2 runs from 1970-2011, meaning that using this crisis variable we can expand our sample greatly. When doing this, grouping countries by average inequality poses a bigger problem, as the within country variation is much larger. But this is more of an opportunity than a problem, because it increases our ability to capture the effect from the movement of inequality. To do this, we create a variable of the average gini for the past 5 years (*MEANGINI*). We also create a variable for growth in *GINI* from 5 years ago to the present (*GINIGROWTH*).

To get a balanced panel covering the whole period, we would have to drop a large amount of countries. When using this time-period we therefore allow for an unbalanced panel. This is done because the sample bias of excluding countries with few observations is deemed higher than excluding country-years without observations. We keep all countries with ten or more consecutive years of observations (sample four). Clearly there is sample bias, as missing years are likely to be correlated with institutional quality, GDP and an array of other factors, but we still view the bias as less than if we were to drop all countries who do not run the entire sample.

Using this complete sample, and lagging all explanatory variables, we get a significant and positive result from *GINIGROWTH* (Table 4). The result is robust to several institutional quality measures, regional dummies, and a time trend, but not robust to yearly dummies. The effect from average inequality levels (*MEANGINI*) is insignificant. When increasing lags of all explanatory variables to three the result from *GINIGROWTH* grows in significance and becomes robust to yearly dummies, as one of very few variables.

This points towards growth in inequality being more important than the level, keeping in mind that the lack of effect from *MEANGINI* could be due to the lower effects from very high levels shown in the last subsection. The result is not very sensitive to sample selection. When using only the countries which have 10 or more consecutive observations up to 2008, the result from *GINIGROWTH* is no longer significant at the first lag of explanatory variables, but *GINIINCREASE* is (see below). Furthermore, *GINIGROWTH* regains significance when taking the third lag of all explanatory variables. Meaning that the growth in inequality over the last 5 years is a significant indicator on if there will be a banking crisis three years in the future. This result is robust to an array of sample selections, also when considering

only balanced panels. The results are robust to including yearly dummies, regional dummies, institutional quality measures, and to deleting all countries with high average standard errors of GINI-estimates (cutoffs of 2 and 3 points were tested – not reported).

The samples used above however allows for *GINIGROWTH* and *MEANGINI* to be calculated for years with missing information. To check if this affects the results, we create a sample containing only years with true observations of the two variables, for all countries with ten or more consecutive observations running up to 2008 (sample 5). The results are also robust to this specification when taking the third lag of all explanatory variables (see *Table 5*). The results are robust to time-trends and all measures of institutional quality, but not yearly dummies. However, *GINIINCREASE* is robust to yearly dummies also in this specification (see below).

We are worried that the estimated effect might come from increasing stability and general growth likely to be found in developing countries in years following conflict, which is likely to be correlated with years of reduced GINI. To check this we separate between decreases and increases in *GINI*. The results (exemplified in specifications 23-24) show that *GINIINCREASE* (positive values of growth, takes the value zero for negative values) is much more significant than *GINIDECREASE* (absolute values of negative growth, takes the value zero for negative values of growth). The results are robust to yearly dummies, and the model performs remarkably well compared to the other models, even at three lags.

Clearly this can be capturing increasing inequality in developing countries indicating or causing higher levels of social unrest and political upheaval, especially in a large panel like sample 4. To make sure we are not capturing such effects, we redo the analysis for only OECD countries.

We do this as an ultimate robustness check. Only OECD countries with observations on *GINI* for 10 consecutive years leading up to 2008 are considered. This leaves us with 17 countries (sample 6), and 20 episodes of crisis. We take the third lag of all explanatory variables. Surprisingly the results are robust (*Table 6*), even to including yearly dummies, which renders all other variables insignificant. The result is however quite sensitive to changing the lag of explanatory variables, where *GINIGROWTH* is insignificant at all lower lags.

The results above indicate that increasing inequality could be causing the movement in the other explanatory variables towards unstable levels in the years

leading up to the crisis. This fits well with our theory, as increased inequality will give higher credit growth within the framework. It also fits well with the relative consumption argument, where keeping relative consumption levels when faced with decreasing relative wages causes household debt levels to reach unsustainable levels.

Table 4, Sample 4: Growth in Inequality.

Dependent variable is first lead of CRISIS2, crisis years are dropped

	(17)	(18)	(19)	(20)
<i>NH*INFLATION</i>	0.044*** (0.016)	0.044*** (0.016)	0.037** (0.015)	0.031 (0.019)
<i>NH*REALINT</i>	0.027*** (0.010)	0.030*** (0.011)	0.028*** (0.010)	0.023* (0.013)
<i>NH*DEPRECIATION</i>	0.003 (0.009)	0.003 (0.010)	0.005 (0.009)	0.005 (0.010)
<i>GROWTH</i>	0.011 (0.025)	0.002 (0.025)	0.006 (0.024)	0.005 (0.027)
<i>GDPPC</i>	-0.041* (0.022)	-0.031 (0.022)	-0.033 (0.023)	-0.008 (0.025)
<i>M2RES</i>	0.021*** (0.004)	0.018*** (0.004)	0.022*** (0.004)	0.021*** (0.005)
<i>PCRATIO</i>	0.009** (0.005)	0.010** (0.005)	0.010** (0.005)	0.009* (0.005)
<i>DEPINS</i>	0.649** (0.271)	0.814*** (0.295)	0.582** (0.289)	0.550* (0.329)
<i>LI.CGROWTH</i>	0.006** (0.003)	0.006** (0.003)	0.006** (0.003)	0.009** (0.003)
<i>HYPER</i>	1.411*** (0.407)	1.398*** (0.406)	1.159*** (0.394)	0.923** (0.415)
<i>MEANGINI</i>	0.000 (0.015)	0.000 (0.015)	0.002 (0.019)	0.011 (0.019)
<i>GINIGROWTH</i>	0.064** (0.032)	0.061** (0.032)	0.060* (0.034)	0.076** (0.035)
<i>POLITY</i>		-0.048** (0.019)	-0.082*** (0.023)	-0.086*** (0.024)
<i>KAOPEN</i>				-0.108 (0.114)
<i>LIBERALIZATION</i>				-0.345* (0.194)
<i>Regional Dummies</i>			YES	YES
Observations	2,119	2,119	2,119	1,947
No of countries	104	104	104	104
Sample no	4	4	4	4
Pseudo R-squared	0.0547	0.0613	0.0813	0.0931
Chi-squared	73.76***	77.36***	112.1***	113.5***
% total correct	80.30	79.90	79.60	79.60
% crisis correct	42.50	41.30	52.50	52.10
% no-crisis correct	81.80	81.40	80.70	80.70

Not reported: constant (always negative and significant).

Robust standard errors in parentheses, clustered by country.

*** p<0.01, ** p<0.05, * p<0.1

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Table 5, Sample 5:

Dependent variable is third lead of *CRISIS2*, crisis years and two years prior to the crisis are dropped

	(21)	(22)	(23)	(24)
<i>NH*INFLATION</i>	0.084*** (0.017)	0.075*** (0.017)	0.116*** (0.032)	0.075*** (0.016)
<i>NH*REALINT</i>	0.047* (0.026)	0.046* (0.025)	0.078*** (0.025)	0.046* (0.025)
<i>NH*DEPRECIATION</i>	-0.029*** (0.011)	-0.027** (0.011)	-0.008 (0.018)	-0.027** (0.011)
<i>GROWTH</i>	0.117*** (0.043)	0.119** (0.055)	0.115*** (0.044)	0.125** (0.051)
<i>GDPPC</i>	-0.084** (0.037)	-0.076** (0.038)	-0.051 (0.040)	-0.073* (0.038)
<i>M2RES</i>	0.037*** (0.014)	0.040*** (0.012)	0.041** (0.019)	0.040*** (0.012)
<i>PCRATIO</i>	0.025*** (0.008)	0.026*** (0.008)	0.027*** (0.009)	0.026*** (0.008)
<i>DEPINS</i>	-0.027 (0.357)	-0.113 (0.526)	0.063 (0.522)	-0.094 (0.551)
<i>L1.CGROWTH</i>	0.002 (0.005)	0.002 (0.004)	0.000 (0.006)	0.002 (0.004)
<i>HYPER</i>	3.089*** (0.652)	2.821*** (0.714)	4.585*** (1.020)	2.817*** (0.718)
<i>MEANGINI</i>	-0.007 (0.026)	-0.006 (0.039)	-0.010 (0.033)	-0.007 (0.039)
<i>GINIGROWTH</i>	0.163*** (0.056)	0.164*** (0.059)		
<i>GININCREASE</i>			0.178** (0.082)	0.192*** (0.066)
<i>GINIDECREASE</i>			-0.059 (0.236)	-0.083 (0.191)
<i>POLITY2</i>		-0.061* (0.035)	-0.063 (0.044)	-0.062* (0.036)
<i>Yearly dummies</i>			YES	
<i>Regional dummies</i>		YES		YES
Observations	921	921	532 ¹²	921
No of countries	49	49	49	49
Sample no	5	5	5	5
Pseudo R-squared	0.130	0.147	0.232	0.147
Chi-squared	142.9***	241.8***	307.0***	246.6***
% total correct	72.5	74.5	62.7	75.5
% crisis correct	64.4	62.2	86.7	64.4
% no-crisis correct	73.0	75.3	60.6	76.0

Not reported: constant (always negative and significant).

Robust standard errors in parentheses, clustered by country.

*** p<0.01, ** p<0.05, * p<0.1

¹² All years with no crisis dropped from this specification, as the yearly dummy will become infinitely negative. Results are similar when including dummies only for years with crises.

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Table 6, Sample 6: OECD

Dependent variable is third lead of *CRISIS2*

	(25)	(26)	(27)	(28)	(29)
<i>NH*INFLATION</i>	-0.035 (0.137)	-0.044 (0.135)	-0.040 (0.137)	-0.108 (0.177)	-0.113 (0.228)
<i>NH*REALINT</i>	-0.109 (0.135)	-0.098 (0.126)	-0.095 (0.123)	-0.201 (0.159)	-0.236 (0.194)
<i>NH*DEPRECIATION</i>	-0.014 (0.028)	-0.015 (0.028)	-0.016 (0.029)	-0.016 (0.033)	-0.024 (0.031)
<i>GROWTH</i>	0.134 (0.087)	0.101 (0.073)	0.092 (0.076)	0.171* (0.100)	0.202 (0.125)
<i>GDPPC</i>	0.008 (0.064)	0.023 (0.066)	0.024 (0.067)	0.046 (0.060)	0.051 (0.063)
<i>M2RES</i>	0.044** (0.017)	0.044** (0.018)	0.040* (0.021)	0.042* (0.023)	0.040* (0.022)
<i>PCRATIO</i>	0.010 (0.012)	0.009 (0.012)	0.010 (0.012)	0.004 (0.013)	-0.005 (0.013)
<i>L1.CGROWTH</i>	-0.013 (0.020)	-0.012 (0.019)	-0.012 (0.017)	-0.014 (0.016)	-0.009 (0.021)
<i>DEPINS</i>	-0.874 (1.120)	-0.785 (1.204)	-0.774 (1.151)	-0.993 (1.440)	-1.597 (1.456)
<i>HYPER</i>	3.078* (1.689)	3.008 (1.842)	3.054** (1.533)	2.438 (1.622)	2.438 (1.641)
<i>MEANGINI</i>	-0.018 (0.086)	-0.018 (0.084)	-0.018 (0.085)	-0.003 (0.091)	0.015 (0.080)
<i>GINIGROWTH</i>	0.331*** (0.107)	0.284*** (0.097)	0.261*** (0.093)	0.241** (0.112)	0.330*** (0.129)
<i>POLITY2</i>		-0.066 (0.056)	-0.074 (0.055)	-0.038 (0.062)	
<i>KAOPEN</i>			0.020 (0.240)	0.076 (0.273)	
<i>LIBERALIZATION</i>				0.305 (0.596)	
<i>FINREFORM</i>					2.882 (2.864)
Observations	431	431	411	370	398
No of countries	17	17	17	17	17
Sample no	6	6	6	6	6
Pseudo R-squared	0.185	0.191	0.188	0.207	0.202
Chi-squared	154.9***	141.7***	1470***	456.4***	310.3***
% total correct	77.0	78.2	75.9	76.2	78.9
% crisis correct	70.0	65.0	75.0	77.7	88.9
% no-crisis correct	77.4	78.8	76.0	76.1	78.4

Not reported: constant (never significant).

Robust standard errors in parentheses, clustered by country.

*** p<0.01, ** p<0.05, * p<0.1

Above we have found clear evidence of inequality being a significant predictor of future SBCs. Both the level of inequality (though not strictly increasing) and the past growth of inequality seem to be increasing the probability of future crises, even when controlling for all relevant controls I could think of. The fact that the effect becomes insignificant when controlling for macro economic variables leading up to the crises suggests that the effect comes from inequality somehow driving these variables, rather than inequality being a proxy for other country-specific factors affecting general fragility.

Increased inequality is predicted to increase household debt levels on the demand side of credit, and increase credit to the entire private sector on the supply side. Within our empirical framework there is basically no way of telling which one of the two (if any) are driving the results. However, as we have seen, the literature on inequality and credit-ratios is quite extensive, and even though the results are disputed, there has been produced much evidence supporting both these channels. In other words, the evidence given above combined with previous research, points towards inequality affecting banking crises through debt levels, both through increasing savings of the rich and increasing the debt of the poor.

5. Concluding Remarks

In this thesis we have seen how existing literature treats the link between inequality and financial fragility. We have been proposed a new formal argument as to why concentration of wealth at the top can cause fragility and why only redistribution can reduce it. We have been presented with new empirical evidence showing a significant increase in the probability of suffering a systemic banking crisis in the future when faced with increasing income inequality in the present. The results are robust to all controls included to deal with institutional quality and other possible omitted variable bias, and the effect of inequality has proven to be more robust than most other variables when considering a crisis three years in the future. This supports the theory of inequality affecting fragility though moving fundamental macro economic variables such as credit ratios.

However we have seen *no conclusive evidence* to indicate what channel the estimated effect from inequality is taking on it's way to systemic banking crises, and the effect seems to be secondary to other variables, most likely

working only through driving them. Thus controlling the level of these other variables directly is likely to be argued as a policy recommendation by many. I disagree for two reasons. Firstly because reducing inequality is an obvious policy goal in itself based on my personal moral sentiment, and secondly because the theoretical framework given in this thesis shows how stability can be secured only through some form of redistribution.

In a time of crisis, the Kalecki-Keynes argument shows how stimulation must come through consumption. These last years have shown us how consumer debt is an unsustainable way of boosting the economy. All evidence points towards the income and wealth share of the very rich being historically high in the countries struck hardest by the recent crisis. This, combined with the evidence given in this thesis, in my eyes paint a clear picture with regard to policy implications. When demand is low while those with much to spend do not spend and those who want to spend do not have the means to, something needs to be done. When the rich lend money to the poor in order to preserve their wealth, and the subsequent crisis comes as a surprise to most, some serious rethinking is justified. It seems self-evident to me that the problem is structural, and that relative returns to labor need to change. I do not know what more to say.

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6. Appendix

6.1. Samples Used

Sample 1:

Time-period: 1980-2002 (balanced)

Countries:

Algeria, Antigua and Barbuda, Argentina, Australia, The Bahamas, Bangladesh, Barbados, Belize, Benin, Bolivia, Botswana, Burkina Faso, Burundi, Cameroon, Canada, Central African Republic, Chad, Chile, Congo (Republic of), Costa Rica, Cote d'Ivoire, Cyprus, Denmark, Dominica, Ecuador, Egypt, Finland, France, Gabon, Gambia, Germany, Ghana, Grenada, Guatemala, Guyana, Honduras, Iceland, India, Ireland, Israel, Italy, Japan, Jordan, Kenya, Korea (Republic of) Lesotho, Madagascar, Malawi, Malaysia, Mali, Malta, Mauritius, Mexico, Nepal, Netherlands, New Zealand, Niger, Nigeria, Norway, Oman, Pakistan, Papua New Guinea, Peru, Philippines, Senegal, Seychelles, Sierra Leone, Singapore, Spain, Sri Lanka, St. Vincent and the Grenadines, Swaziland, Sweden, Syrian Arab Republic, Thailand, Togo, Trinidad and Tobago, Turkey, United Kingdom, United States Uruguay and Venezuela.

Notes:

Rwanda and China are dropped.

Transition years and the two following years for the Euro countries are dropped.

This is because of the lack of continuity in the depreciation and money supply variables. The WB money supply variable post-Euro is not directly comparable to pre-Euro levels, but it is calculated to give good international comparability, and the data is arguably of better quality than for many other countries, so we chose to continue the Euro-series after the transition period. Following DDK (2005) and Caballero (2012), we include countries even though they are clear outliers with regard to inflation, real interest rate and depreciation. We will deal with this through the *HYPER* dummy.

Sample 2:

Time-period: 1980-2002 (balanced)

Countries:

Argentina, Australia, Bahamas, Bangladesh, Canada, Chile, Costa Rica, Cote d'Ivoire, Denmark, Egypt, Finland, France, Germany, Guatemala, India, Ireland, Israel, Italy, Japan, Jordan, Kenya, Korea (Republic of), Madagascar, Malawi, Malaysia, Mauritius, Mexico, Nepal, Netherlands, New Zealand, Norway, Pakistan, Philippines, Sierra Leone, Singapore, Spain, Sri Lanka, Sweden, Thailand, Trinidad and Tobago, Turkey, United Kingdom, United States, Uruguay and Venezuela

When including *POLITY* the Bahamas and Germany are dropped.

When including *LIBERALIZATION* the Netherlands are dropped.

Sample 3:

Time-period: 1980-2002 (balanced)

Countries:

Same as sample one, excluding the following due to less than 6 *GINI* observations:

Dominica, Malta, St. Vincent and the Grenadines, Togo, Gabon, Oman, Seychelles, Congo (Republic of), Antigua and Barbuda, Grenada, Benin, Syrian Arab Republic and Chad

Including *POLITY* drops the Bahamas, Barbados, Belize, Germany and Iceland.

Including *KAOPEN* and *LIBERALIZATION* drops Burkina Faso

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Including *FINREF* drops Central African Republic, Burundi, Niger, Papua New Guinea, Sierra Leone, Mali, Guyana, Botswana, Cyprus, Gambia, Honduras, Lesotho, Malawi, Mauritius, Swaziland, and Trinidad and Tobago, leaving us with 47 countries.

Sample 4:

Time-period: 1970-2011 (unbalanced)

Countries:

Country	First	Last	Country	First	Last	Country	First	Last	Country	First	Last
Algeria	1990	2008	El Salvador	1983	2000	Lithuania	1995	2010	Russian Federation	1995	2010
Angola	1999	2008	Estonia	1997	2010	Macedonia, FYR	1995	2010	Senegal	1995	2008
Argentina	1977	2006	Finland	1972	2004	Madagascar	1980	2008	Sierra Leone	1980	2008
Armenia	1994	2010	France	1977	2010	Malawi	1973	2008	Singapore	1973	2010
Australia	1972	2010	Gambia, The	1996	2006	Malaysia	1972	2008	Slovak Republic	1995	2005
Bangladesh	1976	2010	Georgia	1997	2009	Mali	1993	2009	Slovenia	1993	2010
Belgium	2001	2010	Germany	1990	2007	Mauritius	1978	2009	South Africa	1994	2008
Bolivia	1987	2010	Ghana	1987	2009	Mexico	1976	2010	Spain	1972	2010
Botswana	1985	2008	Guatemala	1975	2009	Moldova	1996	2010	Sri Lanka	1974	2005
Brazil	1990	2010	Guinea-Bissau	1995	2008	Mongolia	1999	2009	Swaziland	1998	2008
Bulgaria	1993	2010	Honduras	1987	2010	Morocco	1994	2010	Sweden	1972	2010
Burundi	1996	2009	Hungary	1984	2010	Namibia	1997	2008	Switzerland	1984	2010
Cambodia	1998	2007	India	1972	2008	Nepal	1980	2007	Tanzania	1992	2004
Cameroon	1987	2005	Indonesia	1982	2010	Netherlands	1973	2010	Thailand	1976	2007
Canada	1972	2008	Ireland	1972	2005	New Zealand	1979	2010	Trinidad and Tobago	1994	2008
Central Afr Rep	1996	2006	Israel	1980	2008	Nicaragua	1996	2008	Tunisia	1972	1996
Chile	1981	2010	Italy	1972	2010	Niger	1996	2008	Turkey	1972	2010
Colombia	1992	2010	Jamaica	1972	2001	Norway	1972	2006	Uganda	1994	2009
Costa Rica	1977	2010	Japan	1972	2004	Pakistan	1972	2008	Ukraine	1994	2010
Cote d'Ivoire	1972	2005	Jordan	1977	2009	Panama	1986	2010	United Kingdom	1972	2010
Croatia	1997	2010	Kazakhstan	1995	2009	Papua New Guinea	1999	2008	United States	1972	2010
Cyprus	1994	2007	Kenya	1972	2008	Paraguay	1994	2010	Venezuela	1972	2010
Czech Republic	1995	2010	Korea, Rep of	1972	2010	Peru	1985	2010	Zambia	1995	2008
Denmark	1972	2010	Kyrgyz Rep	1997	2007	Philippines	1976	2010			
Dominican Rep	1991	2010	Lao	1998	2010	Poland	1992	2010			
Ecuador	1991	2010	Latvia	1995	2010	Portugal	1980	1998			
Egypt	1972	2010	Lesotho	1990	2006	Romania	1998	2010			

Notes:

All countries with ten or more consecutive years of observations on the variables included in sample one plus *MEANGINI* and *GINIGROWTH*. Where *MEANGINI* is the average *GINI* for the last 5 years, and *GINIGROWTH* is the growth in *GINI* in the last 5 years. Both *MEANGINI* and *GINIGROWTH* are allowed to be calculated for observations missing fewer than 3 lags or leads. Azerbadjan, Uruguay, Nigeria, China, Belarus, Rwanda, Vietnam, Guinea Bissau were dropped due to being centrally planned economies and/or outliers. This leaves us with a sample of 104 countries.

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Sample 5:

Time-period: 1970-2008 (unbalanced)

Countries:

Country	First	Last	Country	First	Last	Country	First	Last	Country	First	Last
Armenia	1994	2008	Dominican Rep	1991	2008	Macedonia	1995	2008	Singapore	1973	2008
Australia	1972	2008	Ecuador	1991	2006	Mexico	1976	2008	Slovenia	1993	2006
Bangladesh	1976	2008	Egypt	1972	2008	Moldova	1996	2008	Spain	1972	2008
Bolivia	1987	2008	Estonia	1997	2008	Morocco	1994	2008	Sweden	1972	2008
Brazil	1990	2008	France	1977	2008	Netherlands	1973	2008	Switzerland	1984	2008
Bulgaria	1993	2008	Honduras	1989	2008	New Zealand	1979	2008	Turkey	1972	2008
Canada	1972	2008	Hungary	1984	2008	Panama	1986	2008	Ukraine	1994	2008
Chile	1981	2008	Indonesia	1982	2008	Paraguay	1994	2008	United Kingdom	1972	2008
Colombia	1992	2008	Italy	1972	2008	Peru	1985	2008	United States	1972	2008
Costa Rica	1977	2008	Korea, Rep of	1972	2008	Philippines	1976	2008	Venezuela	1972	2008
Croatia	1997	2008	Lao	1998	2008	Poland	1992	2008			
Czech Rep	1995	2008	Latvia	1995	2008	Romania	1998	2008			
Denmark	1972	2008	Lithuania	1995	2008	Russian Fed	1995	2008			

Notes:

Sample 4 reduced to countries with observations of *GINI* in at least 10 consecutive years leading up to 2008. Only actual observations on *GINIGROWTH* and *MEAGINI* accepted. This leaves us with 49 countries.

Sample 6:

Time-period: 1970-2008 (unbalanced)

Countries:

Country	First	Last	Country	First	Last	Country	First	Last
Canada	1972	2008	Korea, Rep of	1972	2008	Sweden	1972	2008
Czech Republic	1995	2008	Mexico	1976	2008	Switzerland	1984	2008
Denmark	1972	2008	Netherlands	1973	2008	Turkey	1972	2008
France	1977	2008	New Zealand	1979	2008	United Kingdom	1972	2008
Hungary	1984	2008	Poland	1992	2008	United States	1972	2008
Italy	1972	2008	Spain	1972	2008			

Notes:

Sample 5 reduced to include only OECD-members.

6.2. Descriptive Tables

Table 7, Descriptive Statistics:

	obs.	mean	st. dev.	min	max
M2RES	2201	8.289564	13.85161	0.1908626	152.4963
PCRATIO	2201	48.50289	41.74314	0.8152577	250.1187
GROWTH	2201	3.975405	3.819721	-22.93405	26.26858
GINI	2032	38.24483	9.908824	19.7	71.32653
GINIGROWTH	2201	-0.0647323	3.419669	-16.71699	18.58061
CGROWTH	2201	9.291288	19.99754	-75.36097	301.8077
MEANGINI	2201	38.62315	9.932801	19.75496	67.8347
HYPER	2201	0.0345298	0.182627	0	1
NH*REALINT	2201	1.80216	8.00807	-40.88293	69.04538
NH*INFLATION	2201	8.429292	8.309476	-17.63042	49.22587
NH*DEPRECIATION	2201	4.318591	14.20861	-29.3524	153.9479
KAOPEN	2171	0.3504699	1.537108	-1.863972	2.439009
POLITY	2201	5.087687	6.140924	-10	10
FINREFORM	1500	0.5800556	0.2857787	0	1
LIBERALIZATION	2020	0.4364653	0.7804709	0	4.302981
DEPINS	2201	0.4470695	0.4973035	0	1

Table 8, Correlations:

	M2RES	PCRA..	GROW.	GINI	GINIG..	CGRO..	MEAN..	HYPER	NH*RE..	NH*IN..	NH*DE..	KAOPEN	POLITY	FINR...	LIB..	DEPINS
M2RES	1															
PCRATIO	0.4095	1														
GROWTH	-0.1806	-0.0661	1													
GINI	-0.0892	-0.2946	0.0193	1												
GINIGROWTH	-0.0172	0.0541	-0.0713	0.096	1											
CGROWTH	-0.094	0.033	0.32	-0.0517	0.0361	1										
MEANGINI	-0.0854	-0.3011	0.0336	0.9855	-0.0599	-0.0575	1									
HYPER	-0.0649	-0.1308	-0.1779	0.1231	0.0381	-0.0976	0.1157	1								
NH*REALINT	-0.0556	0.004	-0.0017	0.0594	0.1089	0.0857	0.0406	-0.0522	1							
NH*INFL...	-0.1234	-0.3742	-0.0227	0.1781	-0.0631	-0.1061	0.1885	-0.1862	-0.4051	1						
NH*DEPR...	-0.0586	-0.2522	-0.1601	0.1983	0.0084	-0.1341	0.198	-0.0657	-0.1257	0.5611	1					
KAOPEN	0.1821	0.528	-0.0145	-0.2793	0.0677	0.0722	-0.2896	-0.1565	0.1145	-0.3789	-0.2845	1				
POLITY	0.0395	0.328	-0.196	-0.2673	0.0605	-0.0455	-0.2766	0.0116	0.1596	-0.1288	-0.1216	0.3173	1			
FINREFORM	0.0578	0.481	-0.0022	-0.1901	0.1151	0.0716	-0.207	-0.1536	0.1878	-0.3457	-0.2611	0.6765	0.3918	1		
LIBERAL...	-0.1237	-0.1314	0.0001	0.0944	0.0709	0.0826	0.0834	-0.0125	0.1068	0.0235	-0.0078	0.3071	0.0372	0.1945	1	
DEPINS	0.0958	0.2009	-0.0562	-0.2458	0.025	0.0342	-0.2492	0.0429	0.0596	-0.1847	-0.1572	0.2961	0.3688	0.4136	0.0655	1

Regional dummies:

- 1: Middle East & North Africa
- 2: Sub-Saharan Africa
- 3: Latin America & Caribbean
- 4: Europe & Central Asia
- 5: East Asia & Pacific
- 6: South Asia
- 7: North America

Source: The World Bank (2013)