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"Political Determinants of the Depth of Banking Crises"

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Zusammenfassung

Seit dem Ende der Bretton-Woods Ära haben zahlreiche Bankenkrisen Volkswirtschaften immense Kosten auferlegt. Die vorliegende Studie beschäftigt sich mit den politökonomischen Einflussfaktoren auf die Tiefe dieser Bankenkrisen. Es wird argumentiert, dass nur schnelle und tiefgreifende Bankensektorreformpolitik eine fortdauernde Vermehrung der Kosten der Bankenkrise abwenden kann. Die Tiefe der Bankenkrise wird somit zu einer Funktion des Ausmaßes von regulatorischen Wandel, welchen politischen Akteure gegen die Interessen von entgegengesinnten Lobbygruppen durchsetzen. Politische Akteure sind eigennützig und basieren die Entscheidung, ob sie regulatorischen Wandel vorantreiben, auf einer individuellen Kosten-Nutzen-Kalkulation in welche einflussreiche Lobbygruppen positiv, und eine hohe Gefahr der Absetzung durch die Bevölkerung negativ eingehen. Die zentralen Hypothesen dieser Studie sind daher, dass die Anzahl der politischen Akteure auf die Tiefe von Bankenkrisen a) von dem Demokratiegrad eines Landes und b) von der Größe des Bankensektors abhängt. Dadurch hebt sich die Argumentation ab von dem üblicherweise angenommenen uniform-negativen Effekt von der Anzahl der Akteure auf die Reformfähigkeit eines Landes (z.B. Hallerberg and Basinger, 1998)

In einer statistischen Analyse eines Querschnitts von 47 Bankenkrisen in der Zeit von 1980 bis 2002 versucht der empirische Teil dieser Studie, die gebildeten Hypothesen zu belegen. Die Tiefe der Bankenkrise wird dabei approximiert durch die ökonomischen Kosten, die sie begleiten, d.h. durch die Differenz zwischen dem erwirtschafteten Bruttosozialprodukt (BSP) und einem hypothetischen Trend-BSP, welches in Abwesenheit der Krise hätte erwirtschaftet werden können. Die Anzahl der politischen Akteure ist die Anzahl der Veto Spieler, d.h. derjenigen Akteure deren Zustimmung notwendig ist, um eine Änderung des Status Quo vorzunehmen.

Die Ergebnisse der Studie sind gemischt. Einerseits finden die Hypothesen, dass der Einfluss von der Anzahl der Akteure stark mit dem Demokratiegrad und der Größe des Bankensektors variiert starke empirische Bestätigung. Andererseits aber zeigen die Ergebnisse auch die großen Schwierigkeiten, die Nutzenfunktionen von Akteuren a priori zu definieren. Ferner sind die Ergebnisse von bestimmten Parametern der Detrendierungs-Methodologie abhängig. Weiterführende Forschungsarbeiten sollten daher die Auswahl dieser Parameter theoretisch fundieren.

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1. Introduction

In March 1998, the Japanese government injected 1.8 trillion yen (15 billion US$) into its troubled banking sector in a last, unsuccessful effort to prevent the imminent collapse of several large banks. *Long Term Credit Bank*, one of Japan's largest financial institutes failed only 6 months later, in October 1998, and several other financial institutions followed suit. With the benefit of hindsight, one cannot fail to notice the long story of deterioration of credit quality that announced the crash, but at the time regulators largely seemed to lack understanding for the urgency of the situation (Krawczyk, 2004). The sector had had problems with low profitability and deteriorating loan quality for years. Overcapacities and liberalisations had led to fierce competition, in which banks competed for borrowers, even at the cost of willingly loosening credit standards. Combined with simultaneous lax monetary policy by the Japanese government, this lending boom had driven real estate and equity prices to frightening heights that finally collapsed in 1992, revealing enormous imbalances between banks' liabilities and the market value of their assets (Kanaya and Woo, 2001). Nevertheless, it took regulators another five years to take strong policy action. Five years, during which banks were allowed to continuously build up imbalances while waiting for asset prices to return to the pre-crisis levels, instead of decreasing exposure to credit risk and building up loan loss provisions. Five years, during which regulatory authorities, charged with ensuring the economic viability of the banking sector, failed to take even the most basic measures of crisis resolution. In fact, even quasi illiquid banks were allowed to give out earnings in the form of dividends, long after it had become evident that every penny would be needed to strengthen their capital base (see: Table 1.1 on the next page). The question arises, why regulatory authorities that had the power to do so, did not intervene earlier. Today, the Japanese banking crisis is ranked among the longest and most expensive banking crises of all times, with official estimates of non-performing loans as high as 400 billion US$ (Caprio and Klingebiel, 1996) Why were banks allowed to build up such imbalances, without provoking any change in the regulatory environment?

This study will put forward a simple political-economy explanation for such cost-raising forbearance in banking crises. I argue that a banking crisis is an expression of a deeply flawed policy environment and that only swift and efficient policy change will mitigate the detrimental consequences of crises. The economic costs of banking crises, then, depend on the extent to which actors in the political system engage in such policy change, resisting the

incentive to hand out favours to special interest groups opposed to regulatory change. More precisely, I argue that a higher number of *democratically constrained* veto players decreases the profitability of rent-seeking behaviour, thereby increasing the effectiveness of the politics of banking crisis resolution. This argument is in stark contrast to regular political-economy theories of economic change, most of which associate a high number of veto players with high special interest influence and slow and ineffective economic change (e.g. Tsebelis 2002). However, I want to turn this argument around by arguing that veto players essentially consist of self-interested policymakers. Once a certain degree of self interest is taken into account, the sheer *ability* of an unconstrained veto player to change status quo policy promptly will not automatically lead to efficient crisis resolution.

Table 1.1: Aggregated Income Statement for Japanese Banks (Billions of Yen)

	1991	1992	1993	1994	1995	1996	1997	1998
Net Interest Revenue	14,618	19,189	18,456	19,539	19,523	19,080	17,408	10,56
Loan-Loss Provisions	13,193	15,332	15,593	16,654	14,474	14,744	14,888	10,52
Net Income	2,367	1,835	1,515	271	-5,346	-360	-9,683	-7,47
Dividend Paid	750	864	875	892	710	675	687	343

Source: Adapted from Kanaya and Woo (2001)

Government action and banking crises are intimately linked. While, simply speaking, banking crises do not differ much from solvency problems in the non-financial sector – banks basically "go bust" – government intervention and regulation still feature much more important in banking crises than in other crises and have in fact changed the very nature of the phenomenon. Again, the Japanese case can help to illustrate this: while "old school" banking crises, such as the 1929 economic crisis erupted in a sudden bang and were followed by investor and depositor panic, the Japanese crisis loomed on for years and even the highly misaligned liability structures did not result in widespread panic. Most analysts attribute this 'lack of depositor panic' to the implicit deposit insurance schemes in place at that time. Under such government funded regimes, banks troubled with liquidity problems or declining value of assets can apply for government emergency funds to avert illiquidity. Therefore, ailing banks will not immediately be confronted with anxious depositors demanding their funds, which formerly triggered the downwards spiral of forced sales of assets, depressed asset

prices and further demands for liquid funds. Instead, even banks with deeply negative net values can continue operations as long as their liquidity position remains manageable (which it can be for quite some time, as we have seen). The emergence of such situations of "banking distress", i.e. of technical, but not factual insolvency (Sundararajan and Baliño, 1991, p.4), which were largely unknown before the introduction of deposit insurance schemes, has also been reflected in a change of the definition of banking crises: while bank runs were *the* defining feature of banking crises before the introduction of deposit insurance schemes (see: Sundararajan and Baliño, 1991, p.2 for a collection of older definitions), these runs are now a mere epiphenomenon that can, but do not always accompany banking crises. More recent research, such as an IMF (1998) report on banking crises, takes account of this development when it defines a banking crisis to be:

> *A situation in which actual or potential bank runs or failures induce banks to suspend the internal convertibility of their liabilities or which compels the government to intervene to prevent this by extending assistance on a large scale.* (IMF 1998 p. 74)

This coherent and understandable account of banking crises shall be the definition I adopt throughout this study. However, for reasons of data availability I will be constraint to adapt this definition in my empirical analysis to match the one underlying the most widely used dataset on banking crisis.

This study will combine arguments from literature on the determinants of banking crises with arguments from literature on the political economic determinants of economic reform. Hence, the study can contribute to both strands of literature. For one, it will help identifying and evaluating the links between the economic costs of banking crises and the political system, which have been largely ignored in the literature, so far. An unwarranted exclusion, so I will argue, as crisis resolution is inherently political. Moreover, the interpretation of crisis resolution as a political process of economic change opens up an entirely new field of inquiry for researchers on economic change, who so far have been largely confined to look at post communist transformation countries and other rare "natural experiments". Inquiry into the resolution of banking crises can shed light on these dynamics by providing a clear cut case of economic reform, amenable to objective criteria of scientific evaluation.

Nevertheless, this study raises more questions than it resolves. While I do find strong empirical evidence for the idea that the effect of the number of veto players on the economic cost of banking crises is conditional to the level of democracy of the political system, it

cannot be derived with 100 percent certainty that this finding reflects the theoretical claims set out in this study. As the study is clearly limited in terms of its scope, it cannot be but a first gauge of a largely uncharted territory.

The following chapters are organised as follows. Chapter two will provide an extensive survey of the relevant literature, comprising a large section on the theoretical background to banking crises; chapter three will outline the argument and develop the hypotheses that will be empirically tested in a multivariate statistical analysis in chapter four. Chapter five will conclude.

Before finally setting out, it is of importance to note that the *origins* of crises are of no concern in this paper. Whether crises result from asset bubbles, from so called sunspots or any other phenomenon is of no concern. Rather, I treat all crises as roughly comparable incidents of regulatory failure. The sole object of analysis in this study will be the *consequences* banking crises have for the economy of the respective country.

2. Literature

The Literature chapter is divided in four sections. Section one provides the theoretical background to banking crises. Section two surveys the strictly economic literature on the depth of banking crises. Section three introduces the politics of economic change into the debate. In section four I survey the few articles that have tried to combine the two concepts into one coherent framework.

2.1. Theoretical Background

2.1.1. The Introduction of Government Safety Nets

The possibility of a crisis in the banking sector has long been recognised. Information asymmetries and the business of maturity transformation render the sector inherently vulnerable. As was famously analysed by the 19th century British economist Walter Bagehot, the inherent discrepancy between the short maturity of banks' liabilities – mostly deposits – and the longer maturity of banks' assets – mostly loans – leaves the banking business inherently fragile and vulnerable to swings in investors' confidence:

> *By the last return the savings banks - the old and the Post Office together - contain about £60,000,000 of deposits, and against this they hold in the funds securities of the best kind […]. But of cash in ultimate reserve - cash in reserve against a panic - savings' banks have not a sixpence. These banks depend on being able in a panic to realise their securities. But it has been shown over and over again, that in a panic such securities can only be realised by the help of the Bank of England.* (Bagehot 1873, p.330)

These panics, or bank runs, are a form of rational panic among depositors that have been a defining feature of banking crises for most of the past century. They result from the fact that in the event of a bank failure banks serve their customers on a first-come, first-serve base and that hesitant asset holders (i.e. depositors) risk loosing their funds. As bank runs were prevalent in all major episodes of banking crises, including *Black Friday 1929,* they were the prime object of research on banking crises until the late 1990s. Older definitions of banking crises still reflect this theoretic focus: "[A banking crisis is] a demand for reserve money so intense that the demand [can] not be satisfied for all parties simultaneously in the short-run" (Miron 1986, p.126). Bank runs are regarded as especially problematic, because of the market mechanisms inherent to them: increased demand for reserve money induces a rushed sale of assets at *firesale prices,* which incur massive balance sheet losses. These market mechanisms can cause self-confirming bank runs and, if depositors (mis-)take trouble in one bank as

indicator of trouble in the entire industry, they may cause financial turmoil to spread. As Ben Bernanke (1983), who has recently succeeded Alan Greenspan as the Head of American reserve bank, analyses in his famous article on the role of banks in the Great Depression:

> *The fact that liabilities of banks were principally in the form of fixed-price, callable debt (i.e., demand deposits), while many assets were highly illiquid, created the possibility of the perverse expectational equilibrium known as a 'run' on the banks. In a run, fear that a bank may fail induces depositors to withdraw their money, which in turn forces liquidation of the bank's assets. The need to liquidate hastily, or to dump assets on the market when other banks are also liquidating, may generate losses that actually do cause the bank to fail. Thus the expectation of failure, by the mechanism of the run, tends to become self-confirming.* (Bernanke 1983, p.6)

Understandably, then, many governments and central banks have tried to avert bank runs and their contagious tendencies by overcoming the malicious individual incentives that underlie panic – with considerable success. Following the original ideas of Walter Bagehot, which are already insinuated in the sentences quoted above, many central banks nowadays act as *lenders of last resort,* i.e. they commit to give out short-term loans to commercial banks in real or perceived liquidity problems and thereby reassure depositors and investors. A similar tool are *deposit insurance schemes*, i.e. government guarantees of the nominal value of deposit claims in the case of bank failure, which Diamond and Dybvig (1983) have shown to be an optimal policy, if a banking system's stability is threatened by self-confirming bank runs. The first such system of deposit insurance was introduced in the U.S. in the aftermath of the Great Depression, and the policy spread to most OECD countries and an increasing number of developing countries in the post-war period (Demirgüç-Kunt and Detragiache, 2000). Reflecting the phenomenal success of this policy, bank runs "were almost nonexistent in the heyday of Bretton Woods" (Bordo et al. 2001, p.8) and consequently also disappeared as object of scholarly debate.

But banking crises did not disappear altogether. Rather, bank insolvency has become more and more frequent since the late 1970s (Bordo et al. 2001), albeit in a different form. Present day banking crises are qualitatively different from older episodes on several accounts. Modern day banking crises have incurred economic costs on a scale unimaginable to prior crises. Furthermore, recent banking crisis-episodes have not even been associated with declining deposits, but have originated on the asset side of banks' balance sheets (Demirguc-

Kunt, Detragiache and Gupta, 2000). Finally, while crises used to be short and intense, many crises of the past 20 years have lingered on for years, hardly visible for the outsider (Bordo et al. 2001). Not surprisingly, banking crises also had a phenomenal comeback on the academic agenda, so that Caprio and Klingebiel (1996) describe the last twenty years as having been "to economists interested in banking and incentive issues what the 1840s and 1850s were to gold prospectors in California" (p.1). Many of these scholars attribute the re-occurrence of banking crisis at least partially to the introduction of government safety nets, which notwithstanding their advantage of preventing depositor panic, has also had detrimental consequences on banks' regular operative environment: a moral hazard (Demirgüç-Kunt and Kane, 2002). Simply speaking, bank managers, overly comforted by the presence of a government safety net, might discard prudential banking and engage in excessive risk taking to boost their profits. As a result, deposit insurance may contribute to a destabilisation of the banking system and may lead to more bank failures (see Gontermann, 2003 for an introduction of moral hazard into the classical Diamond and Dybvig-model). Furthermore, managers of banks with declining net value might be tempted to engage in such high risk-high profit investment to "gamble for resurrection" (Akerlof and Romer, 1993) instead of filing for bankruptcy (Graf, 1999). While such behaviour *can* restore the banks net value, it is more likely that it will lead to a further deterioration of the asset quality. Therefore, a delay in the restructuring efforts leaves room for the perverse incentives of banking turmoil to take effect and can thereby further aggravate a crisis, giving rise to the idea that quick restructuring can increase the probability of a restructuring success. Several recent banking crises have been analysed with regard to the impact of deposit insurance schemes: Kane (1989) shows, that gambling for resurrection has actually been a cause of the US-American Savings and Loans Crisis of the 1980s. Alper (2001) argues that the failure of the Turkish currency peg and the subsequent bank crisis was only possible in the context of banks' excessive risk taking fed by government insurance schemes. In order to substantiate the overwhelming country-specific evidence, the World Bank started a long term research project that focuses on the effects of deposit insurance since 1999. One of the first papers originating from this project was Demirgüç-Kunt and Detragiache (2000). The authors employ a cross-country statistical research design to test whether the presence of a deposit insurance scheme or any one particular feature of such system systematically increases the risk of a crisis. In their 61 country sample they find some evidence for detrimental effects of deposit insurance, especially in the context of a weak regulatory environment. A possible caveat of the study is that schemes' designs vary largely across countries and are therefore hard to compare. This line of research has been extended,

since. Looking at the effectiveness of deposit insurance schemes in different institutional environments, Demirgüç-Kunt and Kane (2002) explicitly challenge the almost-standard of adopting deposit insurance schemes without addressing observable weaknesses in the institutional environment. They argue that the extent to which moral hazard problems undermine the positive effects of deposit insurance depends on three features of a country's institutional environment: Transparency, deterrency and accountability. "High readings on these dimensions assure that counterparties in private and public sectors can enforce appropriate bank behaviour by evaluating bank activities, disciplining their risk-taking, and resolving their financial difficulties promptly" (Demirgüç-Kunt and Kane, 2002, p.9). While they do not present evidence themselves, the literature they survey largely supports their thesis: "when researchers compare the incidence of observed banking crises in different countries around the world, countries with poorly designed explicit deposit insurance systems are found to experience more crises." (p.24).

2.1.2. Determinants of Banking Crises

Most scholars focus on macroeconomic factors to explain why and when banking crises occur. Often, the image is evoked of the banking system being the "weakest link" that, in the advent of a macroeconomic shock (e.g. the change in the terms of trade or a currency re-evaluation), is likely to break (Gavin and Hausmann 1996) and tear down other parts of the economy. The size of the macroeconomic shock and the "sanity" of the banking sector are taken as determinants of the depth of a crisis. An example of such mechanism might be a bank that borrows in foreign currency and lends in domestic currency. This bank's profitability and liquidity position will be threatened by an unexpected depreciation of the domestic currency. One of the most important studies in this area is the article by Kaminsky and Reinhard (1999) that uses data from developed and developing countries to analyse the behaviour of macroeconomic indicators in the 24 months prior to a banking crisis. They find that crises are preceded by recessions, declines in the terms of trade and real exchange rate appreciations. Extending this forecasting approach, Demirgüç-Kunt and Detragiache (1998) find that on a domestic level, low GDP-growth, high inflation and high interest rates work best in presaging crises. But this rather a-theoretic forecasting approach has its limits, as was shown by Hardy and Pazarbaşioglu (1998). They identify a series of leading macroeconomic indicators of banking crises and test, whether these are of any value in predicting the Asian Crisis. As expected, the identified indicators fare rather bad. The authors therefore see the need to consider different determinants of crises and subsequently turn to microeconomic

determinants of crises, where they find proxies of the vulnerability of the banking and corporate sector to be better warning signs.

Partially as a consequence of the limited success within the macroeconomic research paradigm, other authors have focused on microeconomic factors in the occurrence of banking crises. A starting point of this body of literature is once more the groundbreaking article by Caprio and Klingebiel (1996). They start out by denouncing the "weakest link"-analogy set out earlier in favour of a more sector-specific approach of banking sector stability:

> *The Gavin-Hausmann 'chain' analogy […] breaks down to the extent that the links in the chain— the banks themselves— know that pressures on the links will occur frequently and unpredictably. In such a world putting all the blame on external forces is more an excuse than a justification.* (Caprio and Klingebiel 1996, p. 13)

Thus, a case is made for microeconomic and regulatory determinants of banking crises. Barth, Caprio and Levine (2001) use just this breach, when they draw upon a large database on banking regulation and banking supervision in 107 countries to examine the relationship between bank regulation/supervision and bank stability and performance. They compare two broad approaches to government regulation, the *helping hand* view (benevolent regulation corrects market failures) and the *grabbing hand* view (regulators support political constituencies) and expect to find substantial differences regarding bank performance and stability. Some key patterns emerge in their empirical analysis that are generally inconsistent with the helping-hand view of regulation and more consistent with the grabbing-hand view of government: regulatory and supervisory strategies that focus on empowering the private sector and limiting the adverse incentive effects from generous deposit insurance work best to promote bank performance and stability. But while they do find policies to exert a substantial influence in general, they are very cautious about suggesting global patterns: "There is no evidence, however, that the best practices currently being advocated by international agencies are best, or even better than alternative standards, in every country. There is no evidence that successful practices in the United States, for example, will succeed in countries with different institutional and political environments" (p.1).

Therefore, other authors have focused on singular institutions and have analysed their impact on the stability of the banking sector. One such institution that has been the source of heated debate is the nature of the exchange-rate regime: a fixed rate helps to maintain

exchange rate stability and may minimise the occurrence of domestic shocks by disciplining policymakers (Eichengreen and Rose 1998) and may therefore have stability-enhancing effects. But countries with fixed rates cannot let the exchange rate appreciate in the face of a macroeconomic shock and therefore loose an important tool of macroeconomic stabilisation (Calvo 1999). Addressing this issue empirically, Domaç and Martinez-Peria (2000) find that fixed exchange rates mitigate the likelihood of a banking crisis. But on a sideline, the authors also find that fixed exchange rates also increase the cost of crisis resolution, once crises are underway.

Another issue that has received much scholarly attention was the nexus between banking crises and currency crises. During the past 20 years, these two have tended to occur simultaneously as so called *twin crises* (Kaminsky and Reinhard 1999). But no convincing theoretical explanation on the nature of this nexus has emerged and, in fact, not even the causal direction has been identified unambiguously. What we do know, however, is that twin crises are substantially more expensive and more difficult to resolve than either one of the two crises separately (Bordo et al. 2003). This is why the authors conclude that "[t]he growing prevalence of twin crises is an important change in the fourth quarter of the 20th century" (Bordo et al. 2003 p.29).

2.1.3. Dating Crises

On an empirical level, most studies on banking crises share one common problem: ever since the widespread introduction of government safety nets banking crises are difficult to spot and almost impossible to date. In this, they are unlike currency crises. Whereas a plethora of high-frequency macroeconomic data is available and has been employed to pin down, date and measure currency crises (see for example: Kaminsky 2003), banking crises remain shadowy. Banks are notoriously opaque, accountable only to (not necessarily effective) supervisory agencies and have strong incentives to hide eventual liquidity problems. Therefore, many statistics relevant to banking crises, such as the quality and value of assets, are only available on an annual base, if at all. Additionally, banking crises can manifest in many different ways: as a big bang, as prolonged *banking distress,* etc. Sometimes there might be a particular incident marking the crisis, such as a government enforced merger, but more typically such incident is neither the beginning nor the end of bank insolvency, but only marks the point at which it is revealed to the public. Therefore, an objective procedure, using series of macroeconomic indicators can not readily be employed to date them. Rather, the

classification and dating of crises remains inherently subjective. Consequently, most authors of recent studies on banking crises have at least partially reverted to the judgement of experts and only a fraction of authors writing on banking crises have used more objective indicators (e.g. the volume of banking system deposits or risk premia). In his survey of econometric papers on the length of banking crises Edward Frydl (1999) turns this apparent weakness into a virtue:

> *Reliance on subjective expert views to date crises is appropriate. Simple multiple objective indicators, whether based on bank balance sheet data or financial market prices, are likely to be misleading. The concept of crisis is meaningful principally in terms of its effect on the expectations of participants in financial markets and the economy. Expert opinion can directly reflect these expectations.* (Frydl 1999, p.6)

The downside of this subjective approach, of course, is the lack of consensus on the dating of crises between researchers. Frydl, therefore, laments that "no consensus has been achieved about who should be the arbiter of dating banking crisis" (p.4). However, one such arbiter is de facto emerging from a study by Caprio and Klingebiel (1996), who present a newly compiled global dataset on banking crises[1]. Again, the authors exclusively rely on expert interviews to identify and (more or less precisely) date banking crises in 69 countries since the late 1970s. They distinguish between systemic crises, singular crises and a third category called 'covert borderline cases' that covers cases of financial distress. According to their widely cited definition, a banking crisis is classified as systemic if "the net worth of the banking system has been almost or entirely eliminated." (p.6). This dissection, categorisation and discussion of the phenomenon has greatly contributed to the research paradigm and opened up new paths for research on banking crises. Consequentially, the study by Caprio and Klingebiel has served as a "root source" (Frydl 1999, p.6) for much research on the determinants of the depth of banking crises.

2.2. Economic Literature on the Depth of Crises

Economic research on the depth of crises is a "poor cousin" of research on the probability of crises. Only slowly, as more and more cross country evidence on the conceptual differences between the two emerges, has research on the depth of crises become more prominent. So far, the new paradigm has developed very few theoretical arguments of its own, but has left the theoretical inheritance of its wealthy relative largely unchallenged. The largest

[1] According to Caprio and Klingebiel (1996), the dataset is the "first-ever compilation of data on insolvencies around the world" (Caprio and Klingebiel 1996, p.2)

contributions of this body of literature are therefore empirical in nature and correspondingly, the survey of this body of literature will focus on empirical issues.

The most fundamental challenge for all researchers on the depth of crises has been the opaqueness of banking crises: what exactly is the depth of a banking crisis and how are we to determine it, if we hardly know when a crisis has begun or ended? Researchers have found three distinct approaches to deal with this problem, each of which developed into a distinct research paradigm: assessing the fiscal cost of banking crises; assessing the economic cost of banking crises; and assessing the success of post-crisis bank reform programs directly. The three approaches will be introduced in turn.

2.2.1. Fiscal Cost of Banking Crises

This first research strategy interprets the depth of a banking crisis as the total costs that banking crises incur on governments. Most authors focus on regulatory choices to determine the amount of fiscal cost caused by the crisis. Policies, such as deposit insurance schemes, liquidity support, repeated recapitalisations and regulatory forbearance are inspected with regard to their effect on the fiscal cost. Honohan and Klingebiel (2000) report that in their sample of 40 countries governments spent an average of 12.8 percent of national GDP to clean up their banking systems in the aftermath of a crisis. The authors assign policies to two categories meant to reflect a *strict approach* and a rather *accommodating approach* to crisis management and inspect the implications for the total cost of crises. They find overwhelming support in favour of the *strict approach* of crisis resolution: all policies associated with accommodation positively contribute to fiscal cost of crises – a finding that again echoes the idea that speed of restructuring might strongly contribute to restructuring success. In a similar vein, Tang Zoli and Klytchnikova (2000) compare three different strategies of crisis resolution in 12 transition economies, but find no clear empirical relationship. Rather, they find, that the fiscal costs to a large extent reflect the macroeconomic conditions at the onset of the crisis. In their sample, overall fiscal cost ranged from two percent in the Baltic countries to 42 percent in certain Central European economies.

This research approach can and has been criticized on several accounts. First, as Honohan and Klingebiel readily admit, information on fiscal transfers is hidden in different government finance accounts and may not always be comparable across countries. Therefore the reliability of the data needs to be put into question. Second, budgetary transfers strictly do not represent actual economic costs but rather a transfer of wealth. It is not clear, whether they

are the most relevant measure for assessing the depth of a crisis. Finally, the relation between the amount of fiscal transfers and actual crisis resolution is only addressed implicitly.

2.2.2. Economic Cost of Banking Crises

This has lead researchers to adopt a different approach in which the costs of banking crises are assessed as a loss in GDP. The rationale is simple: banking crises can produce costs for the economy through two main channels: firstly, a sudden reduction of available credit, a so called "credit crunch" and a contraction in the overall stock of money force firms and households to reduce spending and may result in a recession. Secondly, the entire payment system may fail if customers do not sufficiently trust banks to leave funds on bank deposits, or banks loose trust in each other (Hoggarth, Reis and Saporta. 2001). One of the first studies to employ this approach to compare the economic cost of banking crises and currency crises is IMF (1998). The authors calculate an "output-dip" as the difference between actual output and a hypothetical "trend-output" in the crisis-episode. For their 50 country, 1975-97-sample, they find that banking crises usually are more prolonged and more costly than currency crises. On average, they lasted three years and the average cumulative loss in GDP-Growth was 11 ½ percentage points. Economic costs of crises vary significantly between countries with costs on average being higher in developing countries than in industrial countries (p.79). Of course, this approach has the drawback of not measuring the cost *caused* by banking crises, but rather the cost *concurrent* with banking crises. Nevertheless, the measure can be trusted to be unmistakably reliable and is therefore very adequate for cross country comparison. Recognizing this advantage, Hoggarth, Reis and Saporta (2001) employ this approach to analyse the economic cost of banking crises and the effects different crisis resolution policies have on them. In their sample of 47 systemic and non-systemic banking crises, output losses during crises are even larger than 15-20% of GDP on average. Contrary to the findings of IMF (1998), the authors find that losses are higher in developed countries than in developing and emerging countries. In a similar vein, other scholars have used foregone GDP as a proxy for the severity of banking crises. In their seminal article, Bordo et al. (2003) look at crisis-episodes reaching back as far as the 1880s and try to identify secular trends in their occurrence and development. Again, their dependent variable is the size of a hypothetical output dip, defined as the difference between actual output and trend output, calculated using a "band-pass-filter", a modified moving average-procedure. Importantly, they find that while banking crises clearly grown to be more frequent in the post-Bretton Woods era, they have not grown to be more severe (p.60) – a finding, which clearly contradicts other authors'

predictions on the consequences of deposit insurance schemes (see: Demirgüç-Kunt and Detragiache 2000). Instead, the authors find liquidity support to insolvent banks and the presence of an exchange rate peg to be more important determinants of the duration and depth of banking crises.

2.2.3. Success of Post-Crisis Bank Reform Programs

The third strand of literature has addressed the depth of crisis only indirectly. The main focus rather lies on the identification of the main determinants of bank restructuring success. Critically, proponents of this approach argue that crisis recovery is not a strict function of the size of the macroeconomic imbalances that caused a banking crisis in the first place and will therefore not manifest automatically after these have receded. Rather, crisis recovery is a function of government restructuring policy and needs to be "made" through careful policymaking (Caprio and Klingebiel, 1996). The same authors also use their dataset to analyse the determinants of recovery and restructuring. Importantly, they find that only very few countries have been successful in restructuring their banking sectors after a crisis. Their admittedly crude measure for the success of restructuring exercises is a good score on a combination of four indicators used to approximate the performance of the banking sector: ratio of money supply (M2) to GDP, development of real credit, real deposit interest rates and recurrent problems in the banking system after restructuring (p.15). Another study assessing the success of restructuring is the article of Dziobek and Pazarbasioglu (1997), which empirically analyses the relative success of bank restructuring programs in a sample of 24 countries. They construct a success-index reflecting a mixture of good banking performance and high financial system intermediation capacity and use this index to label countries as "good" and "bad" restructurers. They then continue to relate restructuring performance to specific regulatory measures[2] that countries made use of in the course of the restructuring. While the evidence on the importance of individual instruments is scant and rather indicative, their main conclusion nevertheless is strong: "Successful bank restructuring implies *prompt corrective action and a comprehensive approach* […] addressing shortcomings in the *accounting, legal and regulatory framework* while improving *supervision* and compliance" (Dziobek and Pazarbasioglu, 1997, p.3, emphasis in original). As we shall see, the idea, that the *speed of restructuring* might be an important determinant of success of restructuring has

[2] Specific measures range from bank closures and central bank liquidity support to conversion of non-performing loans and setting of incentives for managers and owners

subsequently been discussed by other scholars. Nevertheless, these taxonomist approaches still fail to directly address the mechanisms underlying the success of restructuring.

2.3. Financial Crises and the Politics of Economic Change

As economic approaches failed to deliver a coherent explanation of the regulatory determinants of the depth of crises, it emerged that some relevant factors have not been taken into account so far (see: Frydl 1999 for further discussion). Consequently, political scientists have raised their voices in the hitherto exclusively economic debate. Ultimately, they argue, crisis resolution policies are chosen in the political realm and therefore are inherently political in nature. In this, bank restructuring resembles economic reform in other policy areas and must therefore also be treated similarly, as pointed out by Montinola (2003). The focus of research, thus, shifts from *what measure* is successful in restructuring to a more fundamental *who* restructures successfully. The debate then resembles an old debate about the institutional and political determinants of the success of economic change (see e.g. Milner 1999). Three main strands of arguments have been put forward to explain the large cross country differences in the success of economic reform: arguments linking economic reform to economic crisis, to special interest influence and arguments linking economic reform to the institutional environment. They will be surveyed in turn.

2.3.1. Economic Crisis

One of the most eminent scholars in this area, Mancur Olson, has invested the better part of his academic life to the ideas that "institutions matter" (Olson 2003), and that interest groups play an important role in the making of economic policy. According to Olson, organised interest groups form "distributive coalitions" with people within the political realm to push through suboptimal special interest politics against the interest of the unorganised general public (Olson 1982). He goes on to argue that the German *Wirtschaftswunder* of the 1950s and 60s was possible only after World War II had disrupted these distributive coalitions. One need not follow Olson's argument all the way to understand that a crisis can be the cradle of economic change and reform. Dani Rodrik (1996) presents a formalised model in which policymakers weigh the efficiency gains of economic reforms against the political cost they entail through their redistributional effects. This cost-benefit-analysis, which normally results in maintenance of the status quo, can tip towards economic reform in a crisis situation, where the efficiency gains of economic reform rise. Similarly, Haggard and

Maxfield (1996) argue that the cause of the internationalisation of developing countries' capital markets in the 1980s is to be found in a higher frequency of balance of payment crises:

> *The reasons for this tendency lie in the high costs that countries pay for inward-oriented responses to crises under conditions of increased financial market integration. Maintaining or increasing financial openness in the face of crisis signals foreign investors that they will be able to liquidate their investments, indicates government intentions to maintain fiscal and monetary discipline, and this ultimately increases capital inflows* (Haggard and Maxfield, 1996, p.38)

Thus, crises can undermine the credibility of interest groups in favour of the maintenance of status quo policies and help new interest groups gain more influence. Therefore, a deep crisis can provide a reform-oriented policymaker with more leeway to push through an ambitious agenda. Today, the idea that a crisis can have positive effects on economic reform has become "conventional theory" (Montinola 2003, p.546). Nevertheless, most attempts to empirically link the severity of a crisis to the speed of its resolution have failed because even heavily crisis-ridden countries again and again delay crisis resolution.

2.3.2. Special Interest Influence

The second class of arguments uses the extent of special interest influence as an independent variable in the analysis of economic change. So while literature on the crisis-change nexus argues that crises can serve to break up distributive coalitions, this strand of literature tries to directly asses this impact. Hutchcroft (1998), for example, shows up in impressive detail how cosiness between governments and economic interest groups was conductive to pre-crisis growth in East Asia but negatively affected governments ability to reform economic policy in the advent of crisis. In one of the most widely cited works of modern social science, Mancur Olsen (1971) delivers the underlying logic for the over-proportional influence of small interest groups: members of a large, "latent" interest group have an incentive to free-ride on the public goods provided by the group. Only small organised interest groups can overcome these incentives through monitoring and deterrence of free-riding behaviour. Interest groups often are understood as societal interests ruthlessly competing for a direct access to government funds (e.g. Weede 1990). Alesina and Drazen (1991) model the delay of stabilisation as a "war of attrition" between competing interest groups, who would rather see aggregate cost of economic adjustment rise than accept a reform plan that implies negative distributional consequences for them. Ever since the article by Grossman and Helpman (1994) political favourism is often modelled as a political market on which special interest groups "buy" policy from policymakers by making the highest bid in a

so called *menu-auction*. Policymakers "sell" policy to maximise their *political support function*, which consists of two terms: aggregate welfare and personal contributions received from interest groups. It follows, that specific interest groups' ability to push through their special interests will critically depend on their size.

2.3.3. Institutions 1: Democracy vs. Autocracy

Alongside the crisis change-nexus, political scientists and political economists have also raised their voices in favour of the idea that "regime type matters" in economic reform (see: Tommasi 1995 for an overview). The regime-type hypothesis originally put forward by Skidmore (1977) claims that reform performance will be better in authoritarian regimes, where policymakers are isolated from popular pressures. Underlying is the idea that because true reforms are unpopular and costly in the short run, leaders dependent on popular consent, i.e. democratic leaders, are unlikely to prescribe anything more than cosmetic reform. Authoritarian regimes, however, will be able to push through ambitious reform agendas. Later, the argument was turned around by Remmer (1986), who argued that reforms implemented by democracies will be more sustainable, as democracies have a broader popular base and greater ability to convince. Later, the idea that not democracy, but democratisation can foster reform has received much attention. Democratisation of the political system, so it is argued, increases the size of the *winning coalition*, thereby raising the political costs of pursuing special interest politics. Therefore democratisation reduces a governments' ability to maintain a suboptimal status quo (Kubota and Milner 2005). However, neither theory has been able to amass much empirical evidence in its favour. While the regime-type question remains unresolved today, many scholars have focused on other dimensions along which political systems can be distinguished. Among such intraregime-dimension, the concentration of power within the system and its effects on policy and policy change has received much attention. It is this veto player framework I will turn to now.

2.3.4. Institutions 2: The Veto Player Framework

The by now classical argument regarding the effect of veto players, i.e. the number of "individual or collective actors whose agreement is necessary for a change of the status quo" (Tsebelis 2002, p.36) is that the higher the number of veto players, the more credible government policy will be. Among the first proponents of such veto player arguments were the economic historians Douglass North and Barry Weingast (1989), who argue that

phenomenal growth of capital markets in 18th century England was only possible because of certain constitutional arrangements adopted in the aftermath of the 1688 Glorious Revolution.

> *What established the government's commitment to honoring its agreement - notably the promise not to appropriate wealth and repudiate debt - was that the wealth holders gained a say in each of these decisions through their representatives in Parliament. This meant that only if such changes were in their own interest would they be made. Increasing the number of veto players implied that a larger set of constituencies could protect themselves against political assault, thus markedly reducing the circumstances under which opportunistic behavior by the government could take place.* (North and Weingast 1989, p.829)

A government's ability to commit credibly to upholding the property rights of its citizens, then, becomes the key to economic success, and the *number of veto players* becomes its key determinant. This alleged positive relationship between the number of veto players and long-run economic growth has been confirmed empirically. Henisz (2000) argues that frequent and potentially arbitrary policy changes raise hurdles for investment, especially in long-term projects; and that "institutional environments in which economic returns can easily be secured through political channels lead individuals to reallocate resources from economic to political activity" (p.3). Empirically, he includes his newly developed political constraints index[3], into Barro's (1996) analysis of long run economic growth to test the effect of the number of veto players and finds them to be highly correlated with higher growth. Keefer and Stasavage (2000) extend the argument by saying that the impact of multiple veto players on policy credibility depends on the polarisation of the political system. They apply their argument to a model of monetary policy and central bank independence and find empirical support for their claims.

But while the effect of veto players on long-term growth may be positive, the effect on the likelihood of economic reform is less straightforward. In fact, the arguments uttered by proponents of the veto player-growth nexus mostly run via the credibility enhancing effects of the *infeasibility of change*. Conversely, then, many scholars have linked the number of veto players not to economic growth but to rigidity or, more politely, to *policy stability*. One of the most eminent scholars in this research area, George Tsebelis (2002) makes the key prediction, that a change in a status quo policy is more likely, the lower the number of veto players in a political system[4]. In his terminology, a higher number of veto players diminishes the size of the *winset* of policy change, i.e. of the range of policy outcomes all actors can agree to

[3] The political constraints index represents a concept very similar to veto players as it takes into account independent veto points over policy outcomes and the distribution of these actors' policy preferences.
[4] To be precise, Tsebelis also deems "ideological distances" between veto players, their tenure in office and other factors relevant. As these are of little importance in my arguments, these factors are excluded here.

implement. This prediction has found considerable empirical evidence. Hallerberg and Basinger (1998) argue that higher numbers of veto players decrease a political system's ability to promptly adapt to a change in the economic environment. In a study on tax competition within OECD countries, they show that states with a higher number of veto players are slower in lowering capital taxes, even under substantial economic pressure from increasingly mobile capital. Similarly, Korpi and Palme (2003) examine the effect of the number of veto players on cuts in government' welfare expenditure. For their sample of 18 countries they find that countries with a high number of veto players, such as Germany or Switzerland, have significantly decelerated the remodelling of the welfare state. However, also this finding has not been left unchallenged. In a recent paper, Bodenstein and Schneider (2006) find that an increasing number of veto players has enhanced, rather than limited the scope of economic reform in post-communist transition countries. According to them, veto players have an incentive to support change in transition countries, where constitutional and political choices are made simultaneously and power brokers can increase their power base through supporting change.

2.4. Linking the Depth of Crises to the Political System

This final section of the literature review will try to bridge the gap between the two strands of literature just introduced. It will survey the rare attempts that have been made to link the depth of banking crises to the political system and its institutional features. The arguments roughly mirror the ones uttered in the more general discussion above: either scholars have taken institutions to determine the rigidity / flexibility dimension of decision-making, or they have seen them as a determinant of the influence of special interest groups. The arguments will be surveyed in turn.

2.4.1. Rigidity / Flexibility in Crisis Resolution

The importance of decisiveness in crisis resolution has been stressed over and over in the economic strand of literature (Demirgüç-Kunt and Kane, 2002). Banks balance sheets further deteriorate during crisis through excessive risk taking and gambling for resurrection. Most scholars linking political institutions and crisis resolution adopt a similar stance and argue that policy rigidity slows down crisis resolution and increases economic costs. In this vein, Haggard and MacIntyre (1998b) argue that "if the likelihood of government taking timely and effective remedial policy action is seen to be low or uncertain, investors have

incentives to exit quickly" (p.2). In an extension of this argument, MacIntyre (2001) also takes into account the opposite side, i.e. the negative effect of an overly volatile environment. He assumes the number of veto players to determine the placement of economic policy along a continuum between *policy rigidity* and *policy volatility*, i.e. between a situation "in which governments have extreme difficulty making desired policy adjustments" (p.84) and a situation "in which governments vacillate wildly from one policy position to another" (p.84). In turn, the nature of the economic policy is hypothesized to influence the extent of financial panic in the case of a financial crisis. But MacIntyre does not only stress the credibility enhancing effects of *policy rigidity*, but argues that during a crisis both polar syndromes create significant credibility problems. In his own words: "[i]f the policy status quo were perfectly optimal, rigidity would be desirable – but almost by definition this is not the case when crisis strikes" (p.84). Therefore he hypothesizes a curvilinear relation between the number of veto players and the success of banking crises resolution. Empirically, he employs a comparative case study design to analyse the different crisis resolution policies in the East Asian crisis. His results lend support to his claim: while Thailand suffered from policy rigidity and Malaysia and Indonesia from the opposite syndrome, alone the Philippines, the country with an intermediate number of veto players "were able to maintain a steady course" (p.117) and resolve the crisis efficiently. This analytical framework of institutions navigating between a Scylla and a Charybdis has also been used in a wider sample: Angkinand (2005) tries to determine the effect of the number of veto players on the economic cost of banking crises within a wide sample of 45 banking crises in 27 emerging market economies from 1980 to 2003. Analogous to MacIntyre, she argues that credible government policy "should stop the spread of financial panic and reduce uncertainty about the future investment environment" and that these gains "are traded off against the losses of policy flexibility in responding promptly to exogenous shocks" (p.2). However, she only finds tentative evidence for the alleged curvilinear effect of the number of veto players on the economic cost of crises. This is not overly surprising however, as the importance of investor panic in banking crises is largely attenuated by the confidence raising effect of government safety nets.

2.4.2. Special Interest Groups in Crisis Resolution

Taking into account the attenuated role of *confidence* in banking crises, one might expect the effect of the political system to be straightforward: the more *decisive* they are, the better for the efficiency of crisis resolution. However, researchers have realised that the nature of the political system also exerts another influence on crisis resolution: the extent to which

special interest groups influence policy. One of the first papers within the crisis-paradigm to roughly reflect such ideas was written by Acemoglu et al. (2002). They use the number of veto players as a proxy for the overall quality of the historically determined institutional environment. They "expect a society where elites and politicians are effectively constrained to experience less infighting between various groups to take control of the state, and to pursue more sustainable policies" (p.4). Hence, a more constrained, less *decisive* political system gives interest groups less incentives to pursue special interest politics. Therefore, the authors expect and empirically find a higher number of veto players to be negatively associated with volatility and occurrence of crises. But their study fails to specify an explicit causal mechanism and can only be seen as a very rough approximation of the alleged link. Montinola (2003) addresses the link more directly. In her analysis of the determinants of the duration of banking crises in a sample of 67 developing countries she finds that decisiveness facilitates crisis resolution only in relatively small crises. Paradoxically, larger crises are resolved more efficiently in more constrained political systems. She argues that because more constrained policymakers

> *are compelled to consider the interests of a broadly based political coalition and to make transparent the distribution of resolution costs and are thereby likely to encounter less resistance to and more cooperation from various groups toward the implementation of painful policies.* (Montinola 2003, p.566)

So while the introduction of government safety nets has rendered useless the *confidence* based arguments put forward by North and Weingast (1989) and later MacIntyre (2001), other authors have realised that that these government safety nets also have implications for crisis resolution. Philip Keefer (Keefer 2001, 2001b, n.d.) argues in a series of articles that the fiscal cost of banking crises are largely a function of the extent to which policymakers pay special interest favours to the banking sector. For example, the indemnification of bankers' losses will lead to an increase in high risk assets, which in turn will add to the cost of a crisis. According to Keefer, policymakers base their decision, whether or not to hand out a favour on the amount of "campaign contributions" they receive in return. Critically, this contribution-amount is assumed to decrease with an increasing number of veto players as interest groups need to split the total amount available for contributions among more veto players. In stark opposition to other authors, Keefer therefore hypothesizes the fiscal cost of banking crises to *decrease* with a growing number of veto players. In his (2001) empirical analysis of the fiscal cost of 40 banking crises in 35 countries, he finds surprisingly strong support for his claims.

2.5. Discussion of Literature

So far, we have seen a range of theories, predicting different effects of the nature of political systems on the depth of crises. Each of the theories has also been sustained with more or less convincing empirical evidence. The four main theories are summarized in Table 2.1

Table 2.1: Theories linking veto-players and crisis depth and their predictions

Theory	**Proponent**	**Causal Mechanism** Veto Player Affect:	**Prediction:** More Veto Players…
Decisiveness	Demirgüç-Kunt and Kane (2002), Tsebelis (2002)	Speed of Resolution	→ crisis depth ↑
Confidence	MacIntyre (2001)	Extent of Investor Panic	→ crisis depth ∪
Institutional Economic	Acemoglu et al. (2001)	Uncertain. Less infighting between Interest Groups	→ crisis depth ↓
Political Economy	Keefer et al. (2001), Montinola (2003)	Special Interest Favourism	→ crisis depth ↓

However, the four arguments need not be contradictory, at least not on a theoretical level. The D*ecisiveness* argument states that a rigid political system, i.e. a political system with many veto players, will make crisis resolution less efficient because it *slows down* the process of policymaking. Thus it only makes a statement about the *speed* of crisis resolution. The *Political Economy* argument, by contrast, argues that a rigid political system will make crisis resolution more efficient because it mitigates the influence special interest groups have on policy. Thus, it only makes a statement about the *quality* of crisis resolution.

Furthermore, the empirical tests supporting the arguments all address different aspects of efficiency of crisis resolution. Keefer assesses the fiscal cost of banking crises. Montinola addresses the duration of crises. Ultimately, however, the relevant size to be gauged is neither the amount of fiscal transfer, nor the duration of a crisis, but rather the extent to which people have suffered. The interesting question, then, is to assess how these arguments affect the economic cost of banking crises. Do they cancel out, do they add, are they visible sequentially? These issues will be clarified in the empirical section of this paper. Let me first explain my argument.

3. Theory

Banking regulation in a country in the midst of a banking crisis is very likely to be severely flawed. Capital markets do not function with the usual efficiency, misallocating funds to non-viable companies and giving economic actors incentives to divert from maximising economic value. Reform is a necessity. There is a large body of literature that predicts that a political system will be more apt to cater such needs for reform if it is marked by what is commonly referred to as policy flexibility (e.g. Hallerberg and Basinger 1998). It is argued that each additional veto player in a political system leads to more policy rigidity by increasing the likelihood that an interest opposed to policy change possesses veto power and can block change (McCubbins 1991). But such theory is, at least partially, grounded on false premises: interest groups never posses veto power – veto players do. If the close association between veto players and interest groups is relaxed, the question that needs to be addressed is what drives veto players to pursue certain policies. I want to argue that veto players – whether they be individual or collective – ultimately consist of policymakers who are driven by self interest, rather than being controlled by certain interest groups. Once a certain degree of self interest on the side of policymakers within veto players is taken into account, the sheer *ability* of an unconstrained veto player to change status quo policy promptly will not automatically lead to efficient crisis resolution. Rather, the efficiency of crisis resolution will also be determined by the extent to which the policymakers within veto players have an interest in pursuing efficient crisis resolution policies. Conversely, a high number of veto players need not necessarily be associated with policy rigidity but can also improve the efficiency of crisis resolution if actors within veto players benefit from it.

This chapter will develop this theoretical argument. I will first explain the rationale underlying my thesis, then I will introduce the politics of crisis resolution and the actors involved in it, deriving the hypothesis that will be used to test the argument. The final subsection will summarize the argument.

3.1. Rationale – a Tale of Swift Policymaking

Imagine a country characterised by three features: an absolute concentration of power in a single veto player, an absence of competitive elections and a large banking sector. Now imagine that this country, out of the blue, gets struck by a banking crisis. Banks have

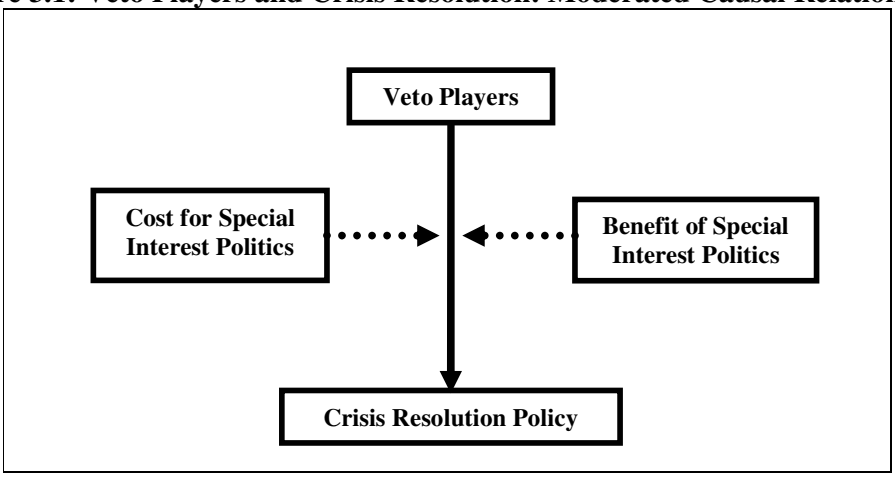
amounted enormous liabilities and are unable to match them with equivalent assets. The policymaker knows that such situations produce substantial costs for the economy and reverts to his IMF-Manual on banking crises to look for a remedy. There, he finds prescribed a rather drastic three-step reform program involving stricter regulation, bank mergers and bank closure. Whilst still skipping through the pages, his *antichambre* is already beginning to be populated by bank managers and bank owners demanding to see him. Upon consultation, they all present high gloss 300-page dossiers, which immaculately *proof* that the IMF-prescribed program would be much too drastic in their particular case and that all they needed to get out of this transitory and rather unfortunate *imbalance* would be a tiny recapitalisation of their banks, a little leeway regarding regulation of loan-loss provisions and an economic upswing. Consequently, they advocate a lenient approach to managing the *imbalance*. In exchange for this little favour, they promise to arrange a skiing trip with a former president of a powerful western country. Of course, our policymaker is very concerned about the welfare of the population as he fears being toppled if people are dissatisfied. However, he incidentally also loves skiing and the calculations presented in the dossiers are absolutely *convincing*. Therefore, he swiftly agrees to adopt a lenient stance towards the banking sector. After all, the IMF-Program can still be implemented after his return from the skiing trip.

Now imagine the same situation in country with by a small banking sector and a political system characterised by competitive elections, in which power is shared between several veto players. The crisis strikes, and bank managers and owners now siege *les antichambres* of all policymakers within veto players. Again, their evaluation of the situation and the appropriate policy measures radically differ from the IMF-Manual. This time, however, the dossiers they present are merely twenty pages long, printed on recycled paper without any illustrations – not very convincing. Additionally, all they offer in exchange for a little lenience towards their unfortunate situation is a joint bus trip into the countryside. Our policymaker, who not only dislikes the countryside, but is also deeply concerned about outcome of the upcoming elections, weighs the benefit of the bus trip against the cost of loosing office if voters dislike his accommodating stance towards the banking sector. Finally, he decides to remain firm and to try to push through the IMF-prescriptions. Even if our policymaker does like the countryside, it is unlikely that all of his colleagues do.

So in the same situation we can observe two entirely different outcomes with respect to the politics of crisis resolution depending on the political system. The *flexible* political system

has adopted the lenient approach; the *rigid* political system has adopted the strict approach. Of course, this example is entirely constructed and the process of policymaking is oversimplified and exaggerated. Nevertheless, it can serve to highlight three cornerstones in the interaction of crisis resolution and the political system: first, crisis resolution is political in nature. Therefore, the introduction of self interested policymakers leads to different approaches to crisis resolution in different political environments. Second, interest groups from the banking sector disapprove of thorough bank restructuring. Therefore, the efficiency of crisis resolution will be higher where the influence of these interest groups is limited. Finally, the extent to which interest groups exert influence on policy depends on three variables: the size of the banking sector, the concentration of power in the political system and the extent to which veto players are constrained by competitive elections. Figure 3.1 illustrates this moderated causal relationship. The next section will further elaborate these points.

Figure 3.1: Veto Players and Crisis Resolution: Moderated Causal Relationship



3.2. Banking Crisis Resolution as Political Reform Process

The question arises, if the strategies chosen by policymakers really will make a difference. After all, the banking sector might be right and IMF prescriptions need not be flawless. I want to argue that swift and thorough resolution policies do make a difference with respect to the economic cost of banking crises. Prior research has shown that policymakers who forbear or only engage in half-hearted measures risk increasing economic cost and perpetuating financial turmoil (Dziobek Pazarbasioglu, 1997; Honohan Klingebiel, 2000). If an asset boom abruptly ends in the bursting of a price bubble, it might be an adequate strategy to sit tight and wait until regular economic activity resumes. However, this is not the case

with banking crisis, as systemic banking crises can only very rarely be resolved within the given policy environment. Rather, efficient resolution of crises in the banking sector involves drastic policy change as the same policy failures that contributed to the occurrence of the crisis continue to add to its depth as the crisis looms on. Three distinct mechanisms are at play:

First: crisis ridden banks cease to fulfil their role as financial intermediators efficiently (Freixas and Rochet, 1997). Banks are concerned about the declining value of their assets and may refuse to extend credit to otherwise viable companies, causing credit crunches and liquidity problems. Moreover, crisis-ridden banks may also be tempted to role over a bad credit to conceal balance sheet problems, thereby allocating resources inefficiently to companies unviable in normal times. In such a crisis situation, economic actors will rarely, if at all, engage in long-term projects and investment, which causes economic costs.

Second: banking crises cause the payment system to fail (Hoggarth, Reis and Saporta, 2001). Modern payment systems are confidence-based. Depositors, who loose confidence in a crisis-ridden bank and store money under their mattresses undermine this system and can cause it to fail. Equally, the system will fail if banks loose trust in one another and stop making inter-bank transfers. Of course, such a breakdown causes economic cost, as consumption and investment are hampered.

Third: crisis situations give rise to extralegal activities such as *economic looting (*Akerlof and Romer, 1993): with government safety nets in place, company owners have an incentive to pay themselves more than their firms are worth and then default on their debt obligations because the government guarantees a firm's debt obligations. In such a situation normal economics of maximising economic value is replaced by "the topsy-turvy economics of maximising current *extractable* value, which tends to drive the firm's economic value deeply negative." (Akerlof and Romer, 1993, p.2 emphasis added).

For these reasons, swift and thorough policy action will lead to smaller economic cost of banking crises. I argue that this holds true irrespective of the concrete policy chosen. Recognising that political systems characterised by high *policy rigidity* are at a disadvantage with respect to the provision of swift policy change, I can already lay out a first hypothesis, which reflects mainstream veto player analysis and will serve as a benchmark hypothesis for other hypotheses:

H1: The higher the number of veto players in a political systems, the higher will be the economic cost of banking crises.

However, as we have seen, this is not the only relevant variable. The fact that an unconstrained policymaker *can* engage in swift policymaking does not necessarily imply that he *will* do so. In the following section I want to argue that this choice is determined by the utility calculations of the veto players. The outcomes of these calculations, then, are influenced by the nature of the political system and the size of the banking sector.

3.3. Actors in Crisis Resolution

As reflected in the constructed example above, I identify two relevant groups of actors in the process of crisis resolution: veto players and interest groups. In accordance with most present day economic research, I assume these actors to be rational in the economic sense and to base their decisions on rational cost-benefit-calculations. Macroeconomic outcomes are assumed to follow from the actions of rational individuals. While modern research on behavioural economics has shown that this assumption cannot be taken to hold at all times (e.g. Egidi 2005), I deem it appropriate in my specific case. I will now analyse the goals and preferences of relevant actors regarding the outcomes of crisis resolution.

3.3.1. Veto Players

Veto players are often portrayed as selflessly representing certain societal interests. A veto player is "controlled" by a certain interest, so it is argued, giving the respective interest group veto power over policy change (e.g. McCubbins 1991). I contend that while veto players may very well be closer to one societal interest than the other, they are never simple footmen to an interest. Rather, veto players consist of policymakers who have an agenda of their own – they are self interested and act in accordance with their own preferences, not necessarily minding policy outcomes, but rather personal utility. Similar to a menu-auction system to the modelling of politics (Grossman and Helpman 1994), I assume policymakers within veto players to choose policies to maximise their *political support function*, which

consists of two terms: aggregate welfare (indicating public contentment and probability of loosing office) and personal contributions received from lobby groups.[5]

Some authors strictly distinguish between individual veto players, e.g. a president, and collective veto players, e.g. a parliament (see for example Tsebelis (2000, p.19). While I recognise this difference, I deem it largely irrelevant for my analysis, as all veto players, irrespective of their individual or collective nature, ultimately are constituted of individual policy makers, who can be described by their strong preference for staying in office and an adjacent utility function. This is clearly the case for a president, but it is also true in the case of a parliament, where re-election probability directly depends on public contentment and aggregate welfare. What will change with the nature of the veto player, however, is the way in which special interest groups try to extract policy favours from veto players, as collective veto players cannot simply be assumed to be *bribed* collectively. Instead, interest groups can transmit information or agitate and mobilize public discontentment in order to influence policymakers within collective veto players. In exchange for a "policy favour", an interest group could offer to *abstain* from mobilising public discontentment. All delegates of the majority fraction in a parliament would see their chances of re-election increased from such abstention. French politics, where strong labour unions only need to threaten to organise one of their much dreaded *grèves*, regularly presents examples for this. Of course, such methods of exerting influence will probably be less effective than direct bribery, but then, this is just what I argue.

In practice this means that a veto player or a policymaker within a veto player will compare the amount an interest group is willing to contribute in exchange for a convenient policy decision to the economic costs he or she will incur if he or she diverts from socially optimal policy and engages in special interest politics. If the amount is higher than his costs, he or she will hand out a favour, if the costs are higher, he or she will stick to socially optimal policy. If he or she pursues suboptimal economic policies or supports such policies in a collective body, aggregate welfare will be below its potential and citizens will be discontent. The weights with which the two arguments of the political support function enter his or her

[5] Theses contributions can be interpreted as legislative campaign contributions in more developed nations, but can and should be interpreted as bribery in less developed countries. Judged in terms of integrity, the difference is almost negligible in any way. However, I assume that lobbyists in crisis situations do not as much try to influence the outcomes of elections, but want to address policies directly. Therefore, the alleged connection between campaign contributions and policy outcomes in the menu-auction model is likely to be oversimplifying in this case (for a discussion see: Ursprung 2000). Elections therefore only play a background role in my theory and I opt for an interpretation of lobby contributions as directly adding to personal welfare.

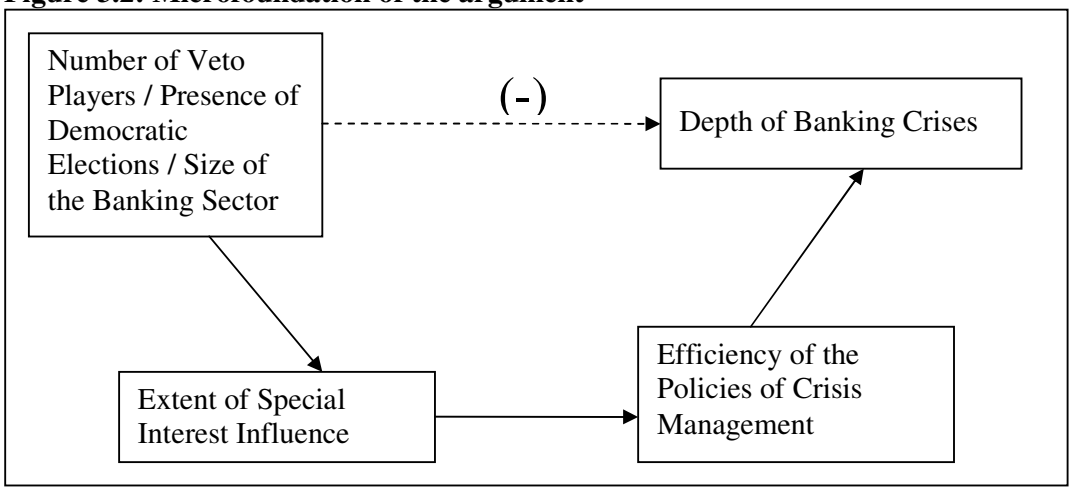
calculation depend on the easiness with which the general population can depose of him or her. If elections are regular and competitive, the relation between public discontentment and the risk of loosing office will be direct and aggregate welfare will enter the calculation strongly. In the case of no elections or no competitive elections, aggregate welfare will only enter his or her calculation with a small weight as the risk of loosing office after pursuing special interest politics is rather small.

What does this imply for the effect of veto players on the depth of banking crises? The benchmark hypothesis states: the more veto players there are, the higher the cost of the banking crisis should be. However, this argument does not take into account the self interest of policymakers within veto players. Self interested actors have a strong interest in acting in the interest of the general public if they risk loosing office by doing otherwise. Thereby, an additional veto player can actually raise the efficiency of crisis resolution if he or she limits the influence of special interest groups in policymaking. It follows that the effect of the number of veto players on the economic cost of banking crises critically depends on the level of democracy present in the political system. Special interest groups in democracies will have to invest more resources into lobbying for the same policy favour, as the offsetting costs that democratic veto players face are higher. Stated differently, the democratic veto player will be *more expensive* to influence than his or her non-democratic colleague. So while undemocratic veto players might be nothing but an obstacle to swift crisis resolution, democratic veto players add to the efficiency of crisis resolution as they raise the cost for rent seeking. This, then, would turn around the effect of veto players on crisis depth. Ceteris paribus the likelihood of a veto player engaging in special interest politics declines with a higher level of democracy. Thus follows my main hypothesis H2:

H2: A high number of veto players will decrease the economic cost of banking crises in a democratic political system.

As stated earlier, this argument is founded on an individual logic, i.e. aggregate outcomes are assumed to be based on the decisions of individuals. In an anticipation of the next section, Figure 3.2 will present the microfoundation of the main arguments.

Figure 3.2: Microfoundation of the argument



3.3.2. Interest Groups

Proponents of societal interests everywhere try to form interest groups with the aim of influencing policy. However, not all interests can be organised equally well. Following Mancur Olson (1971) I assume that special interests are organised asymmetrically. Large groups face problems with individual incentives to free ride on public goods produced by the group and therefore are at an organisational disadvantage to smaller groups, who overcome free riding through monitoring and deterring. Importantly, I assume special interests from the banking sector to be better equipped to overcome collective action problems than other latent interests. I therefore disregard interest groups other than the banking sector. Whatever influence such groups can mobilize, it will always cancel out against influence from the better organised banking sector.

Interest groups from the banking sector have a strong incentive to try to influence the strategy of crisis resolution. On the one hand, swift and thorough bank restructuring imposes a substantial burden on the sector. The Turkish example can help to illustrate this: from October 2000 to October 2001, the *Savings and Deposits Insurance Fund*[6] had to take over a total of 10 banks due to a combination of excessive insider transactions, weak capital, and substantial

[6] SDIF, the *Savings and Deposits Insurance Fund* is the Turkish government agency established in the aftermath of the 1994 crisis to step in in case of liquidity problems. It also was in charge of the restructuring efforts in the aftermath of the 2000/2001 twin crisis.

asset quality problems. Today, only 51 of the 71 banks registered in 2000 remain and three banks are under the trusteeship of the SDIF (Fitch Rating, 2004). Thus, thorough restructuring does not only involve tightening of regulation, but often also implies right-out closure of banks and forced mergers. Additionally, banking turmoil in the context of a government safety net can be quite profitable if bank owners engage in *economic looting* or abuse the system in any other way. On the other hand, banks have much to gain from a lenient approach to crisis resolution: government guarantees give them access to international lending at risk-free interest rates, thereby raising a banks interest margin. Lax regulation gives them the opportunity to engage in high risk-high yield projects; and recapitalisations, e.g. in the form of subsidised central bank credit give access to cheap credits.

Interest groups from the banking sector, thus, try to *buy* certain crisis resolution policies from policymakers within veto players on the *political market for special interest favourism.* As we have seen above, the *price* for a policy change depends on the number of veto players and the extent, to which these are constrained by having to win elections. What will determine the banking sector's willingness to *pay* for a change in policy? Of course, lobbyists will only be willing to pay as long as it pays off for them, i.e. until the contributions equal the expected gains from a change in economic policy. This amount, which is likely to be substantial in the first place, rises with the size of the banking sector as a larger banking sector indicates that the stakes are higher.

So far, we have established that democratic veto players can decrease the economic cost of banking crises, as they increase the cost of lobbying. However, the effect of the size of the banking sector is likely to pull in a different direction as it increases the willingness to pay in exchange for a policy change. If the willingness to pay rises sufficiently high, even a democratically constrained policymaker's reserve price will ultimately be met and he or she will divert from socially optimal policy. This effect will be even stronger in a system characterised by few veto players, because fewer veto players imply that less policy makers need to be *convinced* by contributions. Thus follows hypothesis H3:

H3: A low number of veto players decreases the cost of lobbying and will therefore increase the economic cost of banking crises if the banking sector is large.

However, the effect of the banking sector's size on the cost-benefit analysis of policymakers involved in the process of policymaking is anything but straightforward. While bigger banking sectors imply stronger interest groups and more means to influence policy, bigger banking sectors also imply that banking crises will feature more prominent on the agenda of the general public. This might in turn lead to more public discontentment and a higher risk of loosing office if too much lenience is applied in the process of banking crisis resolution. An opposite effect or the cancelling out of two opposing effects is therefore imaginable. This ambiguity will have to be scrutinized empirically.

3.4. Summary of the Argument

In this section I have argued that the efficiency of banking crisis resolution does not only depend on the speed of crisis resolution, but also on the quality of crisis resolution policies. More precisely, I have identified two classes of central actors, policymakers within veto players and special interest groups and have argued that the political system determines the extent to which it is profitable for these actors to give in to / engage in special interest politics. The extent of special interest politics in the politics of crisis resolution, then, will determine the economic cost of the banking crisis. Table 3.1 summarizes the main hypothesis, which I will use to test my theoretic framework in the next section.

Table 3.1: Summary of the Arguments

Hypothesis	Independent Variable	Expected effect on the economic cost of banking crises
H1	Number of Veto Players	+
H2	Number of Veto Players in a democratic context	-
H3	Number of Veto Players and Size of the Banking Sector	+

4. Empirical Analysis

In the last sections I have laid out, how the number of veto players is linked to the economic cost of banking crises. I have argued that the effect of veto players on the economic cost of banking crises is determined by incentives veto players face. Specifically, I argue that only democracies create an incentive structure that induces veto players to choose policy to minimize economic costs of banking crises. In statistical terms, this means that I hypothesize the effect of the independent variable on the dependent variable to be *moderated* by a third variable, in this case the level of democracy.

It is the purpose of this section to provide empirical evidence for the alleged relationship. To this end, I will employ a multivariate statistical regression analysis of the political determinants of crisis depth. The section is organised the following way: first, I will describe the sample used to test the hypothesis and give an overview of the data and its sources. Thirdly, I will test the hypothesis developed in chapter three. Fourth and finally, I will discuss the results.

4.1. The Sample

The sample used to test the hypothesis contains data on 47 systemic banking crises in 42 countries[7] in the period from 1980 to 2002. The time period is chosen because there were hardly any banking crises in the Bretton Woods era which ended 1973 (see: Bordo et al. 2001). While most prior studies have focused on a subgroup of countries, my sample contains developed and developing countries alike. This reflects my theoretical ambition to provide a global, intra-regime explanation of the political determinants of banking crises. Nevertheless, the sample is clearly dominated by what is commonly referred to as emerging market economies[8], simply because these countries were the ones experiencing the greatest number of banking crises since 1980. The majority of cases are from the Latin America / Caribbean region, the smallest subgroup is the Middle East / North Africa region. A strong caveat to this entire study is that case selection is not based on a random sample but is based mostly on data

[7] Several countries have experienced multiple banking crises within the time span at scrutiny here. These are: Argentina, Bolivia, Cameroon Mexico, the Philippines, Thailand and Turkey.
[8] As no formal definition of this term seems to exist, I take countries with intermediate values 2 or 3 on the World Bank income group scale to be emerging markets.

availability. Table 4.1 provides an overview over the data. A full list of crisis countries and other relevant variables is provided in Annex A.

Table 4.1 Summary Statistics

	Obs[1]	Avg. Duration in Years[2]	Avg. GDP loss (% of GDP)[2]	Std. Dev.	Min.	Max.
Total						
All Crises (Cumulated)	47	3.3	19.55235	20.34001	0	81.0246
Crisis associated with Output loss only (Cumulated)	35	4	26.25601	19.45624	1.0868	81.0246
All Crises Years	136	1	5.841628	5.159137	0	27.06728
Subsets						
Crises in OECD Countries (Cumulated)	5	3.8	15.38895	13.29933	0	31.75512
Crises in Emerging Market Economies (Cumulated)	30	3.5	21.86598	22.33406	0	81.0246
Crises in low income countries (Cumulated)	12	2.5	15.50302	17.61695	0	42.46001

[1] Data on banking crises is taken from Caprio and Klingebiel (1996)
[2] Based on authors calculations. Output loss is the size of "output-dip" calculated with a modified Hodrick-Prescott Filter. Further explanations in the text

The highest calculated output dip in the sample, 81 percent of cumulated GDP, corresponds to the banking crisis in Estonia in early 1993. It is somewhat of an outlier as the next highest calculated output dip, Romania 1990, "only" amounted to 61 percent of cumulated GDP. As sample size is small already, I leave the outlier in the sample for now. Later robustness test will show, however, that results do not change significantly if Estonia is excluded from the sample.

4.2. Data and Specification

This subsection will provide an overview over the data used in this study. Much of the data is freely available on the Internet. I start with the dependent variable and then successively introduce the independent variables and the controls.

4.2.1. The Dependent Variable

The dependent variable is the economic cost of banking crises. As argued above, these costs are difficult to identify and to isolate, as banking crises are never the only things happening in an economy at a given time. I want to approximate these economic costs through the amount of foregone GDP that accompanies a banking crisis. Precisely, then, it is defined as the difference between real GDP and trend GDP in the year of a banking crisis and every subsequent year until absolute GDP returns to its trend value. I prefer this measure over measures of the fiscal cost of banking crises because of its higher empiric reliability and its higher theoretical relevance. Two independent components are necessary to specify this dependent variable: banking crises and losses in GDP. These will be introduced in turn.

Banking Crises

Defining what constitutes a systemic banking crisis is difficult and error prone. Exclusive reliance on objective indicators is likely to be misleading (Frydl 1999). For the purpose of this study, I want to revert to Caprio and Klingebiel's dataset "Episodes of Systemic and Borderline Financial Crises" (2003) which mostly relies on expert interviews to identify and date 93 incidents on systemic banking crises from 1970 to 2002. According to their widely cited definition, a systemic banking crisis is a situation in which "the net worth of the banking system has been almost or entirely eliminated." (Caprio and Klingebiel, 1996, p.6).

I only take the year of onset of crises specified in the data set. Since data on banking crises is almost exclusively available on a yearly basis, and as other data is also not available more frequently, a more fine grained analysis is very difficult to realise. Nevertheless, such an effort would be very worthwhile from a conceptual point of view. Caprio and Klingebiels concept of duration of banking crises reflects different theoretical concepts and will not be

employed in this study[9]. Rather, the duration of a banking crisis is approximated as the duration of the recession that accompanies it.

Foregone GDP

I use GDP data on an annual basis taken from the 2002 version of the *World Development Indicators*, which is published by the World Bank (2002). Most recent data was added using the online version of the World Development Indicators.

To determine foregone GDP, it is necessary to calculate a potential GDP that would have possibly been reached in the absence of the crisis. A straightforward way of doing this is to simply extrapolate past growth on crisis years. However, this approach bears the risk of mistaking other long-term developments in the economy for consequences of a banking crisis. Hoggarth, Reis and Sapora (2001) cite the example of Japan in the 1990s, where long-term output potential fell parallel to the banking crisis for reasons unrelated thereto, such as demography. Nevertheless, such methodology has been employed, albeit with questionable results (Angkinand 2005). A more sophisticated approach is to use moving average procedures to detrend GDP data. One such procedure is the "band-pass-filter" developed by Baxter and King (1995) and used by Bordo et al. (2003). It calculates hypothetical output for one given year as an average of real GDP in a number of neighbouring years. The question then is how many years to include in the calculation of the average. Several studies have shown that banking crises often are preceded by economic booms (see: Kindleberger 2002; Hardy and Pazarbaşioglu 1998), so small windows would risk overstating the economic cost of crises. But large windows are costly in terms of observations as the edges of the sample will remain blank.

Studies employing this methodology not only differ with respect to the filter used, but also regarding what exactly the filter is applied to: *absolute* GDP or GDP g*rowth* data. Most recent studies have used GDP growth data (IMF, 1998; Bordo et al. 2001). However, Hoggarth Reis and Saporta (2001) point out that this approach will bias downwards the true cost of banking crises lasting longer than two years because the GDP growth rate might already return to its growth trend, while the absolute GDP level remains below its pre-crisis capacity.

[9] For reasons of comparison, the duration of banking crisis specified by Caprio and Klingebiel is however given in the complete list of crises in ANNEX I

In this study, I will use a variant of a detrending methodology developed by Robert Hodrick and Edward Prescott (1997) to clean absolute GDP-Data from business cycles. The original Hodrick Prescott Filter is a simple minimization of the following function:

$$Min(\tau_t)\left\{\sum_{t=1}^{T}(y_t-\tau_t)^2+\lambda_1[(\tau_{t+1}-\tau_t)-(\tau_t-\tau_{t-1})]^2\right\}$$

The first term is the sum of the squared deviations $d_t = y_t - \tau_t$. The second term "penalizes" variations in the growth rate of the trend component. λ is the so called *smooth parameter*, i.e. the larger λ the *smoother* the calculated trend curve. Hodrick and Prescott recommend a smooth parameter of 1600 for quarterly data and 100 for yearly data. However, as the calculation of my dependent variable critically depends on this parameter, I use several configurations, which will be discussed below. Again, a caveat is that by only including the direct neighbours of a given year into the calculation of its GDP trend value risks overstating the economic cost of crises, because these tend to be preceded by economic booms. In my analysis I therefore use a variant of the filter, which also includes the second neighbours, i.e. the neighbours of the right and left neighbour of the year questioned in the calculation. The formula finally used to detrend GDP data is therefore the following:

$$Min(\tau_t)\left\{\sum_{t=1}^{T}(y_t-\tau_t)^2+\lambda_1[(\tau_{t+1}-\tau_t)-(\tau_t-\tau_{t-1})]^2+\lambda_2[(\tau_{t+2}-\tau_t)-(\tau_t-\tau_{t-2})]^2\right\}$$

Other variants of the Hodrick Prescott Filter have been used before to compare trend GDP with real GDP in the analysis of the economic cost of banking crises: IMF (1998) and Hoggarth, Reis and Saporta (2001) both employ filter in one way or the other. Unfortunately neither one of the articles gives any indication on what smooth parameter has been used. To give the reader a better impression of the effect of different filter methodologies, Figures 4.1a and 4.1b graphically compare several specifications. In both figures, *smooth* refers to the abovementioned smooth parameter for h-p-filtering and *smooth 100 / 10* indicates that a second neighbour was included into the calculation of detrended values. B-K-Filter indicates that Baxter and King's "band-pass-filter" has been applied to detrend GDP-Data.

Figure 4.1a: GDP and Detrended GDP for South Korea

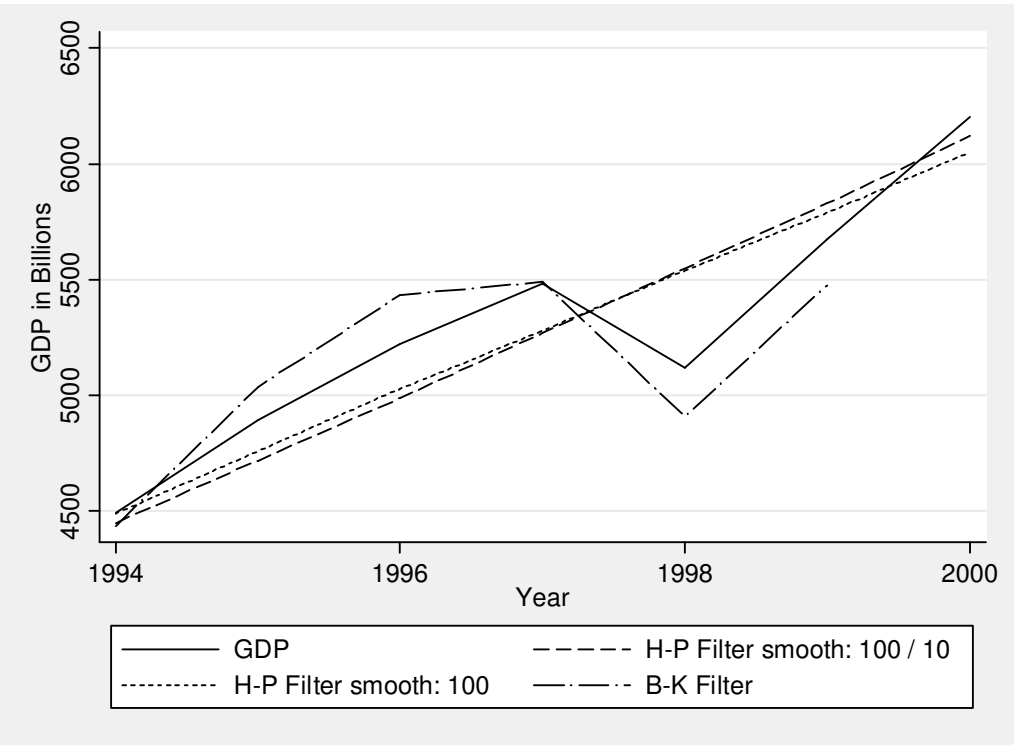
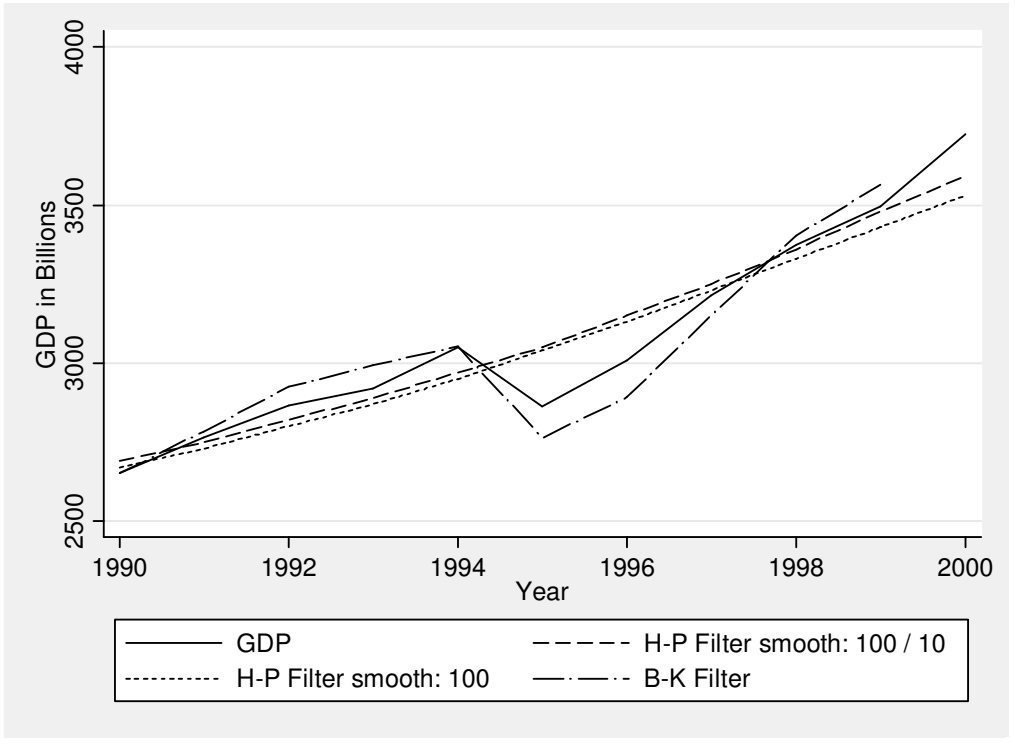


Figure 4.1b: GDP and Detrended GDP for Mexico



Three interesting observations can be made from the figures: First, the inclusion of a second neighbour into the calculation only slightly changes the resulting detrended curve. This is due to the low smooth parameter with which the second year enters the calculation. Second, the results obtained using the moving average procedure developed by Baxter and King are radically different from the results obtained using the Hodrick-Prescott methodology. Trend values obtained with Baxter and King are very sensitive to sudden slumps of GDP, a property that leads to trend values below actual values in economic downturns. Third and finally, the figures lend support to the idea that banking crises are often preceded by economic booms.

The variable I use in my analysis is calculated on the basis of a Hodrick-Prescott filter with two neighbours on each side of each year which enter the calculations with smooth parameters set at 100 and 10 respectively[10]. Only output dips that begin simultaneously with a banking crisis as given by Caprio and Klingebiel (2002) or in the following year are included into the analysis. Other, regular output dips, i.e. recessions are disregarded. Ultimately, I use the logarithm of the sum of the output dips, starting from the year of crisis onset defined by Caprio and Klingebiel until actual GDP returns to trend GDP, expressed as a percentage of total GDP. According to this specification, about a quarter of all banking crises in my sample (12 out of 49) were not accompanied by a recession. Summary statistics can be found in Table 3.1.

One issue that still need to be addressed before turning to other variables is the question of causality. Can the differences between trend GDP and actual GDP be interpreted as economic cost *caused* by banking crises? As a first approximation to this issue, I employ a two sample t-test to see whether recessions concurrent to banking crises systematically differ from recessions without banking crises. As my final sample only includes recessions that are concurrent to banking crises, I revert to a wider dataset including GDP data for 153 countries in 1980 to 2002[11]. Objects of analysis are all years in which actual GDP was below trend GDP. Recessions are expressed as deviations of actual output from trend output as a percentage of total GDP. Results are reported in Table 4.2

[10] This choice was made based on the standard deviations and kurtosis of the resulting distributions of output dips See Annex V for more information

[11] A complete list of all countries included in this sample is given in Annex VI

Table 4.2: Difference between recessions in years with and without banking crisis

Group	Obs	Mean (% of GDP)	Std. Err.	Std. Dev.	95% Conf. Interval	
Without Crisis	1263	5.336463	.1815233	6.451103	4.980342	5.692583
With Crisis	327	7.415804	.5531053	10.00188	6.327697	8.50391
Combined	1590	5.764101	.1847708	7.367698	5.401681	6.126521
Diff		-2.079341	.4543031		-2.970438	-1.188244

Degrees of freedom: 1588 Ho: mean(0) - mean(1) = diff = 0

Ha: diff < 0	Ha: diff != 0	Ha: diff > 0
t = -4.5770	t = -4.5770	t = -4.5770
P < t = 0.0000	P > |t| = 0.0000	P > t = 1.0000

The result from Table 3.2 is straightforward. Recessions in years with a banking crisis are roughly 40 percent more severe than recessions in years without concurrent banking crisis. Of course, this does not imply anything about causation, as more severe output dips during banking crises might simply be due to a higher probability of bank failure during severe recessions. Several authors have given substantial attention to the issue of causality: Hogarth, Reis and Saporta (2001) employ a matched-pairs comparison of crises countries with benchmark neighbouring countries that did not experience banking sector problems at the same time. The results indicate that recessions cum banking crises are significantly deeper than non-banking crises recessions, adding at least another 10-15 percent to the depth of the episode. Bordo et al. (2001) employ a two stage least squares methodology with lagged independent variables to eliminate the possibility of reverse causation. Their results imply an extra 14 percent points of lost GDP growth due to banking crises and the authors decisively reject the assertion that the correlation is due only to the effect of recessions on crises. I take this evidence as conclusive and will not repeat their tests here.

4.2.2. Independent Variables

This next section will give an overview of the independent variables used in the data analysis. Special attention will be given to their specification.

Veto Players

Data on veto players is taken from the 'Database on Political Institutions' by Beck et al. (2001). Newer data was obtained from the updated version of the dataset available on the World Bank website. I use the indicator 'checks' which counts "the number of veto players in a political system, adjusting for whether these veto player are independent of each other, are determined by the level of electoral competitiveness in a system, their respective party affiliations, and the electoral rules" (p.170). This specification is convenient for me, as it only counts the *number* of veto players and does not as much take into account the ideological distances between them. Thus, a higher number indicates a higher number of veto players and a more constrained executive.

In my dataset, 'checks' takes on values between 1 and 6, the mean is at 2.7, median at 3. As I use interaction terms in my analysis, I centre 'checks' by subtracting the mean from every observation, to mitigate disturbances caused by collinearity and to facilitate a meaningful interpretation (Kohler and Kreuter 2001). The resulting variable is called *ctdchecks*

Level of Democracy

Data on the level of democracy is taken from the 'Polity' dataset originally compiled by Keith Jaggers and Ted Gurr. I use the most recent version of the dataset Polity IV (Marshall and Jaggers 2002) which includes data up until 2003. The variable 'POLITY' is the sum of the values on two indexes, 'DEMOC' which measures the level of democracy ascending from 0 to 10; and 'AUTOC', which measures the level of autocracy again ascending from 0 to 10. The value of 'AUTOC' is subtracted from the value of 'DEMOC' to obtain 'POLITY', which therefore ranges from -10 for absolute autocracies to 10 for absolute democracies, including most OECD countries.

In my dataset, I add 10 to 'POLITY' in order to eliminate negative values, thus giving a linear index from 0 to 20. The minimum value in my sample is 1, the maximum is 20, mean is at 12.5, median at 16. Again I use centred values for the analysis to mitigate disturbances caused by collinearity and facilitate a meaningful interpretation. I name the resulting variable *ctdpolity2*. I use this variable to create the interaction term distinguishing between democratic and non-democratic veto players.

Size of the Banking Sector

Data on the size of the financial sector is taken from Thorsten Beck, Asli Demirgüç-Kunt and Ross Levine (1999), which covers the years from 1960 to 2001. Data for 2002 was – where necessary – imputed from previous data[12]. I use their indicator 'Deposit Money Bank Assets to total financial assets', which equals the ratio of deposit money banks assets and the sum of deposit money and central bank assets. This data series has been employed to measure the importance of the banking sector by other authors (e.g. King and Levine, 1993) and my reasoning is the same as theirs: a high amount of deposit money in an economy shows a) a wide availability of banking services and b) that the financial system is widely trusted among private and institutional actors. Both notions approach the idea of a well developed financial system.

In my dataset, the variable takes on ratios ranging from 0.21 to 0.99 with a mean and median both of 0.75. However, I multiply the variable by 10 and centre it to combat collinearity so that final values range from -5.38 to 2.36. Higher values of this variable reflect a bigger banking sector. The resulting variable is called *ctdfindev.* I use this variable to create the interaction term that distinguishes between veto players in the context of a large and a small banking sector.

Interaction Term 1: Veto Players x Democracy

As set forth in the theoretical section, I hypothesise that veto players will have a different effect on the economic cost of banking crises that their non-democratic counterparts (Hypothesis H2). This variable is meant to capture this effect. It is the product of the variables *ctdchecks* and *ctdpolity2*, the sources of which I stated in the respective sub-sections.

The resulting variable, *checks_polity,* takes on values ranging from -6.384 to 26.464 - mean and median at 7.219 and 7.616 – with higher values indicating higher numbers of veto players and/or higher level of democracy.

Interaction Term 2: Veto Players x Size of the Banking Sector

My hypothesis H3 states that the positive direct effect of a high number of veto players will be stronger the bigger the banking sector. This interaction term is meant to capture this effect. To create the interaction variable *invchecks_findev* I use *ctdfindev* and the inverse of *ctdchecks,* in order to obtain unambiguous results. In my sample, *invchecks_findev* takes on values ranging from -10.779 to 9.419 with median 0 and mean 0.205.

[12] However, this was only neccessary in the case of Turkey 2002

4.2.3. Controls

In this section I will introduce the independent variables that will be used to control for other factors that influence the economic cost of banking crises. These controls will reflect two major categories: macroeconomic factors and microeconomic factors.

Economic Growth

The idea that economic growth would influence the economic cost of banking crises is straightforward: if a country is in a phase of robust economic growth, it will be better equipped to absorb the economic shock caused by the banking crisis. The liquidity shortages, which accompany banking crises, will be harder on companies that had been in economic difficulties in the first place. Additionally, my measure of the economic costs of banking crises is based on GDP data. It would be surprising, if this would not translate into a relationship with GDP growth. Data on GDP growth are taken from the 2002 version of the *World Development Indicators* (World Bank, 2002). They enter the regression analysis unchanged.

Creditgrowth

Rapid credit expansions can fuel asset price bubbles. Domaç and Martinez Peria (2000) have found high rates of credit growth to be an important determinant of the duration of crises. Therefore, I take this concept as a proxy for the size of the boom preceding a crisis. Data on the volume of private credit is taken from the online version of the *International Financial Statistics* published by the IMF (2005). I take the variable 'DOMESTIC CREDIT' to express the annual percentage change in credit volume. As the asset boom supposedly lies in the past, I use the average percentage change of the three years, which precede the first year of crisis to form the variable av3_*creditgrowth*.

Currency Crises

Banking crises are often accompanied by currency crises. While it remains unclear whether the former cause the latter or vice versa, most economists agree that the two taken together are much more damaging to an economy than either of two on their own (see: Kaminsky and Reinhard, 1999; Bordo et al. 2001). I include a dummy taking on the value 1 if a banking crisis is accompanied by a currency crisis. Data on currency crises is compiled from Kaminsky (2003) and Hoggarth, Reis and Saporta (2001). Out of the 47 banking crises in my

sample, a total of 18 were accompanied by currency crises. However, a simple two sample t-test reveals that in my dataset the economic cost of twin crises do not significantly differ from the value of banking crisis. This result will be discussed further down.

Intuitively, another control variable would be an obvious inclusion: the duration of the crisis. After all, it could simply be, that the economic cost of banking crises are determined by their length – even more so as my dependent variable follows a strictly additive logic. But the effect of the duration on the depth of banking crisis is not obvious. Theories on the political economy of economic change have shown that a bigger crisis can actually accelerate economic reform (Rodrik 1994), an effect that would, in turn, lead to *shorter* crises being associated with higher economic cost of crises. In any case, the duration of banking crises is anything but exogenous to the depth of crises and the inclusion of the duration as a control would probably lead to misleading results. However, I will tentatively explore this relationship by presenting a model that includes the duration of crises in Annex IV.

4.2.4. Overview of Variables

Table 4.3 will give an overview of the dependent variable as well as the independent variables and their expected effects on the depth of banking crises.

Table 4.3 Overview over the Variables

	Concept	Specification[1]	Expected Effect on the depth of BC's
DV	Depth of Banking Crisis	$\frac{\ln\left(\sum recessiondepth \mid bankingcrisis\right)}{TotalGDP} x100$	
H1	Veto Players (Benchmark)	*ctdchecks*	+
H2	Democratically Constrained Veto Players	*Ctdchecks x ctdpolity2*	-
H3	Size of Banking Sector Influence	inv*(ctdchecks) x ctdfindev10*	+
C1	Economic Growth	ΔGDP	-
C2	Creditgrowth	$L1(\Delta credit) + l2(\Delta credit) + l3(\Delta credit) / 3$	+
C3	Currency Crisis	*curr_crisis* (0/1; no crisis/crisis)	+

[1]See text for further information on the specifications

4.3. Testing of the Hypotheses

In this section I will test the hypotheses developed in the theoretical section. I start with a note on interaction. Then I will discuss my choice of methodology, then I will test the three hypotheses and finally I will present and discuss the results. All calculations presented here are calculated using the 8.2 version of the statistical package 'Stata'.

4.3.1. Modelling Interaction

As my main hypotheses predict an interaction effect of two independent variables on my outcome variable, it is important to clarify the concept of interaction before developing a model and interpreting its results. Interaction effects can be conceptualised in many ways. Most social scientists conceptualize them as what is commonly referred to as the *moderator* approach, albeit without explicitly stating this. In this approach, the researcher designates a *focal independent variable* (x) and a *moderator variable* (z). This latter variable is said to *moderate* the impact that the former exerts on the dependent variable (y), i.e. the researcher hypothesises that the impact of x on y changes with different values of z (Cohen and Cohen, 1983). Applied to the case of this analysis, I designate the number of veto players as focal independent variable and designate the level of democracy (H2), respectively the size of the banking sector (H3) as moderator variables.

Regular regression analysis follows a strict additive logic, i.e. the independent variables are treated as being independent of each other and their respective coefficients as being additive. The most common approach to the modelling of interaction effects, recommended by Cohen and Cohen (1983) departs from this additive logic by calculating a product term xz and introducing it into the regression equation, which then takes on the following form:

$$Y = \alpha + \beta_1(x) + \beta_2(z) + \beta_3(xz) + \varepsilon,$$

where α is the intercept, β_i is the effect of the underlying variables x, z and xz and ε is the error term reflected in the residuals. This introduction permits to test for the presence of a moderated relationship between x and y. In principal, moderated effects between x and y can take on a large variety of forms. However, the modelling via a product term only tests for the presence of what Jaccard and Turrisi (2003) call a *bilinear interaction* (p.21), i.e. an

interaction in which the slope between y and x changes a s a linear function of the values of z. Other forms of interaction relationships, e.g. a nonlinear interaction with a quadratic term, are not tested for and will not be detected by the introduction of a product term.

Interpreting Coefficients in Interaction Terms

In regular regression analysis, regression coefficients are interpreted as change in y for a one unit change in x, holding constant all other variables (Kohler and Kreuter, 2001). However, this interpretation is no longer correct in the presence of interaction terms. Once such terms are present, the coefficients of x and z reflect *conditional* relationships for specific values of z and x. The coefficient for x, β_1, reflects the influence of x on y *when z equals zero*. Conversely, the coefficient for z, β_2, reflects the influence of z on y *when x equals zero* (Jaccard and Turrisi, 2003). The practical interpretation of these coefficients can be difficult if variables naturally do not take on the value zero. As this was the case with my data, I centred data on its mean. In my analysis, coefficients β_1 and β_2 can therefore be interpreted as reflecting the influence of x, respectively z on y, when z, respectively x is held constant at mean levels.

In my analysis, special attention will have to be paid to the interpretation of β_3, the coefficient for the product term xz, as my theory makes a prediction on its value. The coefficient of a product term can be interpreted as "the number of units that the *slope* of y on x changes given a one unit increase in z" (Jaccard and Turrisi, 2003, p.22, emphasis added). Applied to my analysis, in which *polity2* has been designated as moderator variable and *checks* has been designated as focal independent variable, this means that the coefficient of *checks_polity2* indicates the value by which the slope between *checks* and the dependent variable changes, given a one unit increase in *polity2*.

4.3.2. Choice of the Model

Pooled time series cross-section data analysis has become so widely used that "it is no exaggeration to say that it has become difficult to defend the decision not to use panel data" (Kittel and Winner, 2002, p.5). Yet, the political determinants of the depth of banking crises do not particularly lend themselves to pooled data analysis for two reasons. First, it is questionable, whether the singular years of a banking crisis cum recession are theoretically meaningful. After all, my argument puts forth a relationship between the political system and the ability to resolve a banking crisis efficiently, and not between the political system and the

ability to minimise economic cost in any given year. The distribution of these costs among crisis years is not my object of analysis and probably reflects other factors than the political system.

The second reason is methodological. As economic costs of singular banking crises years show a high degree of autocorrelation, only methodologies that control for this problem, such as a *fixed effects-model* including the lagged dependent variable and calculated using robust standard errors would be appropriate (Beck and Katz 1995). But several authors have shown that such approach is problematic under two conditions, both of which are met in my analysis (Plümper, Troeger and Manow, forthcoming). First, if independent variables are time-invariant – such as in my analysis the number of veto players, which varies only very slowly over time – the fixed-effects methodology will introduce a bias and leave very little variance for other the explanatory variables. Second, "if the theory at test predicts an influence of the level of an exogenous variable on changes in the endogenous variable…" – such as in my case the effect of the level of democracy on the change in the depth of the banking crisis – "units fixed effects *must* not be included" (Plümper, Troeger and Manow, forthcoming, p.32, emphasis in the original). An additional reason speaking against the utilisation of the fixed-effects methodology is that regression diagnostics indicate that non-stationarity, i.e. different probability distributions of a variable due to a time trend (Wooldridge 2003) might be a problem in my dataset[13]. As pointed out by Achen (2000, p.24): "when serial correlation is high and the exogenous variables are heavily trended […] the lagged variable will falsely dominate the regression and suppress the legitimate effects of the other variables".

Given these reasons, I choose a simple cross-sectional approach to test my hypothesis. My data has an interval structure, so that multivariate Ordinary Least Squares regression is the baseline model. However, in my case this approach is suboptimal due to censoring of the dependent variable. My data is left censored at a known, fixed point (zero) because output dips, by definition, cannot be negative. Thus, I need to choose an appropriate model that alters the likelihood function to make it a product of the latent dependent variable. Hamilton (2003) suggests a tobit model for the analysis of a fixed point-censored dependent variable. This is also the model that has been utilised by most prior research on the depth of banking crises

[13] Especially, the Durbin Watson statistic remains close to 1 after the inclusion of the lagged dependent variable in some specifications. See Annex IV for further details

(e.g. Bordo et al 2001; Angkinand, 2005). But, the tobit methodology has the important drawback of not allowing for the calculation of robust standard errors. Normal sandwich standard errors are problematic, though, because of heteroskedasticity. Furthermore, several countries in my sample have experienced more than one banking crisis during the period of analysis – it would be reasonable to use standard errors clustered on countries to take account of this lack of independence. Therefore, I follow UCLA (n.d.) and make use of the *intreg* command, which does allow for robust standard errors. This model was actually developed to analyse ranges of a variable where precise values are unknown. To make it work as a tobit model with clustered standard errors, a data transformation of the following form is required: the dependent variable is duplicated, *depth1* and *depth2*, and *depth1* is set to missing in every case of left censoring[14]. *depth1* and *depth2* are then jointly used as dependent variable. In any case, I will also include an OLS model to substantiate the results.

Given these considerations, I fit an *intreg* model of the following form on my data to test my hypotheses:

$$Y\,[y_1, y_2] = \alpha + \beta_1(x_1) + \beta_2(z_1) + \beta_3\,(x_1{*}z_1) \ldots \beta_k\,(x_k) + \varepsilon,$$

where Y is the true dependent variable, α is the constant or intercept, the β's are the regression coefficients of the corresponding x (independent/control) terms, z are the moderator variables in the interaction term and ε is the error term reflected in the residuals.

[14] The variable *depth2* would be set to 'missing' in the case of right censoring. But there are no cases of right censoring in my analysis

4.3.3. Regression Results

I will first establish the effects of the controls and the benchmark hypothesis H1. Table 4.4 provides the results of this effort. I will then turn to the results of the tests of hypotheses H2 and H3.

Table 4.4 Regression Results: The Benchmark Model

	Model 1 (tobit)	Model 2 (tobit)	Model 3 (OLS)
constant	1.738649 (.3613438)****	1.78519 (.3868677)****	2.093765 (.2727432)****
gdpgrowth	-.1532663 (.0513839)***	-.1374026 (.0500288)***	-.1103301 (.0401455)***
av3_creditgrowth	.0005166 (.0002201)**	.0005402 (.0001892)**	.0004563 (.0001613)***
curr_crisis	.6003375 (.5365658)	-	-
ctdchecks	-	.2212337 (.2018911)	.1425261 (.1586929)
n / (left censored)	47 / (12)	47 / (12)	47 / (12)
pseudo-r^2	0.142	0.144	0.1754
Wald χ^2	21.79****	22.18****	-
Log pseudo-likelihood	-82.716385	-82.524505	-
F-Statistic	-	-	10.30****

Notes: Dependent variable: log cumulated output dip of banking crisis; standard errors adjusted for clustering on *countryid* are reported in parentheses; significance levels: *at 0.10 level, **at 0.05 level, ***at 0.01 level, ****at 0.001 level,

The results reported in the above table are mixed. On the one hand side, all models presented in Table 4.4 are highly significant. The relatively small modelfit-statistics indicate that the specification is suboptimal, yet one has to bear in mind that the t-test presented in Table 4.2 has revealed, that only roughly 40 percent of recession depth-variance can be explained by banking crises. Very high values of r^2 would therefore be suspicious. The variable *gdpgrowth* has the expected negative sign and is highly significant in all three specifications. Thus, the idea that economies in phases of positive growth can more easily absorb the shock caused by a banking crisis finds confirmation in the data. Analogous, the coefficient of the *av3_creditgrowth* variable has the expected positive sign and is robustly significant. What is somewhat surprising is the low coefficient of *av3_creditgrowth*, which is only partially explained by the logarithmic transformation of the dependent variable. If we re-transform the normalized beta coefficient of Model 3, we find that the depth of a banking

crisis increases by 1.14 percent of GDP for a one standard deviation increase in *av3_creditgrowth* – surprisingly little, given that other researchers have used the concept to indicate the size of the macroeconomic misalignment in the run-up to the crisis. On the other hand, the results for the variable *curr_crisis* are disappointing. While the coefficient does have the expected positive sign, the effect is far from significant with a t-value of 0.73. This is not surprising bearing in mind that the t-test performed earlier failed to show a significant difference between the economic cost of banking crises and the economic cost twin crises, but it is surprising given the strong positive effect other researchers have found (e.g. Bordo et al. 2001). I will discuss this result at a later stage. The results for *ctdchecks* are equally ambiguous. The coefficient does have the predicted positive sign, but again the effect is far from significant with a t-value of 0.83 in Model 2. However, this result can easily be explained within my theory: if democratically constrained veto players exert a different effect on the depth of banking crises than their non-democratic counterparts, these effects should empirically cancel out in a regression not at all controlling for the level of democracy. So far, I can neither accept, nor clearly reject my benchmark hypothesis H1.

The control for the level of democracy, *ctdchecks,* is introduced into the regression in Model 4, presented in Table 3.5 on the next page. The coefficient for *ctdchecks* preserves its positive sign but still lacks sufficient significance to confirm my hypothesis H1. *gdpgrowth, av3_credictgrowth* and *curr_crisis* all preserve signs and niveau of significance. The regression coefficient of *ctdpolity2* enters positively into the regression, suggesting that a higher level of democracy adds to the depth of a banking crisis. But the coefficient is not significant, either. Additionally, r^2 has not increased significantly, indicating that the inclusion of the level of democracy has not added to model specification.

Table 4.5 Regression Results: The Interaction Effects.

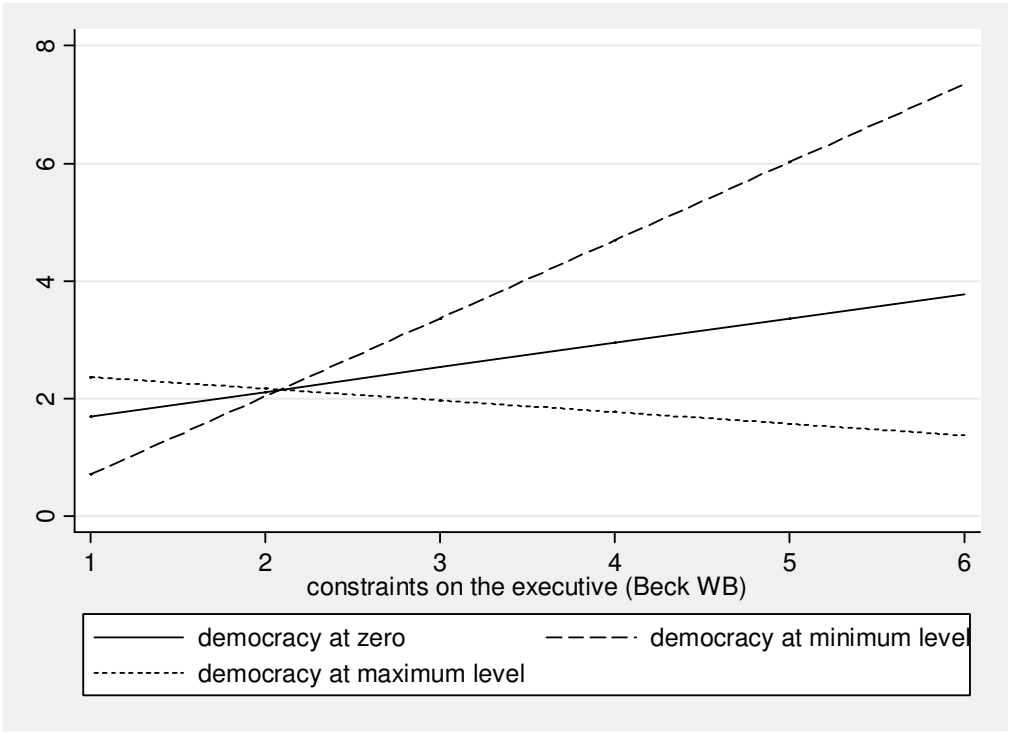
	Model 4 (tobit)	Model 5 (tobit)	Model 6 (tobit)	Model 7 (tobit)	Model 8 (tobit)	Model 9 (OLS)
constant	1.688196	2.283154	1.66308	1.585868	2.232899	2.401232
	(.4224856)****	(.4292676)****	(.40019)****	(.3845743)****	(.4199301)****	(.3866312)****
gdpgrowth	-.1453247	-.1922284	-.1453849	-.1385965	-.1861119	-.1474275
	(.0516736)***	(.0561311)***	(.0553296)***	(.0559147)**	(.0598681)***	(.049862)***
av3_creditgrowth	.0005054	.0002828	.0004838	.0007086	.0005306	.0004772
	(.0002351)**	(.0002117)	(.0002447)**	(.0002137)***	(.0002481)**	(.0002371)*
curr_crisis	.4215379	.7719629	.4446921	.5053586	.8548983	.61073
	(.6004591)	(.6035067)	(.5808511)	(.5956427)	(.6030913)	(.5001052)
ctdchecks	.1504847	.4786627	.1806166	.1562638	.4421489	.3188755
	(.2719111)	(.2413098)***	(.2191363)	(.2035382)	(.21831)**	(.1899381)*
ctdpolity2	.0099568	-.0289809	-.0172128	-	-.022179	-.0242628
	(.0535008)	(.0494028)	(.136839)		(.0483475)	(.0418241)
ctdfindev10	-	-	-.0172128	.0172419	.0410114	.0526736
			(.136839)	(.1230653)	(.1190929)	(.1017251)
checks_polity2	-	-.1207105	-	-	-.1260168	-.0894587
		(.0350179)***			(.0334447)****	(.0290499)***
invchecks_findev	-	-	-	-.1588879	-.1784265	-.1336637
				(.0791019)**	(.0810545)**	(.0644223)**
n / (left censored)	47 / (12)	47 / (12)	47 / (12)	47 / (12)	47 / (12)	47 / (12)
pseudo-r^2	0.151	0.209	0.152	0.186	0.240	0.3335
Wald χ^2	21.23****	26.85****	20.21***	26.92****	35.90****	-
Log pseudo-likelihood	-82.257687	-78.915432	-82.267325	-80.951006	-77.028812	-
F-Statistic	-	-	-	-	-	5.62****

Notes: Dependent variable: log cumulated output dip of banking crisis; standard errors adjusted for clustering on *countryid* are reported in parentheses; significance levels: *at 0.10 level, **at 0.05 level, ***at 0.01 level, ****at 0.001 level; r^2 in tobit models is McKelvey and Zavoina's r^2 obtained through Stata's *fitstat* command

Results on H2: Veto Players and Democracy

Table 3.5 presents the results on the regressions, which include the product terms *checks_polity2* and *invchecks_findev*. Model 5 introduces the product term *checks_polity2*, which tests for the presence of an interaction effect between the number of veto players and the level of democracy. The high significance of the term strongly suggests the presence of such a moderated effect. The corresponding coefficient enters the regression negatively. Recalling the interpretation suggested by Jaccard and Turrisi (2003) the coefficient suggests that for a one unit increase in the level of democracy the slope between the number of veto players and the dependent variable changes by -.1207. Thus, I find empirically confirmed my hypothesis H2, stating that democratically constrained veto players will have a different effect on the depth of crises than non-democratic veto players. This moderated effect is graphically demonstrated in a conditional effects plot in Figure 4.2 in which the level of democracy is held constant at minimum- maximum- and zero level and only the number of veto players is allowed to vary.

Figure 4.2: Effect of *ctdchecks* conditional on the level of democracy



It can be clearly observed in Figure 4.2 that the effect of the number of veto players on the depth of banking crises changes with the level of democracy. In countries with a high

level of democracy, more veto players help to mitigate the cost of crises, whereas more veto players add to the cost of banking crises in countries characterised by a low level of democracy. If no such interaction effect was present, the three lines would be parallel (Kohler and Kreuter, 2001) The fact that the three lines in Figure 4.2 intersect in one point qualifies the interaction as *disordinal* interaction, i.e. as an interaction that cannot simply be due to the metric of the dependent variable (Jaccard and Turrisi, 2003). Additionally, the increase in the value of r^2 from Model 4 to Model 5 shows that the inclusion of the interaction term is a meaningful extension of the model.

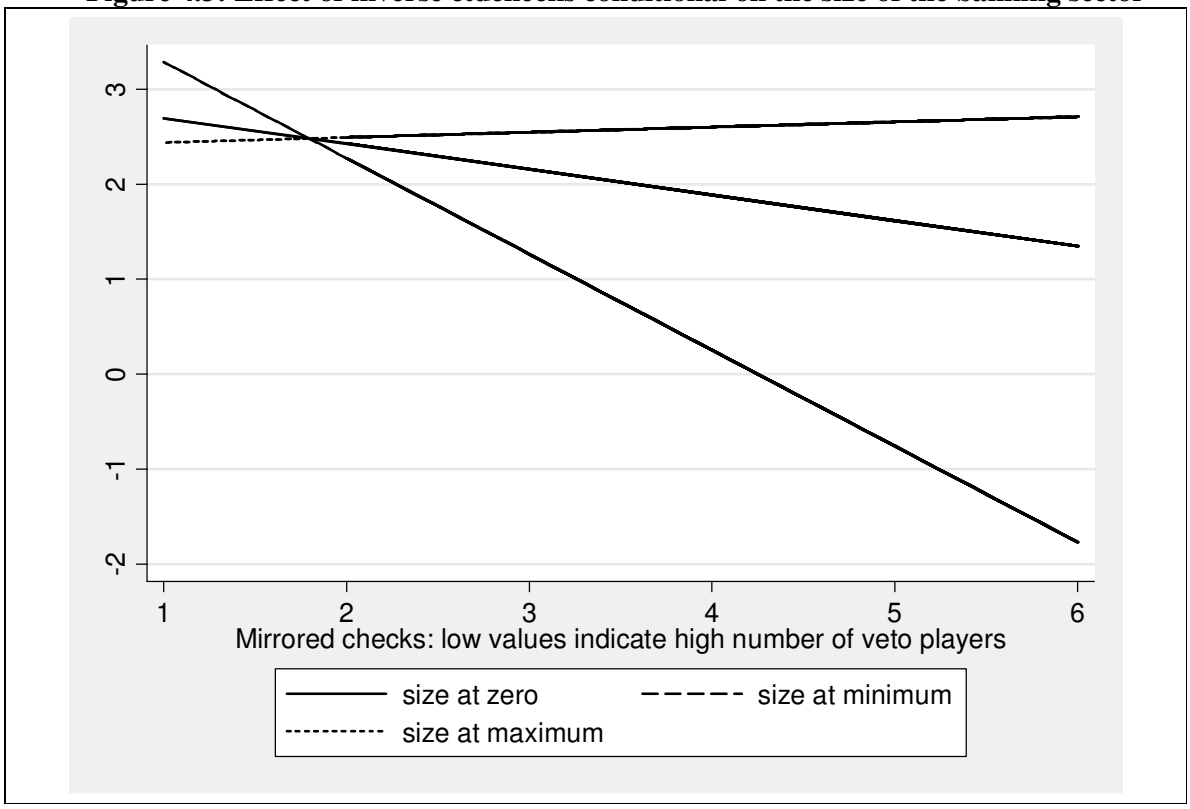
The introduction of the interaction effect *checks_polity2* has also changed the niveau of significance of the variable *ctdchecks,* which now is significant for the first time. This change is due to the fact that the coefficient now reflects a *conditional* relationship whereas it reflected more of a general relationship in the strictly additive models (see section 4.3.1). This conditional relationship is illustrated by the solid line in Figure 4.2, which reflects the relationship between the number of veto players and the economic cost of banking crises with democracy held constant at zero. Of course, the change in the significance also raises suspicion about the collinearity among the variables, a problem often encountered in the presence of interaction variables. However, the mean variance inflation factor (VIF) of the model is at 1.61 – well below the critical value of 10 suggested by Neter et al. (1985). In any case, it should be noted, that only perfect collinearity violates the regression assumptions (Friedrich 1982). Collinearity will remain a subject of concern, as critics speculate that the increase in collinearity impacts the quality of the parameter estimate of the effect of the independent variables on the dependent variable by increasing the covariance and variances of regression coefficients.

Results on H3: Veto Players and the Size of the Banking Sector

Models 6 and 7 subsequently introduce *ctdfindev10*, the measure for the size of the banking sector and *invchecks_findev,* the interaction term reflecting the hypothesized moderated relationship between the number of veto players and the economic cost of banking crises. The coefficient for *invchecks_findev* in Model 7 is significant at the 5 percent level, again confirming the presence of an interaction effect. However, the coefficient enters with the wrong, negative sign, implying that the slope between the *inverse* number of veto players and the economic cost of banking crises changes by -.1588 for a one unit increase in the size of the banking sector. To put it differently: given a big banking sector, an increase in the

number of veto players will *increase* the economic cost of banking crises. Given a small banking sector, an increase in the number of veto players will *decrease* the economic cost of banking crises – a clear contradiction to my hypothesis H3. Again, the moderated effect is graphically demonstrated in a conditional effects plot in Figure 4.3 in which the size of the banking sector is held constant at minimum- maximum- and zero level and only the number of veto players is allowed to vary.

Figure 4.3: Effect of inverse ctdchecks conditional on the size of the banking sector



As above, the introduction of the interaction term *invchecks_findev* into the regression has also affected the coefficient for *ctdfindev10*. In this case, *ctdfindev10* has even changed its sign from negative to positive. Again, this is less an indication of collinearity (mean VIF for Model 7 is 4.08, the condition number takes on the unsatisfactory, but acceptable value of 6.7619[15]) than an expression of the underlying *conditional* relation[16]. Analogous to the above case, *ctdfindev10* in Model 7 reflects the influence of the size of the banking sector on the dependent variable if *inv_checks* is held at zero. But since the coefficient lacks significance

[15] Calculated, using the default oprions of Stata's 'collin' command, i.e. with uncentred data.
[16] Of course, it also matters that *inv_checks* rather than *checks* is used in Model 7.

and as my theory is agnostic about the direct effect of the banking sector's size, I will disregard this finding.

Model 8 then presents the full model. Both interaction effects and *ctdchecks* are highly significant, *checks_polity2* even at the 0.1 percent level. Thus, the findings show substantial robustness to the inclusion of other variables[17]. The modelfit also increases drastically from Model 6 to Model 8, suggesting that the inclusion of the product terms strongly adds to the explanatory power of the models. These results are further substantiated in Model 9, a regular OLS model. Here, modelfit even increases to .33. Given, that only 40 percent of the output dip can be associated with banking crises, this is a remarkable value.

4.4. Discussion of the Results

Overall, three regression results stick out as remarkable. First and most importantly, the strength of the results on hypothesis H2 is surprising. The interaction term *checks_polity2* is highly significant in every regression. As demonstrated in Figure 4.2, a high level of democracy not only mitigates a negative effect of a high number of veto players, but actually reverses the impact of the number of veto players. This finding contradicts conventional theorising on a general effect of the number of veto players on the feasibility of policy reform. Democratic veto players should not simply be equated to non-democratic veto players. While veto players in general will always slow down the process of policy making, democratic veto players more than offset this effect by exerting a positive effect on the quality of policy making, as they help to limit special interest group influence. This finding echoes the results presented by Keefer (2001), which were surveyed in the literature chapter. Similarly Keefer argues that a higher number of veto players can mitigate special interest influence under certain circumstances, but he does not explicitly address the moderator effect of the level of democracy. Further, Keefer uses the amount of fiscal transfers as his outcome variable. My findings go further and show that the effect holds true for what I deem the more relevant measurement of the depth of crises: their economic cost.

The second remarkable result is the finding on H3: Given a strong banking sector, a high number of veto players reduces the economic cost of banking crises, whereas a high number of veto players in the context of a small banking sector adds to the cost. This finding clearly

[17] The findings are further substantiated through the results of a regression analysis in a TSCS-Dataset presented in Annex IV.

contradicts my theorising on the influence of the banking sector on veto players' incentives. But, as insinuated earlier, the size of the banking sector is only a very crude measure for the benefits policymakers obtain in exchange for handing out favours to special interest groups. Conversely, bigger banking sectors also imply higher cost in terms of voter dissatisfaction in the case of a banking crisis due to a higher relevance of the crisis on the public agenda. So, the finding can actually be explained within my theory: if the banking sector is small, policymakers can divert from socially optimal policy without angering voters too much. With increasing size of the sector, the costs policymakers incur in terms of voter dissatisfaction for retarding bank restructuring rise. Again, this finding echoes the findings by Keefer (2001), who takes the ratio M2/GDP to directly reflect the costs that policymakers incur for handing out favours.

The last remarkable result is actually a non-finding, but still has significant thrust: in this paper, I find no empirical evidence for the idea that simultaneous currency crises add to the cost of banking crises. Evidence for higher cost of twin crises is *robustly absent*. Given the substantial prior evidence for the detrimental consequences of twin crises, the cost-rising effect of twin crises must be seen as a fact. Therefore, this finding is more than surprising – it casts a shadow on all other results. If my measurement of the economic cost of crises does not reflect this important effect, the question arises, whether it is at all a meaningful measurement for the economic cost of crises. Other studies that have applied the same methodology have however found twin crises to differ significantly from banking crises in terms of economic cost. IMF (1998) reports average economic cost for banking crises of 11 percent and average economic cost for twin crises of 14 percent. The difference between my and their results could very well be due to the direct detrending parameters. I include two neighbours, i.e. four years to calculate detrended values and smooth with relatively large parameters (λ_1=100; λ_2=10). Given, that currency crises are relatively short incidents compared to banking crises, (IMF 1998 reports an average duration of 1.6 years) it could be that this detrending methodology *"smoothes away"* the effect of currency crises on the economic cost. Unfortunately, IMF (1998) does not give any information about how many years were included and how strong data was smoothed in their study so that an exact comparison of detrending methodologies is not feasible. But if the absence of a positive finding of currency crises on economic cost was due to the large smoothing parameters used, this would imply that the effect would become visible if lower parameters are used. To test this, I compare the economic cost of banking crises and twin crises in a two sample t-test of output-dips obtained

using an H-P Filter with only one neighbour smoothed at λ=10. The results presented in Table 4.6 confirm this idea.

Table 4.6 Difference between Banking Crises and Currency Crises using H-P Filter λ=10

Group	Obs	Mean (% of GDP)	Std. Err.	Std. Dev.	[95% Conf.	Interval]
Banking Crisis	29	7.761619	1.455676	7.839057	4.779801	10.74344
Twin Crisis	18	11.77843	1.747193	7.412713	8.092177	15.46469
combined	47	9.299973	1.1449	7.849042	6.995408	11.60454
diff		-4.016814	2.304723		-8.658764	.6251368

Degrees of freedom: 45 Ho: mean(0) - mean(1) = diff = 0

Ha: diff < 0	Ha: diff != 0	Ha: diff > 0
t = -1.7429	t = -1.7429	t = -1.7429
P < t = 0.0441	P > t = 0.0882	P > t = 0.9559

Thus, the same t-test that failed to show a significant difference between the economic cost of banking crises and twin crises earlier, now shows such a difference. A finding that plainly exposes the sensitivity of the results in this paper to the smoothing parameters used to detrend the data. Given this sensitivity, it is surprising that only very few studies employing this methodology make explicit the exact smoothing parameters they use.

IMF (1998) applies the same detrending methodology, albeit to GDP *growth* data and obtains roughly similar results: banking crises devour roughly 12percent of GDP (19 percent in my analysis[18]) and resolution takes an average three years (three years in my analysis).

[18] Lower GDP-losses in the IMF study might reflect the downwards-bias discussed earlier: Growth data systematically underestimates the losses of crises lasting longer than two years

5. Conclusion

This paper set out to explore the relationship between the depth of banking crises and the political system. I argued that the depth of banking crises is determined by the efficiency of the policies of crisis resolution and that policymakers only engage in efficient crisis resolution, if the benefits of handing out favours to special interest groups from the banking sector are lower than the cost of diverting from socially optimal policy. Further, I argued that the outcome of this cost-benefit analysis largely depends on two factors: the level of democracy present in the political system, which determines how easily voters can depose of policymakers; and the size of the banking sector, which determines how many resources will be invested in rent-seeking activities by special interest groups from the banking sector.

The results obtained from the statistical analysis of 47 banking crises in 40 countries are mixed. I do find strong evidence for the derived hypothesis that the influence of the number of veto players on the depth of banking crises depends on the level of democracy present in the political system. If the level of democracy is high, a higher number of veto players decreases the depth of banking crises. Conversely, if the level of democracy is low, a higher number of veto players increases the depth of banking crises. I conclude from this that democratic veto players face higher cost for engaging in special interest politics than their non-democratic colleagues.

Results regarding the benefit-side of the cost-benefit analysis are less straightforward. The hypothesised negative effect of a large banking sector on the relationship between veto players and the depth of banking crises could not be established empirically. Rather, I find that the depth-reducing effect of a high number of veto players on banking crises is stronger, the bigger the banking sector. It seems that large banking sectors also help to constrain veto players, probably because large banking sectors also imply high visibility of the crisis and high dissatisfaction with the crisis situation. So while large banking sectors might also lead to more resources being invested in rent-seeking, these additional benefits for veto players are outweighed by higher cost in terms of voter-dissatisfaction incurred by diverting from socially optimal policy.

The argument combines elements from two distinct research paradigms: research on the consequences of banking crises and on the political economy of economic change. Therefore,

the results should be evaluated separately in these two contexts. Regarding the political economy of economic change, the implications are rather clear-cut: the results indicate that the number of veto players cannot simply be assumed to exert a uniform influence on the feasibility of economic change in any possible institutional environment. Rather, the acknowledgement that veto players ultimately are manipulated by individual policymakers who base their decisions on cost-benefit calculations helps to improve our understanding of the dynamics of political actors in the process of change. Thereby, the findings can serve as a case against what Dani Rodrik (2003) calls *implicit political theorizing*: the attitude that "governments are run by crooks, so tie their hands" (p.2). In the case of banking crises resolution, this received wisdom is clearly misguided as a *more* of the political system can help to limit their consequences. However, the result regarding the impact of the banking sector on the effect of veto players also clearly shows a caveat to this approach: actors' utility functions are never observable and assumptions about their nature always risk going astray.

As concrete policies of crisis resolution were of no explicit concern in this paper, the implications regarding efficient crisis resolution are limited. "Become more democratic so policymakers don't hand out too many favours to interest groups" is hardly a feasible policy recommendation for countries in the midst of a banking crisis. Nevertheless, the findings do help to substantiate the – by now almost standard – idea that crisis resolution needs to be swift and thorough. The longer policymakers retard thorough restructuring, the more imbalances will be accumulated within the crisis sector. This finding is hardly revolutionary, but the Turkish example shows that its implications are still vastly ignored: it took the country two decades and three financial crises to engage in thorough restructuring. Tentative forecasting now finally sees Turkey on a way to more financial stability (Winkler 2005).

To a certain extent, the drawing of stronger conclusions is inhibited by the methodology that has been applied throughout this paper. The assessment of the depth of banking crises via the concurrent output-dips certainly is much more reliable than other measurements discussed above, especially the assessment of fiscal cost of banking crises. Nevertheless, it remains a crucial caveat of this detrending methodology that not only the strength of the effects, but in fact even the presence / absence of effects depends on the exact smoothing parameters used to detrend the data. The example of the effect of currency crises on the depth of crises helped to illustrate this: while currency crises surprisingly exerted no significant influence on the depth of banking crises calculated with the smoothing parameters used through most of this paper, a

change in these parameters led to the manifestation of the expected positive effect. Attention must therefore be paid in future studies on the choice of the smoothing parameters and the theoretical foundations of this choice. Furthermore, the depth of the output dips is also influenced by a plethora of other factors that could not possibly all be controlled for in this paper. I will present some additional robustness tests in Annex VI though.

The disregarding of concrete policies obviously is a strong limitation to this study – albeit one that is also owed to its limited scope. Future research should therefore introduce such policies and their effects into the framework. If my assertions hold, then the effect of specific policies on the depth of banking crises should similarly vary with the surrounding political system. Of course, data availability problems would probably limit the feasibility of such an effort. Another worthwhile extension of this argument would be the explicit modelling of policymakers' and bank managers' utility functions.

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Annex

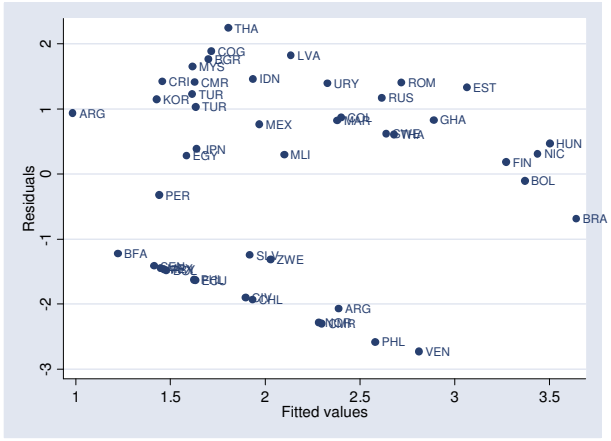
Annex I: Sample and Sample statistics

Countryname	Year of Crisis Onset	Duration of Crisis	Duration following C/K	Cumulated Outputdip (%of GDP)	GDP Growth	Currency Crisis	Checks
Argentina	1990	1	1	6.824383	12.66971	1	5
Argentina	1995	1	1	1.375192	2.84521	0	2
Burkina Faso	1988	1	6	0	6.629533	0	1
Bulgaria	1995	5	2	31.98596	2.860189	0	3
Bolivia	1986	5	2	26.19923	2.573874	1	4
Bolivia	1994	1	?	0	4.667266	0	4
Brazil	1990	5	1+5	19.59643	4.3	1	4
Chile	1981	1	5	0	4.737311	1	1
Cote d'Ivoire	1988	1	3	0	1.136482	0	1
Cameroon	1987	1	6	0	2.146652	0	1
Cameroon	1995	3	3	20.92046	3.3	0	3
Congo, Rep.	1992	8	10	36.70247	2.6	0	1
Colombia	1982	8	5	26.37852	.9484803	1	2
Costa Rica	1987	6	?	17.79068	4.759694	0	2
Ecuador	1981	1	?	0	3.364898	0	3
Egypt, Arab Rep.	1981	2	?	6.446686	3.756081	0	2
Estonia	1993	5	3	81.0246	8.189591	0	3
Finland	1991	6	4	31.75512	6.256455	1	5
Ghana	1982	1	6	41.15255	6.92365	0	1
Hungary	1991	7	5	53.13647	11.89204	0	4
Indonesia	1997	1	5	29.79711	4.699883	1	1
Japan	1991	3	11	6.078247	3.259415	0	3
Korea, Rep.	1997	1	5	13.17584	5.011032	0	3

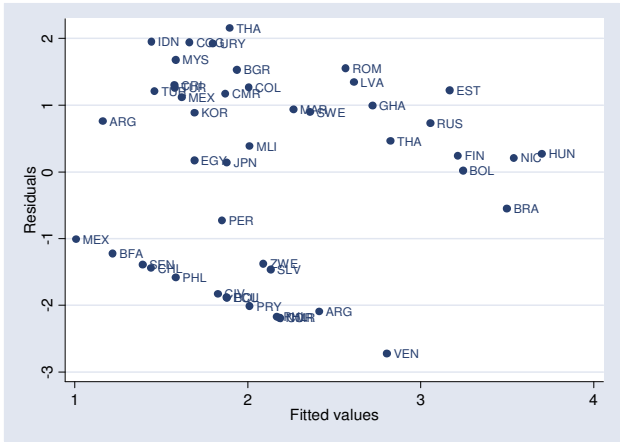
Latvia	1995	3	7	52.30843	.8111686	0	5
Morocco	1981	5	?	24.47042	2.764341	0	1
Mexico	1981	1	10	0	8.7726	1	1
Mexico	1994	4	3	15.41652	4.415326	1	2
Mali	1987	2	3	11.00233	.5182353	0	1
Malaysia	1997	5	5	26.15849	7.322743	1	4
Nicaragua	1989	8	7	42.46001	1.697613	0	2
Norway	1987	1	5	0	1.847243	1	4
Peru	1984	1	6	3.059422	5.2	0	4
Philippines	1981	1	6	0	3.42327	0	1
Philippines	1998	1	5	0	.576722	1	2
Paraguay	1995	1	3	0	4.708716	0	5
Romania	1990	5	10	61.60369	5.600004	0	1
Russian Federation	1995	1	1	44.12308	4.143528	0	5
Senegal	1988	1	3	0	5.068963	0	1
El Salvador	1989	1	1	1.956857	.9621922	0	3
Sweden	1991	8	?	25.93552	1.106241	1	3
Thailand	1984	7	4	57.30881	5.752427	1	5
Thailand	1997	3	5	26.75214	1.371374	1	6
Turkey	1982	4	4	17.13683	3.563356	0	1
Turkey	2000	2	2	14.38548	7.359095	1	3
Uruguay	1981	6	4	41.41119	1.55956	1	1
Venezuela, RB	1994	1	1	1.086872	2.34725	1	5
Zimbabwe	1995	2	5	2.044473	.1580264	0	2

Annex II: Residual Vs. Fitted Plots for the Final Models

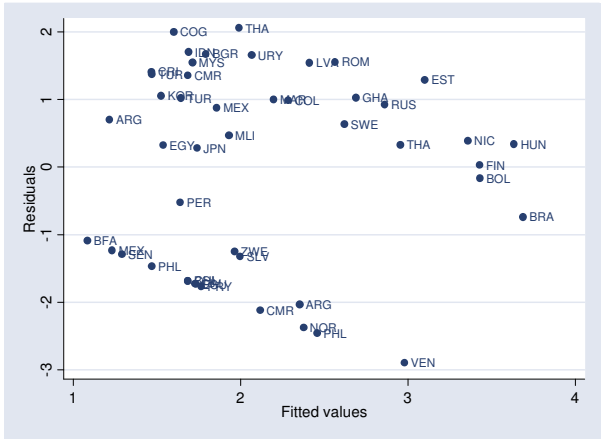
Model 1



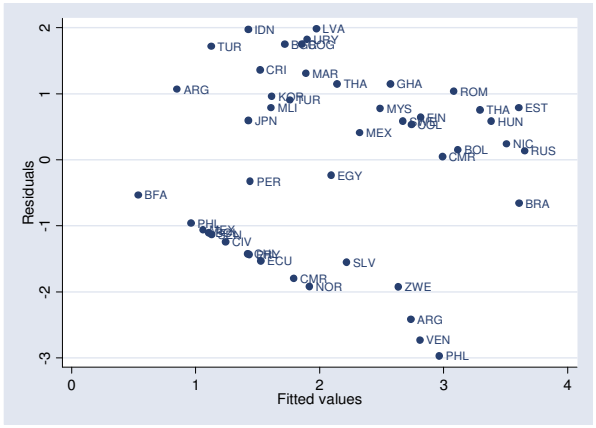
Model 2



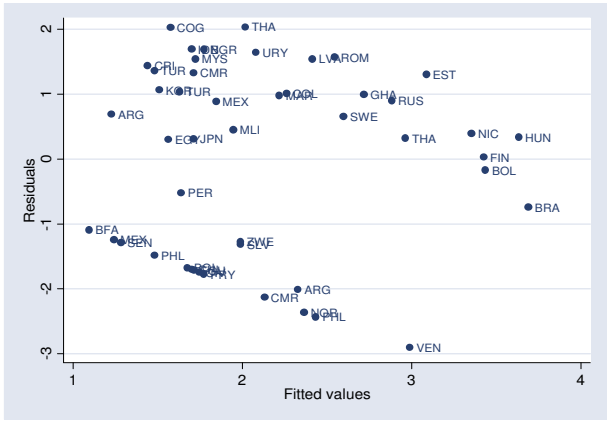
Model 4



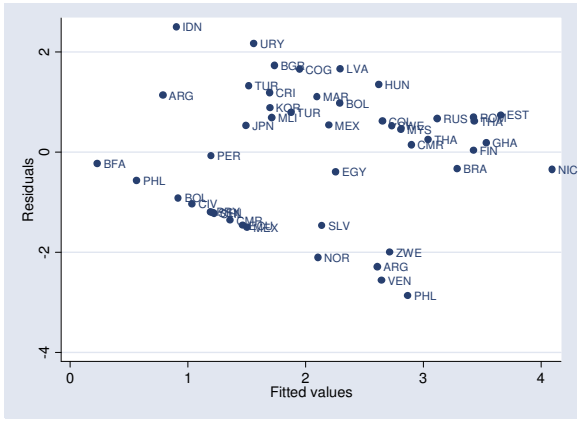
Model 5



Model 6



Model 7



Notes: tobit-regressions do not allow for rvfplots. Models underlying these plots are therefore calculated using regular OLS. Model 3 and 8 under these circumstances are identical to Models 2 and 7 and are therefore excluded, here.

Annex III: Correlation Matrix for Main Models

Model 1	gdpgrowth	creditgrowth	curr_crisis
gdpgrowth	1.0000	-	-
creditgrowth	-0.1901	1.0000	-
curr_crisis	0.1770	0.0629	1.0000

Model 2	gdpgrowth	creditgrowth	checks
gdpgrowth	1.0000	-	-
creditgrowth	-0.1901	1.0000	-
ctdchecks	-0.0732	0.0679	1.0000

Model 4	gdpgrowth	creditgrowth	curr_crisis	checks	polity2
gdpgrowth	1.0000	-	-	-	-
creditgrowth	-0.1901	1.0000	-	-	-
curr_crisis	0.1770	0.0629	1.0000	-	-
ctdchecks	-0.0732	0.0679	0.2705	1.0000	-
ctdpolity2	-0.1308	0.0502	0.2165	0.6953	1.0000

Model 5	gdpgrowth	creditgrowth	curr_crisis	checks	polity2	chks*pol
gdpgrowth	1.0000	-	-	-	-	-
creditgrowth	-0.1901	1.0000	-	-	-	-
curr_crisis	0.1770	0.0629	1.0000	-	-	-
ctdchecks	-0.0732	0.0679	0.2705	1.0000	-	-
ctdpolity2	-0.1308	0.0502	0.2165	0.6953	1.0000	-
chks*pol	-0.2026	-0.0407	0.2299	0.4466	0.1581	1.0000

Model 6	gdpgrowth	creditgrowth	curr_crisis	checks	polity2	findev
gdpgrowth	1.0000	-	-	-	-	-
creditgrowth	-0.1901	1.0000	-	-	-	-
curr_crisis	0.1770	0.0629	1.0000	-	-	-
ctdchecks	-0.0732	0.0679	0.2705	1.0000	-	-
ctdpolity2	-0.1308	0.0502	0.2165	0.6953	1.0000	-
ctdfindev10	0.2569	-0.2942	0.2556	0.0714	0.0982	1.0000

Model 7	gdpgrowth	creditgrowth	curr_crisis	checks	findev	chks*findev
gdpgrowth	1.0000	-	-	-	-	-
creditgrowth	-0.1901	1.0000	-	-	-	-
curr_crisis	0.1770	0.0629	1.0000	-	-	-
ctdchecks	-0.0732	0.0679	0.2705	1.0000	-	-
ctdfindev10	0.2569	-0.2942	0.2556	0.0714	1.0000	-
chks*findev	0.2541	-0.2057	0.2586	0.0496	0.9433	1.0000

Model 8	gdpgrowth	creditgrowth	curr_crisis	checks	polity2	findev	chks*pol	chks*findev
	-	-	-	-	-	-	-	-
gdpgrowth	1.0000	-	-	-	-	-	-	-
creditgrowth	-0.1901	1.0000	-	-	-	-	-	-
curr_crisis	0.1770	0.0629	1.0000	-	-	-	-	-
ctdchecks	-0.0732	0.0679	0.2705	1.0000	-	-	-	-
ctdpolity2	-0.1308	0.0502	0.2165	0.6953	1.0000	-	-	-
ctdfindev10	0.2569	-0.2942	0.2556	0.0714	0.0982	1.0000	-	-
chks*pol	-0.2026	-0.0407	0.2299	0.4466	0.1581	0.0846	1.0000	-
chks*findev	0.2541	-0.2057	0.2586	0.0496	0.0921	0.9433	0.0487	1.0000

Annex IV: Regression Results for Singular Crisis Years

	Model 10 (prais)	Model 11 (prais)	Model 12 (prais)	Model 13 (prais)	Model 14 (prais)	Model 15 (prais)	Model 16 (OLS)
constant	1.081017	1.127621	.3163306	1.072135	1.152832	.339584	.3398981
	(.15233)****	(.15230)****	(.10262)***	(.15987)****	(.16898)****	(.077373)****	(.0793871)****
gdpgrowth	-.0544048	-.0530553	-.0543453	-.0633998	-.0647767	-.0675564	-.0673178
	(.01207)****	(.01200)****	(.00897)****	(.01344)****	(.01282)****	(.0090356)****	(.0091963)****
l_creditboom6	.196729	.2391223	-.0386955	.1674135	.2339456	-.0346535	-.0286984
	(.10206)*	(.1156)**	(.09017)	(.10315)	(.1177)*	(.0850605)	(.0845359)
curr_crisis	-.2921156	-.2517159	.0914655	-.2358028	-.1905019	.134708	.1247669
	(.19631)	(.19008)	(.08348)	(.21276)	(.20511)	(.0697594)*	(.0677424)*
ctdchecks	.166626	.2092678	.0342368	.1704391	.2430033	.0668063	.0716525
	(.04802)***	(.04681)****	(.03450)	(.04099)****	(.03824)****	(.0216322)***	(.0212602)***
ctdpolity2	-.002714	-.0033679	-.0039753	-	-.0037128	-.0067975	-.0072744
	(.01259)	(.0126)	(.00650)		(.01177)	(.006166)	(.0060722)
ctdfindev10	-	-	-	-.0894067	-.0833629	-.0380602	-.0384838
				(.0404388)**	(.0387902)**	(.014621)**	(.014842)**
checks_polity2	-	-.0166635	-.0044491	-	-.0246635	-.0102021	-.0105726
		(.00909)*	(.00540)		(.0090)***	(.0037279)***	(.0036486)***
invchecks_findev	-	-	-	-.0358262	-.0451183	-.0320336	-.0306804
				(.02140)	(.01589)***	(.0134775)**	(.0134295)**
lagged DV	-	-	.7813335	-	-	.7693146	.7626304
			(.04086)****			(.0397823)****	(.0399812)****
n	135	135	135	131	131	131	131
r^2	0.283	0.293	0.765	0.317	0.339	0.802	0.784
DW-Stat	0.432327	0.445637	1.442376	0.434540	0.470492	1.486192	-
Transf. DW-Stat	0.930926	0.930153	1.414164	0.977453	0.988982	1.410265	-
F-Statistic	34.81****	33.23****	913.74****	41.40****	38.08****	700.05****	149.03****

Notes: Dependent variable: log output dip of every recession year following a banking crisis; standard errors adjusted for clustering on countryid are reported in parentheses; significance levels: *at 0.10 level, **at 0.05 level, ***at 0.01 level, ****at 0.001 level; l_creditboom6 is a dummy taking on value 1 if creditgrowth at t-1 > (6x gdpgrowth)

Annex V: Comparison of Different Filtering Methodologies

Filter:	Hodrick-Prescott					Band-Pass
Filter Parameter:	100[S]	100	100x10	20x5	10	3[S]
Economic Cost						
Number of Crises	47	47	47	47	47	47
Crises without Output-Dip	9	12	12	8	9	8
Avg. GDP loss (% of GDP)	20.245	15.674	19.552	19.703	10.373	13.596
Std. Dev.	20.764	17.744	20.34	19.464	8.911	12.606
Skewness	1.137	1.31	0.978	1.236	0.832	0.983
Kurtosis	3.972	4.031	3.208	4.807	3.32	3.323
Min.	0	0	0	0	0	0
Max.	89.586	68.53	81.024	89.886	37.519	48.672
Duration						
Avg. Duration in Years	3.9	3.2	3.3	3.9	3.1	3.8
Min. Duration in Years	1	1	1	1	1	1
Max. Duration in Years	11	10	8	11	9	15
Std. Dev of Duration	2.873	2.293	2.46	2.742	1.88	2.95

Notes: Filter Parameters indicate the smooth parameters (λ) for H-P filters and the number of years included (k) for B-P filter; phi and plo for B-P filter are held at 4 and 8 years, respectively; filters notated with an "S" are calculated using Stata 8.2, all others are calculated using Matlab 7.0.

Annex VI: Robustness Tests for the Main Model

In the following table, I want to forestall criticism of omitted variable bias by introducing a series of other variables into the regression. Specifically, I want to control for international factors, the duration of crises and give roughly gauge the impact of policies.

Viable export-oriented companies could slide into insolvency if their products decline in value. This could adversely affect the quality of banks loans and balance sheets. I take the indicator "Terms of trade (goods and services, 1995 = 100)" from the Global Development Network Growth Database (2005) to form the terms of trade indicators *av3_dToT* and *ToTshock* in Models 17 and 18. "Terms of trade (goods and services, 1995 = 100)" is an index centred around 100 in 1995 and *av3_dToT* is the average of the changes in the index in the three years before a crisis. Negative Numbers indicate an adverse terms of trade movement. *ToTshock* controls for sudden terms of trade shocks. It is a dummy taking on the value one if the terms of trade-index has declined by more than 15 points in the year of the onset of the crisis. As can be seen in the table on the next page, neither indicator is significant and neither one affects the other indicators significantly.

Models 19 and 20 introduce measures of the microeconomic policy environment. *overvaluation* is a measurement of the overvaluation of the currency, which I take, similar to an index of a black market premium, to be an indicator of overall macroeconomic management. Data is taken from the indicator "Real Overvaluation" in the Global Development Network (2005) Growth Database, which equals an index taking on the values higher than 100 if domestic currency is overvaluated, lower than 100 if domestic currency is undervaluated and 100 if otherwise. *dep_ins* controls for the presence of an explicit deposit insurance system. Data is taken from the Banking Regulation Dataset compiled by Caprio and Levine (2003), available on the World Banks Webpage. It takes on the value one if item 8.1 "Is there an explicit deposit insurance scheme?" is answered positively and zero otherwise. As can be seen in the table, neither indicator enters the regression significantly. However, the assessment of deposit insurance schemes is problematic, as these more than often are not stated explicitly to mitigate moral hazard-problems.

In Model 21, the variable *crsdur* controls for the duration of the crises. *crsdur* equals the number of years in which actual GDP was below trend GDP in the presence of a banking crisis, calculated as described above. Given, that the dependent variable is the sum of recessionyears, it is not surprising that the number of summands enters positively into the regression. However, the introduction of the measure also wipes out the significance of the interaction term *checks_polity2* giving rise to robustness-concerns. However, *crsdur* is anything but exogenous to the dependent variable, so that additional inquiry would be necessary to interpret this result.

Table Annex VI: Robustness Tests

	Model 5 (tobit)	Model 17 (tobit)	Model 18 (tobit)	Model 19 (tobit)	Model 20 (tobit)	Model 21 (tobit)
constant	2.283154 (.4292676)****	2.389297 (.5893456)****	2.55267 (.4358604)****	1.193154 (1.054641)	1.552657 (.9445953)*	.3886207 (.4448837)
gdpgrowth	-.1922284 (.0561311)***	-.2328987 (.0851463)***	-.2296763 (.0636572)****	-.1173816 (.0911814)	-.1865134 (.0488557)****	-.1028956 (.0507485)**
av3_creditgrowth	.0002828 (.0002117)	.000177 (.0002638)	.0002788 (.0001819)	.000544 (.0002775)**	.0001211 (.000234)	-.0002849 (.0001758)
curr_crisis	.7719629 (.6035067)	.9567576 (.6422443)	.7570492 (.6002164)	1.426714 (.6338579)**	.4228802 (.5629202)	-.2077554 (.4948913)
ctdchecks	.4786627 (.2413098)***	.4501071 (.2541224)*	.3496718 (.2316059)	.4633934 (.3015238)	.9171329 (.3394036)***	.5077476 (.1726365)***
ctdpolity2	-.0289809 (.0494028)	.0013976 (.0551052)	.0079658 (.0500005)	-.0405928 (.0555198)	-.1519252 (.0662299)**	-.0789919 (.0321873)**
checks_polity2	-.1207105 (.0350179)***	-.1642598 (.0474888)***	-.1475374 (.0417366)****	-.1278969 (.0416896)***	-.1094381 (.0329953)***	-.0402249 (.0305424)
av3_dToT		-.0229938 (.0407537)				
ToTshock			-.4183072 (.8463294)			
overvaluation				.0032668 (.0043527)		
dep_ins					1.110926 (.856028)	
crsdur						.5169837 (.0812372)****
n / (left censored)	47 / (12)	38 / (11)	41 / (11)	40 / (12)	34 / (6)	47 / (12)
pseudo-r^2	0.209	0.269	0.294	0.198	0.270	0.474
Wald χ^2	26.85****	34.28****	40.84****	29.81****	23.73***	62.08****
Log pseudo-likelihood	-78.915432	-59.504202	-65.230393	-65.267715	-55.551913	-66.201465

Notes: Dependent variable: log cumulated output dip of banking crisis; standard errors adjusted for clustering on *countryid* are reported in parentheses; significance levels: *at 0.10 level, **at 0.05 level, ***at 0.01 level, ****at 0.001 level; r^2 in tobit models is McKelvey and Zavoina's r^2 obtained through Stata's *fitstat* command

Annex VI: List of Countries Included in T-Test Table 4.2

Angola
Albania
United Arab Emirates
Argentina
Armenia
Australia
Austria
Azerbaijan
Burundi
Belgium
Benin
Burkina Faso
Bangladesh
Bulgaria
Bahrain
Bosnia and Herzegovina
Belarus
Belize
Bolivia
Brazil
Botswana
Canada
Switzerland
Chile
China
Cote d'Ivoire
Cameroon
Congo, Rep.
Colombia
Cape Verde
Costa Rica
Cyprus
Czech Republic
Germany
Denmark
Dominican Republic
Algeria
Ecuador
Egypt, Arab Rep.
Eritrea
Spain
Estonia
Ethiopia
Finland
Fiji
France
Gabon
United Kingdom
Georgia
Ghana
Guinea
Gambia, The
Equatorial Guinea
Greece
Guatemala
Hong Kong, China
Honduras
Croatia
Haiti
Hungary
Indonesia
India
Ireland
Iran, Islamic Rep.
Iceland
Israel
Italy
Jamaica
Jordan
Japan
Kazakhstan
Kenya
Kyrgyz Republic
Cambodia
Korea, Rep.
Lebanon
Liberia
Sri Lanka
Lithuania
Luxembourg
Latvia
Morocco
Moldova
Madagascar
Maldives
Mexico
Macedonia, FYR
Mali
Mongolia
Mozambique
Mauritania
Mauritius
Malawi
Malaysia
Namibia
Niger
Nigeria
Nicaragua
Netherlands
Norway
Nepal
New Zealand
Oman
Pakistan
Panama
Peru
Philippines
Papua New Guinea
Poland
Puerto Rico
Portugal
Paraguay
Romania
Russian Federation
Rwanda
Saudi Arabia
Sudan
Senegal
Singapore
Solomon Islands
Sierra Leone
El Salvador
Slovak Republic
Slovenia
Sweden
Seychelles
Syrian Arab Republic
Chad
Togo
Thailand
Tajikistan
Turkmenistan
Tunisia
Turkey
Tanzania
Uganda
Ukraine
Uruguay
United States
Uzbekistan
Venezuela, RB
Vietnam
Yemen, Rep.
Serbia and Montenegro
South Africa
Congo, Dem. Rep.
Zambia
Zimbabwe