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Essays in Empirical Macroeconomics on Democracy, Income Inequality and Financial Liberalization

Adam Kuczynski

Essays in Empirical Macroeconomics on Democracy, Income Inequality and Financial Liberalization

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"In Remembrance of the Victims of Fascism and War."

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Chapter 1

Introduction

The objective of this dissertation is to empirically study the interaction between democracy, financial arrangements and economic outcomes. Empirical research on the interaction between democracy and economic outcomes saw its peak in the 1990s and early 2000s, but never reached a consensus on whether democracy is effective and desirable for economies to function well. Financial arrangements have received more attention since the Global Financial Crisis, as economists are questioning to what extent financial development and integration foster economic growth and development. The consensus in the past used to be that finance stimulates economic growth and development, but this consensus is not as robust nowadays, with some recent studies indicating that there may be "too much finance".

The overarching theme connecting democracy, financial arrangements and economic outcomes is the concept of institutions. Institutions can be briefly defined as the formal and informal rules that govern interactions between people (North et al., 1990). Democracy, for instance, outlines whether people have an equal say in determining national policies, while financial regulation determines how financial organizations are allowed to interact.[1]

Within this broad topic, there are three core questions this thesis addresses:

1. Do democratization and democratic breakdowns happen more frequently during economic and financial crises?
2. Does democratization result in lower income inequality?
3. Does capital account liberalization contribute to economic growth?

Each of these three questions has a rich literature backing it up, and this book is not the first to specify or address them as such. However, for all three questions the literature either consists of

[1] Institutions are not to be confused with organizations. Organizations are groups of people with a common goal, whereas institutions determine how such groups of people interact within the organization as well as interactions between organizations. The interested reader is encouraged to check Hodgson (1988), Hodgson (2006) or North et al. (1990) for a far more comprehensive and esoteric discussion on what institutions precisely are and are not.

clashing, empirically untested theories, or consists of clashing empirical results. As such, there is still no consensus on what the answers are to the above questions, calling for further research. This thesis aims to contribute to the literature in two ways. First, by empirically testing seldom-tested theories. Second, by providing methodological updates and data updates to fine-tune existing empirical results.

The questions all have much broader societal implications than may be obvious at first glance. Democracy goes hand in hand with the provision of human rights, hence a better understanding of the resilience or emergence of democracy to crises also provides insights into the resilience or emergence of such human rights. Income inequality is closely linked with poverty, which in turn has a major impact on quality of life. Finally, it is through persistent technological improvements resulting in economic growth that humanity has escaped from Malthusian subsistence life and perpetual poverty. Questions surrounding the driving factors of economic growth are thus fundamentally about the eradication of poverty and betterment of human life.

Each question is studied and presented in a separate chapter in this book. This introduction provides a short summary of the underlying thought and approach behind each chapter, as well as each chapter's main contribution and conclusions. It does so by first briefly introducing the relevant core literature, highlighting potential mechanisms to look out for. After that, it highlights the main contributions of the chapter to the existing body of literature. It then describes the empirical methodology the chapter uses. Finally, it provides a summary of the most important results.

Democratic Breakdowns, Transitions and Economic and Financial Crises

Chapter 2 aims to create a better understanding of the conditions under which democracies emerge and break down. Democratic governments are a relatively new phenomenon. This system of inclusive self-governing has primarily emerged in relatively rich, highly developed countries.[2] Its emergence among such countries has sparked the question whether democracy is a *consequence* of economic development.[3] Based on this question, there are two competing theories on where democratic regimes come from and what makes them endure. The first finds its origins in the works of Lipset (1959), Lerner (1958) and Rustow (1970) and can be dubbed Modernization Theory. It proposes that democratization goes hand in hand with economic development and that democratization is an inevitable outcome as a result of countries' economic growth. The second

[2]The Athenians already enjoyed a form of direct democracy many centuries ago. Though this is often considered the first democracy, it was implemented on a rather small scale. Furthermore, its collapse and disappearance meant that democracy was absent in the world for many centuries.

[3]Naturally, it has also sparked the question whether democracy itself results in economic development. Countries may in part reach a level of high development thanks to being democratic. Such a reversed causality is, too, the subject of many conflicting studies, with no consensus emerging. The lack of an empirically robust connection from democracy toward growth suggests that its thorough inclusion within this dissertation would only considerably increase the complexity of this book, without an apparent purpose or pay-off. It is thus left aside.

theory, suggested by Przeworski and Limongi (1997) is that democracy can emerge (and collapse) at any level of economic development. Consequently, democracy is not endogenous but rather a coincidental outcome. Przeworski et al. (2000) highlights that economic development improves the survival rate of democracies, but in itself does not cause democracy to emerge.

The latter theory suggests that one must look at factors other than economic development to see why democracies emerge or collapse. An often proposed candidate for such a factor is the incidence of economic crises (O'Donnell, 1973; Linz and Stepan, 1978; Epstein, 1984; Acemoglu and Robinson, 2012; Przeworski and Limongi, 1997; Remmer, 1995). Here, too, there are two competing theories on what mechanisms connect crises to changes in regimes. The first theory proposes that crises undermine the extant regime: Democracies collapse, and non-democracies democratize. This view thus flips the narrative from Modernization Theory around, and proposes that regimes that provide stable economic development also remain more stable themselves. The second theory proposes that democracies in particular struggle with crises, as they lack the authority to quickly and effectively intervene and resolve a crisis. Crises then undermine democratic regimes and incumbent political leadership, but they can be resolved relatively easily in non-democracies. At the time of writing, the COVID pandemic is a prime example hereof. It has caused major damage to human life across democratic nations in particular. Many measures for fighting the virus are met with protest, resistance and non-compliance. This is in stark contrast to non-democracies, such as China, where the outbreak began, but where the virus is suppressed via unquestionable, top-down authority. The main distinction between the theories is whether crises undermine both democratic and non-democratic regimes, or only democracies.

The two theories have largely remained untested empirically, and relevant literature on the topic is extremely limited. This is where the contributions of chapter 2 come into play. Research connecting crises to changes in the level of democracy primarily comes in the form of monographic case studies of individual countries, often within Latin America. With such a specific focus, it is difficult to draw general conclusions on the plausible validity of either theory. Providing empirical evidence to form such a general conclusion is the primary purpose and contribution of Chapter 2. Chapter 2 studies the relation between economic and financial crises and changes in democracy as well as changes in political leadership across a broad set of both developing and advanced countries, from as early as 1800. The study distinguishes between 6 different types of economic and financial crises, to better understand which types of crises have historically had the biggest impact on extant regimes, if any. The 6 crisis types considered are: (1) Hyperinflation; (2) Banking crises; (3) Currency crashes; (4) Domestic Debt crises; (5) External debt crises and (6) Stock market crashes. Via a Multinomial Logit approach, the chapter quantitatively estimates how much more likely countries are to experience a change in political leadership and how likely it is to experience democratization or a democratic breakdown during a crisis. The data used in this study cover 70 countries from 1810 till 2016, where the majority of observations come from the period after the Second World War.

There are two main insights drawn from this analysis. The first is that changes in democracy are very infrequent. Countries tend to be either very democratic, or very authoritarian. Most changes in democracy tend to be quite gradual, and major shifts between authoritarianism and democracy are very uncommon. The second insight is that changes in the level of democracy happen more frequently during times of economic and financial crisis, and that political leadership is also more likely to change during crises. Established democracies tend to experience a partial democratic breakdown, and authoritarian countries tend to partially democratize. This chapter thus finds that crises weaken and (partly) undermine the existing regime, regardless of whether it is democratic in nature or not. In relation to the existing literature, this conclusion supports the original premise of Lipset (1959), that regimes require effectiveness and legitimacy to persist. It also adds nuance to the ideas of Remmer (1995), that poor economic performance indeed does not result in major regime changes, but that smaller institutional changes nevertheless do occur.

The Relation between Democratic Changes and Income Redistribution

While Chapter 2 focuses on a potential cause for the emergence and breakdown of democracy, Chapter 3 shifts its focus toward potential *consequences* of democratization and democratic breakdowns. Democracy is the most common system of governance among advanced economies, and a major strand of literature focuses on the extent to which democracy contributes to these economies advancing (Doucouliagos and Ulubaşoğlu, 2008; Acemoglu et al., 2019). The key principle behind democracy is that everyone gets an equal say in determining national policies. That is, people of all economic classes are represented in a democracy. In non-democracies, only the economic upper-class is typically politically represented, often giving rise to a system of kleptocracy (Grossman and Noh, 1990; Grossman, 1999; Acemoglu et al., 2004). Democratization thus constitutes movement away from such a kleptocracy, by extending the influence and power of the economically poor. If the economically poor act in their own self-interest, they are likely to pursue redistributive policies, so as to end or at least diminish their own poverty. Democratization is thus expected to go hand in hand with higher redistribution and lower income inequality.

Meltzer and Richard (1981) formalize this logic in an economic model, highlighting the role of the median voter. When countries democratize, and political power is given toward the economically poor, the median voter becomes poorer. As per Hotelling's Law, the median voter decides the level of desired redistribution. A poorer median voter will demand higher redistribution, thereby decreasing income inequality. In contrast, if a country experiences a democratic breakdown, the median voter becomes richer and the push for redistribution falls, thereby increasing income inequality. The framework is not without its limitations: Public Choice theory highlights that selfishness, ideology, altruism, group identity and (ir)rationality may all play a role in voters' decision making process, making this process much murkier than the simple model implies (Simon, 1995; Lee, 1988;

Rotemberg, 2009; Simon, 1993; Feddersen, 2004; Bonilla, 2004).

Existing empirical evidence on this expected connection between democracy and income inequality is mixed. Some research, such as that of Gradstein and Milanovic (2004), finds the expected effect, while others note that there is no empirical connection at all (Acemoglu et al., 2015). This lack of consensus in the empirical literature is an open invitation to further test the hypothesized connection between democracy and income inequality.

Revisiting the empirical connection between democracy and income inequality nexus is the focal point of Chapter 3. Much of the existing literature finds its results by running country-level cross-sectional or panel regressions, in which the level of democracy is one of the explanatory variables. This approach creates two problems. First, the majority of the data is based on advanced, highly democratic countries, in which both democratization and democratic breakdowns almost never happen. Countries where democratic changes rarely happen are a poor basis for studying the effects of such democratic change. Second, democratic changes are in general very infrequent, displaying far less variation than income inequality This results in weak correlations and almost inevitable statistically insignificant relations. Chapter 3 uses a very different empirical methodology, in the form of a quasi-experiment. The use of this different methodology, which resolves the preceding two problems, is the primary contribution to the literature. The chapter uses Propensity Score Matching (PSM) to mimic the analysis of a Randomized Controlled Trial (RCT) on a macroeconomic scale. At its core, PSM involves creating a trial group of countries, undergoing some form of democratic change, and a control group of countries, which do not undergo any democratic change. The groups are formed in such a way that they are statistically indistinguishable right before the trial (democratization or democratic breakdown) starts. Following the start of the trial, the groups can be further compared, in order to see if the trial has made them diverge. In the context at hand, the comparison of primary interest is whether the two initially identical groups of countries begin to feature different levels of income inequality and/or income redistribution. The data used in this study cover 172 countries from 1960 till 2016.

The results of the empirical analysis reveal that democratization does not result in any measurable changes in income inequality or income redistribution. Similarly, democratic breakdowns are not found to measurably impact income inequality or income redistribution either. The results do not vary depending on which time-frame is considered, and there is no evidence of any short-term nor long-term effects. Policies affecting the pre-redistribution income inequality may obviate the need for income redistribution. There is, however, also no evidence that pre-redistribution income inequality changes as a result of democratization nor democratic breakdowns.

The Impact of Capital Account Liberalization on Economic Growth

Chapter 4 departs from the focus on democratic changes, and instead investigates the effects of changes in financial policies. A considerable strand of economic literature focuses on the benefits and downsides of financial liberalization, notably capital account liberalization. Much of economic theory proposes that free capital markets perform the best, and that financial freedom fosters economic growth. Extensions to neoclassical growth theory suggest that opening up to international capital markets creates a temporary boost in economic growth (Henry, 2007; Klein, 2005; Barro et al., 1995). In particular, developing countries are expected to benefit from this the most, as they gain access to abundant capital from the rest of the world, allowing them to finally overcome local resource constraints. Financial Endogenous Growth Models (FEGMs) use endogenous growth theory as their starting point instead of neoclassical growth theory, but typically provide similarly optimistic expectations. Capital account liberalization encourages financial development, which via the FEGMs has a permanent, positive impact on economic growth.

The various debt crises in Latin America since the 1980's, the Asian Currency Crisis of 1997 and the Global Financial Crisis of 2007-2009 have all repeatedly cast doubt on the purported overall benefits of capital account liberalization. Capital account liberalization results in a higher incidence of financial crises and allows shocks to spillover from one country to another more easily. Stiglitz (2000), Rodrik and Velasco (1999) and Caballero (2016) all highlight how open international capital markets not only make it easier for capital to flow *into* a country but also how it becomes easier for capital to *flee* a country, causing currency crashes and external debt instability.

Empirically, the benefits of capital account liberalization are on shaky ground. Studies on average find a positive relation between liberalization and growth, but statistical significance is typically quite limited (Bumann et al., 2013). A challenge in empirically estimating the effects of capital account liberalization on economic growth is that its effects may be indirect. Capital account liberalization may, for instance, promote economic growth by boosting financial development. Any regression in which capital account liberalization and financial development are two separate explanatory variables would then underestimate the effects of liberalization. Furthermore, there may be bi-directional causality. It may be the case, for instance, that liberalization promotes growth, but that well performing countries are also more likely to pursue liberalization. Finally, Eichengreen et al. (2011), Okada (2013), Asongu (2014) and Kose et al. (2009) all highlight that benefits from capital account liberalization may only clearly materialize when certain requirements are met. Such requirements include that countries must have sufficiently developed property rights, a strong rule of law and absence of corruption. These factors are strongly linked with the level of democracy, with democracies generally performing best in meeting the requirements. In weak or non-democracies, in contrast, the requirements are often not met, suggesting there are few potential benefits from liberalization. Pursuing capital account liberalization as a blanket policy, as has been done extensively under the auspices of the IMF in the past, may therefore be inappropriate.

Chapter 4 revisits the question whether capital account liberalization stimulates economic growth. The analysis is conducted using a Panel Vector Autoregression (PVAR). This methodology can take potential indirect effects, the potential bi-directionality and potential underlying requirements into account. Furthermore, it provides an overview of the longevity of potential effects of capital account liberalization via Impulse Response Functions (IRFs). The primary contribution of this chapter is that it provides a methodological update that considers the preceding confounding factors.

Based on data covering 182 countries over the period 1970-2017, this chapter finds that capital account liberalization generates statistically significantly higher economic growth. Growth is found to increase by approximately 1-2% cumulatively after liberalization, where the effect primarily materializes in the first 5 years after liberalization. There is evidence that liberalization has indirect effects on economic growth, via financial development and FDI. Furthermore, there is evidence of a bi-directional connection between liberalization and economic growth, though this result is sensitive to the measure for liberalization used. The chapter tests whether the results vary across groups of countries based on institutional quality, economic development and involvement of the IMF in the liberalization process. It finds, however, that these three potential confounding factors play no significant role in determining the outcome, hence there is no evidence of underlying requirements. The results from liberalization are equivalent between democracies and non-democracies, for instance. The limited cumulative effect on growth raises the question whether the benefits from liberalization are able to outweigh the increased risk of and exposure to costly economic crises.

The chapters in this book occasionally refer to a supplementary online appendix, along with a clickable link to said appendix. In case this text is being read in print, the full appendix can be accessed via the link: shorturl.at/sFLO3

Chapter 2

Democratic Breakdowns, Transitions and Economic and Financial Crises[1]

2.1 Introduction

Ever since the seminal work of Lipset (1959), economists and political scientists have produced considerable research on the emergence and consequences of democratic systems. There is an extensive body of work looking at the relation between democracy and economic outcomes, with economic growth being the staple outcome. For example, Barro (1996) finds that increasing political rights is positively associated with growth, but only temporarily. Similarly, Helliwell (1994) reports inconclusive results, arguing that democracy both promotes growth, but also has features that stifle growth. Political instability, on the other hand, produces stronger results, with growth being significantly hampered in times of governmental stress (Alesina et al., 1996; Aisen and Veiga, 2013). There is a comparable line of research devoted to democracy's effects on inequality, once again with no fully definitive results. Muller (1988) and Reuveny and Li (2003) show that democracy promotes income equality, while Gradstein and Milanovic (2004) find that this link is not so clear-cut. Acemoglu et al. (2015) provide an extensive study on democracy and inequality, concluding that the effects of democracy are quite complex, and that it does not automatically decrease inequality.

There are fewer empirical studies exploring the conditions during which democracies arise and consolidate. A large part of the literature comes in the form of monographic studies, providing extensive details on specific political transitions, with a particular focus on the Latin American experience since the 1960's.[2] There are some indicators that show under which circumstances democracies generally tend to remain stable (see e.g. Cheibub et al. (1996) and Przeworski and Limongi (1997)), but scholars are split into two camps on the reasons why democracies emerge or break down in the first place.

The first view originates from Lerner (1958) and was further extended by Rustow (1970) and

[1] The work for this chapter was inspired by discussions with Prof. Dr. Bertrand Candelon.

[2] See, for instance, the research presented by Epstein (1984) and Markoff and Beretta (1990).

has been dubbed Modernization Theory. It suggests that democracy is an inevitable outcome of economic development. That is, as a country's citizens gain educational attainment and wealth, they grow intolerant of repression. Consequently, authoritarian rule becomes unstable, forcing the political elite to concede and share their power. Przeworski et al. (1996) refutes the idea that democracy is inevitable. Przeworski argues that the correlation between economic development and democracy need not be causal, but is merely coincidental. Rich countries merely avoided democratic breakdowns, but did not democratize due to their earlier economic growth. Democracy can emerge at any stage of development, and countries reaching a high stage of development simply managed to avoid slipping back toward authoritarianism.[3] Put briefly, the two views state that democratization is either inevitable, or that democratization is only a possible outcome, but one that has coincidentally remained stable. In an attempt to better understand institutional development, testing these two theories empirically has been a focal point, with both sides scoring points and no conclusion being reached as of yet (e.g. Boix and Stokes (2003); Epstein et al. (2006)).

The connection between economic development and democratic changes has been studied extensively, but shocks in the process have received less attention. In particular, there are only very few empirical studies linking economic crises to changes in political or democratic institutions. Though the literature on democracy does regularly refer to economic crises as having a potential impact on the political sphere, there is, to my knowledge, almost no empirical work that emphasises the process of democratization or movement away from democracy.

The main contribution of this paper is to help fill the empirical void by studying the coincidence of political changes in the form of executive power switches, changes in the level of democracy and economic and financial crises. The paper distinguishes between changes in political leadership and changes in democratic institutions. Changes in political leadership can result in altered institutions and vice-versa, but the two events can occur independently. The study considers a long period of time, starting as early as 1800, and a varied sample consisting of polities across all levels of development.[4] Rather than treating crises as a homogeneous category, the study considers different crisis types, in the form of periods of hyperinflation, banking crashes, currency crises, periods of domestic and external debt default and stock market crashes. In doing so, it tests whether crises undermine regimes, and whether they undermine primarily democracies or non-democracies.

The aim of this paper is to study whether crises are indeed regime undermining, and to observe not only whether crises spark political or democratic changes, but also whether they encourage or inhibit democratization. Secondly, establishing which kinds of crises are the most relevant in the democratisation process brings about not only footing for more detailed studies, but also major policy implications for any organisations focused on economic development and human rights.

[3] Przeworski et al. (2000) do argue that democratic institutions are conducive to economic performance, suggesting that improving institutions is a good way to enhance development.

[4] Related literature tends to focus either only on Third World countries or very advanced economies.

The first key observation is that democratic change is very infrequent, and that democracy and authoritarianism are very persistent. There appear to be two quite stable equilibria, with non-democratic polities remaining largely non-democratic, and fully democratic ones also not experiencing much turmoil. From the data, it becomes apparent that the commonly used binary labelling of democracies leaves out the majority of democratic dynamics, which tend to be only minor: The majority of recorded democratic changes do not sway polities from one extreme to the other. Instead, the process is often far more gradual.

The second insight is that changes in heads of state are more common during times of crisis. Similarly, I find that democratic and autocratic institutions tend to change more often during times of crisis. Democratic changes tend to occur either after the start of a crisis or coincide temporally. Changes in democratic institutions seldom precede the start of new crises, however. Periods of excessive inflation stand out as being linked the strongest with democratic changes. I find that established democracies tend to experience democratic breakdowns during crises, while non-democracies tend to emerge with strengthened institutions.

I first review existing literature on the topic in section 2.2. Section 2.3 presents the main hypotheses. Section 2.4 describes the data, followed by an illustration of democratic dynamics via descriptive statistics in Section 2.5. I then explain the statistical methodology employed in Section 2.6, followed by the statistical results in Section 2.7. This is followed by a discussion of the main results along with relevant implications in Section 2.7.4. Finally, I conclude and provide suggestions for avenues for future, follow-up research.

2.2 Literature

The majority of the relevant economic literature on the connection between democratic changes and economic crises comes in the form of monographic studies. These highlight the consequences of singular, often severe crises on the democratic process or public support of democracy in specific countries or regions. The Global Financial Crisis and the various crises in Latin America have received particularly much attention (see e.g. Remmer (1990), Morlino and Quaranta (2016), Córdova and Seligson (2009) and Diamond (2011)).

Outside of many country or crisis-specific monographs, there is a major lack of both theoretical and empirical literature explicitly linking crises to regime changes. [5] As a result, it is challenging to draw a more general conclusion on how commonly economic crises spill over into political changes.

[5] Literature covering the topic more generally typically does not dive into much detail. There is an unfortunate tendency among political scientists to say "Just look at Latin America". The absence of empirical studies may, in part, be due to a lack of precise crisis data and methodological difficulties in establishing causality between crises and democratic changes.

Though theoretical research on the topic is largely absent, there are nevertheless two common views on how crises result in changes in regimes and democratic institutions, based on the influential work of O'Donnell (1973), Linz and Stepan (1978) and Epstein (1984).

The first view is that crises undermine regimes regardless of whether they are democratic or not, and thus trigger regime change in general. This view suggests that crises may result in both transitions towards and away from democracy, depending on what system is in place as a crisis erupts. Any emergence of a crisis is seen as a failure of the political elite to manage a country's economy well. The political elite's blame is also shared by the institutions in place: In non-democracies, the lack of accountability and complete executive discretion can be seen as the underlying cause for a crisis. Strengthening democratic institutions is then used as a tool to ensure that leaders can be replaced when there are red flags surrounding their managing of a country's economy. In democracies, on the other hand, crises can undermine the legitimacy of the regime. A breakdown of democratic institutions need not be met with much resistance, as the regime type was unsuccessful in providing good economic outcomes anyway. Only regimes that provide stable economic outcomes remain stable themselves, regardless of whether they are democratic or non-democratic in nature.

An alternative, second view is that the resolution of economic and financial crises requires policies that are impractical or infeasible in democracies as they are too unpopular and economically painful, necessitating a (temporary) democratic breakdown. Incumbents require more authoritative power to address a crisis via policies that voters would simply not happily comply with. An example of such unpopular policies are the austerity measures undertaken in Greece as a result of the 2009 sovereign debt crisis, which resulted in lengthy riots across Athens. This view thus suggests that crises have a unidirectional result, with democratic breakdowns being the most likely consequence. Similarly, O'Donnell (1973) argues that those who are in a position to change a country's institutions tend to believe that democracy gets in the way of resolving crises, so that these institutions must be removed. Democratic regimes are more susceptible to popular responses, which may be counter-productive to guiding an economy back toward stability.

The only empirical work explicitly and thoroughly linking crises to democratization I have encountered is that of Gasiorowski (1995), which studies the process of democratization among 75 Third World countries since the 1950's in relation to elevated inflation. The reported findings suggest that inflationary crises have a strong, significant connection with periods of democratic breakdown. In a related study of 53 African countries from 1965 till 2008, Bertin et al. (2016) also show that economic and financial crises contribute to political leadership instability. While political instability and democratic instability are not synonymous, the two are expected to be related, based on the first view presented above. Somewhat similarly, in a study of 20 advanced economies over the last 140 years, Funke et al. (2016) show that financial crises contribute to political radicalization and substantial changes in election results towards the far-right, indicating that crises

induce changes in executive power. Brückner and Grüner (2020) reaches a similar conclusion when studying a panel of 16 European countries between 1990 and 2016, with right-wing extremism and political polarization increasing during periods of slow growth.

2.3 Hypotheses

I formulate three testable hypotheses on the basis of the idea that crises are regime undermining. The hypotheses are clarified below. The first hypothesis is:

***Hypothesis 1**: Changes in executive power are more likely to occur during times of crisis than in normal times.*

A change in executive power occurs when a country's leading politicians are at least partly replaced. This can occur in numerous ways, generally depending on the existing type of regime. In presidential democracies, the election of a new president constitutes a clear change in executive power. Even when the new head of state finds their origins in the same political party as the predecessor, the role has nevertheless been passed on to an independent, new person. Independence simply implies that the new incumbent can pursue own, new policy goals which may or may not correspond to those of their predecessor.[6] Parliamentary democracies operate in a similar way, though rather than replacing a single head of state, they create new coalitions and reshuffle cabinets to replace several incumbents at the same time. Non-democracies typically only go through a change in executive power after the death of the incumbent, or after forcefully removing the head of state.[7]

Hypothesis 1 states that heads of state are replaced more often during economic downturns. This is based on the idea that crises undermine existing regimes. If an incumbent is responsible for causing a crisis or shows inability to resolve the crisis, then there is likely to be pressure to replace the mismanaging executive in power. In non-democratic countries, such changes may be connected to riots, revolutions or (military) coups. In democracies, incumbents can be held responsible for mismanagement and replaced via the democratic process, requiring no violence. The replacement procedure can take place both during regularly scheduled elections, or via early elections, if the pressure to resign or to disband the set of heads of state is sufficiently high.

[6]The presidential elections in the United States are an example hereof, through which the head of state is typically replaced once every four or eight years by an independent new candidate. Thus, also Lyndon B. Johnson's unusual ascension to the White House following the assassination of John F. Kennedy resulted in a change in executive power, as he was free to pursue his own policies with his newly found political power.

[7]An example of the former is The Kingdom of Saudi Arabia, which chooses its new King via its Allegiance Council mechanism; a nepotistic, small scale election system. Forceful removals of heads of state are commonplace throughout Thailand's history, with its very frequently occurring military coups.

The second hypothesis takes a closer look at institutional changes rather than incumbent changes:

***Hypothesis 2**: Changes in democratic institutions are more likely to occur during times of economic and financial crisis than in normal times.*

While changes in political leadership and changes in institutions may coincide, they can also occur independently. A change in leadership need not result in any democratization or democratic breakdowns.[8] Similarly, institutional changes can occur without the head of state being replaced.[9]

Two scenarios exemplify how democratic institutions may change due to a crisis. First, if democracy hampers the resolution of crises, due to the necessity to make unpopular decisions, then one could expect a democratic breakdown to occur. Popular influence is curtailed, so that incumbents can freely pursue policies that are painful, but ultimately expected to faster resolve the economic downturn. For instance, sovereign debt problems may be addressed via reduced spending and increased taxes, but such measures are unlikely to win the hearts of many voters. In another (non-democratic) scenario, citizens may demand increased influence in the political sphere so as to prevent future crises from happening via better governmental management. This could occur through an increase in freedom to become an active politician, so that better qualified people can be put in office, or by increasing the accountability of leaders, so that they can be removed if deemed unfit.

Democratic institutions encompass many elements, but all such institutions ultimately determine the level of influence given to those outside the political sphere. The removal of voting rights may seem like the most clear-cut way of removing such influence, but the removal of accountability groups, constitutional limitations and hampering of political competition are other ways in which incumbents can cement their positions as country leaders. Many of today's non-democracies hold regular elections, in which there is often only one candidate to vote for. It is also not uncommon to see term limits removed, so that heads of state can remain in power longer. Regardless of the way in which democratic institutions are altered, the end result is that there is more power and influence available to the head of state.

The third and final hypothesis is split up into two alternative versions:

[8] The Kingdom of Saudia Arabia serves as an example of this. Despite the appointment of a new ruler approximately once every decade, none of the changes in leadership have resulted in any institutional reform, leaving the regime type unchanged.

[9] For instance, Tajikistan's president has continuously been in office since 1992. This presidency has featured several constitutional changes, including an increase and later a removal of the term limit, effectively permitting the incumbent to remain in office indefinitely. Furthermore, the president has been granted lifelong, full immunity from any prosecution.

Hypothesis 3a: *Crises undermine and weaken democracy, resulting solely in a movement away from democracy.*
Hypothesis 3b: *Crises undermine both democracies and non-democracies. Non-democracies are pushed toward democracy, while democracies experience an institutional breakdown.*

Hypotheses 3a and 3b correspond to the two opposing, theoretical views presented in section 2.2. These views differ in whether they expect *all* regimes to experience institutional changes, or primarily democratic ones. Under hypothesis 3a, crises result in democratic breakdowns, while authoritarianism thrives or is at the very least left unaffected. In contrast, hypothesis 3b states that the direction depends on the existing level of democracy, and that crises undermine whichever system is in place. Democratic countries already have strong institutions and legal frameworks in place. The democratic system is designed to be self-correcting, with no institutions standing in the way of changing leadership or policy during times of crisis. Among non-democratic countries, it is less obvious which of the outcomes is more plausible. Since existing institutions are weak, they may be easily removed altogether. Alternatively, new political leaders can offer redistributive policies or more political rights as they take office, to legitimize their place in office and thus partly strengthen democratic institutions.[10]

2.4 Data

2.4.1 Democracy

There are several candidate datasets for capturing the level of democracy. These measures are binary, such as that provided by Cheibub et al. (1996)), and multinary measures, such as the Freedom House data (Freedom House, 2019), Polity IV (Marshall et al., 2017), Polyarchy (Vanhanen, 2000) and Varieties of Democracy (V-Dem) (Lindberg et al., 2014). Binary measures have the downside of capturing primarily very big democratic changes, resulting in very few measured democratic changes and hence very few useful observations. As such, binary measures are not preferred for studying democratic changes. Among the multinary measures, there are differences in the definition and scope of democracy, as well as in the time-span covered by the data. Polyarchy bases its democracy data only on the voter turnout and the share of the biggest political party, which corresponds to a very limited definition of democracy. The Freedom House data, meanwhile, cover the shortest time-span, starting in 1973, whereas the other measures extend to 1900 or earlier. The two remaining main candidates are thus the Polity IV index and V-Dem. The current study chooses the Polity IV index over V-Dem for two reasons: (1) Polity IV has long been an industry

[10]For instance, following the chaotic Rhodesian war, in the 1980's Robert Mugabe of Zimbabwe was seen internationally as somewhat of a hero for denouncing racial division and promoting educational reform. In the end, despite early signs of promoting development, he continuously held executive power until 2017. His successor Emmerson Mnangagwa entered office with the promise of fixing the country's dysfunctional economy and the creation of a new democracy, which may be met with more skepticism after the last incumbent's promises.

standard in studies of democracy, making it easier to place new results in the broader literature; (2) V-Dem is based on a huge breadth of indicators, for some of which the connection to a definition of democracy is questionable (e.g. property rights for men and women, or the extent to which ordinary people discuss major policies among themselves). The latter issue can be resolved by constructing a new democracy index on the basis of the many available indicators, but this introduces a considerable scope creep to the current study. Though V-Dem is not used in the current study, it is slowly gaining in popularity, becoming a new industry standard for measuring democracy, and may prove a fruitful avenue for future research.

Data on democracy therefore comes from the Polity IV dataset (Marshall et al., 2017). This annually updated dataset contains information on democratic properties of 195 polities, from 1800 till as late as 2016, resulting in 17,228 observations.[11]

The dataset splits the scoring system into a democracy (*democ*) and an autocracy (*autoc*) variable. The *democ* index focuses on the ability for people to choose freely between policies and leaders and on the guarantee of civil liberties. The *autoc* index looks at the possibility of political participation and the limits to power.[12] The democracy score ranges from 0 to 10, where 0 represents an absence of democratic institutions and 10 suggests full civil liberties, free elections and strong checks and balances. The autocracy score also ranges from 0 to 10, but in this case 10 means that an incumbent has absolute power and there are considerable barriers preventing the incumbent's replacement. The *democ* and *autoc* variables are generated on the basis of sub-indices, which assign points based on the presence of specific democratic features, as documented at length in the PolityIV Manual. An example of such a sub-index is the level of executive constraints. If an executive can, for instance, routinely ignore constitutional restrictions, has a major influence in accountability groups or can freely rule by decree with no limitations, then the *autoc* index is increased by up to 3 points. By subtracting the autocracy variable from the democracy score, Polity IV combines the two measures into a separate variable, dubbed *polity2*. Due to the subtraction, this variable ranges from -10 to 10, where 10 represents full democratic institutions and an *absence* of autocratic properties, and -10 represents the converse.

The Polity IV dataset contains special modalities for years where the level of democracy is awkward to establish. Years where there appears to be an absence of a standard political system due to foreign invasion are labelled as -66. An example hereof would be the Allied occupation of Germany between 1945 and 1949, during which Germany de facto had several governments ruling over it. Furthermore, there is a -77 label, reserved for polities that experienced a complete removal

[11] The authors of the dataset specify on its website that it covers 167 countries as opposed to 195 polities. This difference stems from the fact that polities come and go. For the year 2016, the cross sectional dimension is 167, but the overall coverage including polities that no longer exist is 195 polities.

[12] The naming used for these two sub-indices is unfortunate. Though the "democracy" labelling of the first sub-index makes it appear to be the go-to measure for democracy, the autocracy measure provides highly relevant information that falls under common definitions of democracy. As such, the sub-index naming should not be taken very literally.

of the central authorities, with no new system stepping into place. This is reserved for periods of anarchy, where the formal government, if there is one, has no *de facto* power. Finally, there are transition periods, during which governments are restructured. For instance, the Austro-Hungarian empire collapsed into two separate nation states following the First World War, which required the formation of new governments. The Polity IV dataset indicates that this process took two years for Austria and 1 year for Hungary, by labelling the democratic scores for the corresponding years as -88. To reduce gaps in the data, Marshall and Jaggers linearly interpolate such observations on the basis of neighbouring democratic and autocratic scores. That is, if at time $t-1$ the *polity2* score is 0, at t it is -88 and at $t+1$ it is 10, then the value at time t is replaced with the linear middle-point between 0 and 10, which is 5.

Switching to and from this -88 label represents an underlying change in a polity's democratic and autocratic institutions. In total, the dataset includes 328 observations that are based on interpolation. Many of these interpolated observations do not result in changes of the democratic score compared to surrounding observations, due to the post-transition government not undergoing any fundamental institutional changes compared to its pre-transition predecessor. Furthermore, the vast majority of these interpolated points do not make it into the final dataset, due to gaps in observations for other variables. As such, I have very little reason to believe that the process is in any way harmful for generating accurate, statistical results. Note that if a -88 observation follows a -77 observation, there is no reasonable way of interpolating the results. Such observations are thus left blank and dropped.

While the vast wealth of underlying indices and information allow for a multitude of analyses, I opt to focus on the final *polity2* index itself. This measure is an 'industry standard' among studies on democracy. Furthermore, the aim of the current study is to establish whether the connection between crises and democracy warrants further studying, hence diving into sub-index based details is rather premature. In the remainder of the paper, I use the *polity2* index and the level of democracy synonymously. While Polity IV makes the distinction between democratic and autocratic institutions, I therefore consider the level of democracy to be the *combination* of both, rather than only the first sub-index.[13]

2.4.2 Economic and Financial Crises

The second main dataset is that compiled by Reinhart and Rogoff (2011) on the incidence of economic and financial crises since 1800 till 2010 (which is also further described by Reinhart (2010)). The data cover 70 countries, resulting in an unbalanced panel of 14770 observations. There are six types of crises considered in this dataset: (1) Inflation crises; (2) Currency crises; (3) Stock market

[13]Most of the existing literature using Polity IV relies on the *polity2* measure to capture democracy, rather than the democracy sub-index. As such, my usage of this data aligns with existing democratic research.

crashes; (4) Banking crises; (5) Domestic sovereign debt crises; (6) External sovereign debt crises. Inflation crises are observed in 1145 observations (7.8%), Currency crises in 1124 observations (8.1%), stock market crashes in 1184 observations (11.4%[14]), banking crises in 732 observations (5.0%) domestic debt crises in 232 observations (1.6%) and external debt crises in 1893 observations (12.8%).

These crises are documented through the use of binary variables, indicating whether there was an ongoing crisis or not.[15] Inflation crises are periods in which the inflation rate exceeds 20% per annum. Unfortunately, there is no consensus in the literature on what inflation rate should be used as a barrier between crisis and non-crisis states. Bruno and Easterly (1996) suggest that 40% may be a more appropriate number to use, as there is evidence that countries managed to sustain inflation rates between 15 and 30 percent for long periods without any catastrophic economic costs. As such, the criterion of 20% may result in inflation crises being *over-represented* in the data, which can result in an underestimation of their importance.

Currency crises are said to take place when the annual depreciation exceeds 15%. A major cause attributed to such crisis periods is to let a currency float after it had been pegged for long periods of time. Furthermore, debasement of a currency can also cause mass depreciation and inflation. Banking crises are said to take place when there is a substantial number of bank runs happening, or when there are numerous bank closures or mergers taking place. Unfortunately, the exact number determining whether it is 'substantial' or not is unclear in this dataset. Furthermore, periods with substantial bank recapitalization by the government also count as crisis periods. External and domestic debt crises take place when governments experience outright default on debt obligations. Periods of debt restructuring also count towards this. External debt crises may involve exclusion from international capital markets, hence one of the main criteria for determining the end of a crisis period is the moment when such access is regained. This, unfortunately, results in some extreme, implausible crisis periods. As Reinhart and Rogoff (2011) point out, Russia is labelled to have a 69-year long external debt crisis, simply because there was no clear point of resolution with creditors. Similarly, Greece was cut off from international markets for 53 years. With such episodes, it is likely that external debt crises are *over-represented* in the data. Conversely, however, domestic debt crises are most likely *underrepresented*. Reinhart and Rogoff note that periods of domestic debt problems often go unnoticed and that their end points are also frequently unclear. Stock market crashes are given no concise quantitative nor qualitative definition in the data files nor in the Chartbook (Reinhart, 2010). These crashes are not documented for all countries. While I do consider the possible effects of stock market crashes in the subsequent statistical analysis, I err on the side of caution when drawing any conclusions.

[14]The state of the stock market has fewer total observations than the other crisis types. This is most likely due to the later emergence of stock markets in developing nations than in developed countries.

[15]Nota bene: This is not the same as labelling the start of a crisis.

I expect that both over- and under-representation result in an underestimation of the relevance of crises. The only solution is, unfortunately, to get more accurate data. Given the limitations of the data at hand, it is worth not fully discrediting statistical results in the subsequent sections that fall short of conventional significance levels ($p < 0.05$ or better). I nevertheless emphasize the role of statistical results that do meet standard significance requirements, as these display the strongest relevance in addressing the hypotheses.

A major drawback of the binary crisis variables is that they do not capture the intensity of the crisis period. The definitions maintained by Reinhart and Rogoff result in the crisis dummies capturing both most economically damaging parts of a crisis, along with the (likely less volatile) recovery period. The aim of the current study is to determine whether crises play any relevant role and to determine which crisis types matter the most. A full analysis involving the various nuances of crises, based on, for instance, their severity and length, both of which likely play a role in determining the outcome, goes beyond the scope of this paper.[16]

2.4.3 Executive Power

I employ the Cross National Time Series Data Archive (CNTS, hereafter) for information on changes in executive power (Banks and Wilson, 2021). The data start as early as 1815 and end in 2012 and cover 235 regions (which includes areas that merged into countries, or areas that are not universally acknowledged to be independent countries), resulting in an unbalanced panel total of 16236 observations. Furthermore, to proxy for educational attainment and economic development, I make use of the literacy rate, also derived from the CNTS. Other potentially interesting explanatory variables are unfortunately severely lacking in the time dimension to be of any use.

Finally, I also consult the Maddison Project (Bolt and van Zanden, 2014) for information on real GDP per capita. This dataset ambitiously includes estimates for output from as early as the year 1 up until the year 2010. There is a handful of observations (41) where the GDP estimate remains unchanged across several years. Similar to CNTS, its cross-sectional availability is sometimes limited. For the year 1900, for instance, it only covers 30 countries. Nevertheless, to my knowledge it is still the most complete data available on estimated GDP when going back in time well beyond the Second World War. The primary use of the GDP data is to capture economic downturns that do not fall into any of the standard crisis types (such as wars and natural disasters) and to proxy, alongside the literacy rate, for the attained level of economic development.

[16] Reinhart and Rogoff do provide more detailed data for all crises which should allow for such an analysis, but due to the methodological challenges posed in sufficiently addressing the topic in a complete manner, the endeavour is left for future research.

2.5 Descriptive Statistics

The development of economic and financial crises is largely covered by Reinhart (2010); Reinhart and Rogoff (2011) and various papers published by the IMF (see, among others, Claessens and Kose (2013); Kannan et al. (2009); Pazarbasioglu et al. (2011)). Long run democratic developments have received less attention, however. Hence, while I discuss several basic features of the crisis variables, I emphasize the information found in the *polity2* index in this section.

	Variable	Mean	SD	Min	Max	N
	polity2	-0.55	7.07	-10	10	16992
	Executive	0.20	0.40	0	1	11651
Crises	Inflation	0.10	0.29	0	1	9436
Crises	Banking	0.07	0.26	0	1	9435
Crises	Currency	0.11	0.31	0	1	9343
Crises	External Debt	0.18	0.38	0	1	9436
Crises	Domestic Debt	0.02	0.15	0	1	9436
Crises	Stock Market	0.16	0.36	0	1	7296
	GDP per capita	4455	5074	203	42916	10010
	GDP Growth (%)	1.86	6.71	-61.47	86.90	9830
	Literacy (%)	66.24	39.64	1.30	99.99	8196

Table 2.1: Descriptive statistics. Note: The number of observations is based on the final, full merger of all datasets. Due to a lack of full overlap in regions across these datasets, there is an inevitably large number of crisis measurements that cannot be used.

Table 2.1 illustrates the means, standard deviations, minima and maxima, and the number of observations available for the *polity2* index, the binary executive power measure, the binary crisis variables, the level of real GDP per capita, the growth of real GDP per capita and the literacy rate. The average *polity2* level is close to the index' constructed middle point, indicating that on average countries have only been partial democracies. The high standard deviation shows that there is much variation in this index across the various countries and years, however. The binary variables' means depict the portion of observations that feature a positive outcome. That is, there is a change in executive power approximately once for every five observations. Similarly, inflation crises cover approximately one in ten observations. The average income level across the observations is quite low, sitting at only one tenth of the maximum measured value. This is likely due to both the global nature of the data, which includes a substantial number of economically poor countries, and due to the lengthy time-span covered, during which the now-rich countries featured far lower income levels. The vast majority of countries grow at a reasonable rate, but several extreme outliers do occur. The most severe drop in income occurred in Iraq, in 1991, following UN sanctions which obliterated the Iraqi economy that was heavily dependent on oil exports. The highest growth is

recorded in Bosnia in the year 1996. This period of extraordinary GDP changes was preceded by the Bosnian War, during which the region's economy came to a standstill. The period of extremely high growth therefore marks a major reconstruction of the nation. The literacy rate displays major variation across countries, with some countries being almost entirely analphabetic, and others being fully literate. This major split in literacy rates persists throughout the years, and illustrates that the level of education varies considerably across the sample.

The average measured level of democracy has been increasing over time, as illustrated in figure 2.1. The interbellum period coincides with a visible deviation from a preceding, overall upward trend. The temporary drop in the average during the Second World War can be explained by the fact that it affected the majority of the democratic world at the time. Due to the labelling used by the Polity data, observations in which countries are experiencing periods of foreign occupation and anarchy are excluded in the calculation of the average (so that the average is based only on countries that were left untouched or unoccupied by, for instance, the Second World War). Democratic institutions were never permanently removed due to WW2, but rather temporarily inhibited, as can be seen from the revival of the democratic average immediately after the war's conclusion (where the average once again includes previously occupied countries). Average democracy declines in the post-war period, until the early 1990's. This drop cannot be solely attributed to the emergence of the Soviet bloc, because most countries are simply labelled as entirely undemocratic from the conception of this bloc onward. Several other notable authoritarian regimes emerged or consolidated during this time, such as the Franco regime in Spain (1939-1975), Pinochet in Chile (1973-1990) and the many, often volatile, post-colonial regimes in countries like Burundi and Morocco. Nevertheless, the collapse of the Soviet occupation shows up clearly throughout the early 1990's, as countries experienced fundamental governmental restructuring towards democracy.[17]

Table 2.2 further illustrates the dynamics of democratic change by presenting a matrix that shows how the *democ* variable changes from year to year.[18] Each column represents a baseline level of democracy and each row represents the level of democracy one year later. Each cell then shows how often countries in the data have moved from the column's value to the row's value. For instance, the second row in the first column shows that there are 42 instances where the *democ* score changes from 0 to 1 - a minor form of democratization. Similarly, the first row in the final column shows that there is only one case where the democracy index has changed from 10 to 0 - a complete democratic breakdown. Every entry under the diagonal therefore represents democratization, while every element above the diagonal shows a democratic breakdown. The further it is from the diagonal (vertically), the larger the degree of institutional change.

[17] The corresponding graphs for the *autoc* and *democ* variables can be found in the Supplementary Online Appendix.

[18] For practical purposes, I do not show the equivalent matrix for *polity2*. The main reason for this is that *polity2* has 21 modalities, resulting in a 21x21 matrix. The resulting table is quite awkward to read but results in the same interpretation. I also omit the *autoc* table, because it looks essentially the same and also provides the same interpretation as the *democ* one.

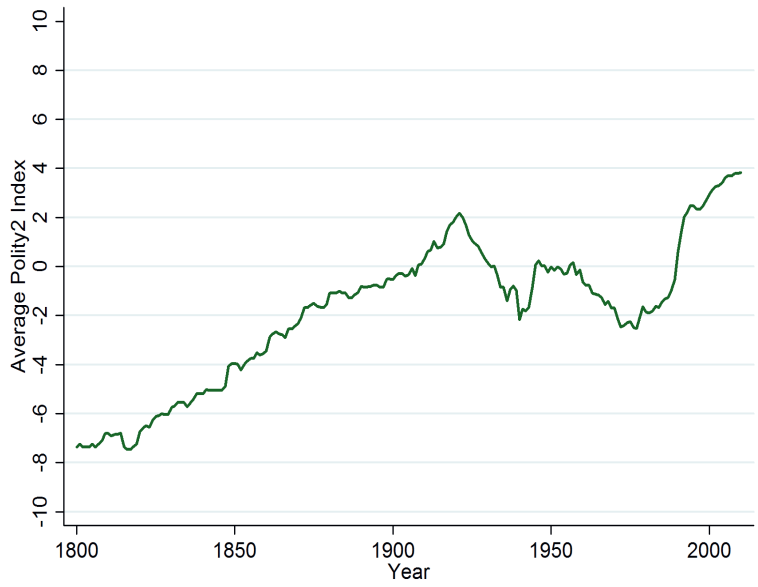

Figure 2.1: Average global democracy score over time. Note that the sample is not constant in size throughout the years, as the cross-sectional dimension regularly increases. Source: Polity IV and author's calculations.

Three features stand out in this table. First, the overwhelming majority of observations lies along the diagonal, where the current level of democracy is the same as the preceding level of democracy. Changes in democratic institutions are infrequent, and thus the existing institutional quality is very persistent. Once a certain level of democracy has been established, it is thus unlikely to change over time. This suggests that democratic change is a rather unique event that is most likely instigated by another event, such as an economic or financial crisis. Institutional persistence may make it difficult to predict any changes in democracy. Secondly, many of the measured democratic changes are quite marginal and lie very close to the diagonal. Almost half of the reported democratic changes had at most a magnitude of 2 on the *democ* variable scale (from 0 to 10). When looking at changes in the *polity2* variable, more than half of the changes had a magnitude of 2 or less (on a 0 to 20 scale). This implies that a simple distinction between democracies and non-democracies is too superficial, as change tends to be quite gradual. Thirdly, the measured levels of democracy are concentrated heavily on the lower end of the scale, and at the very top of the scale. Combined with the first feature, this means that a complete absence of democracy is difficult to alter and that fully established democracies are very unlikely to regress. Nevertheless, there are proportionally far more cases of absolute non-democracies entering a democratization process, however small it may be, than there are absolute democracies regressing to less democratic institutions. Democracies appear very self-sustaining and do not collapse often. This difference

		Time T										
	Mod	0	1	2	3	4	5	6	7	8	9	10
Time T+1	0	5863	41	9	15	17	11	11	7	13	3	1
	1	42	2522	8	11	6	7	3	1	2	0	0
	2	23	15	403	8	2	0	0	3	4	1	0
	3	14	21	14	427	5	8	4	1	1	0	2
	4	22	6	9	15	546	4	5	2	4	1	0
	5	10	5	3	6	10	609	13	3	0	0	1
	6	12	6	1	11	10	11	580	15	4	1	1
	7	7	3	6	2	3	7	22	820	9	4	1
	8	8	2	4	1	13	5	4	25	711	8	5
	9	2	1	1	0	1	0	4	9	28	634	4
	10	2	0	0	0	2	0	1	4	7	17	2384

Table 2.2: Dynamics of the degree of democracy as measured by the *democ* variable."Mod" refers to the modality of this degree. The column labels denote the original democracy index, and the rows represent the index attained one year later. Values below the diagonal indicate *increase* in the democracy index from one year to the next, while values above the diagonal indicate *decreases* in de democracy index. Source: Polity IV and author's calculations

provides support for the hypothesis that the effect of crises may differ between democracies and non-democracies, as the latter display more volatility.

I observe in the data that serial changes are common. In other words, once a country experiences either democratic transition or democratic breakdown, it frequently does so several years in a row, creating some form of path dependence.[19] Both the strong persistence of the democracy data and the serial changes in democracy suggest that a dynamic model will most accurately capture the series' development over time.

Table 2.3 depicts the correlation coefficients between all variables. It illustrates that changes in executive power occur more frequently in democratic nations. This reflects that democracies tend to feature regular elections that facilitate a change in the head of state, regardless of the reason, whereas autocracies feature rulers who stay in power for prolonged periods of time. Countries with high democratic scores also tend to have higher GDP per capita and higher literacy rates, which is the primary empirical observation driving Modernization Theory.

For the crisis data, I observe that most observed crisis periods featured singular crises and that there is generally no strong relation between the different types. Two exceptions do stand out, however. There is a substantial correlation between currency and inflation crises (0.48). Exchange rates and inflation are both tied to monetary policy. Extreme expansionary monetary policy results in hyperinflation, but also exchange rate crashes. Currency crashes can also have other causes than hyperinflation, and currency crises also need not result in hyperinflation, hence the overlap between

[19]There may be parallels to the Big Push style models, where starting a process (in this case institutional change) requires a tremendous effort, but is easier to keep going once the process has already started.

Table 2.3: Contemporaneous correlation coefficients between the measure of democracy, the binary crisis variables, real GDP growth, and two proxies for economic development (real GDP per capita and the literacy rate).

	polity2	Executive	Inflation	Banking	Currency	Ext. Debt	Dom. Debt	Stock Market	GDP per Capita	GDO Growth	Literacy
polity2	1										
Executive	0.12***	1									
Inflation	-0.14***	0.025	1								
Banking	0.016	0.06***	0.13***	1							
Currency	-0.07***	0.019	0.48***	0.14***	1						
Ext. Debt	-0.22***	-0.01	0.29***	0.07***	0.22***	1					
Dom. Debt	-0.05**	0.06***	0.07***	0.07***	0.09***	0.34***	1				
Stock Market	0.06***	0.045**	0.07***	0.12***	0.09***	-0.02	0.01	1			
GDP Per Capita	0.49***	-0.029	-0.17***	-0.01	-0.11***	-0.21***	-0.09***	0.05**	1		
GDP Growth	0.027	-0.01	-0.08***	-0.12***	-0.09***	-0.04*	-0.01	-0.05**	0.05**	1	
Literacy	0.49***	-0.02	-0.03	0.06**	0.02	-0.12***	-0.06**	0.09***	0.57***	0.046**	1

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

the two crisis types is only partial. The Asian currency crisis is a prime example of major currency crashes that were accompanied by only very moderate increases in inflation. External and domestic debt crises correlate (0.33). With domestic debt crises most likely being underreported, the true overlap between these two types of crises may be somewhat higher.[20]

The data show that in any given year, each crisis type makes an appearance in a substantial part of the world. The prevalence of each crisis type does, however, vary over time.[21] There have been several long periods during which a large number of countries simultaneously struggled with high inflation. Two of the peaks appear to directly follow the two World Wars. For banking crises, the Great Depression stands out, along with the unstable period in the 1980's and 1990's. The 2007-09 global financial crisis shows the most recent peak. However, it affected relatively few countries compared to the other two periods. Currency crises have become more frequent since the start of the 1900's, possibly due to the increasing relevance and development of international trade and capital markets, along with the collapse of the gold standard.

External debt crises appear to be particularly persistent and long-lasting, while the other types of crises are resolved much faster. Trebesch et al. (2012) shed some light on this and explain that domestic sovereign defaults are much easier to resolve than external defaults, as the legal process involved is much simpler. The authors note that recent periods of domestic debt restructuring were finalized within months. External debt restructuring, in contrast, can take several years due to its

[20]To account for potentially elevated standard errors due to collinearity in the subsequent regression models, I present not only models with all crisis variables combined, but also with all crisis variables separated.

[21]See the Supplementary Online Appendix for the relevant figures.

legal complexity, since there is no supranational legal framework to resolve such matters, and due to the fact that the restructuring process now also has to take currency mismatches into consideration. Furthermore, there are likely stronger incentives to resolve domestic debt problems as it also ultimately affects the domestic real economy and the domestic electorate, while external creditors play a less pivotal role in the domestic economy. Since debt restructuring was one of the defining criteria for external debt crises, these periods show up as being quite persistent. Unfortunately, there are few recorded instances of domestic debt crises (44 unique starting points for domestic debt crises, compared to the 173 external debt crises), making it difficult to compare the dynamics of the two in greater detail.

Stock market crashes appear to have become considerably more common since the early 1900's compared to the 19th century. This is most likely a consequence of the drastic change in scale and scope of equity markets, as more and more countries experienced industrialization and the accompanying growth of firm size and change in firm activity financing.

Overall, the changing frequency of the various crisis types warrants for a robustness check based on checking results for different temporal sub-samples.

2.6 Methodology

2.6.1 Hypothesis 1: Crises versus Executive Power

I make use of a Logit model to establish whether economic and financial crises make changes in executive power more *likely* to occur.[22] The regression equation takes on the following form:

$$ln\left(\frac{P(Y_{i,t}=1}{P(Y_{i,t}=0}\right) = \pi_{i,t} = \alpha + \beta X_{i,t} + \gamma Z_{i,t} + \delta D_{i,t} + \epsilon_{i,t} \tag{2.1}$$

In this equation, the probability of observing a positive outcome is given by $P(Y_{i,t}=1)$, while the probability of a negative outcome is denoted by $P(Y_{i,t}=0)$. As long as these two probabilities are non-zero, the odds-ratio is clearly defined. A large odds ratio represents a large probability to see a change in executive power compared to the probability of seeing no change. Similarly, a low odds ratio indicates that the probability to observe a change in executive power is low compared

[22]Under some conditions, linear probability models can provide an advantage over Logit when studying binary outcomes. In the case at hand a linear regression model would, however, too often predict meaningless values for the outcome variable. The Logit model circumvents this problem by changing the outcome variable to the log-transformed odds ratio, which has a meaningful range spanning all of $\mathbb{R}$.

to seeing no change.[23]

The regression equation includes dummy variables for each crisis type, denoted by $X_{i,t}$. The crisis types used here are inflation crises, banking crises, currency crises, domestic debt crises, external debt crises, stock market crashes and periods of negative GDP growth. The equation also contains the level of democracy measured one year before the outcome variable, the contemporaneous levels of real GDP per capita and real GDP growth, along with the literacy rate. These variables are denoted by $Z_{i,t}$. Finally, the regression model a dummy variable denoting whether there was a change in executive power one year in the preceding year, along with country-specific fixed effects, and decade-specific fixed effects.[24] These dummy variables are denoted by $D_{i,t}$ in equation 2.1.

2.6.2 Hypotheses 2 & 3: Direction of Institutional Change

I make use of a Multinomial Logit specification to determine whether institutional changes occur more frequently during times of crisis. The Multinomial specification differentiates upwards changes and downwards changes as separate outcomes. The outcome variable is defined as:

$$\Delta polity2_{i,t} = \begin{cases} 0 & \text{if} \quad polity2_{i,t} < polity2_{i,t-1} \\ 1 & \text{if} \quad polity2_{i,t} = polity2_{i,t-1} \\ 2 & \text{if} \quad polity2_{i,t} > polity2_{i,t-1} \end{cases}$$

If the *polity2* index has decreased from one year to the next by any amount, this outcome variable takes on the value 0. If it has remained unchanged, the variable is given the value 1 and if it has increased by any amount, the value is set to 2. The modality where $\Delta polity2$ is 0 represents all democratic breakdowns, regardless of how big or small they are. The modality in which $\Delta polity2$ is 2 denotes all increases in democracy, once again regardless of how big or small these increases are. The corresponding regression equation takes on the following form:

$$ln\left(\frac{P(\Delta polity2_{i,t}=j}{P(\Delta polity2_{i,t}=1}\right) = \pi^j_{i,t} = \alpha_j + \beta_j X_{i,t} + \gamma_j Z_{i,t} + \delta_j D_{i,t} + \epsilon^j_{i,t} \qquad j \in \{0,2\} \tag{2.2}$$

The right hand side includes all crisis variables ($X_{i,t}$). Furthermore, $Z_{i,t}$ once again denotes real

[23] Using the fact that $P(Y_{i,t}=0) + P(Y_{i,t}=1) = 1$, one can rearrange equation 2.1 to calculate the probabilities of observing a positive outcome. The expression can easily be shown to be $P(Y_{i,t}=1) = \frac{e^{\alpha+\beta X_{i,t}+\gamma Z_{i,t}+\delta D_{i,t}}}{1+e^{\alpha+\beta X_{i,t}+\gamma Z_{i,t}+\delta D_{i,t}}}$

[24] Though the inclusion of year-specific fixed effects is technically possible, such a specification proved to result in too many convergence issues.

GDP per capita, real GDP growth, the literacy rate and the level of the polity2 index lagged by one year. D now includes two dummy variables listing whether the preceding year featured a drop in *polity2* or an increase in *polity2*. There are two log-odds ratios to work with simultaneously. When j is equal to 0, the equation shows the odds of experiencing a democratic breakdown compared to a baseline of there being no change at all. If instead j is equal to 2, the equation shows the odds of there being a strengthening of democratic institutions compared to the baseline of there being no change in institutions. The parameters of interest are those that fall under β_j. The joint estimation of the Multinomial Logit model produces a set of coefficients for each non-baseline modality. These parameters are likely to differ. If all β_0 have the opposite sign from β_2, then economic and financial crises are associated with democratic breakdown *or* democratization *but not both*. If both are positive, then both democratic breakdowns and democratization happen more frequently during crises. Crises then undermine regime stability, but leave the precise direction of the outcome ambivalent.

Equations 2.1 and 2.2 include several dummy variables, including time-based (per decade) and cross-section based fixed effects. There is a wealth of literature on the dangers of simply adding such dummy variables to (Multinomial) Logistic models, as it can induce bias in the estimators due to the incidental parameters problem (Greene, 2004; Neyman and Scott, 1948). Increasing the sample size in the cross-sectional dimension results in the need to estimate a new variable for each modality. Consequently, such increases in the sample size do not result in consistent estimators. The estimator bias is instead shown to be inversely related to the time dimension ($O(T^{-1})$). Given the large time dimension employed in this study, the use of dummy variable fixed effects is entirely justifiable, as any remaining bias is negligible (Greene, 2004; Katz, 2001).

One aspect that I cannot address fully, but do not ignore, is the potential problem of endogeneity. It could be that political changes themselves result in economic and financial crises, producing a feedback process. This is elaborated upon further in section 2.7.3.

2.7 Regression results

2.7.1 Incumbent changes during times of economic and financial crisis

Changes in incumbents

I address the hypotheses presented in section 2.3 consecutively. Hence, the initial focus is on hypothesis 1, which is:

Hypothesis 1: *Changes in executive power are more likely to occur during times of crisis than in normal times.*

As elaborated upon in the methodology section, this hypothesis is addressed by running the following regression equation:

$$ln\left(\frac{P(Y_{i,t}=1}{P(Y_{i,t}=0}\right) = \pi_{i,t} = \alpha + \beta X_{i,t} + \gamma Z_{i,t} + \delta D_{i,t} + \epsilon_{i,t} \tag{2.3}$$

in which the parameters of primary interest are the β coefficients, as they measure the effects of the crisis variables (X).

Table 2.4 presents the results of regression equation 2.3. These results show that several crisis types are statistically linked to changes in executive power, in accordance with the above hypothesis. Models 1 through 6 include only one unique crisis type, while model 7 includes all different crises. The first 6 rows in each model represent, respectively, periods of high inflation, banking crises, currency crises, domestic debt crises, external debt crises and stock market crashes. The $Executive_{t-1}$ variable captures whether there was a change in executive power in the preceding year. $Polity2_{t-1}$ denotes the level of the *polity2* index in the preceding year. The variables GDP_t and $GDPGrowth_t$ represent the level of real GDP per capita and the growth rate of real GDP per capita. Finally, $Literacy_t$ captures the effect of schooling, via the literacy rate.

Models 1-6 reveal that periods of high inflation, banking crises and domestic debt crises have the strongest statistical association with contemporaneous changes in political leadership. The inclusion of all different crisis types simultaneously (model (7)) does not change this conclusion. This is in line with the idea of hypothesis 1, that executive power is undermined by economic and financial crises, resulting in a change of leadership. Hypothesis 1 refers to crises in general, but the results indicate that not all crisis types are statistically relevant. It therefore does not appear to be true that crises in general undermine political leadership, but only several kinds do.

Due to the nonlinear nature of the model, the precise impact of a covariate on the *probability* (rather than the log-odds ratio) to observe a change in executive power depends on the values of all other covariates. Hence, while the coefficient for inflation crises may be slightly bigger in model 7 than it is in 1, that does not automatically mean that the effect is larger. The coefficients merely indicate the marginal effect on the log-odds ratio of observing a change in executive power versus the baseline of there being no change. The positive sign of all coefficients shows that changes in executive power occur more often during times of economic crisis. With the exception of their sign, coefficients cannot be interpreted without taking all the other coefficients and covariates into consideration. The average marginal effect (AME)[25] can be derived from these coefficients. The

[25]In this case the AME is derived as $\Lambda(\eta A) - \Lambda(\eta B)$ where Λ is the logistic cumulative distribution function, A is the matrix of all covariates with the crisis variable of interest set to 1, and B is a copy of X, but with the same crisis variable set to 0. In other words, it is the comparison of two sets of predicted probabilities, one of which is based on there being no crisis in any observation and the other being based on there being a crisis in every observation.

Table 2.4: Logit regression on changes in Executive Power.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Inflation$_t$	0.302*						0.360*
	(2.16)						(2.04)
Banking$_t$		0.418**					0.398**
		(3.06)					(2.59)
Currency$_t$			0.168				0.0855
			(1.39)				(0.60)
Dom. Debt$_t$				0.618**			0.810**
				(2.65)			(2.75)
Ext. Debt$_t$					0.100		0.0133
					(0.75)		(0.07)
Stock Market$_t$						0.167	0.137
						(1.59)	(1.29)
Executive$_{t-1}$	-0.00637	-0.00821	-0.00200	-0.00615	0.00113	0.0641	0.0265
	(-0.07)	(-0.09)	(-0.02)	(-0.07)	(0.01)	(0.63)	(0.26)
polity$_{t-1}$	0.0493***	0.0495***	0.0496***	0.0481***	0.0494***	0.0667***	0.0645***
	(5.02)	(5.04)	(5.06)	(4.89)	(5.03)	(5.97)	(5.75)
GDP$_t$	-0.0313	-0.0305	-0.0340	-0.0319	-0.0351+	-0.0434+	-0.0274
	(-1.50)	(-1.46)	(-1.63)	(-1.53)	(-1.69)	(-1.82)	(-1.13)
GDP Growth$_t$	-2.896**	-2.689**	-2.918**	-3.000***	-3.058***	-3.143**	-2.456*
	(-3.21)	(-2.97)	(-3.23)	(-3.32)	(-3.40)	(-2.90)	(-2.23)
Literacy$_t$	-0.109+	-0.108+	-0.112+	-0.108+	-0.110+	-0.123+	-0.116+
	(-1.86)	(-1.85)	(-1.91)	(-1.85)	(-1.87)	(-1.89)	(-1.78)
N	4134	4133	4134	4134	4134	3218	3217
pseudo R^2	0.087	0.088	0.087	0.088	0.086	0.092	0.098
AIC	4252.1	4247.2	4254.8	4249.9	4256.2	3329.6	3317.2
APP—Exec$_t$=1 (%)	23.1	23.1	23.1	23.1	23.1	23.5	23.5

t statistics in parentheses
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
APP refers to the unconditional average predicted probability for observing a change in executive power.

AME reveals that, on average, the probability to see a change in the head of state during an inflation crisis (when compared to there being no crisis) is increased by approximately 5% in model 1 and 6% in model 7. For banking crises, this effect is approximately 7% in both models 2 and 7. Domestic debt crises show the strongest effect, increasing the probability of seeing a change in executive power by 11% in model 4 and 15% in model 7.

The high significance of the *polity2*$_{t-1}$ coefficients across all models reveals that political leadership changes more frequently in strong democracies than in weak ones. Given that the level of democracy is partly based on the level of political competition, this result was to be expected. High political competition implies that there are many candidates to choose from, and the system at hand also facilitates the election of different candidates. Non-democracies by definition have little competition, and generally lack the institutions that facilitate potential changes in executive power.

Interestingly, the coefficient for GDP growth is negative, large and significant across all specifications. This indicates that while poor economic performance, as may be the case during crises,

results in political leaders to be removed from office, good economic performance is 'rewarded' by incumbents staying in power for longer. Politicians therefore seem to be held accountable for their managing of economies in both good and bad times.

The unconditional average predicted probability for observing a change in executive power ranges from 23.1 to 23.5 between models 1 and 7. Of the crisis variables, domestic debt crises have the proportionally biggest impact on this average predicted probability. The incidence of a debt crisis adds, for instance, 15% to the probability of observing a change in executive power in model 7. The probabilities of observing a change in power are nevertheless generally not pushed to very high levels (or even above 50%), hence while inflation, banking and debt crises do act as an indicator toward changes in executive power, the outcome remains uncertain. The primary reason for this is most likely that crisis dummy variables do not provide information on the severity of the crisis in any given year. Crises generally involve brief moments of severe chaos, after which economies gradually revert back to more stability. The dummy variables list countries as being in a crisis until that stability has been fully reached, and thus encompass both the severe chaos (which intuitively should have the biggest impact on the political sphere) and the calming of the storm, which, intuitively, should not cause as much uproar.

The statistical relevance of inflation and debt crises is particularly interesting, as these are crises often directly caused by governmental mismanagement. Hyperinflation is often a consequence of imprudent fiscal and monetary policy. The observed association with changes in executive power may thus be a form of holding incumbents accountable.

Anecdotal evidence shows that there are many cases where the unsustainable debt levels definitely resulted in the replacement of heads of state. Examples include the 2001 Argentinian crisis, the 2009 sovereign debt crisis in Greece and the Venezuelan debt crisis in the 1980's. Banking crises result in the highest costs in terms of output (see e.g. Claessens et al. (2014) or Laeven and Valencia (2010)), hence there may be a large desire for discontinuity of existing policies, which corresponds to a switch of leadership. The results from table 2.4 suggest that the pattern persists beyond an anecdotal level, and that heads of state are changed more often during times of crisis. It is particularly interesting that external debt crises are so statistically insignificant, while domestic debt crises appear to play a much bigger role. It may be the case that domestic creditors play a bigger role in determining political leadership than external ones, and that they therefore have a bigger impact on electoral results. Naturally, leadership that proves incapable of repaying debt can then be more easily replaced.

2.7.2 Changes in democracy

The preceding section shows that economic crises coincide with political change, but it does not establish whether that ultimately affects the process of democratization as well. I run regressions on the multinary versions measuring change in *polity2*, where I make a distinction between democratization and democratic breakdowns.[26] The relevant hypotheses are:

Hypothesis 2: *Changes in democratic and autocratic institutions are more likely to occur during times of economic and financial crisis than in normal times.*

Hypothesis 3a: *Crises undermine and weaken democracy, resulting solely in a movement away from democracy.*

Hypothesis 3b: *Crises undermine both democracies and non-democracies. Non-democracies are pushed toward democracy, while democracies experience an institutional breakdown.*

These hypotheses are addressed via three sets of Multinomial Logit models, all of which are based on the same regression equation. These sets of models differ only in which sub-sample they consider. Hypothesis 2 and 3a are tested by running the Multinomial Logit model on the full sample of countries. Hypothesis 3b is instead addressed by running the same model on a sub-sample of democratic countries and a sub-sample of non-democratic countries. In the former, the hypothesis predicts that institutions are eroded, while in the latter, institutions should be strengthened. The regression equation for all hypotheses looks as follows:

$$ln\left(\frac{P(\Delta polity2_{i,t}=j}{P(\Delta polity2_{i,t}=1}\right)=\pi^{j}_{i,t}=\alpha_j+\beta_j X_{i,t}+\gamma_j Z_{i,t}+\delta_j D_{i,t}+\epsilon^{j}_{i,t} \qquad j\in\{0,2\} \tag{2.4}$$

Here, the coefficients of primary interest are the β coefficients, since these capture the effect of the crisis variables (X). There are now two sets of coefficients for all explanatory variables. One set denotes the effect on the odds of a democratic breakdown, while the other captures the effects on the odds of democratization. For both odds, the baseline of comparison is that there is no change at all. The results relevant to hypothesis 2 and 3a are listed in table 2.5, which spans two pages. The first half of table 2.5 shows the effects on the odds of there being a democratic breakdown, while the second half of table 2.5 illustrates the relation with democratization. Any positive coefficient indicates that the odds in favour of that outcome have increased (compared to the baseline).[27]

[26] Results were also calculated on the basis of the *polity2* sub-indices, as well as the Polyarchy measure for democracy. These results remain qualitatively similar. Unreported results are available from the author upon request.

[27] Note that this is not the same as saying that the probability has increased. It could be the case that the odds ratio increases for both modalities, but that the underlying probability has only increased for one modality. A positive coefficient nevertheless captures that the outcome in which institutions remain unchanged is unambiguously less likely.

The estimation results indicate that inflation crises are the most relevant crisis type, as seen by the coefficients in the top row of models 1 and 7 in both the first and second half of table 2.5. Other crisis types show the expected, positive sign, but do not reach a reasonable significance level. Inflation is associated with both increases and decreases of the *polity2* score. While there is a clear statistical relation between hyperinflation and institutional change, it does not point uniquely toward democratization nor democratic breakdowns. The coefficients for inflation crises do not differ statistically significantly between the democratic breakdown modality and the democratization modality. A t-test comparing the coefficients between the democratization and democratic breakdown modalities for inflation crises in model 1 produces a p-value of 0.95. A similar t-test for model 7 yields a p-value of 0.92. Despite the coefficients being insignificantly different from one another, there is nevertheless a slight difference in the AME. In model 7, the AME of inflation crises is 1.8% toward democratic breakdowns, and 3.1% toward democratization. Hence, during a time of excessive inflation, both democratic breakdowns and democratization are more likely to happen than a lack of institutional change, and democratization is slightly favoured. Though most of the crisis types are not significantly related to institutional changes, inflation crises do not coincide uniquely with democratic breakdowns. Hypothesis 3a is therefore rejected. Since democratic changes do occur more frequently during times of crisis, hypothesis 2 is, however, not rejected.

The lagged *polity2* variable indicates that countries with strong institutions are more likely to experience an institutional downturn, and are less likely to become democratically stronger. The opposite is true for countries with weak institutions, as measured by a low existing *polity2* score. This may partly be caused by the inclusion of observations at the two ends of the democratic spectrum: Full autocracies cannot go further down in their democratic score, hence any democratic change must be upwards. In other words, if a country has a *polity2* score of -10, then it cannot go any lower: It can only stay at the same level or go up. The opposite holds true for full democracies, which cannot democratize any further, but can by construction only experience a breakdown. Excluding the observations at the boundaries of the *polity2* distribution and re-running the shown models does not, however, alter the shown findings.

Table 2.5: Multinomial Logit regression results with *polity2* as the dependent variable. The first 6 rows of coefficients on this page and the next test the predictions of hypotheses 2, 3a and 3b. The last modality (anarchy and foreign occupation) is excluded from this table.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Modality: Decrease in overall level of democracy							
Inflation$_t$	0.529*						0.692+
	(1.97)						(1.94)
Banking$_t$		-0.344					-0.506
		(-1.01)					(-1.22)
Currency$_t$			-0.0211				0.0107
			(-0.08)				(0.03)
Dom. Debt$_t$				0.357			0.203
				(0.77)			(0.32)
Ext. Debt$_t$					0.338		0.209
					(1.29)		(0.57)
Stock Market$_t$						0.0161	0.0701
						(0.05)	(0.23)
Polity2$_{t-1}$	0.115***	0.116***	0.115***	0.113***	0.115***	0.120***	0.120***
	(5.13)	(5.20)	(5.18)	(5.07)	(5.17)	(4.35)	(4.24)
GDP$_t$	-0.215*	-0.255**	-0.247**	-0.238**	-0.234**	-0.161+	-0.110
	(-2.54)	(-3.01)	(-2.92)	(-2.84)	(-2.80)	(-1.67)	(-1.12)
GDP Growth$_t$	-4.287*	-4.757**	-4.617*	-4.613*	-4.668*	-4.010+	-3.851+
	(-2.39)	(-2.60)	(-2.51)	(-2.54)	(-2.57)	(-1.84)	(-1.81)

Continued on next page

Table 2.5 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Modality: Increase in overall level of democracy							
Inflation$_t$	0.508*						0.649*
	(2.39)						(2.32)
Banking$_t$		0.290					0.357
		(1.29)					(1.31)
Currency$_t$			0.295				0.232
			(1.49)				(0.93)
Dom. Debt$_t$				0.361			-0.961
				(0.98)			(-1.21)
Ext. Debt$_t$					0.118		-0.0884
					(0.57)		(-0.32)
Stock Market$_t$						0.172	0.139
						(0.82)	(0.65)
Polity2$_{t-1}$	-0.139***	-0.138***	-0.138***	-0.139***	-0.137***	-0.128***	-0.132***
	(-7.61)	(-7.58)	(-7.57)	(-7.59)	(-7.54)	(-6.13)	(-6.29)
GDP$_t$	-0.0525	-0.0613	-0.0591	-0.0619	-0.0632	-0.120*	-0.0843
	(-1.12)	(-1.31)	(-1.26)	(-1.32)	(-1.34)	(-1.96)	(-1.39)
GDP Growth$_t$	-0.256	-0.230	-0.225	-0.384	-0.415	0.871	1.470
	(-0.20)	(-0.17)	(-0.17)	(-0.29)	(-0.32)	(0.55)	(0.92)
Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Time Effects	YES	YES	YES	YES	YES	YES	YES
Dynamic model	YES	YES	YES	YES	YES	YES	YES
N	4092	4091	4092	4092	4092	3196	3195
pseudo R^2	0.214	0.212	0.212	0.212	0.212	0.257	0.266
AIC	2743.7	2749.8	2750.5	2751.3	2750.8	1908.8	1910.2
APP (%) polity2↓	3.26	3.29	3.28	3.28	3.28	2.84	2.84
APP (%) polity2↑	6.17	6.16	6.16	6.15	6.16	5.45	5.59

t statistics in parentheses
$+ \ p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

There is a notable difference in the effects of GDP growth. The first half of table 2.5 shows that democratic breakdowns coincide more frequently with periods of negative GDP growth, and less frequently when GDP growth is positive. This relation does not apply to the second half of table 2.5, where the GDP growth coefficients are very close to 0 and statistically irrelevant.

To check whether democracies respond differently from non-democracies, the sample is split up on the basis of polities' democratic score. Following the classification by Marshall et al. (2017), I label countries with *polity2* scores greater or equal to 15 as democracies and those with scores smaller or equal to 5 as autocracies. I then run the preceding Multinomial Logit model on these two sub-samples, with the intent to observe whether coefficients point in different directions. The corresponding results are shown in tables 2.6 and 2.7. Table 2.6 shows the relation between crises and institutional change for non-democracies and table 2.7 shows the relation for democracies. The primary use for these results tables is to compare the coefficients for all the crisis variables and see whether their signs and significance are comparable.

In table 2.6, only currency crises show up as marginally significant in model 7 for the decreasing democracy modality (the first half of table 2.6). Thus, this model suggests that only currency crisis have a destabilizing effect on democratic institutions. All crisis variables for this modality are insignificantly related in all models, suggesting that this link is very weak and the direction is very uncertain. For the increasing democracy modality (the second half of table 2.6), the results suggest that inflation crises are mostly associated with democratization in non-democratic countries. Domestic debt crises appear to have a significant, positive relation with the democratization process in model 4, but the coefficient and its significance disappear in model 7. The large coefficient for the lagged *polity2* level shows that countries that do not sit close to the bottom of the *polity2* distribution have the biggest chance of experiencing an institutional breakdown. Such countries essentially have more democratic institutions that can be eroded, whereas full autocracies have nothing to erode. Interestingly, the growth rate is accompanied by a very large, negative coefficient throughout models 1-6 in the lower half of table 2.6, suggesting that good economic performance can legitimize an existing regime, even if it has very poor democratic institutions.

The highly democratic countries display a different pattern. Table 2.7 shows that inflation crises are now only significantly related to decreases in the democracy index (the first half of table 2.7) in model 1, but not in model 7. Domestic debt crises are associated with democratic breakdowns in models 4 and 7, though this relation is only weakly significant. None of the crisis variables display any significant relation to increases in the democracy score for already democratic countries. Crises are more significantly associated with democratic breakdowns than with democratic upturns (shown in the second half of table 2.7). Note that despite the larger coefficients, the AME is quite very small. For instance, the AME of domestic debt crises is just over 3% in model 7. Though the estimated effect on the probability is small, it is worth noting that the crisis measure is quite

crude and that democratic changes are infrequent, rendering it difficult to measure the pattern. The average predicted probabilities at the bottom of table 2.7 highlight that institutional changes are particularly infrequent among democratic countries.

I conclude that there is evidence in favour of hypothesis 3b, as the relevant crisis types affect both democracies and non-democracies and these effects differ between the two sub-samples. Non-democratic countries tend to increase the strength of their institutions during times of crisis, while democratic countries do the opposite. There is therefore indeed an asymmetric effect on the democratization versus anti-democratization process, rejecting the view that crises only induce democratic breakdown.

In summary

Overall, there are three conclusions from these preceding results. The first is that I cannot reject the hypothesis that changes in executive power occur more frequently during times of crisis. Several crisis types are significantly related with changes in leadership. This suggests that leaders are held accountable for economic performance, and replaced in office more frequently during times of crisis. The second is that democratic institutions change more frequently during crises, with inflation crises having the strongest connection to such periods of change. Inflation crises are also related to changes in executive power, hence it may be the case that this crisis type results in the greatest demand for structural reforms next to changes in leadership. There is evidence that inflation crises undermine existing regimes and create a push toward institutional changes. Finally, crises play a different role in democracies than in non-democracies. Non-democracies experience a strengthening of their institutions during periods of high inflation. There is some evidence that democracies tend to experience a (partial) democratic breakdown during crises, with domestic debt crises playing the biggest role. Overall, institutional changes are more common among non-democratic countries than among democratic ones, and the statistical relation between (inflation) crises and institutional change is much stronger for non-democracies. While crises undermine both democratic and non-democratic regimes, they do not correspond solely with the removal of democratic institutions.

Table 2.6: Multinomial Logit regression results with *polity2* as the dependent variable, based on the sample of weak democracies. The last modality (anarchy and foreign occupation) is excluded from this table.

	Low *Polity2* score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Modality: Decrease in overall level of democracy							
Inflation$_t$	0.521						1.407
	(0.54)						(0.78)
Banking$_t$		-1.135					-1.110
		(-0.87)					(-0.44)
Currency$_t$			1.067				1.848+
			(1.53)				(1.68)
Dom. Debt$_t$				-14.52			-15.36
				(-0.01)			(-0.00)
Ext. Debt$_t$					0.384		0.0641
					(0.54)		(0.06)
Stock Market$_t$						1.625	1.273
						(1.08)	(0.77)
Polity2$_{t-1}$	1.690***	1.705***	1.701***	1.688***	1.713***	3.662***	3.603**
	(4.30)	(4.34)	(4.37)	(4.29)	(4.39)	(3.43)	(3.13)
GDP$_t$	-0.893	-0.919+	-0.889	-0.870	-0.884	-1.056	-0.980
	(-1.63)	(-1.68)	(-1.61)	(-1.60)	(-1.62)	(-1.09)	(-0.86)
GDP Growth$_t$	-1.606	-2.178	-1.127	-1.436	-1.563	2.929	9.990
	(-0.35)	(-0.47)	(-0.24)	(-0.31)	(-0.34)	(0.41)	(0.92)

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Table 2.6 – continued from previous page

	Low *Polity2* score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Modality: Increase in overall level of democracy							
Inflation$_t$	0.804*						1.003*
	(2.49)						(2.40)
Banking$_t$		-0.125					-0.000624
		(-0.30)					(-0.00)
Currency$_t$			0.341				-0.00847
			(1.05)				(-0.02)
Dom. Debt$_t$				1.406*			0.705
				(2.00)			(0.71)
Ext. Debt$_t$					0.505		0.0895
					(1.64)		(0.22)
Stock Market$_t$						0.266	0.245
						(0.73)	(0.66)
Polity2$_{t-1}$	-0.190	-0.186	-0.189	-0.184	-0.180	-0.211	-0.245
	(-1.43)	(-1.39)	(-1.41)	(-1.38)	(-1.36)	(-1.15)	(-1.32)
GDP$_t$	0.131	0.0980	0.111	0.132	0.127	0.242	0.343+
	(0.90)	(0.68)	(0.77)	(0.91)	(0.87)	(1.31)	(1.80)
GDP Growth$_t$	-5.195**	-5.624**	-5.243**	-5.042**	-5.557**	-6.663**	-2.413
	(-2.79)	(-3.00)	(-2.80)	(-2.71)	(-2.99)	(-2.73)	(-0.69)
Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Time Effects	YES	YES	YES	YES	YES	YES	YES
Dynamic model	YES	YES	YES	YES	YES	YES	YES
N	1405	1405	1405	1405	1405	1041	1041
pseudo R^2	0.220	0.215	0.217	0.219	0.217	0.263	0.285
AIC	1107.6	1113.0	1110.6	1109.3	1111.0	790.0	797.4
APP (%) polity2↓	1.70	1.70	1.70	1.70	1.70	1.67	1.67
APP (%) polity2↑	8.66	8.59	8.63	8.54	8.58	8.27	8.51

t statistics in parentheses
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.7: Multinomial Logit regression results with *polity2* as the dependent variable, based on the sample of strong democracies. The last modality (anarchy and foreign occupation) is excluded from this table.

	High *Polity2* score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Modality: Decrease in overall level of democracy							
Inflation$_t$	0.948*						0.674
	(2.52)						(1.33)
Banking$_t$		0.0748					0.233
		(0.15)					(0.39)
Currency$_t$			0.663+				0.0266
			(1.82)				(0.05)
Dom. Debt$_t$				1.009+			1.349+
				(1.65)			(1.81)
Ext. Debt$_t$					0.258		0.00222
					(0.66)		(0.00)
Stock Market$_t$						-0.165	-0.122
						(-0.40)	(-0.29)
Polity2$_{t-1}$	-0.281**	-0.320***	-0.303**	-0.320***	-0.317***	-0.357**	-0.334**
	(-2.90)	(-3.42)	(-3.20)	(-3.39)	(-3.38)	(-3.22)	(-2.73)
GDP$_t$	-0.0715	-0.0857+	-0.0774	-0.0799+	-0.0819+	-0.0369	-0.0195
	(-1.50)	(-1.82)	(-1.64)	(-1.70)	(-1.73)	(-0.76)	(-0.38)
GDP Growth$_t$	-6.595*	-7.671*	-6.813*	-7.617*	-7.696*	-10.50**	-4.580
	(-2.19)	(-2.54)	(-2.24)	(-2.52)	(-2.57)	(-3.03)	(-1.08)

Continued on next page

Table 2.7 – continued from previous page

	High *Polity2* score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Modality: Increase in overall level of democracy							
Inflation$_t$	0.404						0.199
	(1.16)						(0.41)
Banking$_t$		0.296					0.0205
		(0.81)					(0.04)
Currency$_t$			0.358				-0.0841
			(1.11)				(-0.18)
Dom. Debt$_t$				-1.043			-14.99
				(-1.00)			(-0.01)
Ext. Debt$_t$					0.0263		-0.321
					(0.07)		(-0.56)
Stock Market$_t$						0.0546	0.0133
						(0.16)	(0.04)
Polity2$_{t-1}$	-0.687***	-0.683***	-0.693***	-0.688***	-0.692***	-0.800***	-0.808***
	(-7.11)	(-7.08)	(-7.17)	(-7.18)	(-7.19)	(-6.67)	(-6.28)
GDP$_t$	-0.0456	-0.0511	-0.0459	-0.0556	-0.0521	-0.109*	-0.110*
	(-1.19)	(-1.35)	(-1.20)	(-1.47)	(-1.37)	(-2.13)	(-2.05)
GDP Growth$_t$	2.832	2.757	2.726	2.811	2.582	3.865	6.515*
	(1.07)	(1.03)	(1.06)	(1.03)	(0.96)	(1.41)	(2.13)
Fixed Effects	NO	NO	NO	NO	NO	NO	NO
Time Effects	YES	YES	YES	YES	YES	YES	YES
Dynamic model	YES	YES	YES	YES	YES	YES	YES
N	2268	2268	2268	2268	2268	1950	1950
pseudo R^2	0.191	0.185	0.188	0.188	0.185	0.235	0.252
AIC	969.2	977.6	972.1	974.4	977.8	722.9	740.4
APP (%) polity2↓	2.17	2.17	2.17	2.18	2.17	1.81	1.85
APP (%) polity2↑	2.87	2.86	2.86	2.85	2.86	2.53	2.52

t statistics in parentheses; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2.7.3 Alternative Specifications

Precedence

I run all preceding models with lagged crisis variables rather than contemporaneous ones. Such models test whether the economic and financial crises break-out before any changes in executive power or democratic institutions occur. Precedence makes for a stronger case that causality is unidirectional, and that crises are causing changes in leadership and institutions, rather than the other way around (which, with contemporaneous variables is impossible to prove). It is not clear whether the relation between crises and democratic institutional changes is a rapid one or takes time to appear. In other words, it may be that lagging variables by one year is too long of a period. The Arab Spring of 2011 was, for instance, only just preceded by high food inflation in both Algeria and Tunisia (among other, longer-existing institutional issues). It seems implausible that such inflation problems would have to persist for an entire year before there is any kind of social unrest. Another example is set by the riots and protests against Suharto, whose three decade reign ended very rapidly after the Asian Currency Crisis hit Indonesia. Here, too, it seems implausible that the economic crisis must already be ongoing for at least a year before it induces a change of leadership.[28] This paper aims to establish whether political and democratic changes occur more frequently *during* economic crises, for which contemporaneous data are sufficient. More granular data which better captures the evolution and severity of crises may help in introducing and determining the optimal lags to more rigorously establish the causal nature of the relation, but such data is not available for the current study.

The models on changes in executive power reveal that only banking crises remain highly significantly related, while inflation and debt crises lose their significant relation. That is, banking crises begin before there is a change in leadership. Though the loss of significance for debt crises does require further research, it may not be all that surprising. It is not uncommon for governments to struggle with debt, only to fully begin a restructuring process after a change in head of state. An anecdotal example of this is Argentina, which saw its bond yields soar throughout 2001, but only officially defaulted after changing heads of state at the very end of the year. Thus, while the country's debt problems began prior to the change in power, the crisis period only began once the debt restructuring commenced. It may thus be one of many data limitations that prevents the relation from showing up clearly.

In the case of the Multinomial models, the effect of crises is generally no longer statistically significant. If there is a causal relation between crises and political changes, the speed at which this relation manifests itself is unknown. Crises are diverse, with some being short-lived and minor, and others being persistent and heavily damaging. Capturing such diversity with only an annual binary variable is unlikely to produce the strongest results. If the response to crises is rapid, then

[28] In Suharto's case the crisis was, much like with the Arab Spring, not the only problem pushing the country towards a change of leadership.

an annual measurement of crises and political changes may be too infrequent to accurately capture their interrelation. When running the *polity2* regressions in binary form (where there is no distinction made between the directions of change), inflation crises remain significant throughout the various specifications, as they were in the contemporaneous regressions. That is, inflation crises start before any institutional changes happen, making it unlikely that the causality runs the other way around.

Dynamic Logit

I make use of a more extensive dynamic model, as presented by Kauppi and Saikkonen (2008) and Kauppi (2012), which includes the lagged log-odds ratio as an explanatory variable. The inclusion hereof should further capture the persistence of democracy and non-democracy. However, the models including this extra variable perform no better than the models shown in the preceding tables, and the results remain virtually identical. The lagged log-odds ratio does not improve the fit, and is entirely insignificant in all models.

Non-Random Sampling

To counteract the possibility of non-random sampling I use Inverse Probability Weighting to assign weights to observations based on how likely they are to make it into the final regression models (see, e.g. Wooldridge (2002, 2007)). Crisis data and CNTS variables appear to be more readily available for the more democratic and rich countries than non-democratic or poorer ones, hence such countries are more likely to end up in the final sample. Thus, despite the use of a global sample, the results may primarily be based on the history of the West rather than the world. Countries with low democratic scores are given higher weights in all regression models, based on their probability to make it into the final regression models. While it is not a perfect cure for potential non-random sampling, it can at least serve as an indicator whether results are too heavily dependent on how representative the sample is. I find that the use of such weights has no appreciable impact on parameter estimates and their significance in any of the models.

Sub-samples

The regression models are re-run emphasizing various temporal sub-samples, to check whether the estimated effects remain visible over time. These sub-samples include comparing the pre-WW2 to the post-WW2 era, and including dummy variables to account for the Cold War. For the models involving changes in executive power I find that some economic crises coefficients become marginally more significant when looking at a post-1945 sub-sample, but no new conclusions can be drawn. Re-running the regressions involving *polity2* suggests that the link between crises and democratic

change has become stronger over time, with the coefficients' sign remaining unchanged.

Magnitude of change

Additionally, I re-run the models on democratic change in which I consider only changes in the polity2 variable of 2 or greater, which indicate a more substantial change in a country's democratic and autocratic institutions and may be less prone to measurement errors. Despite the reduction in variation of the dependent variable, I find that using this stricter definition does not alter the results. I find that increasing the required polity2 score change to be at least 2 only marginally reduces the significance of inflation crises. Thus, even when the criterion for what constitutes a democratic change is made more strict, the statistical link with inflation crises remains largely unchanged. Increasing this cut-off point to 3 results in a further reduction of significance beyond an acceptable level. This most likely stems from the fact that increasing the threshold substantially reduces variation in the dependent variable, to the point where it becomes too rare of an event to base estimates on. Creating additional modalities for the $\Delta Polity2$ variable based on the magnitude of the polity change, and re-running the preceding models based on these modalities reveals that parameter estimates do not vary across the modalities and all magnitudes of institutional change are equally associated particularly with inflation crises.

Spill-overs

Finally, I check for regional spill-overs in democratization. After all, it may be the case that civil unrest and political changes in one country spark a similar situation in nearby countries, as could be observed during the 2011 Arab Spring. One can generate many arbitrary groups of countries based on cultural, economic and geographic factors. The grouping is based on present day membership of organizations such as the European Union, OECD, Arab League and ASEAN. Furthermore, I check whether democratic changes spill over to nearby countries within the same continent, but also find no evidence in favour of such spill-overs. Admittedly, this test for international spill-overs may be too simple to capture their existence. There are anecdotal cases throughout history where democratic change was incited by volatility in neighbouring countries, but these may have primarily been related to major democratic overhauls rather than the gradual cases studied in this paper, and thus appear to be isolated cases rather than the norm.

Reverse causality

Thus far, I have focused on crises bringing about changes in executive power and institutions, but the effect may also run in the opposite direction. Changes in leadership or institutions may spark new economic or financial crises. A sudden regime change may bring about uncertainty resulting

in economic or financial instability. Funke et al. (2016) make the argument that polarization or fractionalization contribute to slow recoveries in advanced economies. Their study finds that policy making and reforms become much harder in the wake of financial crises, thereby lengthening such crises. Linking this back to my own results, I see that being in a crisis state significantly contributes towards political change. While I have no measure of whether there is any continuity in the pursued governmental policies after crises, the potentially lengthened macroeconomic instability could result in further political turmoil. In short, there may be a feedback loop between crises and political change, where a crisis causes a change in leadership or institutions, which deepens the crisis, causing further leadership or institutional changes, and so forth. There is an unfortunate deficit in the empirical literature explicitly linking political stability to economic crises. In a recent study, Bertin et al. (2016) study this link by looking at political crises in African states since 1965. They find that financial crises have predictive power vis-a-vis political crises, but that there is weak predictive power between political crises and outbreaks of financial crises. Formulating a proper structural model that would allow us to more accurately deal with such endogeneity has, to my knowledge, not been done yet.

There are several statistical techniques available to address the potential endogeneity problem. Candidate model specifications or estimation methods are the Simultaneous Equation Model[29] (using 2SLS), or estimation via IV-Logit. However, each method relies on the availability of valid instrumental variables, which proves to be very problematic in the case at hand. Suitable instruments must meet several critical conditions: They must be correlated with the endogenous variables and outcome variable, but must be uncorrelated with the error term and have no meaningful causal relation to the outcome. In the case at hand, the endogenous variables are the binary crisis variables. Lagged observations are typical candidates as IVs, but here they would overfit the endogenous variables and not eliminate the potential bias. First-differenced observations are plausibly exogenous, but are only very weakly correlated to the endogenous variables. Outside of these inapplicable IV candidates, I find no suitable variables that meet all the requirements. As such, this paper cannot fully address endogeneity with the data at hand.

To nevertheless get an indication of how problematic endogeneity by potential reverse causality may be, I perform an alternative, simple test. I generate a dummy variable for each crisis type denoting whether a year featured the start of a specific economic crisis type or not. I then make use of a Logistic regression model and regress these dummy variables on the incidence of executive power changes with their corresponding lagged versions. Furthermore, I include GDP growth and the level of GDP in the regression. I find that preceding changes in executive power have no significant relation to the emergence of new crises states, with the only exception being banking crises. Of the 258 observed starting points of banking crises, there are 62 cases where the emergence of a

[29]This would sketch the clearest picture of how the relation may run both ways, but does require certain parametric assumptions prior to any estimation.

banking crisis is directly preceded by a change in executive power. Thus, there may be a feedback process between banking crises and changes in executive power. A more thorough study of this specific feedback loop is left for future research.

Non-democratic regimes are found to feature relatively many debt crises compared to democratic ones, suggesting that the absence of democratic institutions increases the likelihood of observing such crises, which could then feedback into more political instability. I find, however, no pattern indicating that democratic changes directly precede economic crises. As was shown in the descriptive statistics, most democratic changes are quite minor in nature, which makes it unlikely that they will have large destabilizing effects on the real economy or financial markets. As such, I deem the reverse causal link unlikely, but leave the topic for future research.

I cannot exclude the possibility that *expectations* of forthcoming political, democratic or autocratic changes affect the economic and financial system. Elections generally take place at regular intervals, and incumbents often have a term limit. As such, a potential change in power can be pin-pointed to a quite specific date well in advance. Markets may start performing better due to the expectation of a politician proposing favourable macroeconomic policies taking power in the near future. Similarly, the expectation of a discontinuation of favourable policies may hamper economic performance and spark a crisis. This seems particularly relevant for banking, stock market and currency crises as the values of the various relevant assets largely depend on future expectations, but may also play a role for debt crises. Hyperinflation is primarily caused by major commodity price shocks or inappropriate monetary policy. While expectations can play a major role in determining the inflation rate, it seems implausible that expectations could suddenly bring it up to hyperinflation levels without there being a major change in economic fundamentals. Without such a major change in economic fundamentals, expectations likely make inflation persist at a high level rather than causing it to suddenly jump from a low to a very high level.[30] Inflation crises show the strongest statistical relation with political and democratic changes throughout the various regression models run. As expectations probably play the smallest role for sparking such crises, and inflation crises generally precede political and democratic changes, it seems most reasonable that the causality runs in one direction.

2.7.4 Discussion of findings

In this subsection, I briefly summarize the main takeaways. First, changes in executive power occur more frequently when countries are in a crisis state. These changes in political power may reflect the societal need to alter macroeconomic policy in response to such crises. These findings are in line with the results of Funke et al. (2016), who show that financial crises result in an in-

[30] Cagan (1956) highlights that periods of (severe) hyperinflation can be driven in large by momentum, via adaptive expectations. Such momentum, however, generally requires a shock or imprudent monetary policy to get going.

crease in civil dissatisfaction and political polarization, with voters turning to more populist parties.

When looking at changes in democratic and autocratic institutions, I find that economic crises provide an environment that induces not only a change in political leadership, but also facilitates changes in democratic institutions. In countries with high democratic scores, such institutions are eroded, resulting in (minor) democratic breakdowns. In countries with low democratic scores, the opposite happens and crises foster democratization.

The findings have a relation with Modernization Theory and with Przeworski's theory. With regard to the former, it shows that developed, rich countries may nevertheless experience democratic breakdowns. The idea that full democracy is an inevitable, persistent outcome of economic development is thus too blunt. Countries with strong existing institutions do exhibit a major resilience against institutional breakdowns, but such breakdowns are still facilitated by the incidence of economic crises.

While Przeworski does not argue that crises play a pivotal role in spurring democratic changes, it does fit his ideas that democratization can take place independently of economic development. Democratization among non-democratic countries occurs more frequently during unexpected shocks, but is negatively related to economic growth. Good economic performance can legitimize an existing autocratic regime and does not automatically result in a stronger push toward democracy.

The conditional tendency towards both democratization and democratic collapse supports the view that crises are regime undermining, but rejects the idea that they only induce democratic breakdown. What was beyond the scope to address in this paper is what the actual consequences are of such democratic changes. The tendency towards democratization suggests that it may be seen as either a suitable way of addressing ongoing crises, or to provide a framework that prevents future crises from happening. Full democracies, however, do not have such an option, as there is little room to further strengthen democratic institutions. With democratization not being a feasible commitment, those disenfranchised by poor democratic performance may turn instead to populist proposals as an alternative (see also Oesch (2008) for a more detailed study). That is, if what could be called conventional democratic leaders fail to produce the desired results, then there is no incentive to maintain not only the status quo in leadership, but also the type of leadership. Economic and financial crises do not play a unique role in this regard, but perhaps one of the most visible ones. Other factors, which may display large dynamics during crises, may generally undermine heads of state. Consider, for instance, the seminal findings of Piketty and Saez (2014), who show that inequality has been on the rise in developed, highly democratic countries since the 1960s. A common view of democracy is that it is redistributive, yet its performance in that regard appears to be lacking. With such rising inequality and in the wake of the 2007-2009 Global Financial Crisis, followed by the European Debt Crisis, it should thus come as no surprise that conventional politics

is being rejected.

In non-democracies, the emergence of a crisis can spur democratization due to a reduction of the costs of revolting against the incumbent. As political power tends to be primarily held by the economic elite, the disenfranchised are generally poor and any repression will be relatively painful. During crises, however, there is less to expropriate and less to lose for this group, hence the opportunity cost of demanding a change is reduced. Consequently, repression becomes less effective and incumbents must make way for institutional changes. The fact that inflation in particular stands out as the most relevant crisis fits this idea well: High inflation is very costly for all inhabitants of a country, rather than being limited to a select group (as would be the case with, for instance, stock market crashes), and is much more likely to create dissatisfaction among a large group of citizens. The more widespread the dissatisfaction is, the more repression has to be used, or the more likely incumbents are to partly yield their power.

2.8 Concluding remarks

This paper fills part of the empirical void on democracy by studying the political consequences of economic and financial crises. My contribution is to establish whether economic and financial crises undermine regimes and political leadership. Secondly, I establish which types of crisis are the most relevant and warrant for further studies.

The findings indicate that economic crises coincide with changes in the democratic nature of countries and result in political instability. Changes in executive power occur more frequently during times of crisis with banking crises playing the biggest role. Furthermore, I find that while democratic and autocratic institutions are very persistent, they change more frequently during times of crisis. I observe that there are differences between democratic and non-democratic nations. Democracy is self-sufficient and self-sustaining by design, allowing the electorate to change incumbents without completely altering the existing framework. Mismanaging politicians can be replaced via elections, thus obviating the need to change democratic institutions. Nevertheless, such times of change are associated with weakened democratic institutions. In contrast, non-democratic countries featuring weak institutions often move towards being more democratic during times of crisis. Inflation crises in particular stand out as being the most relevant. As high inflation is often the result of poor macroeconomic policy and the costs of high inflation are not limited to only a sub-population of a country, their incidence undermines regimes and fosters structural changes.

These results have important policy implications. The primary one is that economic and financial crises can act as indicators for impending democratization for weak democracies, and weakened institutions for already established democracies. This result is useful for organizations promoting in-

ternational economic development, cooperation and coordination. While economic damage caused by crises warrants much attention in itself, the destabilizing effect of crises may have persistent effects when it results in democratic collapse. At the time of writing, the COVID19 pandemic is wreaking worldwide havoc. Events such as this are likely to cause major economic damage, which can then undermine Western democracies. A swift resolution of any potential economic crisis is important to prevent such democratic decay.

I speculate that the inability for full democracies to democratize even further creates room for populism to gain popularity. Democratization cannot be used as a commitment tool for preventing or managing crises better, hence a change in executive power may be accompanied by radicalization and polarization.

The empirical gap surrounding democratization is still large, hence the study leaves many venues for further research. One aspect I am unable to cover fully is the potential existence of feedback loops and endogeneity. Though I find an indication that crises almost always precede democratic change, there may be further dynamics that affect democratic consolidation. Another point of interest is that there is no clear theoretical nor empirical information yet on what the underlying mechanisms are connecting the crisis types to both political and to democratic changes. I speculate that the government's direct involvement and responsibility in causing such crises is the primary reason for this strong link, but it would be beneficial to develop the theory behind this connection further. Thirdly, I am not able to assess the role that the severity of crises plays. Knowledge hereof would enhance the policy implications behind the findings in this paper.

Chapter 3

The Relation between Democratic Changes and Income Redistribution

3.1 Introduction

The idea that democracy promotes income redistribution is not a new one. Since the days of Aristotle, it has been argued that democracy functions best when income inequality is low and that for democracy to persist, it must thus redistribute income. Modern economics has taking this as a starting point and developed it further, introducing a wide variety of theoretical models explaining the connection between democracy and income inequality (Meltzer and Richard, 1981; Acemoglu et al., 2015; Roemer, 1998; Acemoglu and Robinson, 2001b). A common element in all models is that democracy is a system that favours the poor. Under a democracy, the poor have influence in determining the level of taxation and redistribution. Therefore, they make use of it to alleviate their poverty by redistributing income from the rich to themselves. Opposition against democracy primarily comes from the economic elite, who incur the costs of the system without reaping the benefits. The theoretical prediction is that any extension of democratic rights goes toward the less economically privileged, and that these less privileged increase income redistribution. Hence, post-tax income inequality could be inversely related to the level of democracy, as democracy expands the possibility of countering inequality via greater income redistribution. Empirical evidence is much less clear on how democracy, income inequality and income redistribution are connected, however, with evidence pointing in a wide variety of (often conflicting) directions. At present, there is no conclusive evidence whether democratic changes influence income inequality nor income redistribution.[1]

This paper aims to contribute to the literature in two ways. Firstly, the paper applies a considerably different methodology compared to the existing literature. The paper makes use of

[1]Note that the two terms are not synonymous. Income redistribution refers solely to government policies that directly alter the income distribution. Income inequality captures the resulting distribution, which is only partly determined by such redistributive policies, but depends on many other factors.

Propensity Score Matching, in which moments of democratization and democratic breakdowns constitute "treatments". The matching procedure provides estimates on whether democratic changes affect income inequality and income redistribution within a short time period (2 years) or long time period (6 years). This methodology resolves several major issues that the existing empirical literature has not yet addressed.[2] Two such issues are that the empirical results are typically based on an unrepresentative sample of the world's, which makes existing results difficult to generalize, and that democratic changes are rare events. Both issues can have a major impact on the accuracy and efficiency of parameter estimates. Secondly, while other research on democratic developments tends to emphasize large democratic changes, this paper also includes smaller changes. Most measurements of democracy show that changes are quite gradual, and that focusing solely on large dynamics ignores a lot of variation in an otherwise persistent variable.

The primary finding is that democratization does not have any measurable impact both in the short and long run. Both the process of democratization as well as democratic breakdowns are not found to have any robust statistically significant connection to changes in pre-tax inequality or post-tax inequality. Pre-tax income inequality captures income inequality *before* any taxation and income transfers, whereas post-tax income inequality captures income inequality *after* any taxation and transfers. Redistribution is by definition the gap between pre-tax and post-tax inequality. Hence, if both of these inequalities are unaffected, so too does redistribution remain the same. This relation is absent regardless of the magnitude of the democratic change considered. Similarly, the extent to which a democratic change is permanent and consolidated plays no detectable role in the absent relation between democratization, democratic breakdowns and income inequality. The findings of the paper therefore find no evidence in favour of the existing theory connecting democracy to redistribution.

The paper is structured as follows. Section 3.2 introduces the existing theoretical background on the topic along with key empirical results from existing literature. Section 3.3 sets out two testable hypotheses based on the reviewed literature. After that, section 3.4 describes the data gathered, after which section 3.5 shows key statistics. Next, in section 3.6, I discuss the methodology, followed by the empirical results in section 3.7. The paper further discusses and interprets the results and is wrapped up with brief concluding remarks.

[2]No methodology is without its limitations, hence both the upsides and downsides of PSM are discussed in section 3.6.

3.2 Literature Review

3.2.1 Theoretical background

The theorized connection between democratization and changes in income inequality dates back far in time, but gained prominence with the works of Lipset (1959) and Lenski (1966).[3] The main premise Lipset and Lenski put forth is that in the absence of democracy, it is mostly the economically rich who also have major political influence. The economically rich make use of this influence to ensure that their wealth grows as much as possible, generally at the expense of the lower economic classes. Democracy is a framework which allows for the lower economic classes to also exert political influence, so that political power is not limited to the elite. Under a functioning democracy, a poor electorate is given the opportunity to act in its self-interest and to improve its own quality of life, by demanding policies that favour the poor. One natural demand the non-elite can make is to push for more equality and redistribution of income, by taxing those at the top of the income distribution. A switch from non-democracy, in which the elite hold all the power, toward a democracy, in which both the rich and poor have power, is thus expected to bring about a reduction in income inequality. Democracy therefore goes hand in hand with a widening between pre-tax income inequality and post-tax income inequality. The extent of income redistribution from the rich toward the poor is thus defined by the size of the gap between pre-tax and post-tax income inequality.[4]

This line of thinking is formalized by Meltzer and Richard (1981) in the form of an economic model (hereafter referred to as the MRF, or Meltzer-Richard Framework). In this model, society is split into a group of the economic elite, and a group of the economic poor. Each individual's income is taxed via a proportional tax, and all tax revenue is used solely for redistributive purposes. The tax rate, and hence the extent of redistribution, is based on what the electorate votes for. In such a scenario, it is the median voter who decides the implemented tax rate.[5] In the case where voting rights are limited to the economic elite, the preferred tax rate is low, as the median voter is part of the elite. Naturally, the elite have no intention to altruistically redistribute their income without getting anything in return. When voting rights are extended to the economically poorer, the median voter also becomes poorer, and hence the preferred level of taxation increases. The more voting rights are extended to the poor, the lower the median voter's income is and the higher taxation and redistribution are. The model unambiguously predicts that democratization results

[3]There is also a parallel discourse on the topic of wealth inequality, which has gained prominence with research by e.g. Piketty and Saez (2014). The current study focuses on income inequality, however, as that is also the focal point of existing theory. Additionally, data on wealth inequality is considerably more limited than on income inequality.

[4]The distinction between the two types of income inequality and redistribution is important. If post-tax income inequality falls, this could be because pre-tax income inequality falls, rather than via increased redistribution. Similarly, it is possible for post-tax income inequality to increase, despite increases in income redistribution, if pre-tax income inequality rises *more*. Keeping track of both pre-tax and post-tax inequality reveals what the driving forces are behind any changes.

[5]The logic behind this is essentially the same as Hotelling's Law. Rather than determining product differentiation, it instead determines tax policy differentiation, with one tax rate ultimately proving to be the most popular.

in a bigger gap between pre-tax and post-tax income inequality. The framework also predicts what happens in the case of democratic breakdowns. Rather than extending voting rights to the poor, a democratic breakdown limits voting rights to the economic elite. This implies that the median voter's income increases, and that the preferred level of taxation is lower, resulting in lower redistribution. While democratization is accompanied by lower income inequality, democratic breakdowns should thus result in higher income inequality.

This framework has formed the dominant theoretical basis in the literature connecting democracy to income inequality. There are numerous extensions to the model, of which those by Alesina and Rodrik (1994) and Persson and Tabellini (1994) are two notable ones. These extensions endogenize economic growth and create a trade-off between taxation and growth. Income is partly devoted toward capital investments, which drive economic growth. Similar to the MRF, higher taxation results in higher redistribution, but also reduces economic growth. While the metaphoric pie may be divided more evenly, it does not grow as rapidly, hence high taxation may not be unambiguously preferred by the economic poor. Voters thus have to solve an optimal control problem to determine what the preferred level of taxation is. Similar to the MRF, the median voter is also the decisive voter, and democratization is defined as extending voting rights to the economically less fortunate. Despite the introduction of the trade-off between growth and taxation/redistribution, the models nevertheless conclude that democratization results in higher taxation and higher redistribution. Once again, the results are symmetric and democratic breakdowns simply mirror the process of democratization: Breakdowns result in a higher income of the median, decisive voter, resulting in lower taxation and lower income redistribution.

Though the MRF and its derivatives use voting rights as the sole, defining feature of democracy, the logic can be easily extended to other aspects of democracy. There exists no consensus on what the precise definition of democracy is, the vast majority of definitions emphasizes two elements: (1) Incumbents are held accountable and are replaceable via a process of elections and (2) there is an ability to freely organize against or critique politicians. These two elements extend well beyond only voting rights. At the time of writing, voting rights are common throughout both democracies and non-democracies.[6] The process of democratization or democratic breakdowns therefore goes beyond mere changes in voting rights, but must include other features of democracy. For instance, an increase in political competition allows for more ideologies to be represented. Such increased competition therefore generally provides more room for politicians who favour redistribution than there is under a non-competitive election process.[7] A reduction in political competition naturally has the opposite effect, and results in an entrenchment of ideologies of those with political influence (which tend to be the redistribution-disapproving elite). Changes in accountability determine how strongly desired policies can be changed by an incumbent in office. Lower accountability lets lead-

[6] See for instance Egorov and Sonin (2018) for a thorough explanation on why autocrats hold elections.

[7] Roemer (1998) presents a clear case of why a lack of ideological representation may result in an inability to implement policies that are broadly desired. See also Hibbs Jr (1977) for an introduction to Partisan Theory.

ers renege on their promises, while high accountability ensures that leaders must go through with promised policies. Higher accountability therefore results in the electorate's redistributive policies being more accurately represented, whereas lower accountability likely results in a departure from such accurate representation. While voting rights are an essential component of any democracy, the extent to which voting rights result in materialized policies is equally if not more important. The more democratic institutions are in place that eliminate voter suppression, provide political competition and a choice between different ideologies and policies, the more effective voting rights are. While the existence of voting rights as used in the MRF is too simple of a representation of democracy, a re-interpretation of voting rights as *effective* voting rights reconciles the extended definition of democracy with the MRF.[8]

Acemoglu and Robinson (2001b) add to the theory the idea that inequality itself is a driving factor behind pushes for democracy. If the poor consider income inequality to be excessive, they can threaten to overthrow the economic elite via a revolution and install a democratic government. The main aim of such a democratic government is to increase income redistribution. The costs of a revolution are large for the elite, hence it is in their own interest to concede some of the political power, and to accept the demands for higher taxation and redistribution. If income inequality is very high and the resulting post-democratization increased taxation is too costly for the economic elite, they can also launch a coup and overthrow any attempts at there being a democratic government. Consequently, democracy may be too short-lived to address any redistributional concerns, keeping the income inequality conflict alive. Acemoglu and Robinson (2000) discuss that the extension of the democratic franchise toward the poor in Britain, France, Germany and Sweden was precisely an effort to stave off revolutions, which were fuelled by the dissatisfaction caused by high income inequality. While the MRF states that democracy reduces income inequality, income inequality itself may also be driving factor behind a push for democracy.

Several frameworks suggest that income inequality may also rise after a period of democratization. Kuznets (1955) argues that countries in the early stages of development first experience a rise in income inequality as they democratize and develop. The primary reasoning behind this inverted relation is that democracy is conducive to economic growth, but that not everyone benefits equally from higher economic growth. As countries reach a high level of development, accompanied by a higher level of democracy, there are fewer stragglers, resulting in a more egalitarian income distribution.

Similarly, Perotti (1993) argues that the prevailing income distribution determines to what ex-

[8] Acemoglu and Robinson (2006) use a similar line of reasoning to distinguish between *de jure* and *de facto* democracy. The elite typically have access to extensive economic resources, with which they can circumvent the legal (*de jure*) democracy and maintain control (resulting in a weak *de facto* democracy). Chong and Gradstein (2007) similarly emphasize that institutions cannot be studied without taking their quality into consideration. Low quality results in their circumvention, while high quality lets institutions fulfil their purpose.

tent agents are able to invest in human capital, which is the key determinant of future income. Democratization may initially provide more redistribution, via the same median voter mechanism as in the MRF. This redistribution results in some income groups gaining the ability to invest in human capital, which results in these groups moving up in the income distribution. Eventually, a middle class emerges, after which the median voter is no longer poor and redistribution is no longer desired. The effects of democratization may thus only be short lived. Galor and Zeira (1993) present a similar framework, in which redistribution can help overcome an obstacle in initial investment in profitable projects, but where redistribution is no longer as desirable with the gradual rise of a middle class. Consequently, democratization may initially increase both the pre-tax and post-tax level of income inequality, with there being an initial big difference between the two inequality measures, but that pre-tax and post-tax income inequality eventually converge to a similar level.

While there may be different consequences for the overall level of pre-tax and post-tax income inequality across the above models, the underlying mechanism is always that increases in democracy allow for more income redistribution to take place. The primary difference is whether this redistribution also has a long-run impact on the pre-tax income distribution or not.

3.2.2 Empirical background

Next to the theoretical literature, there have been several attempts at determining whether the predictions hold up empirically. An early literature review by Sirowy and Inkeles (1990) reveals that half the surveyed literature finds that democracy and income inequality are significantly related, while the other half finds no relation at all. For instance, Muller (1988) finds that democracy is negatively related to income inequality, while Bollen and Jackman (1985) find that the two are not statistically related. These early empirical studies were severely limited by the availability of data. Nevertheless, improved availability of data has not provided more conclusive results. The more recent literature still clashes on whether democratic institutions drive redistribution. As pointed out by Savoia et al. (2010), the MRF-based idea that democratization reduces income inequality does not go unchallenged. Gradstein and Milanovic (2004) survey recent papers and conclude that democracy and income inequality are inversely related to one another. Gradstein and Milanovic (2004) argue that democracy must first be consolidated before it can have any effects on redistribution, and that democratic changes take time to show their full effects. In other words, the effects of democratization on income inequality may require a long time to appear. In contrast, after extensive testing, Acemoglu et al. (2015) find no robust relation between democratization and income inequality, though they do find that government spending is positively related to democracy. This suggests that budgetary concerns and taxation are not constraining the possible level of income redistribution, but that redistribution is simply not a pursued policy. Scheve and Stasavage (2007) suggest that inequality developments throughout the twentieth century can largely be explained by events such as crises and wars rather than institutional changes.

The aforementioned literature focuses on income inequality rather than income redistribution. Though the two are closely related, these terms are not synonymous. It may very well be, for instance, that post-tax income inequality has risen, despite increases in income redistribution. This would be the case if pre-tax inequality has increased rapidly, and the increase in redistribution was not sufficiently large to offset it. Most of the existing empirical research has not explicitly tested the predictions of the MRF about redistribution.

3.2.3 Possible extensions and limitations

There is a rich empirical and theoretical Public Choice literature featuring alternatives to the Median Voter Theorem, which is central to the MRF, or highlighting the limitations of its simplest version and the conclusions it can generate. Among others, the MRF assumes that the policy space is uni-dimensional, whereas policies may come in packages. Furthermore, voters have imperfect information, and must spend effort to acquire information on redistributive policies (Larcinese et al., 2002). Lack of awareness of redistributive options is likely to result in a gap between voters' desired redistribution versus realized redistribution. Additionally, voters may base their redistribution decisions not only on economic self-interest, but also on the basis of *inter alia* social identity (Lindqvist and Östling, 2013), group power dynamics (Acemoglu and Robinson, 2001a), ideology (Bonilla, 2004) or a sense of altruism (Simon, 1993; Widerquist, 2003). Finally, governments may struggle to implement desired policies due to limitations in state capacity (Besley and Persson, 2009, 2010; Bäck and Hadenius, 2008).

The above provides a great deal of possible extensions and alternatives to consider for the MRF. However, with its mixed results, the extant empirical literature has not yet determined the validity of the baseline MRF. As such, the current study focuses on determining the merit of this baseline MRF rather than its multitude of possible extensions, leaving these extensions for future research. An additional limitation to the inclusion of more nuanced mechanisms highlighted in the Public Choice literature is that data availability is considerably more limited. Since democratic changes are rare events, and data are already generally limited for much of the non-Western world, this paper therefore focuses on improving our understanding of the merits of the basic MRF.

3.2.4 Main takeaway

To summarize, much of the theoretical literature primarily posits that democratization should go hand in hand with a reduction of post-tax income inequality, due to increases in redistribution. Some theory predicts that redistribution may only be a temporary phenomenon, resulting in the effects of democratization no longer being visible in the long-run. Public Choice literature highlights that there are numerous mechanisms that may complicate the relationship between democracy

and income redistribution. The results from the empirical literature are mixed, with some researchers finding that income inequality and democracy are entirely unrelated, and others finding that democracy is inversely related to income inequality. Little emphasis has been put on the role of redistribution thus far, hence the accuracy of the MRF has not yet fully been addressed empirically, leaving an hole in the literature to be filled.

3.3 Hypotheses

The existing literature described in the preceding section makes several testable predictions about the consequences of changes in democratic institutions. Since the MRF and its derivatives play a dominant role in the literature, I derive two hypotheses based on these frameworks, to determine whether the frameworks hold up to empirical scrutiny. First and foremost, the MRF makes an unambiguous prediction that increases in the level of democracy go hand in hand with increases in the amount of redistribution. An extension of effective voting power toward poorer parts of the population, as is typically the case with democratization, results in the interests of the poor being better represented. If increasing the tax burden for the rich and increasing redistribution toward the poor is feasible, then the poor will use of their newly gained political influence to push for policies that make such redistribution happen. The first hypothesis can therefore be formulated as:

Hypothesis 1: Democratization results in an increase of income redistribution (an increase in the gap between pre-tax and post-tax income inequality).

Note that this hypothesis does not make any predictions on the direction of change for pre- nor post-tax income inequality, but focuses on changes in the gap between these two measures instead. As pointed out in section 3.2, it is possible that democratization indirectly results in increasing both pre- and post-tax income inequality. If post-tax income inequality does not rise as rapidly, however, it implies that redistribution is nevertheless increased, possibly to counter such indirect effects.

Though the MRF and its derivatives do not devote much explicit attention to it, the models are symmetrical in nature. That is, democratic breakdowns have the opposite result of democratization. When the poor lose political influence, their interests are less represented by politicians, and their preferred policies are less likely to be pursued. The median decisive voter's income is higher after a democratic breakdown. As such, the economic elite once again has a bigger role in determining the realized policies, which are likely to be based on a reduction of taxes for the rich and a reduction of redistribution toward the poor. Therefore, the MRF predicts that:

Hypothesis 2: Democratic breakdowns result in a reduction of income redistribution (a decrease in the gap between pre-tax and post-tax income inequality).

The primary outcome of interest for both hypotheses is whether pre-tax and post-tax income inequality develop differently after democratization or democratic breakdowns, and whether the gap between these inequality measures widens or becomes narrower.

3.4 Data

3.4.1 Democracy

I construct a yearly panel of 172 countries, starting in 1960. Due to gaps in data, primarily for developing nations, this panel is unbalanced. Data on democracy comes from two separate datasets: The PolityIV database, and the Freedom House database.[9] PolityIV provides an index within a range of -10 to 10 denoting to what extent a country features democratic and autocratic institutions. A score of -10 indicates that a country is entirely authoritarian, while a score of 10 indicates that a country is fully democratic. This index is already discussed at greater length in section 2.4 of chapter 2. As the usage and interpretation of this index are unchanged, a repetition of its discussion is omitted here. The *polity2* index is an industry-standard measure of democracy throughout the literature, and it serves as the first candidate for measuring democracy here.

The Freedom House data provides measures of formal political rights and civil liberties from 1972 onward. Both political rights and civil liberties are represented along a 7-point scale, where a score of 1 indicates strong political rights or robust civil liberties, while a score of 7 indicates that there are strong limitations to political rights or civil liberties. Contrary to PolityIV's *polity2*, these scores for political rights and civil liberties are not based on any clear set of sub-indices or criteria. Freedom House documents and publishes lengthy reports on countries' major developments in political rights and civil liberties, but it does not make clear how these developments translate into any score changes. The reports emphasize a very broad range of countries' features, which are shown to have major implications for the functioning of democratic institutions. An example of such a feature is the way in which protests are responded to. A repressive response results in a poorer scores for political rights and civil liberties, though it is unclear how strong the effect is. Another feature affecting both political rights and civil liberties is whether the voting process in elections is hampered by terrorist activities. Even if the legal institutions in place are democratic, terrorism may render these ineffective, so that the *de facto* level of democracy is limited. Despite the somewhat obfuscated scoring methods, Freedom House data is a second industry standard next to *polity2* in empirical studies on democracy.[10]

[9] The primary reason for using two data sources is that there is no unanimously accepted measurement of democracy. While democratic indices may correlate very strongly with one another, they may capture entirely different elements of democracy, all of which may be relevant. See Bollen and Paxton (2000) for a thorough elaboration.

[10] This index was not used throughout chapter 2 due to the considerably shorter available time range, which is less problematic here.

To make the set of Freedom House scores more comparable to the single *polity2* index, I add up the political rights score and the civil liberties score to form an overall democratic index, which is hereafter referred to as the Freedom House Index. Since both sub-scores are within a range from 1 to 7, with high values indicating absent rights and absent liberties, the sum of the two ranges from 2 to 14. Since high scores of this summed Freedom House Index imply non-democracy, and high *polity2* scores imply democracy, a direct comparison is rather awkward. I therefore reverse the scores in the Freedom House Index along with the meaning of the range. A country previously labelled as having 14 points (non-democracy) now gets relabelled as having 2 points (which now means non-democracy), for instance. Finally, for convenience, I subtract 2 points from each score, so that the score is shifted from a range of 2 to 14 to a range of 0 to 12. Ultimately, a score of 0 therefore represents a complete absence of civil liberties and political rights, while a score of 12 indicates a strong upholding of civil liberties and political rights.

Both the *polity2* index and the Freedom House index have the advantage of being multinary rather than binary. Such a ranking allows for the study of both minor and large democratic developments, whereas binary rankings only allow for major institutional changes to be studied. Non-binary measurements allow for more nuances in evaluating democracies. Dichotomous democracy measures render borderline cases awkward to study, and inaccurately lump partial, flawed democracies together with either authoritarian regimes or with fully consolidated, strong democracies. While both the *polity2* index or Freedom House index are discrete rather than continuous, they feature a sufficient number of modalities to capture many of the benefits a continuous measure would have.[11]

Due to their different construction, the *polity2* index and the Freedom House index may only partly overlap, and may capture both different elements of democracy and different changes in democracy. If the subsequent empirical findings are independent of the choice of the democracy index, then these findings are likely robust across other democratic definitions too. Any emerging differences can provide room for a more in-depth comparison of the two indices, to establish which sub-element is driving any difference in results.

Figure 3.1 depicts the average level of democracy as measured by the Freedom House Index and the *polity2* index. For the sole purpose of this figure, the Freedom House Index is re-scaled to have the same range as the *polity2* index (hence it is temporarily changed to range from -10 to 10, rather than 0 to 12). Without such re-scaling, the Freedom House Index would automatically display less variation than the *polity2* index. In all subsequent sections, the Freedom House Index is always used in its original form, ranging from 0 to 12. Figure 3.1 depicts that though both indices show an increasing average global level of democracy, the *polity2* index features larger changes. In particular, the period right after the collapse of the Berlin Wall is captured differently by both

[11] For a deeper discussion on the topic, see Bollen and Jackman (1989).

indices, with *polity2* suggesting a large increase in democracy, while the Freedom House Index only displays a relatively modest increase. Periods of political turmoil are thus not evaluated the same by both indices, and there is indeed only partial overlap in these democracy measures.

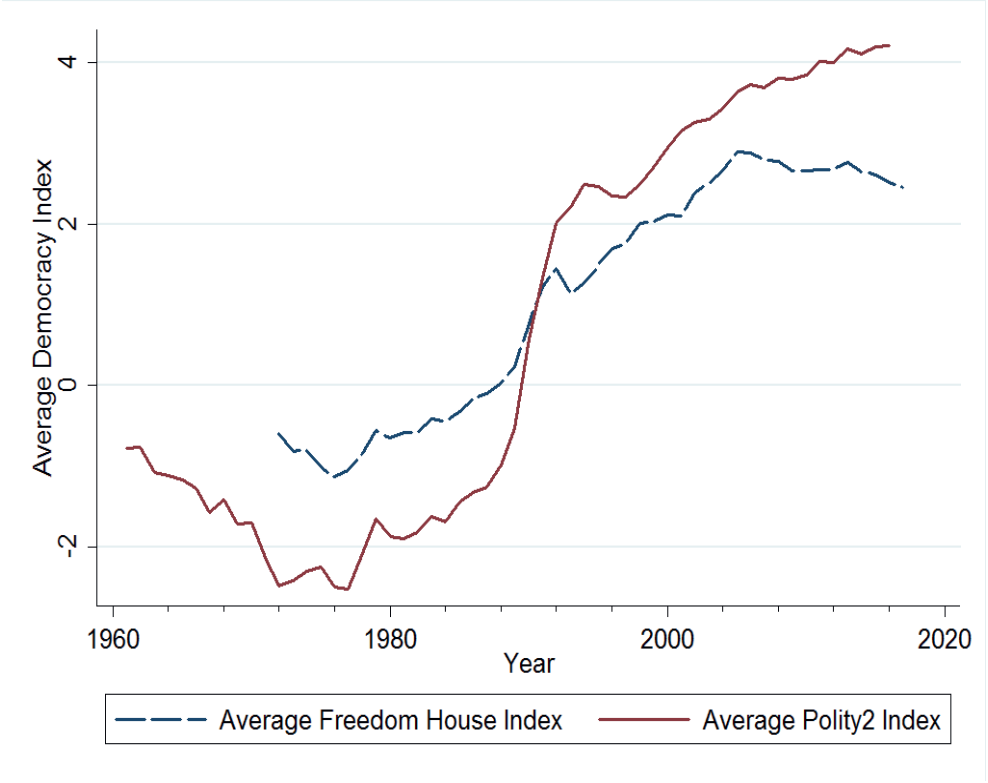

Figure 3.1: Average level of democracy as captured by the *polity2* index and a re-scaled Freedom House Index. For the sole purpose of this figure, both indices range from -10 to 10. In all subsequent sections, the Freedom House Index range is reverted back to its original 0 to 12 range.

3.4.2 Income inequality

Data on income inequality stems from the Standardized World Income Inequality Databse (SWIID) (Solt, 2016), which covers 163 countries and territories from 1961 until 2016. This dataset has the advantage of covering a longer time-span than other inequality datasets, such as the Luxembourg Inequality Study (LIS) or the World Income Inequality Database (WIID). Furthermore, it standardizes sources and measures so as to improve comparability between countries. The third and biggest benefit is that it provides Gini coefficients on the level of income inequality before taxes and redistribution (pre-tax inequality) and after taxes and redistribution (post-tax inequality)[12].

[12]The Gini coefficient is constructed by comparing a hypothetical perfectly equal income distribution to the actual income distribution. If the two distributions are identical, then there is no gap in the cumulative income at any

Therefore, the data make it possible to observe to what extent taxation and redistribution change over time and how such changes affect the income inequality a country's citizens ultimately face. If the gap between the pre-tax and the post-tax Gini coefficients increases, it indicates that either the taxation system has become more progressive, that there are more redistributive policies favouring the poor in place, or both. If both the pre-tax and post-tax Gini coefficients change by the same amount, then the level of income inequality is different, but the change is caused by factors unrelated to taxation and redistribution. Note that it is also possible that pre-tax income inequality increases, but that post-tax income inequality stays at the same level. This indicates that redistribution has increased, but that the pre-tax distribution deteriorated so much that net income inequality remains unchanged.

3.4.3 Control variables

The above data on the level of democracy and income inequality are combined with several economic indicators, which are obtained from the World Development Indicators, as provided by the The World Bank (2018). More specifically, this data covers 265 countries and territories[13] from 1960 till 2017 and includes real GDP per capita, trade as a share of GDP, gross primary school enrollment rates, net FDI inflows and life expectancy. These variables are connected to both the process of democratization and to changes in income inequality, and must thus be accounted for so as to avoid any major biases from omitted variables. Life expectancy, for instance, ensures that observations of Botswana during the peak of the HIV epidemic are not treated as viable candidates for the matching procedure (described in more detail in the next section). Meschi and Vivarelli (2009); Clark et al. (2011) and Abdullah et al. (2015) provide a thorough explanation of the role of trade, health and education in affecting income inequality. Controlling for such variables thus makes it far more plausible that countries follow similar paths prior to experiencing democratic changes, so that subsequent changes in income inequality can more accurately be attributed to democratic changes.

Table 3.1 provides an overview of all variables used in the study, along with a brief definition and their source.

percentile. As soon as the distributions differ at any percentile, a gap forms between the two distributions. The Gini coefficient represents this gap. The bigger the gap, the bigger the Gini coefficient.

[13]The number of territories exceeds the presently existing number of countries, as it also includes inter alia groupings of independent nations, such as the EU.

Variable	Measure	Source
Polity2	Ranks democracy on a scale from -10 to 10. The value -10 represents a complete absence of democracy, while a score of 10 represents a full democracy.	(Marshall et al., 2017)
Freedom House Index	Ranks democracy on a scale from 0 to 12. The value 0 represents absence of democracy, while a score of 12 represents a full democracy.	(Freedom House, 2019)
Pre-tax Gini	Measures the degree of income inequality, prior to taxation and redistribution. Ranges from 0 to 100, where 0 represents perfect equality, and 100 represents complete inequality.	(Solt, 2016)
Post-tax Gini	Measures the degree of income inequality after taxation and redistribution. Ranges from 0 to 100, where 0 represents perfect equality, and 100 represents complete inequality.	(Solt, 2016)
Trade/GDP	The aggregate value of imports and exports, divided by the level of GDP. Captures how important of a role trade plays in a country. Minimum possible value is 0. The higher the value, the more important trade is.	(The World Bank, 2018)
Net FDI/GDP	The difference between FDI inflows and outflows, divided by the level of GDP. Negative values indicate that investments outflows from a country exceed investment inflows, while positive values represent the opposite.	(The World Bank, 2018)
Life Expectancy	Expected life span at birth in years. Low values indicate that the average life-span is short, likely due to poor health standards or conflicts.	(The World Bank, 2018)
GDP/Capita	Average real level of output produced by a person within one country during a year. High values indicate a high level of productivity and development.	(The World Bank, 2018)
Primary School Enrollment (Gross)	The number of students in primary school compared to the primary school eligible population. Low values indicate that school attendance is low, and neither children nor adults attain primary education. High values indicate that attendance is high.	(The World Bank, 2018)

Table 3.1: List of all variables, along with a brief definition and source.

3.5 Descriptive Statistics

This section depicts key statistics of the variables described in the preceding section. Table 3.2 lists the mean, standard deviation, minimum, maximum and number of observations for each variable, both across the cross-sectional and the temporal dimension. The means of both the *polity2* and Freedom House index lie close to the middle point. These indices display considerable variation and cover all possible values. The average pre-tax and post-tax Gini coefficients differ substantially, indicating that pre-tax income inequality is much higher than post-tax income inequality ($t = 70.51$).

Several variables feature excessive variation, caused by large outliers among the observations. For instance, the trade/GDP ratio displays a few anomalous values, mostly originating from very small, open economies. The maximum value of 860.8% is reached by the US Virgin Isles in 2008, after which it soon plummets down to approximately 140%. FDI reaches extremely outlying values for similarly small nations and territories. An FDI of over 400% is observed in the Cayman Islands, for instance. Though outliers can form a problem for applying the matching procedure used in this paper (described in the next section), their removal is not always strictly required. The precise requirements and handling of outliers is elaborated upon in section 4.4. In practice, it is predominantly the FDI rate that causes issues in applying the method, while outliers in other variables generally do not impede the matching procedure. Observations featuring an FDI rate over 30% of GDP are excluded from the dataset.[14] Table 3.2 therefore shows the net FDI statistics after the extreme upward outliers have been removed. Life expectancy at birth reaches its lowest value for Cambodia, during the Cambodian genocide in the 1970s, dropping below even 20 years, though it swiftly recovers after the conclusion of the genocide. GDP growth features several extreme outliers caused by conflicts and post-war reconstruction. Primary school enrollment reaches a maximum value of more than 100%, because it compares the number of enrolled students to the number of people in the relevant age bracket for primary school. Hence, major educational overhauls in countries such as Micronesia saw a large number of people enter primary education despite being adults, resulting in a very high level of student enrollment.

Both the *polity2* index and Freedom House index display considerable variation across the cross-sectional dimension, but do not change very frequently over time. In total, there are only 430 cases where the *polity2* score increases, and only 238 instances where the *polity2* score decreases from one year to the next. For the Freedom House index, there are 793 increases and 647 decreases, rendering it much more volatile.

Table 3.3 shows the correlation coefficients between all variables, along with their significance levels. Several correlations stand out in this table. Firstly, the correlation between the *polity2* index and the Freedom House index is high, but not 1, indicating that the two measures do not

[14] This cut-off point is arbitrary, but generally proves to be sufficient to let the matching method work.

	Mean	SD	Min	Max	N
polity2	1.46	7.40	-10	10	7089
FH Index	6.75	4.07	0	12	7184
Pre-tax Gini	45.77	6.63	21.20	68.50	4406
Post-tax Gini	38.24	8.97	19.80	60.90	4406
Trade (% of GDP)	72.68	50.10	.02	860.80	10526
Net FDI (% of GDP)	2.70	4.43	-82.89	29.96	8876
Life Expectancy (years)	63.72	11.07	18.90	85.42	13512
GDP per Capita (log)	8.20	1.48	4.75	11.88	10929
GDP Growth (%)	2.11	5.66	-64.99	140.50	10679
Primary School Enrollment (% of population)	96.50	21.19	2.83	222.03	9237

Table 3.2: Descriptive statistics

capture identical features of democracy. One can therefore also expect that these two indices capture different changes in democracy at different times.

Secondly, both democracy indices are positively correlated with pre-tax income inequality, but negatively with post-tax income inequality. This suggests that democracy goes hand in hand with higher pre-tax inequality, but that income redistribution is sufficiently large so that the post-tax inequality is lower than in non-democracies.

Figure 3.2 illustrates where the negative correlation between post-tax income inequality and the democracy indices originates from. It depicts the average levels of pre-tax (left) and post-tax (right) income inequality across different levels of the *polity2* index, along with their 95% confidence interval. The graphs do not show any clear upward, downward nor curvilinear trend in the two income inequality measures. However, countries with the maximum *polity2* score of 10 display by far the lowest post-tax income inequality. Such countries have very stable democratic institutions in place, and tend to be rich and highly developed. Hence, their data are a poor frame of reference when studying democratic changes, since such events are observed quite infrequently. A major part of the dataset behind figure 3.2 is comprised of countries that have the maximum democracy score, thereby driving the negative correlation between income inequality and the level of democracy. With the exclusion of the highest democratic modality, there does not appear to be a clear-cut connection between the level of democracy and the level of post-tax income inequality anymore.

The correlation coefficients do not appear to be largely driven by observations with the maximum *polity2* score. Only the correlation between democracy and life expectancy, and the correlation between democracy and GDP per capita are affected when observations with the highest *polity2* score are left out. These correlations fall from 0.53 to 0.36 and from 0.58 to 0.32 respectively, indicating that countries with the highest democracy score have disproportionately high life expectancy and income per capita.

	polity2	FH Index	Pre-tax Gini	Post-tax Gini	Trade (% of GDP)	Net FDI (% of GDP)	Life Expectancy	GDP (log)	GDP Growth	Prim. School
polity2	1.00									
FH Index	0.89	1.00								
	(0.00)									
Pre-tax Gini	0.14	0.15	1.00							
	(0.00)	(0.00)								
Post-tax Gini	-0.25	-0.31	0.62	1.00						
	(0.00)	(0.00)	(0.00)							
Trade (% of GDP)	0.03	0.08	-0.00	-0.06	1.00					
	(0.01)	(0.00)	(0.86)	(0.00)						
Net FDI (% of GDP)	0.06	0.08	-0.03	-0.07	0.34	1.00				
	(0.00)	(0.00)	(0.11)	(0.00)	(0.00)					
Life Expectancy	0.53	0.57	-0.06	-0.39	0.27	0.10	1.00			
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)				
GDP (log)	0.48	0.58	0.08	-0.43	0.26	0.07	0.79	1.00		
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)			
GDP Growth	0.02	0.03	-0.02	0.00	0.08	0.09	0.07	0.03	1.00	
	(0.13)	(0.03)	(0.18)	(1.00)	(0.00)	(0.00)	(0.00)	(0.00)		
Prim. School	0.33	0.30	0.17	0.10	0.15	0.06	0.55	0.37	0.09	1.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	

Table 3.3: Cross-correlation table. The p-values are in parentheses.

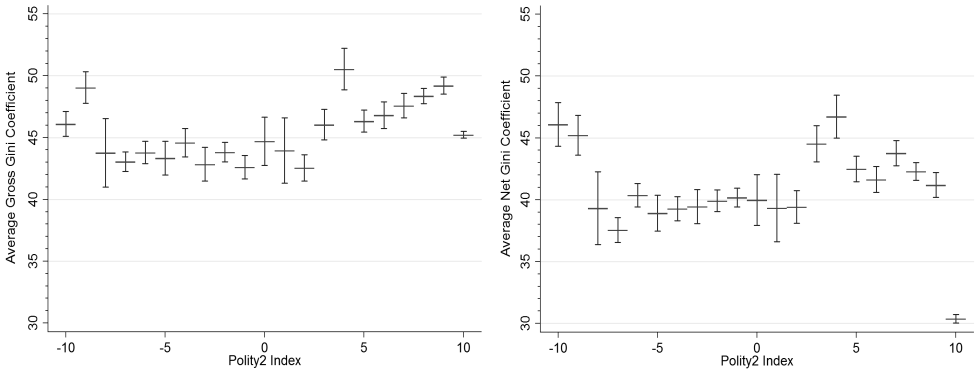

Figure 3.2: Level of pre-tax and post-tax income inequality across different levels of democracy as captured by the *polity2* index. The vertical lines depict the 95% confidence interval based on each average.

3.6 Methodology

Previous empirical studies have primarily made use of standard statistical methods such as OLS or Panel Fixed Effects. Such standard methods have at least two weaknesses that renders them less appropriate for the topic at hand. Firstly, standard methods require a representative sample of the population in the data. In other words, the data should accurately represent the portion of rich and poor countries, and of democratic and non-democratic countries. This assumption is unrealistic and inaccurate. For instance, fully democratic, rich nations have more readily available data than non-democratic poor ones, and are overrepresented. Such countries feature stable democratic institutions and thus make for a poor basis to determine the consequences of democratization. After all, the effects of democratization are inapplicable and irrelevant for countries that cannot realistically undergo much further democratization. Any regression results based primarily on data from countries where the research question at hand is irrelevant and inaccurate are equally inaccurate.[15] Ross (2006) provides an overview of how selection bias has plagued empirical studies on the effects of democracy.

Secondly, changes in democratic institutions are infrequent, which means that any measure of either the level of democracy or changes in democracy inevitably features little variation. While changes in democratic institutions are theoretically expected to have an impact on income inequality, income inequality is unlikely to be driven solely by such institutional changes. Consequently, income inequality displays considerably more variation than democratic institutions, automatically rendering any correlation between income inequality and institutions weak, even if there is a causal relationship between the two variables. The result of this weak correlation is that any estimated

[15]Note that for democratic breakdowns, data originating from fully democratic countries are potentially relevant again.

effect of changes in democratic institutions using standard methods can ex ante be expected to be very small and statistically insignificant.[16]

3.6.1 Propensity Score Matching

Strengths and weaknesses

Rosenbaum and Rubin (1983) develop a method called Propensity Score Matching, which remedies both aforementioned problems by performing a quasi-experiment. The method matches the group of countries that undergo an institutional change to a group of countries that remain institutionally unchanged, and compares the developments in income inequality between the two groups of countries. In the current study, this comparison is made 2 years and 6 years after undergoing the institutional change. The two groups are matched in a way so that they are statistically indistinguishable before undergoing any institutional change. Consequently, the effects on income inequality can be attributed to the subsequent institutional change, as that is the element that sets the two groups apart.

There are several noteworthy strengths and weaknesses of PSM that are worth highlighting. PSM is not the only matching method that allows one to perform a quasi-experiment. One of the major challenges in matching procedures is how to match based on multiple variables simultaneously. When there are multiple variables involved, it becomes awkward to define when observations are deemed similar enough for a match (Heinrich et al., 2010; Streiner and Norman, 2012). The major advantages of PSM is that it simplifies multivariate matching problems to a single dimension, by combining all variables into a singular propensity score. Even with this singular dimension, there are several difficulties in following an appropriate matching procedure. In particular, one must consider which covariates are combined to generate the propensity scores, how observations are matched with one another (e.g. k-nearest neighbour versus calliper), whether there is sufficient overlap between all matched groups, how to handle imperfect matching and whether there unobserved heterogeneity could bias the results. Though Caliendo and Kopeinig (2008) and Rosenbaum and Rubin (1985) provide guidelines on how to tackle some of these questions, there are generally no well-established rules to follow on how to exactly implement PSM.

The potential issue of unobserved heterogeneity is particularly relevant to the current study. As seen in section 3.4, the scope of the included variables is intentionally left limited, in order to be able to include more developing countries, for which the study of democratic changes is particularly relevant. As such, there is an inherent risk that some unobserved variables influence the estimation results. Note that this risk is not unique to PSM, but would also be applicable to more traditional estimation methods. The current study assumes that the included variables sufficiently capture all

[16] Results from existing research finding an inverse relation between income inequality and democracy were often based on exploiting cross-sectional variation between countries rather than looking at developments within countries.

relevant observed and unobserved processes determining the estimation results.

Procedure

For clarity, the full PSM procedure is summarized in figure 3.3, of which all steps are described in this subsection. The first step is to define a treatment that is expected to affect an outcome variable. In the case at hand, I consider democratization and democratic breakdowns as two possible treatments, which are expected to affect income inequality (i.e., a treatment effect). More specifically, the democratization treatment is based on any increase in the *polity2* score from one year to the next. The democratic breakdown treatment is defined as any decrease in the *polity2* index from one year to the next. I also form a second, separate set of treatment indicators based on the Freedom House Index. These two democracy measures are used separately to ensure the robustness of results. The effects of the Freedom House Index treatment indicators is studied entirely separately from the procedure based on the *polity2* index. Changes in the democracy index sometimes happen several years in a row. If a year of democratization is preceded by a period of democratization during the two preceding years, it is not considered as a treatment and the observation dropped from the sample. This is to ensure that multi-year periods of democratization or democratic breakdowns are not incorrectly counted as multiple, independent treatments, but that only the beginning of such a period is counted as a democratic change.[17]

The treatment of democratization and the treatment of democratic breakdowns are therefore defined as:

$$Democratization_{i,t} = \begin{cases} 1 & \text{if} \quad \Pi_{i,t} > \Pi_{i,t-1} \quad \& \quad \Pi_{i,t-3} \not< \Pi_{i,t-1} \\ 0 & \text{if} \quad \Pi_{i,t} \leq \Pi_{i,t-1} \end{cases}$$

$$Breakdown_{i,t} = \begin{cases} 1 & \text{if} \quad \Pi_{i,t} < \Pi_{i,t-1} \quad \& \quad \Pi_{i,t-3} \not> \Pi_{i,t-1} \\ 0 & \text{if} \quad \Pi_{i,t} \geq \Pi_{i,t-1} \end{cases}$$

where $\Pi_{i,t}$ denotes the *polity2* index or the Freedom House Index for country i in year t. The condition $\Pi_{i,t-3} \not< \Pi_{i,t-1}$ ensures that the democracy index has not increased over the course of the two prior years, so that the change between time $t-1$ and t marks the first period of democratization. Similarly, the condition $\Pi_{i,t-3} \not> \Pi_{i,t-1}$ makes sure that democracy has not decreased in the

[17] There are approximately 30 cases in which countries undergo repeated institutional changes. PSM does not take the panel structure of the data into account, and would simply see repeated institutional changes as entirely independent observations. Countries undergoing long periods of institutional change would then *de facto* be given an excessive weight in the overall procedure, and any estimated treatment effects would be unjustifiably primarily based on such countries.

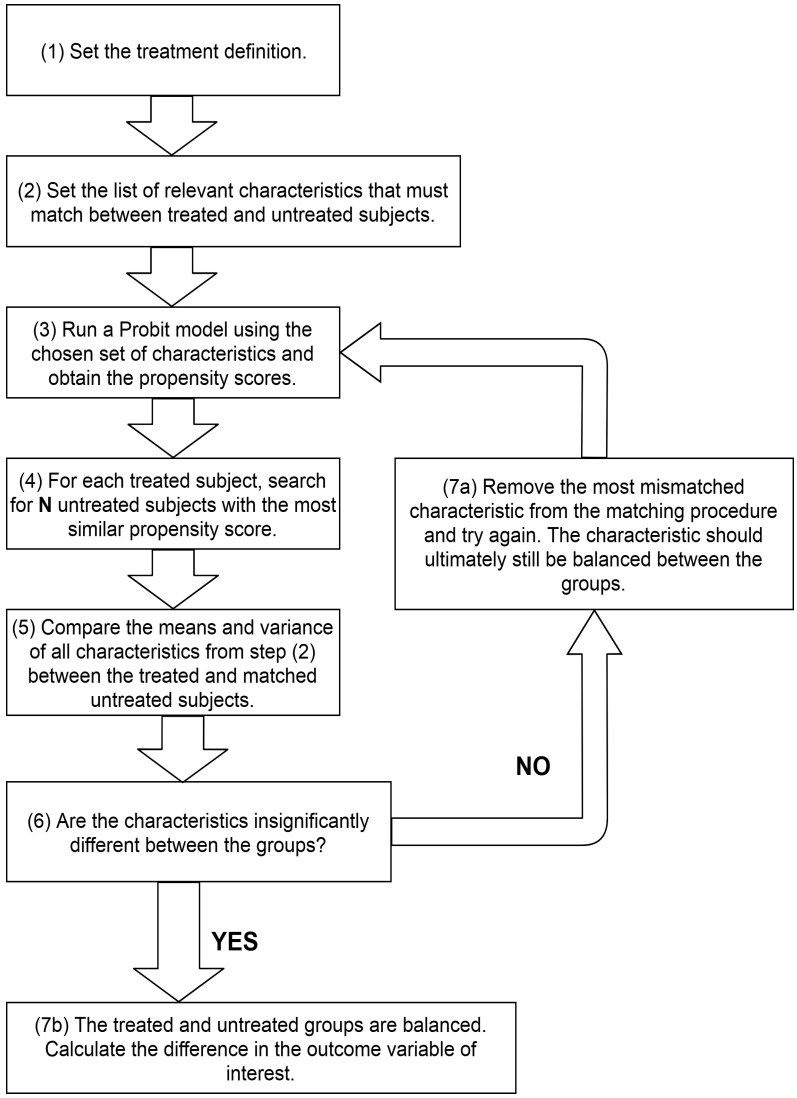

Figure 3.3: Flow chart of the matching procedure. If PSM results in any significant differences in characteristics, the process is re-run on the basis of other characteristics. The parameter **N** can be modified. The number of matches used in the results reported in the subsequent section 3.3, but other matching numbers have also been tested.

two prior years. In total, there are four possible treatments: (1) An increase in the *polity2* index, (2) a decrease in the *polity2* index, (3) an increase in the Freedom House Index and (4) a decrease in the Freedom House Index. The effects of all four treatments are studied via PSM separately.

Ideally, each country undergoing a period of institutional change (i.e., a treatment) would simply be compared to a counterfactual of itself *not* undergoing the same institutional change. Let Y_i^1 denote the potential outcome for country i after undergoing a treatment and Y_i^0 denote the potential outcome for country i after not undergoing a treatment. The exact treatment effect D caused by the treatment status T for a specific country i is then the difference between these two potential outcomes, or $Y_i^1 - Y_i^0$. Averaging it out over the countries of interest, this can be represented as:

$$D = E[Y_i^1 - Y_i^0], \qquad i = 1, 2, \ldots, n \tag{3.1}$$

One can look at the treated countries specifically, to observe what the average treatment effect on treated subjects is.[18] Let TR represent the set of countries undergoing the treatment. Then equation 3.1 can easily be changed into:

$$D_T = E[Y_i^1 - Y_i^0] = E[Y_i^1] - E[Y_i^0], \qquad i \in TR \tag{3.2}$$

Clearly, the potential outcome without treatment can never be observed for subjects undergoing the treatment, hence it is impossible in practice to calculate $E[Y_i^0]$ if $i \in TR$. The true effect of the treatment is therefore impossible to calculate. In an experimental setting, if treatment is assigned randomly, it is possible to use untreated observations as a substitute for the unobservable counterfactual. Let NTR represent the set of countries that have, by sheer chance, *not* undergone the treatment. Then:

$$E[Y_i^0] = E[Y_j^0], \qquad i \in TR, \qquad j \in NTR, \qquad TR \cap NTR = \{\emptyset\} \tag{3.3}$$

The unobservable counterfactual can simply be substituted with the randomly untreated observations. In the study at hand, the treatments of democratization and democratic breakdowns are unlikely to be completely random, so it is unlikely that equation 3.3 is true. For instance, rich, very democratic countries are extremely unlikely to undergo a process of democratization, so they would be an inappropriate counterfactual for poorer, flawed democracies that do undergo such a process of democratization. Instead, it is necessary to adjust the sample so as to select the most appropriate candidates for the untreated control group, so that equation 3.3 holds. The primary purpose of the PSM method is to create an accurate control group through a matchmaking pro-

[18] The distinction between the average treatment effect and the average treatment effect on the treated is further elaborated upon in section 3.6.2

cedure, so that the treatment effect can be isolated from any other confounding factors. Selecting the potential confounding factors constitutes the second step in figure 3.3. This set of potential confounding factors is composed of the level of pre-tax and the level of post-tax income inequality, the change in pre-tax and post-tax income inequality from one year to the next, the trade to GDP ratio, the net level of FDI as a percentage of GDP, the life expectancy at birth, the log-level of GDP per capita, the growth rate of GDP per capita, the primary school enrollment rate and the level of democratic institutions.

The matchmaking portion of the PSM procedure begins with running a separate binary Probit regression for each of the four treatment indicators (step 3 in figure 3.3). The Probit regression takes on the form:

$$P(T_{i,t} = 1|X_{i,t-1}) = \Phi(\beta X_{i,t-1}) \tag{3.4}$$

In this equation, $T_{i,t}$ denotes that country i underwent one of the aforementioned four treatments in year t. The vector $X_{i,t-1}$ represents the set of characteristics one year prior to the treatment status $(t-1)$. The Probit regression tries to predict the odds of undergoing a treatment on the basis of countries' characteristics that are established before the treatment status. The aim of the Probit regression is not to create a model that perfectly predicts the treatment status. Instead, it is merely a step in ensuring that all characteristics are ultimately closely matched between the treated and untreated observations. Note that the set of characteristics prior to the treatment can also include the outcome variable of interest (Wooldridge, 2007).[19]

The results from the Probit regression are used to calculate the probabilities of undergoing a treatment (hereafter referred to as the propensity scores). If two countries share a very similar propensity score, then they were equally likely to undergo the treatment, based on the observed characteristics. Such a similarity in the propensity score indicates that countries also had very similar characteristics prior to their treatment status. This propensity score therefore summarizes whether countries feature comparable characteristics in a single statistic. Instead of having to match numerous variables separately, the use of propensity scores greatly simplifies the process by having to only match one variable instead. Each treated observation is matched with its most similar, untreated observations (nearest neighbors) on the basis of their propensity scores. In other words, each treated observation is matched with a counterpart that was just as likely to undergo the treatment, but did not. The matching procedure therefore ensures that the treatment can no longer be predicted on the basis of pre-treatment characteristics, making it just as random as it

[19]Ideally, countries should not only be a match in all pre-treatment characteristics, but should also feature parallel trends prior to treatment. Given the relatively small cross-sectional dimension of the sample, guaranteeing that all trends are parallel for all matches is infeasible. However, simulation results indicate that matching on lagged outcomes can validate estimated treatment effects even if one cannot guarantee parallel trends prior to the treatment (O'Neill et al., 2016).

would have been in a randomized controlled trial. As a consequence, rich, full democracies are no longer greatly overrepresented in the data. Furthermore, changes in democratic institutions are no longer rare in the data, as all irrelevant observations where such institutional changes are extremely unlikely have been filtered out from the data.

Step 4 of the procedure is to determine how many untreated observations to match each treated observation with. Each treated observation can be matched to a single, untreated observation, but it is also possible to match each treated observation with multiple untreated observations ('oversampling'). When looking at the effects of the treatment, increasing the required number of matches results in a trade-off between bias and variance. A higher number of matches results in a bigger bias, since each treated observation is now matched with increasingly dissimilar untreated observations, but the variance of the treatment effect is reduced, since each treated observation is compared to more counterfactuals. In the current study, I choose to match each treated observation to its three nearest neighbours, as this results in a major reduction in the variance of the treatment effect, without causing much bias.

Matching quality

Steps 5 and 6 in Figure 3.3 involve checking whether the matching procedure is successful. There are several criteria used to determine whether the matching procedure is accurate. Firstly, if the matching procedure is successful, then re-running the original Probit regression (equation 3.4) using only the treated and matched untreated observations should result in a very low goodness of fit, which is illustrated via a very low pseudo-R^2, along with a low likelihood-ratio test statistic[20] and its associated high p-value. Secondly, the mean and median biases indicate the remaining average percentage difference between the means and medians of the treated and control groups prior to the treatment. Though there is no established criterion for how high these may be, they should be reasonably close to zero. An additional statistic is Rubin's B, which illustrates the (absolute) standardized difference in the means of the propensity score between the treated group and the untreated control group. Rubin (2001) suggests that the value of B should lie below 25, as this indicates that propensity scores are not only similar in absolute terms but also when standardized.[21] Finally, Rubin's R denotes the variance ratio of the propensity scores between the treated group and untreated control group. Rubin suggests that this should lie between 0.5 and 2, as it indicates

[20]The constrained model simply excludes all parameters other than a constant. If the log-likelihoods are similar for both models, the unconstrained model is of very little value, indicating that all variables in the unconstrained model no longer have any predictive power for the treatment status.

[21]In Probit models that do not have a high degree of predictive power, the estimated propensity scores may all lie very close to zero, so that the absolute difference between them is automatically very small. Standardizing the differences illustrates whether the propensity scores are close to one another, given the overall distribution of all propensity scores. Propensity scores therefore have to be not only absolutely but also relatively close to one another.

that there are no distributional differences in the underlying matching characteristics.[22]

Outliers in data can hamper the matching procedure, as they may have a major impact on the quality of the Probit regression, and the resulting propensity scores. The effect of outliers can be evaluated by checking the ultimate matching quality, and comparing the density functions between the treated and untreated groups. If the outlier occurs in an untreated observation, the outlier is unlikely to have much effect on the matching procedure. After all, such an observation simply forms a poor match for any treated observation and is automatically filtered out of the control group. If a treated observation features a severe outlier, then the procedure may struggle to find an appropriate match. In such a case, the propensity scores would not align sufficiently, and the first two moments of the underlying variables would not end up close to one another. Such cases are easily spotted, as Rubin's B and Rubin's R would exceed appropriate cut-off values. Furthermore, a glance at the kernel density functions for all variables in the treated and untreated groups reveals whether any severe outliers remain after the matching procedure. The problem can be remedied by excluding observations featuring severe outliers from the entire matching procedure, so that they are considered neither a treated nor a control observation.[23]

It is also possible that two sets of very different characteristics produce the same propensity score, and that countries with similar scores therefore differ greatly in their pre-treatment status observable characteristics. This would not be captured by Rubin's B nor Rubin's R. To ensure that this is not the case, I compare all the means and variances of all characteristics used in the Probit regression to ensure that there are no significant differences between the treated and the untreated control group before the treatment has taken place. If a characteristic turns out to feature a significantly different mean or variance, I first check whether this is caused by any major outliers in any of the characteristics. If this is the case, then observations featuring such outliers are excluded from the sample, and the matching Probit and matching process is repeated. If there is another cause, the mismatched variable is excluded from the Probit regression. Though it is not explicitly part of the matching procedure, the first and second moments of the excluded variable are still compared between the treated and the control group, to ensure that it is statistically indistinguishable between the two groups. Variables are predominantly excluded when they have no strong predictive power on the treatment status. In the current study, all matching inaccuracies are successfully resolved via the removal of outliers, or removal of variables from the Probit regression.[24]

[22] The relevant formula is that Rubin's $B = 100 * \frac{|\mu_1 - \mu_2|}{\sqrt{(Var(ps_1)+Var(ps_2))/2}}$, where μ_1 and μ_2 are the weighted means of the propensity scores of the treated sample and the untreated sample, respectively, and $Var(ps_1)$ and $Var(ps_2)$ are the variances of the propensity scores for the treated and untreated sample respectively. Rubin's R is the ratio of these two variances.

[23] The exact cut-off points for outliers for each matching procedure are tabulated in the supplementary online appendix.

[24] Caliendo and Kopeinig (2008) elaborate on various options in resolving matching inaccuracies. The procedure used in this paper is not the only one, but proved to be the most successful in the case at hand.

It must be emphasized that countries can undergo multiple treatments. Furthermore, almost every country in the dataset undergoes a treatment at some point. By default, the estimation procedure pools all observations together, treating them as entirely independent from one another.[25] This can result in a considerable amount of incorrect matches. For instance, if a country undergoes a treatment in year t, PSM may match that country to itself in year $t-1$, since the matched variables are likely to generate very similar propensity scores in these two years. Similarly, a treated observation may be matched to an untreated observation that only just precedes treatment. For instance, if two countries A and B undergo treatment at time t, then PSM may match the treated observation of A to an observation of B at time $t-1$ and match the treated observation of country B to an observation of A at time $t-1$. In order to ensure that such incorrect matching within countries and between countries does not occur, all observations surrounding a treated observation are strictly excluded from the PSM process. The time window determining this exclusion is based on the time-span over which the outcome variable is studied. In the current study, the outcome is observed after 2 years and after 6 years.[26]

When the matching procedure has been completely satisfactory and the treated and untreated countries are indistinguishable prior to the treatment, the outcome variables of the treated and untreated groups are compared to determine how the two groups have evolved differently as a result of the treatment (step 7 in figure 3.3). In the current study, there are two outcome variables of interest: The change in pre-tax income inequality, and the change in post-tax income inequality.[27] The combination of these two outcome variables displays whether redistribution has changed.[28] This change in pre-tax and post-tax income inequality is measured over the course of 2 years and 6 years from the treatment year onward. The two time-spans make it possible to observe whether changes in redistribution, if any, occur swiftly or not. Furthermore, if such changes do occur swiftly, then their persistence can be observed. A positive (negative) treatment effect on pre-tax (post-tax) income inequality indicates that the treatment has resulted in pre-tax (post-tax) income inequality

[25]The closest parallel to this is how panel data models can be estimated via Pooled OLS or methods that take the temporal structure explicitly into consideration.

[26]Note that this solution does not fully exclude matching within countries. There is, however, a minimum gap in time between matched observations within countries to ensure that the (non-)treatment outcomes have no impact on one another. Matching within countries may, in fact, provide an additional benefit of matching on unobserved, country-specific factors, thus controlling for more than the variables included in the explicit matching procedure.

[27]Using the level of pre-tax and post-tax income inequality would have been more intuitive, but unfortunately results in awkward treatment effects. While the matching procedure manages to make the levels of income inequality statistically insignificantly different between the treated and untreated groups, the difference is not exactly zero. The treatment effect then partly based on the remaining difference between the treated and untreated group, and overestimated. By subtracting the starting level of income inequality and focusing on the change in income inequality compared to the treatment year, this remaining difference is accounted for and no longer biases the treatment effect.

[28]The main benefit of using two outcome variables rather than estimating a treatment effect on redistribution directly is that it provides a more complete overview of the effects of democratic changes. Suppose that pre-tax and post-tax income inequality both fall by the same amount following democratization, whereas they stay constant when there is no democratic change. Then democratization would be estimated to have no effect on income redistribution. However, clearly democratization has resulted in lower income inequality, thereby reducing the *need* for income redistribution. Though this is not the exact prediction of the MRF, it is quite close to it. Focusing solely on redistribution would, instead, fully reject the MRF.

being higher (lower) than it would have been if the country had not undergone the treatment.

The gap between pre-tax and post-tax income inequality by definition represent the level of redistribution and the progressiveness of taxation. The only way in which this gap can widen or narrow is if pre-tax income inequality and post-tax income inequality are changing at different rates. This can happen in a variety of ways. Suppose that pre-tax income inequality increases. If post-tax income inequality increases even more, then redistribution has decreased. Alternatively, suppose that pre-tax income inequality decreases, and post-tax income inequality decreases at a lesser rate, then redistribution has fallen.[29] These two examples illustrate that looking at the change in pre-tax and post-tax income inequality separately (and in isolation) does not provide sufficient evidence to draw any conclusions on the accuracy of the MRF: It is possible for inequality to increase (decrease), and redistribution to increase (decrease) at the same time. Instead, one must look at whether pre-tax and post-tax income inequality are changing significantly differently from one another to draw such conclusions. If pre-tax income inequality changes significantly differently from the post-tax income inequality, then redistribution or taxation must also have changed.

3.6.2 Interpretation of results

The PSM method provides a major benefit over other, standard methods, such as panel data methods or OLS, by making a distinction between the Average Treatment Effect (ATE) and the Average Treatment Effect on the Treated (ATT). The ATE represents the estimated effect of a treatment on *any* subject in the sample. This is the typical outcome of standard regression models. The ATT, in contrast, represents the effect the treatment has had on subjects that have actually undergone the treatment. These two can differ substantially if the treated group's characteristics are very different from those of the untreated group and the treatment status is non-random. If the characteristics of all treated subjects are distributed identically to those of the untreated subjects and these characteristics cannot explain the treatment status, the ATT and ATE are equivalent. In such a case, the treated group and the untreated group are indistinguishable, apart from the treatment status, so any difference in the outcome stems from this treatment difference. If the characteristics feature significantly different distributions between the treated and the untreated group, then the treatment status is no longer the only difference between the two groups, and the outcome differences may be (partly) driven by the differences in underlying characteristics.[30] In this study, the effects of democratization are most relevant for countries that have room left to undergo such a process. If the treatment effects are primarily based on the comparison between

[29] Note that if the pre-tax inequality ends up being *lower* than the post-tax inequality, then redistribution is happening from the poor toward the rich rather than the other way around.

[30] To illustrate this further, consider a study testing the effectiveness of a new drug. If the study only tests the drug on young male participants, and the control group also (correctly) consists solely of young men, then the ATT would reflect the drug's treatment effect on young men. The treatment effect would not be valid for women, as the treated group did not include any female participants, or older men, as they were also not among the participants. The ATE would (incorrectly) extrapolate the treatment effect to both women and older men, while the ATT recognizes that the treatment effect is only valid for young men.

poor, partial democracies such as Gabon or Botswana, then these treatment effects may not be appropriate for rich, highly democratic countries such as Norway or Sweden. Similarly, if the treatment effects of democratic breakdowns are based on partial democracies, then these effects may be of little relevance to countries that have almost no democratic institutions in place, such as Saudi Arabia. Standard models, which calculate the ATE, are therefore not appropriate, while PSM, which calculates the ATT, provides correct results.

The treatment effects can be estimated in two ways. One can use the difference between pre-tax and post-tax income inequality as the outcome variable and see whether it changes as a result of the treatment. Alternatively, one can look at the treatment effects on changes in pre-tax and post-tax inequality separately, and then calculate whether the effects differ significantly. The main benefit to applying PSM to changes in pre-tax and post-tax income inequality separately is that it becomes possible to see whether the treatments have an effect not only on redistribution but also the pre-tax income distribution. It may be the case, for instance, that a population's income distribution is equalized via indirect rather than direct methods, such as changes in investment in better health care or education for the poor. If such investment would be increased, as may be the case after a period of democratization, then it may result in a drop in both the pre-tax and post-tax income inequality, without affecting redistribution itself. Though the MRF specifically only predicts direct income redistribution, such a result would indicate that it may be happening indirectly instead. By looking at both pre-tax and post-tax income inequality, such indirect effects, if any, can easily be observed. Though at first glance, estimating treatment effects for pre-tax and post-tax income inequality would double the amount of matching procedures, this is not the case. The Probit model and observations used are identical when looking at pre-tax and post-tax income inequality, so that the exact same matching procedure provides treatment effects for both outcomes. Hence, in this paper I calculate the effects on pre-tax and post-tax income inequality separately, so as to make use of the additional information this provides.

In short, I perform a quasi-experiment that transforms the dataset via PSM, so that it seems as if it were from a randomized controlled trial. The effects of democratization and democratic breakdowns on pre-tax and post-tax income inequality are then calculated via a difference-in-difference process. The theoretical expectation is that there is a significant difference in the change of the pre-tax and post-tax income inequality *between* treated and untreated countries, and that there are significant differences between the changes in pre-tax and post-tax income inequality *within* treated countries. Democratization should result in a widening of the gap between pre-tax and post-tax income inequality, while democratic breakdowns should result in a narrowing of this same gap.

3.7 Empirical Results

3.7.1 Results

Naive OLS Regression

For the sake of comparison, I run a basic OLS regression with fixed effects (FE) to establish a baseline of results. The relevant regression equation is:

$$Y_{i,t+k} = \beta X_{i,t} + \gamma Z_{i,t} + \delta_i + e_{i,t} \qquad i = 1, 2, ...N \qquad t = 1, 2, ...T \tag{3.5}$$

Here, Y is the the outcome variable, which is either the change in pre-tax Gini or post-tax Gini in the 2 or 6 years (k) after time t. X denotes the matrix of explanatory variables, which are the level of pre-tax Gini and post-tax Gini, the level of democracy (as captured by either the *polity2* index or the FH Index), the log-level of real GDP per capita, the growth rate of real GDP per capita, the Trade/GDP ratio, the net FDI inflows, the life expectancy in years and the primary school enrollment rate. Z contains two dummy variables capturing whether the level of democracy has increased or decreased compared to the preceding year. The vector δ denotes the country-specific fixed effects, and e is the vector denoting the error terms.[31]

Table 3.4 provides a highly condensed overview of all the regression results, omitting control variables for the sake of brevity. This table highlights the regression coefficients of the dummy variables capturing: (1) an increase in the *polity2* index; (2) a decrease in the *polity2* index; (3) an increase in the FH Index and (4) a decrease in the FH Index. The t-statistic underneath each pair of coefficients denotes how the effect on pre-tax and post-tax income inequality differs after a democratic change. For instance, the effect of increases in the *polity2* index on both pre-tax and post-tax income inequality after 2 years are both negative (-0.036 and -0.0244), but these effects are not statistically different from one another ($t = -0.16$). Among the tabulated results, only the change in the post-tax Gini after 6 years is significantly ($p < 0.05$) related to decreases in the *polity2* index (-0.377) as well as increases in the FH Index (0.172). However, as the pre-tax Gini is estimated to change similarly in both cases, the resulting change in redistribution is not statistically significant. In other words, democratic changes in either direction, as captured by both *polity2* and the Freedom House Index, are not associated with noteworthy changes in income redistribution. The lack of significant results is not a surprise: democratic changes are very rare events, hence significant relations to any changes in income inequality are unlikely to appear. Furthermore, as most of the sample is composed of rich, highly democratic countries, it is hard to justify extrapolating these estimation results to weaker democracies. To verify whether these estimation results are merely an artifact of the estimation method and the underlying data, or truly reflect

[31] A random-effects model is also run, but provides equivalent results. Furthermore, I also run a variation with lagged variables, which results in qualitatively similar results.

	(1) Δpre-tax Gini (2 years)	(2) Δpost-tax Gini (2 years)	(3) Δpre-tax Gini (6 years)	(4) Δpost-tax Gini (6 years)
polity2 increase	-0.0360	-0.0244	-0.0469	-0.0103
	(-0.68)	(-0.51)	(-0.41)	(-0.10)
Tt-statistic	-0.16		-0.23	
(Δpre-tax = Δpost-tax)				
polity2 decrease	0.115	0.0649	-0.266	-0.377**
	(1.36)	(0.84)	(-1.40)	(-2.12)
t-statistic	0.44		0.42	
(Δpre-tax = Δpost-tax)				
Total N	2491	2491	2324	2324
FH Index increase	0.0244	0.0700*	0.117	0.172**
	(0.58)	(1.83)	(1.27)	(2.02)
t-statistic	-0.80		-0.44	
(Δpre-tax = Δpost-tax)				
FH Index decrease	0.0346	0.0155	-0.058	-0.0803
	(0.70)	(0.35)	(-0.54)	(-0.80)
t-statistic	0.28		0.15	
(Δpre-tax = Δpost-tax)				
Total N	2460	2460	2290	2290

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.4: The estimated effects on changes in the pre-tax and post-tax Gini coefficients over a period of 2 years and 6 years respectively. The t-statistic displays whether the change in the pre-tax income inequality is significantly different from the change in post-tax income inequality.

the a lack of empirical support for the MRF, the next section makes use of PSM instead of FE-OLS.

Propensity Score Matching

The first step in establishing the effect of democratic changes is to use a Probit model through which the propensity scores are estimated (step 3 in the afore shown figure 3.3). The treated and untreated observations are then matched based on these propensity scores, and the outcomes are compared across the two groups. The Probit equation takes on the form:

$$P(T_{i,t} = 1|X_{i,t-1}) = \Phi(\beta X_{i,t-1}) \tag{3.6}$$

The explanatory variables, $X_{i,t-1}$, used in this Probit model are all lagged by one year, so that

they capture countries' characteristics prior to undergoing any potential institutional changes. The baseline set incorporates the lagged change in pre-tax Gini, the lagged change in post-tax Gini, the lagged level of both the pre-tax and post-tax Gini, the lagged level of democracy as captured by either the *polity2* index or the Freedom House Index, the lagged Trade/GDP ratio, the lagged Net FDI/GDP ratio, lagged life expectancy in years, lagged output growth and the lagged level of educational attainment.[32] The set of variables selected for each Probit regressions varies depending on whether the explicit inclusion of a variable improves or hampers the accuracy of the matching process. Hence, some of the aforementioned variables are excluded in some of the Probit specifications. Note that when a variable is excluded from the Probit model, both the mean and variance of this variable are still required to be insignificantly different when comparing the post-matching treated and control groups. In other words, such a variable is not used explicitly in the match-making process, but is still required to be insignificantly different between the treated and untreated group.

As there are two possible treatments (an increase in democracy, and a decrease in democracy), the treatment is defined via two separate indices (the *polity2* index and the Freedom House index) and the outcome is measured at two different times (after 2 years and after 6 years) for two separate outcome variables (changes in the pre-tax Gini coefficient, and changes in the post-tax Gini coefficient), there are 16 matching procedures to complete.[33] The number of matching models can be reduced to only 8, however. The matching models for the treatment effects on changes in the pre-tax Gini coefficient are identical to those for changes in the post-tax Gini coefficient within a given year, due to the underlying data being identical for both matching procedures. Therefore, the resulting treated and control groups are also identical when looking at the two outcome variables within a given year based on the same treatment.

Table 3.5 denotes the treatments and outcome variables corresponding to each Probit model. The Probit regression results are then listed in Table 3.7. The coefficients of all Probit regressions are not of primary interest for addressing the research question at hand by themselves. Nevertheless, they indicate whether treatment was non-random prior to the matching process. If a variable acts as a (statistically significant) predictor for the treatment status but is also correlated with the treatment outcome, it would inevitably bias the treatment effect. After all, the treated and untreated control group would differ not only in their treatment status, but also in their distribution of the treatment-predicting variable. For instance, columns 1, 2, 5, 6, 7 and 8 in table 3.7

[32]The lagged changes in post-tax and pre-tax income inequality are included to ensure that there was no major shock just prior to the treatment. If there were such a shock, one could expect to see e.g. a reversion to the mean, which would bias the subsequent comparison between outcomes. After all, it would be the shock rather than the treatment responsible for determining the outcome.

[33]If there were no gaps in the underlying data, one matching model would be sufficient to study all outcomes. Studying the outcome variables after 2 years and after 6 years results in the underlying data changing. This is due to the fact that there are fewer treated and untreated countries for which there are uninterrupted data available for both time-spans.

Treatment	Horizon	Outcome	Model number in table 3.7
Increase in polity2	2 years	Δpre-tax Gini and Δpost-tax Gini	1
Increase in polity2	6 years	Δpre-tax Gini and Δpost-tax Gini	2
Decrease in polity2	2 years	Δpre-tax Gini and Δpost-tax Gini	3
Decrease in polity2	6 years	Δpre-tax Gini and Δpost-tax Gini	4
Increase in FH Index	2 years	Δpre-tax Gini and Δpost-tax Gini	5
Increase in FH Index	6 years	Δpre-tax Gini and Δpost-tax Gini	6
Decrease in FH Index	2 years	Δpre-tax Gini and Δpost-tax Gini	7
Decrease in FH Index	6 years	Δpre-tax Gini and Δpost-tax Gini	8

Table 3.5: List of treatment and outcome combinations associated with all models in table 3.6 and 3.7.

illustrate that GDP growth is significantly negatively related with undergoing democratization. If GDP growth is not only related to this treatment status, but also has a positive causal effect on income inequality, then a direct comparison of income inequality changes between treated and untreated countries without the matching procedure would be invalid and biased. After all, countries with high (low) growth would see their income inequality increase (decrease), but at the same time countries with high (low) growth would be far more likely to end up in the untreated (treated) group, making it infeasible to determine which part of the overall change in income inequality is caused by a country's growth rate and which part is caused by the treatment status.

The goodness of fit for all the Probit models is expressed as a pseudo-R^2. This goodness of fit measure ranges from a minimum of 0.091 in model 3 to a maximum of 0.243 in model 6. These values are quite low, and indicate that predicting the treatment status on the basis of these characteristics is difficult. This is not surprising, as both democratization and democratic breakdowns are infrequent events. The treatment status is nevertheless non-random prior to the matching procedure, as the employed variables jointly hold both individual and joint predictive power. The accuracy of the comparison of outcomes is therefore improved by the matching procedure.

Matching Goodness of Fit

Table 3.6 shows the summary statistics of the matching quality connected to each of the Probit models. The goal of the matching procedure is to bring the characteristics of the treated and untreated group sufficiently close together so that they no longer have any power in predicting the treatment status. The very low pseudo-R^2, the low likelihood-ratio test statistics and the very high p-values indicate that these characteristics are jointly entirely incapable of predicting whether a country undergoes any institutional change.

Model	Pseudo R^2	LR χ^2	$p > \chi^2$	Mean Bias (%)	Median Bias (%)	Rubin's B	Rubin's R
1	0.010	3.88	0.868	6.3	7.4	23.1	0.95
2	0.010	3.81	0.923	5.4	3.9	23.4	0.76
3	0.005	0.65	1.000	5.6	5.8	16.8	1.22
4	0.004	0.64	1.000	3.3	1.8	14.8	1.48
5	0.007	4.51	0.808	6.9	5.9	19.9	0.85
6	0.008	4.48	0.723	6.9	6.6	21.1	1.08
7	0.006	3.33	0.912	4.4	3.7	18.5	1.03
8	0.004	1.83	0.768	5.0	4.1	14.0	1.31

Table 3.6: Summary statistics for the matching quality. The pseudo-R^2, the likelihood ratio test and the corresponding p-values all indicate whether the remaining differences between the treated and untreated control observations have any predictive power in the treatment status. The mean and median bias indicate the percentage difference between the characteristics in the treated versus the untreated control group. Rubin's B illustrates the absolute standardized difference in the propensity scores, which should not exceed 25. Rubin's R shows the variance ratio of the propensity scores, which should be within the range of 0.5 to 2, and as close as possible to 1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
pre-tax Gini$_{t-1}$		0.0111		-0.0438*				
		(0.77)		(-2.48)				
post-tax Gini$_{t-1}$	0.0203**	0.0124	-0.0106	0.0208	0.00514	0.0105	0.0137*	
	(3.20)	(1.10)	(-1.15)	(1.45)	(0.95)	(1.41)	(2.32)	
Δpre-tax Gini$_{t-1}$		-0.127						
		(-0.92)						
polity2$_{t-1}$	-0.0563***	-0.0558***	0.0215	0.0414**				
	(-7.02)	(-5.52)	(1.63)	(3.10)				
FH Index$_{t-1}$					-0.104***	-0.0813***	0.0242	0.0332
					(-6.48)	(-3.53)	(1.40)	(1.53)
Trade$_{t-1}$	-0.0028	-0.0034*	0.0001	0.0020	-0.0029*	-0.0031	-0.0013	-0.0017
	(-1.89)	(-2.34)	(0.03)	(1.45)	(-2.28)	(-1.88)	(-1.04)	(-1.05)
post-tax FDI$_{t-1}$	0.0119	0.00411	0.0235		-0.00771		-0.0292*	
	(0.87)	(0.28)	(1.42)		(-0.58)		(-2.45)	
Life Exp.$_{t-1}$	0.0318***	0.0197*	0.00592	-0.0124	0.0146	0.0273*	0.0167*	0.0274
	(3.68)	(1.98)	(0.47)	(-0.99)	(1.86)	(2.35)	(2.08)	(1.52)
ln(GDP$_{t-1}$)	-0.239***	-0.317***	-0.345***	-0.294**	-0.136*	-0.396***	-0.393***	-0.6542***
	(-4.28)	(-3.97)	(-4.23)	(-2.95)	(-2.54)	(-5.26)	(-6.85)	(-9.32)
GDP Growth$_{t-1}$	-2.492*	-2.817*	-2.052	-0.331	-2.591**	-4.550**	-2.395*	-3.1134*
	(-2.36)	(-1.98)	(-1.38)	(-0.18)	(-2.69)	(-2.91)	(-2.35)	(-2.05)
Prim. School$_{t-1}$	-0.0077**		-0.0017		-0.0037	0.0002	-0.0016	
	(-2.68)		(-0.43)		(-1.37)	(0.05)	(-0.59)	
Constant	-0.744	0.145	0.615	1.830*	0.285	1.503*	0.570	2.7451***
	(-1.60)	(0.24)	(0.92)	(2.52)	(0.64)	(2.23)	(1.25)	(7.22)
Observations	2136	1421	2136	1720	1734	813	1822	1083
Pseudo R^2	0.159	0.233	0.091	0.148	0.126	0.243	0.130	0.239

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.7: Probit regressions used for obtaining propensity scores, using all available treated and untreated observations. The regression is based on equation 3.6. Models 1-8 are summarized in table 3.5.

All Rubin's B statistics fall below the suggested value of 25, indicating that the propensity scores do not differ substantially between the treated and untreated groups. Furthermore, all Rubin's R statistics are close to 1, indicating that the variance ratios of the propensity scores are very similar between the treated and the untreated control group. There is no standard benchmark for the mean or median bias percentages. These percentages can only be interpreted as a crude indicator for how close the matched variables lie to one another, hence the smaller the bias, the better the match.[34] Based on the Rubin's B and Rubin's R statistics, the matching procedure is successful in removing pre-treatment differences in observable characteristics (steps 5 and 6 in figure 3.3).

Treatment Effects

Table 3.8 displays all the estimated treatment effects on the change in pre-tax and post-tax income inequality after 2 years and after 6 years, following a period of democratization or a democratic breakdown. Positive coefficients indicate that the treatment resulted in upward pressure on income inequality, while negative coefficients show that there was downward pressure on income inequality. The table also depicts the number of observations used in the underlying Probit model (treated observations and candidates for the untreated group), along with how many treated observations were ultimately used.

In the case of increases in the *polity2* index, all estimated treatment effects are negative. The coefficients range from -0.0103 to -0.397, indicating that the corresponding Gini coefficients are between 0.0103 and 0.397 points lower than they would have been if the country had not undergone any democratization. Though the negative coefficients indicate that both pre-tax and post-tax income inequality ended up being lower in countries undergoing democratization than in those that did not, their size is minimal. The pre-tax and post-tax Gini coefficients range from approximately 20 to 68 in the overall sample, hence the estimated treatment effect is minute. All coefficients feature low t-statistics, and are insignificantly different from 0. Only the effect on post-tax Gini after 6 years is significant at 10%. More importantly, two t-statistics reveal that the change in the difference between the pre-tax Gini coefficient and post-tax Gini coefficient due to undergoing democratization is insignificant after 2 years ($t = 0.38$) and after 6 years ($t = 0.42$). Consequently, increases in the *polity2* index are not associated with any subsequent changes in income inequality, nor income redistribution.

The second set of treatment effects is based on decreases in the *polity2* index, and reveals a different story. Decreases in the *polity2* index are also associated with downward pressure on both pre-tax and post-tax income inequality, with the effect ranging from -0.0637 to -0.688. This time, the treatment effect after 2 years remains very insignificant, but turns significant after 6 years.

[34] For a more detailed overview of the matching quality for each matching procedure, see the supplementary online appendix.

	(1) Δpre-tax Gini (2 years)	(2) Δpost-tax Gini (2 years)	(3) Δpre-tax Gini (6 years)	(4) Δpost-tax Gini (6 years)
polity2 increase	-0.0103	-0.0526	-0.264	-0.397
	(-0.13)	(-0.66)	(-1.20)	(-1.71)
t-statistic (Δpre-tax = ΔNet)		0.38		0.42
Total N	2136	2136	1421	1421
Treated N	145	145	140	140
polity2 decrease	-0.0637	-0.104	-0.688**	-0.631*
	(-0.71)	(-0.91)	(-2.64)	(-2.24)
t-statistic (Δpre-tax = ΔNet)		0.28		-0.15
Total N	2136	2136	1720	1720
Treated N	45	45	54	54
FH Index increase	0.0569	0.0735	0.128	0.0609
	(0.91)	(1.18)	(0.54)	(0.25)
t-statistic (Δpre-tax = ΔNet)		-0.19		0.20
Total N	1734	1734	813	813
Treated N	228	228	201	201
FH Index decrease	0.0294	-0.0269	-0.032	0.029
	(0.43)	(-0.40)	(-0.17)	(0.16)
t-statistic (Δpre-tax = ΔNet)		0.58		-0.24
Total N	1822	1822	1083	1083
Treated N	193	193	184	184

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.8: The estimated treatment effects on changes in the pre-tax and post-tax Gini coefficients over a period of 2 years and 6 years respectively. The t-statistic displays whether the change in the pre-tax income inequality is significantly different from the change in post-tax income inequality.

Countries that have experienced a democratic breakdown end up having significantly lower pre-tax and post-tax income inequality, with the respective Gini coefficients moving away by -0.688 points and -0.631 points when compared to the untreated countries. Once again, however, the t-statistics reveal that the pre-tax and post-tax income inequality changes are insignificantly different after 2 years ($t = 0.28$) and after 6 years ($t = -0.15$), indicating that there is no change in income redistribution. While income inequality ends up being significantly different after a democratic breakdown, redistribution within the treated countries remains unchanged.

Income inequality is statistically unrelated to democratization, but shows a strong connection to democratic breakdowns when using the *polity2* index. Considering that the post-tax inequality change is almost identical to the pre-tax inequality change, it is the pre-tax income inequality that is primarily affected. Despite the decreasing income inequality, the level of redistribution remains unaffected. It may be that the drop in income inequality is the result of a policy change targeting pre-tax income inequality, which would automatically affect post-tax income inequality to the same extent. Non-democracies may try to gain legitimacy by pursuing policies that reduce income inequality without the use of redistribution. An example hereof are the mass literacy programmes launched in Zimbabwe, after Mugabe's ascension. Such a literacy programme is likely to primarily benefit the poor, improving their chances of finding well paying work, thereby reducing income inequality. Ultimately, the main result to take from the segment describing *polity2* decreases in table 3.8 is that democratic breakdowns do not affect redistribution.[35]

The third set of treatment effects corresponds to increases in the Freedom House index. Compared to the *polity2* counterpart, these coefficients show the opposite sign, suggesting that democratization increases both pre-tax and post-tax income inequality. This effect ranges from 0.0569 to 0.128. Once again, the coefficients are insignificantly different from 0, and are insignificantly different from one another after 2 years ($t = -0.19$) and after 6 years ($t = 0.20$). Hence, redistribution is also unaffected by upward changes in the Freedom House index.

The final set of treatment effects shows the consequences of democratic breakdowns, as measured by the Freedom House index. These coefficients illustrate that democratic breakdowns result in different Gini coefficients, with the effect ranging from 0.294 to -0.032. These effects, too, are entirely insignificantly different from 0, and insignificantly different from one another after 2 years ($t = 0.58$) and 6 years ($t = -0.24$). In contrast to democratic breakdowns as measured by downward changes in the *polity2* index, democratic breakdowns measured by downward changes in the Freedom House index are associated with neither pre-tax or post-tax income inequality changes,

[35]There may also be indirect effects of democratization on income inequality. Democracy is generally seen as a cause for increasing, among other factors, FDI (see e.g. Asiedu and Lien (2011) or (Li, 2009) on the topic). FDI is conducive to income inequality (Chintrakarn et al., 2012; Clark et al., 2011). Consequently, periods of democratization may increase FDI, which in turn increases income inequality. Similarly, the reverse can be expected from periods of democratic breakdowns.

nor with any changes in income redistribution. The relation between democratic breakdowns and income inequality is therefore not robust to the choice of the democracy data.

These results fully contradict the MRF. The hypotheses formed on the basis of the MRF and its derivatives are:

Hypothesis 1: Democratization results in an increase of income redistribution (an increase in the gap between pre-tax and post-tax income inequality).
Hypothesis 2: Democratic breakdowns result in a reduction of income redistribution (a decrease in the gap between pre-tax and post-tax income inequality).

There is no evidence in favour of hypothesis 1, since income redistribution remains unchanged as a result of democratization. Similarly, hypothesis 2 is rejected, since income redistribution is unaltered after democratic breakdowns. The significant decrease in pre-tax and post-tax income inequality after democratic breakdowns, as captured by the *polity2* index, does not provide any evidence in favour of this hypothesis, since income redistribution is left unaltered. Both hypotheses are rejected regardless of the choice for the measure of democracy. The results found via PSM match those based on the linear regression, typically denoting even less significant relations between changes in democratic institutions and changes in income inequality. By addressing the limitations of methods based on linear regressions, results that are skeptical of the basic MRF are thus bolstered.

Note that the results do not suggest that there are any attempts for governments try to influence income inequality via non-redistributive methods after periods of democratization. Governments could, after all, aim to equalize pre-tax income distribution, so that there is no need for any redistribution. If institutional changes would be accompanied by policies aimed at affecting pre-tax income inequality such as increases in education or improved health-care for the poor, which should result in the ability to earn more, then both pre-tax and post-tax income inequality would fall.[36] Such policies would reduce the need for redistributive policies, but would nevertheless affect the level of post-tax income inequality. The corresponding treatment effects do not show up for countries undergoing democratization, indicating that there is no clear pattern toward such policies.[37]

[36]Technically, it is possible that the effect of policies aimed at reducing pre-tax income inequality would be cancelled out by the non-policy related income inequality-increasing effects of democratization, such as the aforementioned FDI channel. It would be an incredible coincidence for these effects to cancel out in all specifications, and across all time-spans, however, hence such an explanation seems implausible.

[37]Note that policies affecting pre-tax income inequality may also take a longer time to emerge than 6 years. The matching process proves too inaccurate at time-frames longer than 10 years, hence such results are at present too difficult to measure with the data at hand.

Robustness Checks

I make use of several alternative specifications to determine the robustness of the above findings. Each alternative specification requires the re-estimation of every matching procedure and every treatment effect. For the sake of space, the corresponding tables for the Probit regressions, the matching quality and the treatment effects are omitted in this section. [38]

The lack of any significant effects on income redistribution may be the result of the treatment being set to broadly. In the preceding section, treatment is defined to take place when there is a change in the *polity2* index or the Freedom House index by *any* amount. Small changes in these indices may simply represent too minute democratic changes to realistically have an impact on income inequality or income redistribution. I therefore re-run all above matching procedures and treatment effect estimations with treatment variables that only assign a positive treatment status when the respective democracy indices change by at least 2 points from one year to another. While this redefinition results in a stronger relation between the treatment effects, these effects are nevertheless all insignificant. Filtering out minor changes in democracy therefore does not significantly alter the level of pre-tax nor post-tax income inequality. Furthermore, the T-statistics comparing pre-tax to post-tax income inequality changes remain very low, indicating that income redistribution also remains unchanged. Increasing the required change in the democracy index to 3 results in too few treated observations to accurately perform the matching procedure, hence such treatment effects cannot be estimated with the data at hand.

Research starting from Kuznets (1955) suggests that the relation may be curvilinear, and that the treatment effect is conditional on the existing level of democracy. I therefore split the data into sub-samples based on their existing *polity2* score and based on their existing Freedom House index. The cut-off point based on the *polity2* score is 0 (the middle point of the index' range), so that countries below this point are labelled as non-democratic, and those with higher scores are labelled as democratic. The resulting sub-samples based on this split produce the same results, with all treatment effects being insignificantly different from 0. Furthermore, all changes in income redistribution are also insignificantly different from 0. The cut-off point based on the Freedom House index is 6 (the middle point of the index' range). The split based on the Freedom House index produces equivalent results, once again indicating that there is no change in income redistribution.

If democratization does not persist and the new level of democracy is not consolidated, then it may come as no surprise that there are no visible effects on income inequality. To avoid such democratic reversals, I add the condition that a country only undergoes democratization if its *polity2* score or its Freedom House index do not drop within 2 years or 6 years (corresponding with the

[38] Due to the reduced number of observations in some of the below specifications, the underlying matching procedure is sometimes less strict than in the main results, as treated and untreated observations cannot always be as easily matched anymore.

time-spans of the outcome variables). In practice, such democratic reversals only occur in a handful of cases, and the ultimate conclusion remains unchanged. Neither democratization nor democratic breakdowns have a significant effect on income redistribution.

3.8 Discussion

The results show no evidence in favour of the main prediction of the MRF and its derivatives, which state that democratization should result increasing income redistribution. There may be several reasons why the relation fails to appear.

First, it may be that countries simply lack the capacity to change their taxation and redistributive system, and simply try but fail to generate the desired result of a tighter income distribution. Such organizational incapacity is plausible for the developing world, where infrastructure is limited, and know-how might be lacking. However, both Acemoglu et al. (2015) and Yi and Woo (2015) find that democratization is positively associated with an increase in government spending and taxation. At the very least, the capacity for governments to access new funds is increased after a period of democratization. These funds are nevertheless not devoted to the poor in the form of an income transfer. If there is no redistributive system in place, then any redistributive policy simply cannot be implemented. This is a doubtful explanation, however. If governments can raise more funds, they must have a functional taxation system in place. Redistribution can take place not only through income transfers to the poor, but also through more progressive taxation systems. Therefore, if post-tax income inequality is unaltered, but government spending and taxation have increased, then altering the level of income redistribution may simply not have been one of the government's goals. The lack of any significant effect on pre-tax income inequality indicates that the government does not make any measurable efforts to affect income inequality via means other than redistribution. Acemoglu et al. (2015) stress that the role of fiscal mechanisms may be overstated, but if this were the case, then countries experiencing institutional changes should still display significantly different pre-tax income distributions compared to their unchanged counterparts. This, however, does not emerge from the results in this paper.[39]

A second reason for why inequality does not drop after democratization is that democracy is captured either by the middle class (Director's Law (Stigler, 1970) or its extended version (Acemoglu et al., 2015)) or by the economic elite (Krieger and Meierrieks, 2016). If the middle class becomes the politically dominant group, it can choose to distribute resources away from the tails of the income distribution toward itself. Thus, the rich are made poorer, but the poor also lose out.

[39]There is an extensive literature on state capacity, some of which highlights that one should not only look at taxation amounts but also the taxation methods. For instance, Di John (2011) highlights that Latin American countries increased VAT, which can be regressive in itself. If such regressive taxes are then used for progressive redistribution, the net effect may be null. As such, it may be warranted to further explore the potential interaction between state capacity and income inequality in future research.

If the economic elite capture and overwhelm the democratic process with their extensive economic resources, then there is little chance that any redistributive policies are pursued. The economic elite has the tools to lobby against increases in taxation, to create propaganda to dissuade the electorate from pursuing it or make use of loopholes to evade taxation. The process of democratization can be countered or made ineffective via the *de facto* power gained via economic resources.[40] Accounting for the elite's power through economic resources and incorporating dynamics of the power's elite goes well beyond the scope of this paper. The absence of the elite's economic power in the MRF may therefore be a driving factor in the MRF's empirical failure.

Perhaps the most obvious reason for the theory being rejected is that the inherent assumption in the MRF about the poor wanting to redistribute money toward themselves is wrong. Knutsen and Wegmann (2016) study the World Value Survey and find evidence that redistribution from the rich to the poor is not a commonly wanted feature, nor is it associated with democracy. Any extension of political influence to the poor is unlikely to result in income redistribution if the poor have no interest in direct income transfers. An extension of the franchise toward the lower economic classes then has no impact on the gap between pre-tax and post-tax income inequality. The World Value Survey only recently introduced the topic of redistributional preferences in its questionnaire. Its inclusion in any empirical framework is as of yet infeasible, but may nevertheless provide a foundation for augmenting theoretical models. Furthermore, its future data may prove useful in determining the necessary conditions for redistribution to take place.

Another explanation is that the poor do care about income redistribution, but that other policies matter more and are therefore prioritized more. Roemer (1998) shows how the ranking of ideological preferences is the driving force behind which policies are pursued. Even if people have a preference in favour of redistribution, it can be entirely dominated by other, more pressing topics, so that there is ultimately no change in redistributive policies.[41] Redistribution may not even be seen as fair or desirable, if people expect to climb the social ladder and escape poverty that way, or if the consensus is simply that the rich are rich and the poor are poor entirely due to their merit.

In addition to this, Norton and Ariely (2011) and Niehues (2014) show on the basis of survey results that societies seem incapable of accurately assessing how unequal the income distribution is. Inequality is commonly both substantially over- and underestimated. If unequal societies underestimate the extent to which redistribution is possible, then there is no reason to expect any strong pressure toward more redistribution.

These above factors have not yet been tested in the context of the current study. Their incor-

[40] See also Acemoglu and Robinson (2006) on this topic.

[41] The ultimate goal of redistributing income toward oneself would be to increase the quality of life by increasing the set of consumption options. Such options can also simply be provided directly, leaving out the income transfer (e.g. schooling, health care, and so forth). See Gerring et al. (2012) for an example hereof.

poration therefore provides the next logical step in addressing the MRF's shortcomings.

3.9 Conclusion

Income inequality and income redistribution have received much attention in economic theory, but their connection to democracy is not yet conclusively documented. The dominant theory predicts that democratization increases income redistribution by giving political power to the poor, who demand income redistribution so as to improve their own economic status. Conversely, democratic breakdowns remove this political power from the poor and place it back with the country's economic and political elite, therefore reducing the need and desire for redistributive policies.

This paper tests this predicted connection between democracy and income redistribution. Past empirical research largely does not take into consideration that data are more readily available for countries in which democracy is already strong and long established. This availability results in an overrepresentation of such countries, which renders it infeasible to accurately estimate the effects of a democratic change. The current study addresses this selection bias by applying Propensity Score Matching, which creates a balanced sample between countries that undergo democratic changes and their closest unchanging counterparts. Based on this matching procedure, I find that democratization is not statistically significantly related to an increase in income redistribution. Similarly, I find that democratic breakdowns have no significant impact on income redistribution. Neither democratization nor democratic breakdowns affect income redistribution after a period of 2 years nor 6 years. This relation remains insignificant when accounting for the magnitude of democratic change, the persistence of democratic change and the existing level of democracy.

The results in this paper provide no evidence in favour of the theoretical predictions of the dominant framework devised by Meltzer and Richard (1981) and its subsequent derivatives. There are several possible causes for why the theoretical predictions fail to emerge. These provide logical avenues for future research, in order to determine whether the original framework merely needs more detailed extensions, or whether it can be rejected in its entirety. For instance, it assumes that the economically poor always have a desire for higher redistribution, but factors such as perceived social mobility and ideology may result in higher redistribution being undesirable or left unpursued. The effect of democracy on redistribution and income inequality may therefore be more nuanced than theory has thus far suggested. Hopefully, future research will examine these nuances in more detail, so that theory and empirical evidence can be reconciled.

Chapter 4

The Impact of Capital Account Liberalization on Economic Growth

4.1 Introduction

There is a prominent strand of literature predicting and studying the effects of financial liberalization. To this day it remains inconclusive on whether the pursuit of financial liberalization leaves countries better or worse off. Early theory on financial liberalization has predicted positive outcomes. Free capital markets, as opposed to repressed ones, are said to allocate funding more efficiently, both domestically and across borders, thereby increasing investment and economic growth (McKinnon, 1973; Shaw, 1973; Levine, 2001). It is with such outcomes in mind that countries pursued extensive liberalizing reforms in the latter half of the 20th century, with a major emphasis on access to international capital markets. Early empirical studies, often cross-sectional in nature, predominantly found that financial liberalization enhances economic performance. However, events such as the various Latin American external debt crises since the 1980's and the Global Financial Crisis of 2007-2009 have cast major doubt on the benefits of such reforms. Furthermore, countries such as China have displayed remarkably strong, persistent economic growth, despite the absence of extensive liberalization reforms. Both theory and empirical studies have undergone great improvements, as a result of greater data availability and methodological improvements. Nevertheless, no consensus has emerged. A major strand of literature highlights the dangers or futility of financial liberalization, while others see plenty of room for benefits, albeit with many nuances attached (Stiglitz, 2000; Rodrik and Subramanian, 2009; Kose et al., 2009). As such, both the necessity and sufficiency of financial liberalization in stimulating economic growth are presently uncertain.

This paper revisits the question whether financial liberalization enhances economic growth, in the context of capital account liberalization. The paper aims to contribute to the literature in two ways. The primary contribution is a methodological one. The current paper makes use of Panel Vector Autoregression (PVAR) to study whether capital account liberalization has direct or indi-

rect effects on economic growth. A primary benefit of this method is that it accounts for potential endogeneity problems by making use of an Instrumental Variable Generalized Method of Moments (IV-GMM) estimation process. This method enables the capture of potential feedback processes between variables of interest, and provides an overview on the longevity of any potential effects of financial liberalization on economic growth via Impulse Response Functions (IRFs). These aspects have, to my knowledge, not been explored in existing literature, which has thus far primarily taken the form of either cross-sectional studies, or dynamic panel models. The second contribution is that this paper tests whether the effects of liberalization are conditional on institutional quality or economic development. These factors may play a large role in determining whether financial liberalization is able to generate any benefits or not. It also looks at whether IMF lending, which has often been conditional on liberalization reforms, has an impact on the outcome of reforms.

The main finding of this paper is that financial liberalization in the form of capital account liberalization is positively associated with economic growth. A moderate capital account liberalization reform is estimated to generate a 1 to 2 percentage points worth of additional economic growth on average. The effect of liberalization is found to be short-lived, with growth returning to its pre-liberalization level within 5 years. The paper also finds that there is no evidence that strong institutions are a prerequisite for capital account liberalization to generate such growth. This is in contrast with recent theoretical literature, which argues that the absence of strong institutions undermines the benefits of liberalization. An additional result is that the estimated benefits of financial liberalization are largely the same between highly developed and less developed countries. This, too, is not an obvious result. The prevalent theoretical expectation is that less developed countries are starved for capital, which can fuel high-yield investments. International capital markets can provide this much-needed capital. A final result is that there is little evidence of any impact of IMF intervention on the effects of financial liberalization.

The paper is structured as follows. Section 4.2 first presents a synthesis of the existing literature. Section 4.3 then summarizes the hypotheses studied in the current paper. After that, section 4.4 explains several limitations of the methodologies employed in the existing literature, and outlines the methodology used in this paper. Following this, section 4.5 presents the datasets used in this paper, after which section 4.6 provides an overview of descriptive statistics alongside a preliminary analysis of the data. Section 4.7 covers the main findings. Finally, section 4.8 provides a brief conclusion along with suggestions for further research.

4.2 Literature Review

4.2.1 Theory

An important strand of theory on economic growth is based on the Solow-Swan model (Solow, 1956; Swan, 1956). These models provide an explanation for economic growth by highlighting the role of savings in fueling physical capital accumulation. In such models, capital accumulation can only temporarily generate economic growth, as it is typically assumed to run into diminishing returns. These diminishing returns render further capital accumulation increasingly less productive and difficult. A core feature of the model is that productivity increases via capital accumulation eventually grind to a halt, resulting in a steady state. At this point, economic growth is entirely based on factors exogenous to the model, such as technological progress. Based on this framework, one can therefore make the distinction between long-term steady state growth, and growth leading up to the steady state.

In closed economies, investments can only be fueled by domestic savings, which in turn are limited by domestic output. Open-economy extensions to the Solow-Swan framework explore the effects on investment when countries liberalize and allow for international capital mobility. As highlighted by Henry (2007), liberalization primarily shows its impact through the cost of capital. If a country has a small stock of capital per worker compared to the rest of the world, its returns to capital are relatively high. When there is a wedge between the marginal returns to capital and the cost of capital, capital inflows increase dramatically. Under perfect capital mobility, a country's capital stock increases instantly to the point where the marginal returns to capital match the world cost of capital. Small countries, which have no influence on this world cost of capital, can therefore instantly move into a steady state with higher output per capita than before the liberalization. The absence of any transition dynamics is an unrealistic feature of the model that has been resolved by introducing capital mobility imperfections (Barro et al., 1995), institutional quality requirements (Klein, 2005) or a central planner slowly providing essential public capital (Clarida, 1993). These extensions slow down the process with which liberalizing countries reach their steady state, but ultimate still rely on exogenous factors to explain long-term growth once such a steady state is reached. Capital account liberalization may have a permanent impact on the *level* of output, but it only temporarily increases economic growth, and it has no impact on countries' long-term steady state growth.

The lack of an endogenous explanation for where long-term growth comes from has stimulated the emergence of Endogenous Growth Models (EGMs). These models highlight the roles of research and development (R&D), human capital, international trade and various other factors in determining long-run growth (Romer, 1986; Aghion and Howitt, 1992; Rivera-Batiz and Romer, 1991). Instead of having an exogenous rate at which technology improves, EGMs aim to explain where this rate comes from and how it may be affected by policy decisions. One strand of endoge-

nous growth models focuses on the role of financial systems in determining or influencing long-term economic growth, also dubbed Financial Endogenous Growth Models (FEGMs). These models explain how R&D or human capital accumulation can be improved via financial systems. Bencivenga and Smith (1991) provide a comprehensive model explaining the positive role of financial intermediaries in allocating savings efficiently. In this framework, entrepreneurs are constrained in their access to long-term credit due to savers' liquidity preferences. The introduction of financial intermediaries, such as banks, resolves this mismatch, resulting in a higher and more efficient allocation of savings towards productive capital. Such productive capital is, in turn, useful for sectors such as R&D, which determine long-term growth. King and Levine (1993) present a Schumpeterian model where the introduction of well-functioning financial intermediaries leads to a greater allocation of resources toward the creation of productivity enhancements. In this framework, the more developed a financial system is, the better it can mobilize resources and the better it can pick out the most promising entrepreneurs. Such entrepreneurs continuously invent and implement more productive technologies, making them the source for long-term growth. In this context, liberalization is likely to further improve allocative efficiency, by fostering competition, thereby further increasing long-term growth. Rancière and Tornell (2016) create a two-sector model, in which financial liberalization reduces the limitations imposed on the credit constrained sector by making new financial instruments available. The model highlights the role of debt-denomination mismatching, where entrepreneurs' income is denominated in a different currency than their debt. The availability of such a debt-denomination mismatch increases lending and borrowing, resulting in higher and more efficient allocation of capital to the sector that drives long-term growth.[1] Liberalization thus reduces credit constraints and improves allocative efficiency, increasing long-term growth.

While many FEGMs do not focus on the sole role of international capital markets specifically, their underlying mechanisms are directly tied to capital account liberalization. Access to international capital markets, for instance, allows for greater risk diversification for financial intermediaries, as risk can be more easily spread internationally. Consequently, risk premia for lending fall when the capital account is opened up, thereby reducing lending rates. This increases entrepreneurial access to capital, which boosts investment, thereby increasing economic growth. When connecting this to Schumpeterian frameworks, such increases in investment do not only result in short-run growth increases, but permanently affect the rate at which productivity improves. Furthermore, as the number of sources for capital increases, competition between financial intermediaries also increases. Profit margins are squeezed and lending rates fall, once again increasing access to capital. The result is higher investment and (temporarily) higher economic growth (Bumann et al., 2013). Additionally, there may be a positive feedback loop connecting capital account liberalization, financial development, financial integration and growth. It may be the case that capital account

[1] As per the authors' comments, this model permanently affects growth not via Schumpeterian mechanisms focusing on technological improvements, but rather via changes in Juglar cycles.

liberalization results in larger FDI flows, which result in higher economic growth. This in turn may attract even more FDI (Agénor, 2001). It may also be the case that the prospects of economic growth drive the decision for capital account liberalization. If it indeed does generate higher economic growth, this may generate a push toward further liberalization, thereby generating another feedback process.

There are several extensions to the aforementioned frameworks, focusing on the conditions under which capital account liberalization can generate the promised positive effects on growth. Good institutions, such as a strong judicial system or protection of property rights, stable governmental policies and well developed financial markets can enhance the positive effects of liberalization (Kose et al., 2009; Rodrik and Subramanian, 2009; Asongu, 2014). Strong institutions, such as well established property rights and a strong rule of law, ensure that investments will not be expropriated. This reduces riskiness, resulting in greater cross-border capital flows. Without such institutions in place, foreign investors may be unwilling to provide any capital, as the chance of having no returns becomes too great. Strong institutions also limit the extent of corruption, which otherwise erodes the returns to investment (Kunieda et al., 2014). Stable governmental policies render the returns to investment more stable, as they are unlikely to be affected by unexpected regulatory changes, thus once again reducing riskiness. Well developed financial markets make it more likely that capital flows to where it is most productive, as there is accurate information about possible investments, and investments can better be monitored (Levine, 2005). The role of institutions has become particularly relevant since developing countries have started to liberalize their financial systems. Prior to such liberalization, much of international capital moved within the developed world, where the quality of institutions is relatively homogeneous.

A major weakness of the neoclassical framework and many of the FEGMs is that the underlying assumptions are far removed from reality. The absence of any asymmetric information in many of these frameworks is particularly troublesome. Many FEGMs rely on a higher allocation of funding toward entrepreneurs. Asymmetric information may, for instance, result in a lemon market, where productive entrepreneurs are driven out of the market. Stiglitz and Weiss (1981, 1992) provide a series of models pointing out that the presence of asymmetric information results in sub-optimal credit provision for investments. When lenders are unable to accurately determine the quality of an investment and hedge themselves against potential downside risk, the result is a low amount of lending. Well-developed financial intermediaries are able to partly resolve information asymmetries through relationship building between debtors and creditors. As pointed out earlier, financial liberalization is likely to increase competition between creditors, making such relationship building less likely to occur. Consequently, the problem of asymmetric information may become greater in financially open countries. This undermines the potential benefits highlighted in FEGMs, as productivity-enhancing entrepreneurs will find it more difficult to attract funding. The asymmetric information problem is likely linked to the aforementioned institutional requirements. Strong

legal frameworks, along with rules and regulations surrounding financial discipline and disclosure may offset asymmetric information problems. A clear structure outlining what to do in case of an entrepreneur's insolvency, for instance, reduces the risk of lending for financial intermediaries.

Stiglitz (2000) points out that capital market liberalization is likely to produce instability rather than growth. One reason for this is that the risk of sudden stop crises and currency crashes is increased. While open capital accounts may make it easier for capital to flow into a country, it also makes it simpler for capital to leave the country rapidly or at the very least stop flowing into the country (as e.g. FDI funding is likely transformed into illiquid physical assets). Emerging markets are particularly vulnerable to this. Any change in perception from foreign investors may result in a major redirection of capital flows. Furthermore, as pointed out by Caballero (2016), open capital markets can expose emerging markets to large surges in capital inflows. These are not allocated efficiently but merely create lending booms and bubbles. Consequently, banking crises are more likely to occur after financial liberalization. Rodrik and Velasco (1999) provide a concise theoretical framework illustrating how countries borrowing on international capital markets are vulnerable to sudden changes in their external creditors' preferences, generating self-fulfilling debt crises. Such sudden changes may be based on unsustainable macroeconomic policies, or on Keynesian animal spirits when investors' home country is undergoing economic trouble. Capital flights are often accompanied by currency crashes, which render external debt insurmountable, thereby deepening the problem.[2]

Rodrik and Subramanian (2009) argue that developing countries are just as likely to be constrained in investment opportunities as being constrained by available savings. Rather than having a lack of available funding, there can be a lack of worthwhile, profitable investment opportunities. In other words, the core premise of the neoclassical model, suggesting that capital can be put to better use in less developed countries than highly developed ones, need not be true. The aforementioned large surges in capital inflows following capital account liberalization may result in a nominal exchange rate appreciation. As prices are likely to be considerably less flexible than exchange rates and are particularly unlikely to plummet by the same percentage as the nominal exchange rate has increased, the real exchange rate also appreciates, thus hurting the competitiveness of exporting industries. Opening up the capital account can therefore result in a *reduction* of profitable investment opportunities, thereby hampering economic growth.

In summary, neoclassical theory and various FEGMs predict that capital account liberalization generates economic growth. Neoclassical theory suggests temporary benefits to liberalization (until a steady state is reached), while FEGMs imply that economic growth can be increased both in the short run and the long run. There may be potential virtuous cycles connecting liberalization,

[2] A prime example of a country stuck with a seemingly perpetual external debt default cycle is Argentina. Extensive financial liberalization in this country has contributed to its poor economic performance, as a result of external debt problems, alongside lengthy austerity measures and hyperinflation.

international capital and economic growth. The effectiveness of capital account liberalization may be conditional on the existing quality of institutions, the level of financial development and the level of economic development, indicating that capital account openness requires a specific environment before generating any benefits. Critics of financial liberalization point at the increased risk of crises and instability and that there is insufficient effective use for major capital inflows. Capital account openness therefore may not generate any clear-cut benefits. Furthermore, the presence of asymmetric information results in inefficient capital allocation and may negate the purported benefits of financial liberalization. Theory is thus overall inconclusive on whether liberalization results in any short term or long term growth.

4.2.2 Empirical studies

From a theoretical perspective, the effects of capital account openness and its expected, accompanying high international capital mobility on economic growth are ambiguous. The empirical literature has long attempted to test the accuracy of the baseline neoclassical model, but has found ambiguous results. A literature review by Eichengreen (2001) concludes that early empirical studies do not provide robust results on any positive effects of liberalization on economic growth, though this may partly be caused by data and methodological limitations. Later reviews by Prasad et al. (2003) and Edison et al. (2004) also reach the conclusion that there is little empirical evidence supporting the theorized positive link between liberalization and growth. A meta-analysis of 60 empirical papers by Bumann et al. (2013) finds that most papers find a positive relation between liberalization and economic growth, but that the statistical significance is generally only limited. While exploring possible causes for heterogeneity in results, these authors find that studies controlling for the level of financial development find a weaker, often insignificant connection between liberalization and economic growth. Financial development may be a mediator for financial liberalization, though. Hence, liberalization may lead to higher growth *through* financial development. A strong correlation between these two variables inevitably results in higher standard errors and thus less statistically significant results.

Research explicitly considering the role of institutions and financial development provides more conclusive insights. Eichengreen et al. (2011) explore the role of thresholds in institutional quality and financial development in determining the effects of capital account liberalization in a sample of 95 countries from 1976 till 1995. These authors find that benefits from financial openness are limited to countries with well developed financial systems, strong institutions and a high level of economic development. Hassan et al. (2011) study a panel of 168 countries from 1980 till 2007 and show that financial development is necessary, though not sufficient for high economic growth. In a study of 112 countries, covering the years 1985 till 2009, Okada (2013) finds that institutional quality and capital account openness are not sufficient on their own to explain capital inflow volumes, but that their interaction is positively related to capital inflows. In a study of 95 countries between 1980 and 1997, Bekaert et al. (2005), too, find that the effects of financial liberalization is

conditional on the quality of institutions. Similarly, Javorcik and Wei (2009) use data from 22 transition economies covering the years 1989 till 1995 and find that the existence of corruption reduces international capital flows, and alters company ownership structures to mitigate problems caused by corruption. Thus, financial development, economic development and the quality of institutions may all play a role in determining the effectiveness of financial liberalization.

A recent strand of literature casts doubt on the positive effects of financial development on economic growth, coining the idea that there can be 'too much finance'. For instance, Hook Law and Sing (2014) study 87 countries from 1980 till 2010, finding that financial development provides positive effects only up to a threshold. More finance beyond this threshold is found to suppress growth. Similarly, a study by Gambacorta et al. (2014) of 41 countries from 1989-2011 reveals that there is a point at which financial activity is excessive and growth-reducing. Capelle-Blancard and Labonne (2016) study the connection between financial depth and economic growth for 24 OECD countries from 1970 till 2008 and find no positive relation between financial deepening and economic growth. A meta-analysis of 68 empirical studies since the late 1990s by Bijlsma et al. (2018) suggests that past studies have overestimated the effects of financial development on growth, though overall evidence does still point toward a positive relation. Capital account liberalization may influence growth partly via financial development. If the positive effects of financial development on growth are only minor, or in some cases even negative, then liberalization reforms are likely to also have only minor or even negative effects on growth.

Bumann et al. (2013) point out that endogeneity is a major issue in establishing the connection between capital account openness and economic growth that has largely been left unaddressed. It is challenging to determine which direction causality runs in between capital account liberalization and economic growth. Is it liberalization that generates growth, or do well performing countries liberalize more? Could both be true, resulting in a feedback process? Bekaert et al. (2005) point out that the exogeneity of the liberalization decision is particularly relevant for countries considering EU membership, which includes both higher capital account openness and potentially good conditions for economic growth at the same time.

An additional source of endogeneity is that capital account liberalization has been pursued extensively as a result of IMF intervention. In particular, the IMF intervenes when countries hold unsustainable amounts of external debt and are struggling to service this debt. Countries experiencing IMF intervention often undergo extensive economy-wide restructuring beyond only capital account liberalization, which has been subject to considerable criticism (e.g. Stiglitz (2000)). Such non-random liberalization may, too, thwart prior studies in accurately determining the effects of opening the capital account on economic growth.

4.2.3 Summary

Neoclassical theory indicates that opening the capital account increases the amount of resources available for investing in productive, physical capital. This increased access speeds up the rate at which countries can accumulate capital, until they reach a steady state. Hence, capital account liberalization is expected to generate a temporary increase in economic growth, until countries reach their steady state. FEGMs suggest that this increase in growth need not be only temporary. Well-developed financial systems provide long-term growth benefits, and capital account liberalization provides conditions which promote financial systems to develop, by increasing competition among financial intermediaries. While both the neoclassical framework and the FEGMs predominantly emphasize the benefits of finance, several factors cast doubt on these benefits. It may be the case that capital account liberalization increases asymmetric information problems, thereby reducing investments rather than increasing them. Furthermore, capital account liberalization may result in a major increase in volatility, exposing countries to the risk of economically damaging financial crises. Such crises may negate any growth benefits. Additionally, much of the theory is founded on the idea that credit constraints hamper growth. It may instead be the case that there is a lack of worthwhile, productive investment projects to allocate credit to. In other words, the premise that capital account liberalization enhances growth *at all* is uncertain.

The empirical literature is inconclusive on which side of the argument carries more merit. There is evidence pointing in favour of the role of institutions, financial development and economic development. Existing literature, however, has not extensively addressed the possible existence of endogenous liberalization, as is the case in the presence of potential feedback loops or liberalization in the wake of IMF interventions. The primary concern is that unaddressed endogeneity results in biased and inconsistent econometric results. Consequently, the empirical question on the existence and direction of the connection between financial liberalization and economic growth is yet unresolved.

4.3 Hypotheses

This section discusses three hypotheses that the current study addresses, which are largely based on the preceding literature review. Figure 4.1 presents a flow-chart overview of the hypothesized relations between financial liberalization, financial development, financial integration and economic growth. Both neoclassical theory as well as the FEGMs indicate that capital account liberalization results in greater international capital mobility, and promotes financial development (arrow A). More access to international capital and better developed financial systems provide improved conditions to invest in productive physical capital and to fund productivity-enhancing entrepreneurs. The result hereof is higher economic growth (arrow B).

It may be the case that the relation between capital account liberalization, financial development, financial integration and economic growth depends on the surrounding conditions. Theory suggests that strong institutions may be a requirement for liberalization to pay off. In the neoclassical framework, less developed countries feature high marginal returns to physical capital, whereas the opposite is true for developed countries. Less developed countries are thus expected to see major financial capital inflows, which fund the accumulation of physical capital and foster growth. This potential relevance of underlying conditions is highlighted by arrow C. The existence of arrow C is explored by stratifying the empirical analysis along the level of development and the level of institutional quality (further described in section 4.7.1).

Arrow D highlights that the involvement of the IMF may have an impact on the outcomes of liberalization policies. Involvement of the IMF typically results in considerable reforms beyond capital account liberalization, which may either synergize or clash with such liberalization efforts.

Arrow E in figure 4.1 denotes that other variables may influence the impact of financial liberalization, development and integration. Two examples hereof are human capital and openness to trade. Such variables are not of primary interest to the study at hand, but are included as control variables. They may capture non-financial reforms that are likely to have an impact on economic and financial performance.

Finally, arrows F, G and H denote that the connection between capital account liberalization, financial development, financial integration, and economic growth may be bi-directional.[3] If this bi-directional connection forms a virtuous cycle, then the effects of liberalization may be larger and more persistent than cross sectional or dynamic panel regressions would imply. Even if arrows A and B are of primary interest, it is necessary to take arrows F, G and H into consideration. The methodology employed in this paper (discussed in section 4.4) explicitly allows for bi-directionality. This makes it possible to spot potential feedback loops, if they exist.

Arrows A and B correspond with the predictions of the neoclassical model alongside the FEGMs. These theories suggest that capital account liberalization results in higher capital inflows (more financial integration) and higher levels of financial development, which ultimately result in higher economic growth. The first hypothesis is therefore formulated as:

Hypothesis 1: Financial liberalization in the form of capital account liberalization results in higher economic growth.

This hypothesis thus aims to capture the joint, cumulative effect of financial liberalization on

[3]Though not depicted in figure 4.1 (for simplicity), arrows F, G and H may also depend on similar underlying conditions as arrows A and B. That is, arrows C, D and E could also connect to F, G and H.

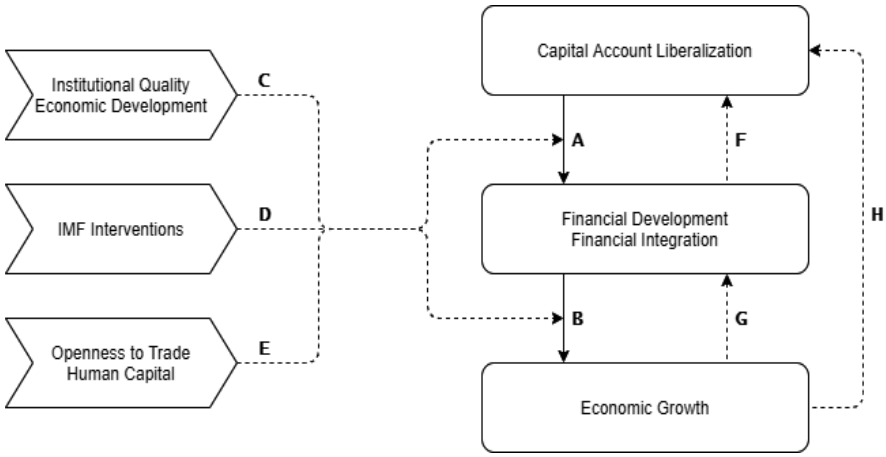

Figure 4.1: Structure depicting the main hypothesized relations of interest. It may be the case that there are feedback loops and that capital account liberalization, integration, development and economic growth affect one another bidirectionally. Institutional quality and economic development may generate threshold requirements that determine the effects of capital account liberalization. Finally, countries undergoing IMF interventions tend to be in an unusual macroeconomic position that may result in notably different effects of financial liberalization.

growth, which is expected to go through financial integration and through financial development.

The second hypothesis builds on the first hypothesis by zooming in on the potential roles of institutional quality and economic development. It aims to capture the conditionality highlighted by arrow C in figure 4.1.

Hypothesis 2: The effect of capital account liberalization on economic growth is positively related to the level of:

(a) Institutional quality

(b) Economic development

Institutional quality is expected to be pivotal in determining the benefits of capital account liberalization. Poor institutions increase the risk of investments and their returns being expropriated, thus making potential investors reluctant to invest. Strong institutions, in contrast, remove this risk and encourage capital inflows. The second part of this hypothesis tests the finding from Eichengreen et al. (2011), that the benefits of capital account liberalization are limited to high-income countries. Neoclassical growth theory would rather expect that developing countries benefit the most from liberalization, as the marginal returns to capital have been less exhausted in low-income countries than in high-income ones.

The third and final hypothesis touches upon the role of the IMF. There may be notable heterogeneity in the effectiveness of liberalization reforms based on whether a country is making use

of IMF credit lines or not, as indicated by arrow D in figure 4.1. The use of IMF credit is not of primary interest in itself. Countries making use of such credit are typically in a poor macroeconomic state, and reforms are imposed upon them rather than decided upon autonomously. The poor macroeconomic state and lack of autonomous interest to reform may spill over into the post-liberalization economic performance.[4] Note that the aim of this hypothesis is not to establish the effectiveness of IMF interventions, but rather to distinguish the unusual state in which countries undergoing such intervention find themselves in.

Hypothesis 3: The effect of capital account liberalization is different for countries undergoing an IMF intervention than those that do not undergo such intervention.

In summary, the paper focuses on three hypothesis, through which I test whether financial liberalization is associated with higher economic growth. Furthermore, I test whether this effect on economic growth is universal or conditional on the quality of institutions, the existing level of development, and interventions by the IMF. There may be bi-directional causality, potentially resulting in feedback loops. This paper explicitly accounts for such potential loops, providing more accurate estimation results and more insights into potential transmission mechanisms of capital account liberalization.

4.4 Methodology

As noted in section 4.2, it is possible that liberalization affects economic growth through financial integration and financial development. Furthermore, there may be various feedback processes connecting capital account liberalization, financial integration, financial development and economic growth. Past literature has not extensively explored such indirect effects and feedback processes, but rather studied the matter via uni-directional models, such as cross-sectional growth regressions or dynamic panel models. This may unduly strengthen or weaken empirical estimates, depending on what kinds of indirect connections and feedback processes exist. A VAR structure provides the benefit of being able to explicitly take into account and model potential indirect connections and feedback loops between variables. Furthermore, it provides a tool in the form of impulse response functions to see how shocks in individual variables propagate through the entire set of variables. When studying VAR structures across multiple countries, it becomes necessary to use its panel variant (PVAR hereafter), to account for possible subject-specific fixed effects. The estimation process of such a fixed-effects PVAR is explained in greater detail below.

This section first introduces the PVAR model and explains why estimating it via OLS results in biased and inconsistent parameter estimates. It then presents the IV-GMM method, which resolves

[4] A country undergoing a sovereign debt crisis is unlikely to be an attractive destination for international capital. Consequently, there may be no positive effects of capital account liberalization for such a country.

the issues OLS suffers from. Next, it outlines how the number of lags in the model is determined. After that, it describes which instruments are selected for the estimation process, along with which challenges appear in this selection process. Finally, the section concludes with an explanation of how the estimation results can be interpreted and connected back to the hypotheses outlined in section 4.3.

4.4.1 Model specification

Baseline model

The baseline PVAR model takes on the form shown in equation 4.1 below.

$$y_{i,t} = \sum_{l=1}^{k} A^l y_{i,t-l} + Bx_{i,t-1} + u_{i,t} \qquad i = 1, 2, ...N \qquad t = 1, 2, ...T \tag{4.1}$$

$$u_{i,t} = m_i + e_{i,t} \tag{4.2}$$

$$\mathbb{E}[e_{i,t} m_i] = 0 \tag{4.3}$$

In these equations, $y_{i,t}$ is an $m \times 1$ vector of endogenous variables and $x_{i,t-1}$ denotes the $p \times 1$ vector of strictly exogenous variables (independent from contemporaneous and past error terms) and $u_{i,t}$ is the error term, composed of both an $m \times 1$ vector of subject-specific, time-invariant fixed effects (m_i) and and $m \times 1$ vector of idiosyncratic errors. In the context of this study, the vector of endogenous variables is composed of measures of financial liberalization, financial development, financial integration and economic growth, while the exogenous variables include the quality of institutions, the level of openness to trade and the level of educational attainment. In the current study, institutional quality is treated as exogenous. While high institutional quality is highlighted to theoretically impact the effects of liberalization, it seems questionable to expect such institutional quality to *cause* the decision to undergo liberalization. Furthermore, it is an extremely persistent series, displaying minimal variation over time.[5] Similarly, the stock of human capital is too persistent to include as an endogenous variable. There is no ex ante clear reason to expect a rapid enough *bi*directional connection between human capital accumulation and the financial sector for a PVAR model to pick up on.[6] Openness to trade is only slightly less persistent, but it is intuitively more closely linked to financial integration in particular. Its inclusion as an

[5] Running the PVAR with institutions as one of the endogenous variables does not prove to be fruitful; transforming the variable into a stationary form renders it insignificantly related to all other variables.

[6] Much of human capital accumulation is centered around education, which is a slow process. Any potential feedback mechanism would thus also appear only with too big of a delay that can realistically be considered in a PVAR model.

endogenous variable generates methodological problems beyond the scope of this paper, however, hence its treatment as an exogenous variable is necessary for the tractability of the models and results.[7] A and B are $m \times m$ and $m \times p$ matrices respectively, containing the parameters that capture the statistical relevance of the l lagged endogenous variables ($y_{i,t-1}$) and lagged exogenous variables ($x_{i,t-1}$).[8] It is assumed that the idiosyncratic errors are i.i.d. for all subjects i and all time periods t. Furthermore, $\mathbb{E}[e_{i,t}] = 0$ and $Var[e_{i,t}] = \Sigma_e$, where Σ_e is a positive semidefinite matrix.[9]

Estimation of equation (1) via OLS would result in biased estimates, since the lagged endogenous variables are inherently correlated with the fixed effects in the error term. In standard panel models, an often used solution is to remove the fixed effects by transforming all variables in the regression equation, either by taking their first-difference or by subtracting the within-subject variable means. Alternatively, one can also add $N - 1$ dummy variables, thereby directly estimating each subject specific fixed effect (LSDV, hereafter).

As described by Nickell (1981), the former method does not fully resolve the endogeneity issue. The regression equation in first differences transforms the first lag of the dependent variable from $y_{i,t-1}$ into $\Delta y_{i,t-1} = y_{i,t-1} - y_{i,t-2}$ and the error term from $u_{i,t}$ into $\Delta u_{i,t} = \Delta e_{i,t} = e_{i,t} - e_{i,t-1}$. While the fixed effects may have been purged from the regression equation, $y_{i,t-1}$ depends on $e_{i,t-1}$ and hence one of the explanatory variables still correlates with the error term, making OLS biased and inconsistent. Similarly, subtracting the within-subject mean from each variable results in all lagged dependent variables to be correlated with the error term, since the mean of the error term by construction includes all time periods. If T is very large, then the problem practically disappears, but this is largely not the case with macroeconomic data. Judson and Owen (1999) show through the use of simulations that when $T = 30$, estimated coefficients are still biased by as much as 20%.

Estimation via LSDV is also shown to be biased (Nickell, 1981). Roodman (2009a) points out that there are post-estimation corrections to the LSDV-induced bias, such as those found by Kiviet (1995), but requirements for their feasibility includes having a balanced panel.[10]

[7] Though there are many anecdotal examples of countries undergoing trade liberalization prior to financial liberalization, the timing between such events varies greatly. Furthermore, the former tends to occur much more gradually and with a limited scope, whereas the latter has a broader scope. Finally, the effects of trade liberalization on economic growth may already occur prior to any liberalization policy being implemented. Firms can, for instance, anticipate the implementation of new trade policies and restructure in advance. At the time of writing, the UK has been negotiating its withdrawal from the European Union's Single Market and subsequent relations vis-a-vis the EU for several years, but firms have not been waiting till the announcement of the conclusion of these negotiations before restructuring.

[8] Much like in dynamic panel models, PVAR models allow for subject-specific heterogeneity in parameters. This option is not pursued here, and it is instead assumed that parameters are homogenous. Heterogeneity is partly explored through stratification and sub-sampling instead.

[9] This does not need to be a diagonal matrix, meaning that the error terms are allowed to be related to one another.

[10] Furthermore, if other regressors are endogenous, then such corrections still leave the endogeneity problem unresolved.

While transformations such as via first-differences and within-subject mean subtraction do resolve the fixed effects problem, the remaining endogeneity requires an instrumental variable (IV) approach. Fortunately, there are still plenty of candidates for such IVs, producing a large number of potential moment conditions.

Establishing the moment conditions

Without loss of generality, suppose that the model features only one lag, no exogenous variables and thus takes on the form of the equation below:

$$y_{i,t} = \rho y_{i,t-1} + m_i + e_{i,t} \tag{4.4}$$

Transforming this equation via first differences (FD) produces:

$$\Delta y_{i,t} = y_{i,t} - y_{i,t-1} = (\rho - 1) y_{i,t-1} + m_i + e_{i,t} \tag{4.5}$$

$$= \rho(y_{i,t-1} - y_{i,t-2}) + (e_{i,t} - e_{i,t-1}) \tag{4.6}$$

Provided that the data are stationary (and hence $\rho < \mathbf{1}$) and that the idiosyncratic errors are not serially correlated (as per the i.i.d. assumption), one can see that $\forall t = 3, ..., T \qquad \mathbb{E}[y_{i,t-2}\Delta y_{i,t-1}] \neq 0$, that $\mathbb{E}[y_{i,t-2}\Delta y_{i,t}] \neq 0$ but that $\mathbb{E}[y_{i,t-2}\Delta e_{i,t}] = 0$. In other words, $y_{i,t-2}$ is correlated with the regressand, is correlated with the endogenous explanatory variable, but is uncorrelated with the error. Consequently, it is a perfectly viable candidate as an IV for $\Delta y_{i,t-1}$. Any lag beyond $t-2$ is, in fact, strictly exogenous and hence a candidate as an IV for the endogenous explanatory variable.

If only the first lag in levels is selected as an IV, the estimation process is exactly identified. The later the observation is, the more lags there are available as potential instruments. For observation $y_{i,T}$, for instance, there are $T-2$ viable instruments to use. This brings about potential problems caused by overidentification, which are discussed in section 4.4.1.

Rather than taking first differences and using second lags and onward as instruments, Arellano and Bover (1995) propose an alternative transformation of the regression equation. This transformation is commonly referred to as *forward orthogonal deviations* (FOD), and bears some resemblance to subtracting within-subject variable means. Instead of using the full sample of observations to determine the within-subject variable means, FOD makes use of only future observations. For observations at time t, one can subtract the mean of observations from time $t+1$ till T. A result of this transformation is that the list of viable instruments begins with the first (un-transformed) lag rather than the second lag, as the transformation introduces only future error terms into the

regression equation rather than lagged ones. FOD has a much easier time in dealing with gaps in the data, as the transformation is always available, provided there is at least one future observation. FD, in contrast, has to drop observations whenever there is a gap in the data, as the transformation cannot be performed. Various simulations show that the FOD transformation tends to outperform FD (Hayakawa et al., 2009; Phillips, 2020). For these reasons, the current study makes sole use of the FOD transformation.

Extending equation 4.4 by adding further lags of the dependent variables, and including exogenous explanatory variables (i.e. turning it back into equation 4.1) does not affect the GMM estimation described above. Strictly exogenous variables remain strictly exogenous after both the FD and FOD transformations, and additional lags of dependent variables have a large number of instruments available, similar to the first lag.

Lag length selection of endogenous variables

The optimal number of lags k in the baseline model can be obtained using two Model and Moment Selection Criteria (MMSC). These were introduced by Andrews and Lu (2001) for the context of GMM and their application is similar to the Bayesian Information Criterion (BIC) and Hannan-Quinn information criterion (HQIC). The primary purpose of these tests is to check whether the inclusion of an additional lag provides sufficient explanatory information to justify such inclusion. For the sake of brevity, the formulas are omitted in this paper. The models presented in the empirical results section are tested on the basis of the BIC and HQIC.

Instrument selection

Roodman (2009b) makes a note that while the availability of many potential instruments seems wonderful, it does bring about the problem of instrument proliferation. Weak instruments receive much attention in the empirical literature, but the issue of (extreme) overidentification is of equal importance. Roodman points out several major problems. One can overfit endogenous variables, to the point that the endogenous components are not removed. A trivial example hereof is when the number of instruments is equal to the number of observations, in which results from the use of IVs would be identical to those from standard OLS. Furthermore, standard overidentification tests (such as the Hansen test or the Sargan test) may fail to detect model miss-specifications and instrument invalidity, giving a false sense of security. An additional problem pointed out by Sigmund and Ferstl (2019) is that the use of a large numbers of instruments may be computationally infeasible. Reducing the number of moment conditions may be necessary to realistically get any results at all.

There is no clear-cut solution to determine what an appropriate number of IVs to use is. To address possible issues caused by excessive use of IVs, Roodman (2009b) proposes that the number of instruments is reported alongside the results, and that one conducts sensitivity-analysis with respect to the number of instruments used.[11]

Quite notably, Roodman (2009b) shows that a (very extensively cited) study by Levine et al. (2000) connecting financial development to economic growth made use of a valid GMM estimator, but used far too many instruments, which the Hansen test failed to pick up as being invalid. As such, while there was an attempt to address endogeneity through the use of IV, excessive use thereof resulted in the statistical results remaining invalid. Hence, as a robustness check, this paper includes a sensitivity test to the number of IVs employed, to avoid this pitfall.

4.4.2 Interpretation of results

The coefficients of a PVAR model have the same interpretation as those of a standard VAR, with the addition that they capture the average dynamics of a panel rather than a singular country. Such parameter estimates can be difficult to interpret directly. Instead, it is possible to estimate Impulse Response Functions (IRF), which help visualise the effects that endogenous variables have on one another. In particular, their estimation requires that the PVAR model is stable. Stability can be calculated by rewriting the PVAR model in companion form, computing the eigenvalues of the corresponding companion matrix and checking whether they lie within the unit circle (and thus have moduli < 1). Stability implies that shocks only have temporary effects on endogenous variables. Instability, in contrast, means that the effects of shocks last indefinitely and (if a modulus is > 1) are 'explosive', akin to exponential growth. Given the general absence of such 'explosive' behaviour of macroeconomic data, any PVAR model that is unstable is likely to be miss-specified (e.g. due to non-stationarity).

If the idiosyncratic error matrix (Σ_e) is non-diagonal, IRF will depict the cumulative effect of multiple simultaneous shocks rather than shocks to individual endogenous variables. As long as Σ_e is positive semidefinite, it is possible to orthogonalize the errors and obtain Orthogonalized Impulse Response Functions (OIRF), so as to filter out the effects of individual shocks. This orthogonalization is performed by generating the (unique) Cholesky decomposition of Σ_e and using the resulting triangular matrix to generate orthogonalized errors. Cholesky decomposition has the downside that it is conditional on the order of the original matrix' elements. In other words, while

[11] It is also possible to drastically reduce the number of moment conditions and instruments without sacrificing (much) information by collapsing the instrument matrix (for which the procedure is shown by Roodman (2009a) in the context of a single dynamic panel, and by Sigmund and Ferstl (2019) for the PVAR context). The second approach in particular is shown to make the Hansen test functional and useful again, making it easier to correctly specify models and sets of instruments (Roodman, 2009b). Unfortunately, usage of the PVAR package created by Sigmund and Ferstl proves computationally infeasible, hence this option is left largely unexplored.

the order of the endogenous variables in a PVAR model has no impact on the parameter estimates, it does affect the generation of OIRF. Consequently, it is useful to re-estimate these OIRF under alternative variable orderings, to check their sensitivity.[12]

Should the OIRF be consistent with one another, they can visualise the effects of shocks to individual (endogenous) variables. In the current study, this can be used to show how financial liberalization possibly (temporarily) affects economic growth directly and indirectly (through financial development and financial integration). Confidence intervals for such OIRF can be generated via bootstrapping. According to Kapetanios (2008), re-sampling is best done in the cross-sectional dimension, rather than re-sampling on the time dimension or a combination of the cross-sectional and time dimension.

If financial liberalization has the hypothesized, positive effect on economic growth, then one can expect that it has a visibly significant effect on economic growth as depicted in the OIRF.

4.5 Data

This section first briefly lists which sources are used to obtain proxies for each of the aforementioned variables, and then presents a deeper overview of these sources and interpretation of the proxies. Table 4.1 provides an overview of all relevant variables and their proxies, along with a brief description of these proxies and their source.

4.5.1 Capital account liberalization

This paper defines capital account liberalization as a change in the level of capital account openness. Capital account openness is measured using data from two sources. The first source is generated by Chinn and Ito (2008) and the second by Abiad et al. (2010). The reason for using two separate sources is to test the sensitivity of the PVAR models to how capital account liberalization is measured. The two sources of data are described briefly below.

The dataset generated by Chinn and Ito (2008) (hereafter referred to as *ka_open*) covers 182 countries, providing annual observations from 1970 till 2017. The set of countries covered is very broad, and includes the majority of both developed and developing countries. The measure of capital account openness is based on the Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER), as compiled by the IMF. Chinn and Ito take four binary variables from

[12]There exists also a Generalized Impulse Response function, which does not depend on the ordering of the variables. Its availability in existing statistical packages appears to be limited, however.

the AREAER, and combine them to obtain a single index.[13] The binary variables taken as a basis for measuring capital account openness capture: (1) whether there are multiple exchange rates present; (2) whether there are restrictions on current account transactions; (3) whether there are restrictions on capital account transactions and (4) whether export proceeds are required to be surrendered.[14] Chinn and Ito specify that this measure captures both *de jure* and *de facto* capital account openness. In other words, the measure denotes not only legislative limitations, but also practically implemented limitations to the capital account. The *ka_open* measure is non-binary and ranges from 0 to 1, where 0 denotes a closed capital account, and 1 represents an unrestricted capital account.

The second source of data on capital account openness is constructed by Abiad et al. (2010), covering 91 countries via annual observations from 1973 till 2005. This second dataset thus covers fewer countries than the *ka_open* measure. On average, the two datasets cover both developed and developing nations in very similar proportions. The index is a quaternary index, which is based on three binary sub-indices. These sub-indices capture: (1) whether or not there are multiple exchange rates in use; (2) whether there are restrictions on capital inflows and (3) whether there are restrictions on capital outflows. The quaternary index aggregates these three binary indices, thus ranging from 0 to 3. For this index, the value 0 represents a severely restricted capital account, while a value of 3 represents an unrestricted capital account. The index is hereafter referred to as 'intlcapital'. Though the index is limited in the years it covers compared to *ka_open*, it is nevertheless an often used, literature standard measure for financial reforms. As such, it enhances the comparability to other studies about capital account liberalization. A limitation of this measure is that it is not continuous, but discrete. In this paper, it is nevertheless treated as if it were continuous. Intermediate values between the discrete denominations still have an intuitive interpretation. While a change in this index of size, say, 0.5 is never measured, such a change can easily be interpreted as a *partial* removal of capital flow restrictions, or a reduction (but not elimination) of the multitude of simultaneous exchange rates in use.

Section 4.6 elaborates on several statistical differences between the data for *ka_open* and intlcapital.

[13] Note that rather than summing the binary indices, these overall index is generated using Principal Component Analysis. As such, not every binary measure is given an equal weight when calculating the overall index.

[14] From 1996 onwards, these four binary variables are disaggregated, so as to provide more specificity and nuance. Chinn and Ito (2008) do not provide an explicit overview on how the disaggregated data are handled beyond citing Mody and Murshid (2005), but it appears the sub-indices are made use of in a PCA similar to the one based on the four main categories.

4.5.2 Institutional quality

As highlighted in Chapter 1, institutions and democracy are not equivalent terms, as the latter is a subset of the former. Nevertheless, democracy can act as a strong proxy to capture the quality of institutions more broadly. Strong democratic institutions are often a necessity for other institutions to function correctly, but they can also themselves be dependent on other institutions functioning well. For instance, strong democratic institutions are associated with a strong legal system, the upholding of property rights, low incidence of corruption, free media, and so forth (see e.g. Knutsen (2011), Nieman and Thies (2012), Rigobon and Rodrik (2005), Rock (2009) and Whitten-Woodring (2009)). As with any proxy, the correlation with other institutional quality is not perfect, but it is sufficiently strong that the inclusion of democracy in the estimation process should capture the effects of many non-democratic institutions as well. The primary benefit of relying on such a proxy is that data on democracy is far more readily available than for other measures of institutional quality. The Polity Project, provided by Marshall et al. (2017), provides a measure for the quality and strength of democratic institutions for 167 countries from 1800 onward. This measure, hereafter referred to as *polity2*, gives countries a score based on sub-indices that illustrate the strength of certain institutional features. These sub-indices include, for instance, whether the political system is competitive, whether executives are subject to accountability groups, whether there are clear transitions in power from one executive to a successor, and many others. The ultimate score ranges from -10 to 10, with -10 representing an absence of any democratic properties and 10 indicating that a country is a full democracy. For more details on this measure, see section 2.4 of chapter 2.

4.5.3 Remaining variables

Data for the remaining variables come from the Financial Sector dataset, as part of the World Development Indicators (WDI) dataset (The World Bank, 2018). This dataset covers 265 countries and territories[15] from 1961 onward. Six measures are extracted from this dataset: (1) Net FDI inflows (as a percentage of GDP); (2) Domestic credit provided to the private sector (as a percentage of GDP); (3) Real GDP per capita; (4) the Trade/GDP ratio; (5) the primary schooling enrollment rate and (6) the use of IMF credit facilities.

The FDI rate serves as a proxy for financial integration, as it captures international capital flows. The presence of such international capital flows indicates that capital markets are internationally connected. The domestic credit provision is used as a proxy for financial development. Domestic credit can only be provided extensively if domestic financial institutions are also well developed and active. Though it is an incomplete measure for financial development, it has the benefit of being the most readily available measure for financial activity. [16] The real GDP per capita is transformed

[15]Territories include groupings of countries, such as the EU, highly developed economies and less developed economies. Furthermore, it includes countries that at the time of writing no longer exist.

[16]Potential sensitivity issues based on this proxy choice are explored in the section covering the empirical results in

into a growth rate, and used to determine economic performance. The trade/GDP ratio captures a country's openness to trade by summing a country's exports and imports, and dividing them by GDP. Large changes in this ratio may indicate trade reforms, which may affect the need to open the capital account, financial integration, and economic performance. The primary school enrollment rate acts as a proxy for a country's educational attainment. This captures, to some extent, the level of development of a country. Finally, the use of IMF credit captures whether a country has made use of any of the IMF's credit lines (beyond a country's reserve tranche). Use of such credit is generally accompanied by reform requirements, as indicated by the IMF (2021), such as privatization of government owned companies, liberalization of trade and the current and capital accounts. As such IMF-induced liberalization typically occurs during harsh economic downturns, the use of IMF credit serves as a basis for sub-sampling, to determine whether countries undergoing such non-random liberalization performed better or worse as a result of it. The timing of reforms related to the use of IMF credit lines is not fixed. It may be the case that reforms are required prior to the extension of credit, or ex post. As such, without a historical context of every country making use of such credit lines, it is a relatively crude measure to capture IMF intervention.

Table 4.1 provides an overview of all variables, along with their source and a brief definition.[17]

this paper. If the data were to feature severe gaps, it may bring into question the validity of using an autoregressive model, hence data continuity is prioritised here. Initially, a principal component analysis was considered here, combining several proxies for financial development, but this proved impractical due to the presence of too many gaps in the data.

[17]Though there are potentially more variables of interest to include here, the methodology employed in this paper is best applicable when there are regular observations available, without frequent gaps. In the interest of including many non-Western countries in the analysis, the variable set is kept limited. Additionally, there are many periods of financial liberalization in the early years within the sample, during which data on other potentially interesting variables is too scarce to make their inclusion viable.

PVAR Variable	Proxy	Measure	Source
Capital Account openness	*ka_open*	Measure capturing the openness of the capital account. It ranges from 0 to 1, where 0 indicates a severely restricted capital account, and 1 indicates an unrestricted one.	Chinn and Ito (2008)
Capital Account Openness	intlcapital	Quaternary measure capturing the openness of the capital account. It ranges from 0 to 3, where 0 indicates a severely restricted capital account, and 3 indicates an unrestricted one.	Abiad et al. (2010)
Financial Integration	FDI	The difference between FDI inflows and outflows, divided by the level of GDP. Negative values indicate that investments outflows from a country exceed investment inflows, while positive values represent the opposite.	The World Bank (2018)
Financial Development	Fin. Dev	Domestic credit provided to the private sector (as a percentage of GDP). This measure captures the extent to which credit is available from any domestic source, and to what extent such credit sources are made use of.	The World Bank (2018)
Economic performance	GDP/Capita	Average level of output per person (PPP) within one country during a year. High values suggest a high level of productivity and development.	The World Bank (2018)
Economic performance	GDP Growth	The yearly growth rate of real GDP per capita. High values indicate that a country is experiencing rapid development and increases in income.	The World Bank (2018)
Institutional quality	Polity2	Ranks democracy on a scale from -10 to 10. The value -10 represents a complete absence of democracy, while a score of 10 represents a full democracy.	Marshall et al. (2017)
Openness to trade	Trade/GDP	The sum of imports and exports, divided by the level of GDP. Captures how important of a role trade plays in a country. Minimum possible value is 0. The higher the value, the more important trade is.	The World Bank (2018)
Human Capital	Primary School Enrollment (Gross)	The number of students in primary school compared to the primary school eligible population. Low values indicate that school attendance is low, and neither children nor adults attain primary education. High values indicate that attendance is high.	The World Bank (2018)
IMF intervention	IMF credit line position	The volume of credit provided by the IMF. A non-zero amount indicates the IMF is actively providing credit to the corresponding country, meaning the country is likely to undergo extensive reforms.	The World Bank (2018)

Table 4.1: List of all variables, along with a brief definition and source.

4.6 Descriptive Statistics and Preliminary Analysis

This section illustrates key descriptive statistics, and presents the results of the stationarity tests necessary for running the PVAR models. Table 4.2 indicates the means, standard deviations, minima, maxima and number of the observations for all variables. These summary statistics denote the variables' distributional properties across both the cross-sectional and the time dimension. The observations used in generating these summary statistics are limited to those that actually end up being used in the PVAR estimation process. Due to the use of two different measures for capital account openness with differing numbers of observations, there are two samples used for estimating the PVAR models - one in which *ka_open* and one in which intlcapital measures capital account openness. There is a sizeable difference in the number of observations available for both capital account measures, so the properties of the two samples may also differ substantially. To account for this, table 4.2 also shows the relevant summary statistics separately for both samples. The top half of this table shows the descriptive statistics for the sample using the *ka_open* measure, while the bottom half does the same for the sample using the intlcapital measure.

The means and standard deviations for both *ka_open* and intlcapital suggest that there is a wide range of capital account openness across the sample, with the average country being only partly open. The average levels of financial integration and financial development are not particularly high, but display a high level of variation. Similarly, the sample covers a wide variety of levels of institutional quality, openness to trade and educational attainment. The two samples are statistically significantly different from each other when looking at the means of FDI, trade/GDP and primary schooling. The sample based on intlcapital features slightly less net FDI inflows, lower openness to trade and slightly higher educational attainment. Differences in results based on these two samples may therefore in part be driven by the differences in the underlying distributions of these variables.

4.6.1 Capital account openness over time

Ex ante, it is unclear which of the two capital account openness measures is more accurate and preferable. Any differences between the two measures can be exploited to test the PVAR models' sensitivity to the choice of how capital account openness is measured. As shown in figure 4.2, both *ka_open* and intlcapital show a clear upward trend, indicating that there has been a tendency in recent decades to liberalize the capital account. The intlcapital measure shows greater variation over time than *ka_open*. This difference in variation is in part caused by the difference in which countries are covered by both measures. The *ka_open* measure is based on a broader set of countries than intlcapital. When comparing the average *ka_open* value for the same set of countries as covered by intlcapital, the two lines become more similar, but intlcapital nevertheless displays more variation. While both measures agree on the rise of average capital openness over time, they disagree on the

PVAR variable	Data	Mean	SD	Median	Min	Max	N
	ka_open	0.47	0.36	0.42	0	1	3375
Capital account liberalization	Δ*ka_open*	0.00	0.08	0	-0.76	0.76	3375
Financial integration	FDI	3.07	6.84	1.64	-82.89	198	3375
Financial development	Fin. Dev	42.41	40.52	27.81	0.40	255.31	3375
Economic performance	GDP Growth	0.02	0.05	0.02	-0.29	0.57	3375
Institutional quality	polity2	3.03	6.86	6	-10	10	3375
Openness to trade	Trade/GDP	72.76	38.19	64.40	6.32	354.98	3375
Human capital	Prim. Schooling	97.10	21.27	101.42	12.14	152.21	3375

PVAR variable	Data	Mean	SD	Median	Min	Max	N
	intlcapital	1.71	1.10	2	0	3	1349
Capital account liberalization	Δintlcapital	0.05	0.43	0	-3	3	1349
Financial integration	FDI	1.89	3.42	0.99	-5.67	55.08	1349
Financial development	Fin. Dev	41.71	38.79	26.54	0.40	221.23	1349
Economic performance	GDP Growth	0.02	0.04	0.02	-0.23	0.30	1349
Institutional quality	polity2	3.22	6.86	7	-10	10	1349
Openness to trade	Trade/GDP	57.94	29.61	53.13	6.32	220.41	1349
Human capital	Prim. Schooling	99.61	15.28	101.26	39.64	146.82	1349

Table 4.2: Summary statistics. See table 4.1 for a brief definition of all variables. The upper half of the table reports the summary statistics based on the sample using *ka_open* as the measure for capital account openness, while the bottom half is based on the sample which uses intlcapital. Note that the GDP Growth summary statistics are reported as the percentage divided by 100. Hence, GDP growth of 0.02 corresponds to 2%. Summary statistics on the GDP/capita ratio and IMF credit are not reported here, as this data is only used for sub-sampling.

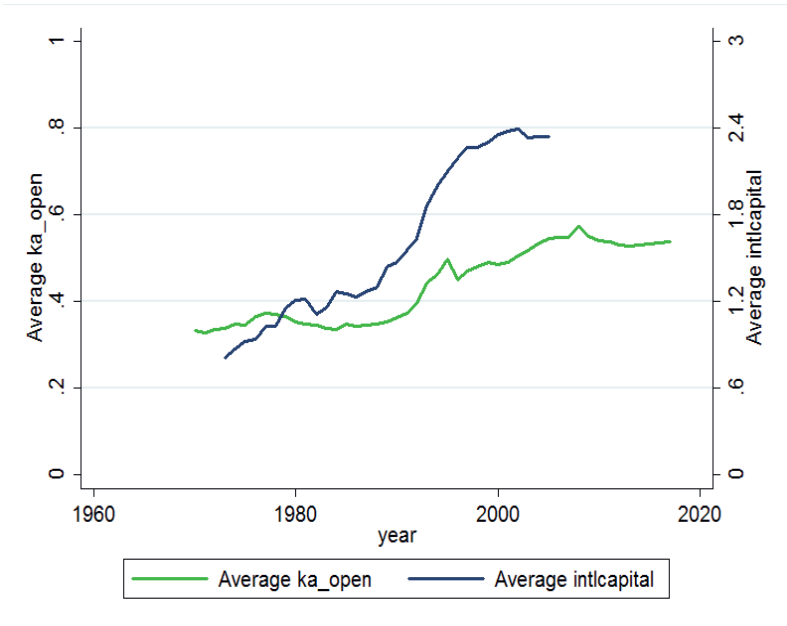

Figure 4.2: Average level over time of *ka_open* and intlcapital.

extent to which capital accounts have been opened up. As the primary interest of the current study is to study the effects of liberalization policy (and hence the *change* in capital account openness), figure 4.2 thus indicates that the two measures do not capture the capital account liberalization process in exactly the same way.

4.6.2 Correlations

Table 4.3 shows the pairwise contemporaneous correlations for all available observations (so not limited to either sample) along with their significance between all variables of interest.[18] The correlation between *ka_open* and intlcapital is high at 0.71, but nevertheless substantially far away from 1. The use of both measures provides an opportunity to test the sensitivity of the PVAR model to how capital account openness and changes therein are measured. The difference between the two measures is highlighted by the correlation between Δ*ka_open* and Δintlcapital, which is only 0.20. The correlation related to both capital account openness measures on the one hand and FDI on the other hand also differs quite substantially, with the intlcapital being more strongly linked to FDI. An additional observation is that the three exogenous variables (polity2, the trade/GDP ratio and the primary school enrollment rate) are all positively related with capital account openness, indicating that countries with better institutions, countries with higher trade volumes and countries

[18]Restricting the observations to either the *ka_open* or intlcapital sample does not have any noteworthy impact on the correlation coefficients.

with high education tend to have more open capital accounts. The correlations between the endogenous variables indicate whether there are any sizeable contemporaneous connections between them. As the PVAR model is specified in lags, such contemporaneous connections would not be picked up strongly in the estimation process. Among these endogenous variables, the Δka_open measure is only significantly correlated to GDP growth, while Δintlcapital is only significantly correlated with the measure for financial development. Based on these weak correlations, it seems unlikely that there are any major contemporaneous effects between the variables. The sign reversal of the correlation coefficient between liberalization and FDI or financial development is unusual. These correlations are not significant for both liberalization measures, hence this is not a major concern.

	ka_open	intlcapital	Δka_open	Δintlcapital	Net FDI (% of GDP)	Financial Development	GDP Growth	polity2	Trade (% of GDP)	Primary Schooling
ka_open	1.00									
intlcapital	0.72	1.00								
	(0.00)									
Δka_open	0.13	0.11	1.00							
	(0.00)	(0.00)								
Δintlcapital	-0.02	0.17	0.20	1.00						
	(0.44)	(0.00)	(0.00)							
FDI	0.10	0.29	0.03	-0.01	1.00					
	(0.00)	(0.00)	(0.04)	(0.65)						
Fin. Dev	0.07	0.10	0.02	-0.10	0.03	1.00				
	(0.00)	(0.00)	(0.13)	(0.00)	(0.03)					
GDP Growth	0.00	0.14	0.04	0.02	0.09	-0.01	1.00			
	(0.79)	(0.00)	(0.00)	(0.28)	(0.00)	(0.63)				
polity2	0.32	0.44	0.05	0.05	0.06	0.03	0.02	1.00		
	(0.00)	(0.00)	(0.00)	(0.03)	(0.14)	(0.03)	(0.14)			
Trade/GDP	0.22	0.29	0.00	-0.02	0.34	0.01	0.08	0.03	1.00	
	(0.00)	(0.00)	(0.71)	(0.41)	(0.00)	(0.03)	(0.00)	(0.00)		
Prim. Schooling	0.16	0.31	0.02	0.01	0.06	0.05	0.09	0.33	0.15	1.00
	(0.00)	(0.00)	(0.14)	(0.63)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	

Table 4.3: Cross-correlation table. The p-values are in parentheses.

4.6.3 Stationarity test

One of the requirements for a PVAR model to be stable, and to generate (O)IRFs is that the underlying data are stationary. To test this stationarity, I conduct a panel unit root test on *ka_open*,

intlcapital, net FDI, Financial Development and GDP Growth for all available observations. There are several possible unit root tests one can conduct, but most are inapplicable due to requiring strongly balanced data. In the current study, I use the Fisher panel unit root test on each series to determine whether there is a unit root present.[19] The tests reveal that intlcapital and the measure for financial development feature unit roots, but that *ka_open* and FDI do not. Regressing *ka_open* on its lagged values reveals that the measure nevertheless features a root close enough to unity to cause stability problems for the PVAR model. As a result, *ka_open*, intlcapital and the financial development measure are transformed by taking their first difference. The remaining variables are left unchanged.[20]

4.7 Empirical Results

This section discusses the empirical results. It first presents the model, based on specification and stability tests. It then displays the estimation results based on the full sample via OIRFs. The section then elaborates on the estimation results based on several sub-samples. These sub-samples are based on: (1) the level of institutional quality; (2) the level of economic development and (3) the use of IMF lending facilities.[21] The sub-sections covering the full sample results and the sub-sample results end with a comparison to existing literature. The aim of this comparison is to see whether the new methodology also generates novel results, or whether it solidifies prior empirical results. Finally, this section conducts several robustness tests, to check the stability and sensitivity of the OIRFs. These robustness checks focus on the validity of the IVs and the sensitivity of the results to using alternative measures for capturing the level of financial development.

4.7.1 Model specification

The PVAR takes on the following functional form, as previously described in section 4.4:

$$y_{i,t} = \sum_{l=1}^{k} A^l y_{i,t-l} + Bx_{i,t-1} + u_{i,t} \qquad i = 1, 2, ...N \qquad t = 1, 2, ...T \tag{4.7}$$

In this model, $y_{i,t}$ is the vector denoting all endogenous variables. In the current study, this vector takes on the form $\{\Delta$ *ka_open*, *FDI*, Δ *FinDev*, *GDPGrowth*$\}$. A second specification is run where Δ*ka_open* is replaced by Δ*intlcapital* instead. It then thus takes on the form $\{\Delta$*intlcapital*, *FDI*, Δ*FinDev*, *GDPGrowth*$\}$. The ordering of these variables is specified here explicitly, as it can

[19] More specifically, I use the Augmented Dickey-Fuller version of this test.

[20] For the sake of brevity, the tables featuring the unit root tests results are omitted here and left to the Supplementary Online Appendix.

[21] The corresponding PVAR parameter estimates for both the full sample and the various sub-samples are left to the Supplementary Online Appendix. The tables containing such parameter estimates are considerably more difficult to interpret than the OIRFs, and thus add little interpretive value given the space they take up.

potentially have an impact on the estimation of the OIRFs. The potential effect of this ordering is explored in greater detail as a robustness check. The vector $x_{i,t-1}$ is composed of the exogenous variables, which measure institutional quality (via the *polity2* for democratic institutional quality), openness to trade (via the trade to GDP ratio) and education attainment (via the primary school enrollment rate).

The number of lags included in the model is determined on the basis of information criteria, for which the calculations are presented by Andrews and Lu (2001). Based on both the Bayesian Information Criterion and the Hannan-Quinn Information Criterion, the preferred model includes only the first lag of all endogenous variables.[22]

As elaborated upon in section 4.4, in order to generate reasonable OIRFs (in which shocks do not propagate exponentially, but eventually peter out), the estimated PVAR models must be stable. The *pvar* module in Stata by Abrigo and Love (2016) provides a tool to explicitly test a PVAR model's stability, and such a test is performed after every estimation process. Instability issues are generally caused by the data being non-stationary, due to the presence of unit roots. Both measures for capital account openness and the measure for financial development feature unit roots. These variables are replaced with their first-differenced counterpart. The results of the test for PVAR stability show that each model is stable once all unit roots are removed. These models thus generate correct OIRFs.[23]

Equation 4.7 is estimated using GMM, where the IVs are based on the first 20 lags of the endogenous variables and their validity is verified using a Hansen test. That is, while the model only includes the first lag of endogenous variables as explanatory variables, their relation is estimated using IVs based on the second lag onward.[24] This number of IVs is arbitrary, as there exists no clear method to determine which amount is too large or too small. As a robustness check, section 4.7.4 checks the sensitivity of the results based on the number of IVs used. The variables in equation 4.7 are transformed using forward orthogonal deviations, making every lag from the first one onward a potentially valid instrument.

4.7.2 Impulse Response Functions

This section presents the impulse response functions generated by the PVAR model. Figures 4.3 and 4.4 depict the OIRFs created using the PVAR estimates for the full sample. Figure 4.3 is generated using Δka_open, while figure 4.4 uses $\Delta intlcapital$. All OIRFs include a 95% confidence band, which is obtained by running Monte Carlo simulations with 1000 repetitions each. Each plot

[22] The Akaike Information Criterion is, according to Sigmund and Ferstl (2019), inappropriate.

[23] The outcomes of the stability tests can be found in the online appendix.

[24] Note that this means the number of IVs is as high as $4 * 20 + 3 = 83$ when all lags are present in the data.

within the figure corresponds to the impulse response to a shock. The first row considers the effects of a one standard deviation shock in GDP growth on GDP growth itself (first row, first column), on financial development (first row, second column), on FDI (first row, third column) and capital account openness (first row, fourth column). The second row switches from there being a GDP growth shock to there being a shock in financial development. The third row considers a shock in FDI, and the fourth row (which is of primary interest) a shock in capital account openness. These shocks are tracked for a period of 10 years, using steps of one year each.

The scale of the GDP growth plots is that a value of 0.01 along the vertical axis corresponds to a 1% increase in the GDP growth rate. For the FinDev plots, a value of 0.01 corresponds to a 1 percentage point increase in the *change* in financial development. Positive values after a shock therefore indicate that financial development is increasing by 1% more than it would have without the shock.[25] For the FDI plots, a value of 0.1 indicates an 0.1% increase in net FDI inflows (as a percentage of GDP). Finally, in the capital account openness plots a value of 0.1 on the vertical axis corresponds to a 0.1 point increase in the *change* in capital account openness. Any positive value in these plots indicate that capital account openness is higher[26] than it would have been without the shock.

For reference, there is a horizontal, red reference line added at the 0 level. A clearly visible red, reference line indicates that the estimated effect is significantly different from 0. If the reference line is barely or not visible, then it is covered by the 95% confidence band, and the estimated effect is thus insignificantly different from 0.

The variable ordering may have an impact on the OIRFs generated, to which the sensitivity is explored in section 4.7.4.[27] The ordering used throughout this section is $\Delta ka_open \rightarrow FDI \rightarrow FinDev \rightarrow GDP\ Growth$ for figure 4.3 and Δintlcapital $\rightarrow FDI \rightarrow FinDev \rightarrow GDP\ Growth$ for figure 4.4. This ordering corresponds closest to the structure presented in figure 4.1 in section 4.3. The underlying assumption in this ordering is that liberalization shocks can have an immediate effect on FDI, FinDev and GDP growth, but that shocks in the latter three variables are unlikely to instantly result in new liberalization policy. Furthermore, the ordering assumes that FDI and FinDev have a more rapid impact on GDP growth than the other way around.[28]

[25] Notice that this does not guarantee that the resulting overall change is positive. It could be, for instance, that without the impulse financial development would shrink by 2%, but that after a shock it only shrinks by 1%.

[26] The scale of capital account openness using Δka_open ranges from 0 to 1, and for $\Delta intlcapital$ from 0 to 3. Therefore, if the same height along the vertical axis is reached in both OIRFs, Δ *ka_open* would imply a bigger impact on the capital account openness score.

[27] The orthogonalization of the impulse response functions requires an assumption for the causal nature of contemporaneous error terms. Causality is limited in only one direction. As a simple analogy: If a variable X is correlated to a variable Z, then the variable Z is also trivially correlated to variable X. The relevant causal assumption would be that X *causes* Z, but not the other way around. Such an assumption thus identifies X as the starting point of any shock.

[28] This assumption is made plausible by the fact that GDP growth is typically only measured and observed *ex post*, whereas financial activity is reported on a much more continuous basis. It takes the side of (Schumpeter, 1911) rather

Results: ka_open

Figure 4.3 shows several interesting patterns. The first row represents the effects of GDP growth impulses. Higher levels of GDP growth are associated with increased financial development, increased FDI and capital account liberalization. The second row reveals that financial development impulses are not significantly related to the other endogenous variables. There is only weak, insignificant evidence that financial development negatively impacts economic growth. The third row illustrates that FDI impulses positively contribute to GDP growth and financial development, but have no appreciable impact on capital account openness.[29]

The fourth row is of primary interest, as it depicts the effects of capital account liberalization. The overall effect on GDP growth is positive. A one standard deviation shock in the measure for changes in capital account openness results in up to 1% higher growth two periods after this shock.[30] The cumulative effect on GDP growth is that it gives an additional 2 percentage points worth of growth over the years. The effect of capital account openness on financial development is also positive, indicating that domestic credit provision to the private sector grows by up to approximately 0.5% of GDP. The effect on FDI is at first marginally negative, though insignificant, after which the effect becomes significantly positive.

As highlighted in section 4.3, it may be the case that there is a feedback process connecting capital account liberalization and the other endogenous variables. Figure 4.3 suggests that this is indeed the case. Capital account liberalization generates higher economic growth, but higher economic growth itself also fuels liberalization. The strength of this feedback process is limited, as the impulse responses are not particularly big. The existence of a feedback process nevertheless renders the effects of liberalization more persistent. There is no evidence that there is also a feedback process directly connecting financial liberalization, FDI and financial development. Liberalization does promote financial development and FDI, but the latter two have no significant impact on the former. Even without a strong feedback process, the indirect effect of liberalization on growth via FDI and financial development is noteworthy. Prior studies have largely treated these variables as independent of one another. Given the OIRFs shown here, such an approach may be inappropriate, as it does not capture the full effect of liberalization.

than (Robinson, 1952) in seeing finance as an essential driver of growth rather than only a consequence.

[29]Note that FDI is normally part of the capital account, and would thus be by definition positively related. The capital account openness measures are not, however, based on capital flow amounts, but rather on restrictions and inefficiencies in place.

[30]Such a shock in the capital account measure constitutes a sizeable, but not huge capital account reform. For Δka_open, for instance, a one standard deviation shock is a very commonly observed reform.

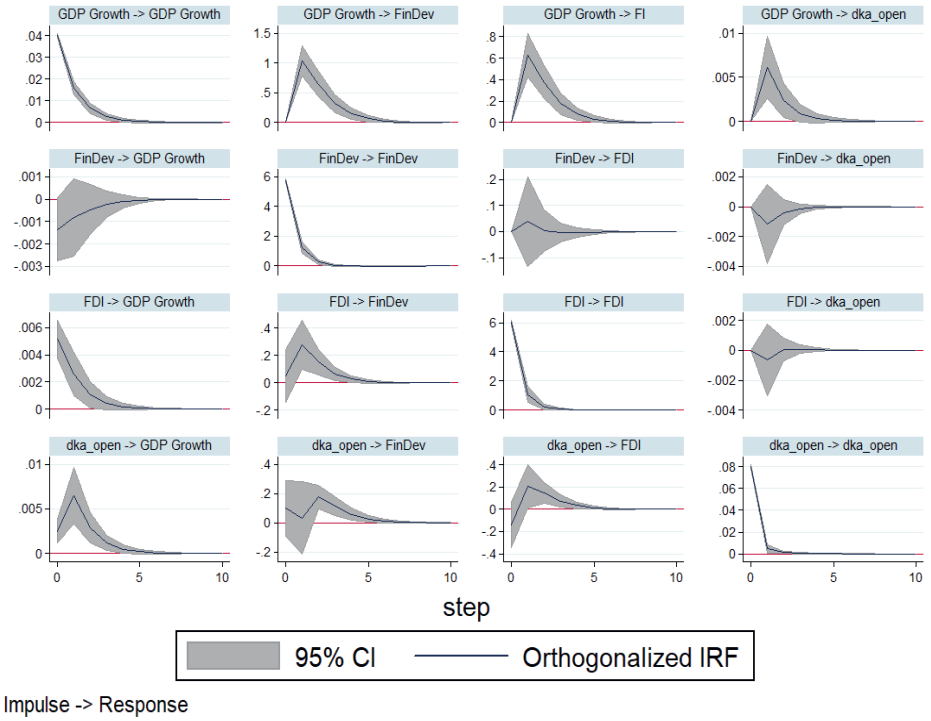

Figure 4.3: Orthogonalized Impulse Response Functions depicting the effect of impulses in singular endogenous variables on the rest of the system. The top row displays the effects of an impulse in GDP growth, the second row an impulse of financial development, the third row an impulse of FDI and the fourth row an impulse in capital account openness.

Results: intlcapital

To check the robustness of the results, the PVAR model is run using *intlcapital* as the measure for capital account openness. These results are shown in figure 4.4. The results are similar, though not identical. Differences in these results are partly driven by having fewer observations available for this second capital account openness measure and partly by differences in the covered sample period.[31]

Similar to figure 4.3, GDP is still positively associated with causing financial development, but the positive effect on FDI and on opening up the capital account is absent. In contrast to figure 4.3, figure 4.4 shows that financial development is positively associated with FDI. A positive shock in the level of financial development is associated with a cumulative effect of a 0.4% increase in net FDI inflows. Considering that such net FDI inflows range from -80% to over 400%, a 0.4% change does not appear to be particularly big. The third row of figure 4.4 shows that FDI is positively associated with GDP growth and with financial development. Furthermore, it is also now significantly positively associated with financial development. Figure 4.4 also reveals that FDI is negatively associated with further capital account openness.

The fourth row of figure 4.4 is once again of primary interest to this paper, as it depicts the effects of changes in capital account openness on the other endogenous variables. The figure reveals that, once again, capital account openness positively contributes to economic growth. Though the effect depicted in figure 4.4 is smaller than that of figure 4.3, the cumulative gain in economic growth over the years is still positive at approximately 1%. Capital account openness is also positively related to financial development, where the cumulative effects over the years indicate that domestic credit provision goes up by 0.5% of GDP. The effect on FDI is overall not significantly different from 0.

Figure 4.4 does not show any signs of a feedback process between capital account liberalization and GPD growth. The feedback process found in figure 4.3 is thus not robust to the choice of measure for capital account openness. Instead, the OIRFs show that FDI and financial development may form a positive feedback loop. FDI impulses result in higher financial development, and financial development impulses result in higher FDI.

Discussion

The primary takeaway from figures 4.3 and 4.4 is that capital account liberalization on average contributes to higher GDP growth. The effects do not persist for particularly long, but the cumulative gain in the growth percentage is estimated to be around 1 to 2%. Furthermore, opening the capital account promotes financial development, spurring domestic credit provision. The relevant

[31] Restricting the model based on *ka_open* to observations available for *intlcapital* results in a similar set of OIRF as shown in figure 4.4

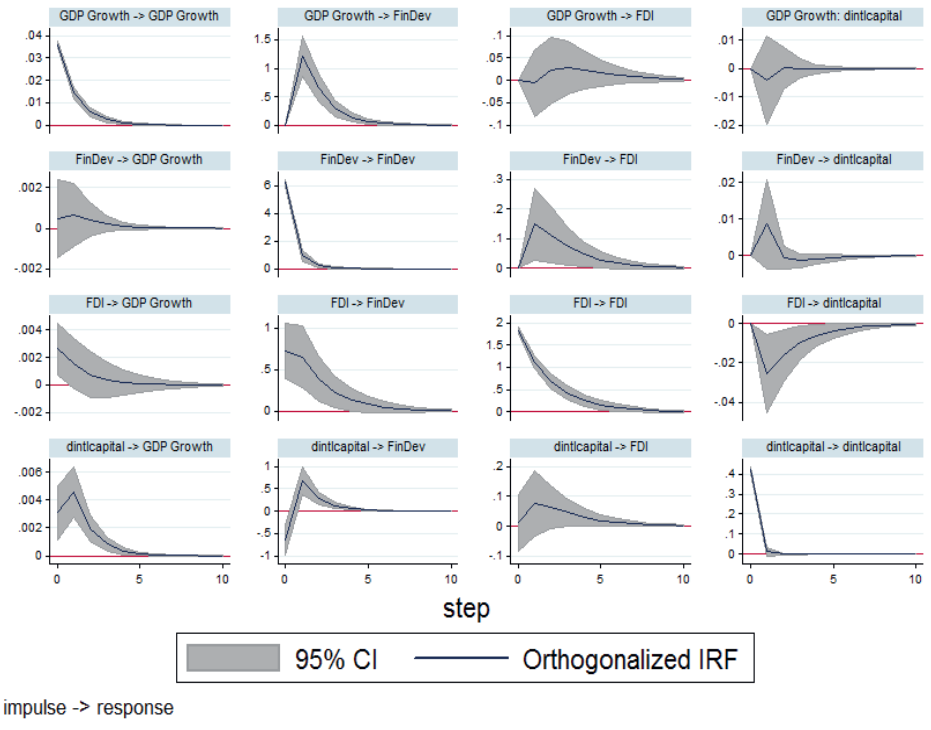

Figure 4.4: Orthogonalized Impulse Response Functions depicting the effect of impulses in singular endogenous variables on the rest of the system. The top row displays the effects of an impulse in GDP growth, the second row an impulse of financial development, the third row an impulse of FDI and the fourth row an impulse in capital account openness.

hypothesis from section 4.3 states that:

Hypothesis 1: Financial liberalization in the form of capital account liberalization results in higher economic growth.

Given the results presented in this subsection, there is evidence in favour of this hypothesis, hence I fail to reject it. The results of these PVAR models is in line with the neoclassical model, as discussed in section 4.2. The neoclassical framework predicts that capital account liberalization results in a short-run increase in economic growth. As the effects of the increase in growth are not very long-lasting, steady-state long-run growth is left unaffected. FEGMs suggest the opposite, namely that there is a persistent effect of financial liberalization.[32]

The estimated effect of financial liberalization is positive, but considerably smaller than that found by e.g. Bekaert et al. (2005), Arteta et al. (2001), Edison et al. (2004) or Quinn and Toyoda (2008), who find an increase in the annual average real growth rate of 1-2% as opposed to a *cumulative* increase in growth of 1-2%. This difference is likely the result of the different methodology and availability of more data. The cited studies make use of cross-sectional growth regressions, where the dependent variable is an average growth rate across several years. This approach reduces the time series information used, as it instead focuses on the effects of average openness on average growth. The longer the time period considered in these averages, the less reflective it is of the effects of individual reforms. A second difference is that the existing literature focuses largely on the direct correlation between capital account openness and growth, rather than also including indirect effects. As shown in the above results, there is evidence that liberalization affects both financial development and FDI, though in the case of FDI this evidence is mixed. This finding is in line with Mukerji (2009). Treating financial development, FDI and capital account openness as separate independent variables is therefore inappropriate. Overall, the results in this paper counter the findings of Rodrik (1998), who finds no benefits to capital account liberalization. The effects found in the current study are quite small, however.

As highlighted by Stiglitz (2000), Rodrik (1998), Mukerji (2009), Rancière and Tornell (2016), Kose et al. (2009) and many others, capital account liberalization goes hand in hand with an increase in the risk of financial crises. Such crises are costly, generally greatly reducing GDP without any prospects of recovering the losses once a crisis is resolved Claessens and Kose (2013). The estimated benefits of liberalization have to be weighed against the risk of such damaging crises. Liberalization is estimated to result in permanently higher GDP per capita, even after the effects on the growth

[32]This may partly be the result of the data stationarity requirements for generating the OIRF, which inevitably result in effects diminishing over time. While unit roots may not be present in the underlying data, series are nevertheless allowed to be persistent, hence a rapidly flattening OIRF still suggests a short-lived effect. A fully conclusive answer to the longevity requires an exploration and use of other methodology, exploiting potential cointegration relations. Such an analysis goes beyond the scope of this paper and is left for future research.

rate peter out. The overall gained GDP per capita depends highly on whether countries are able to avert such financial crises after liberalization, so that they can enjoy the higher GDP per capita for as long as possible. If a country is hit by a crisis soon after liberalization, then the benefits of liberalization are not only short lived when looking at growth, but also the level of GDP per capita.

On the other hand, highly developed countries are on average highly liberalized. This begs the question whether liberalization is a necessary step to reach a high level of development. The PVAR models' results suggest that the contribution of liberalization to reaching a higher standard of living may be only limited. Furthermore, there are various countries that reached a high level of development prior to extensive liberalization.[33] As such, the results echo the sentiment of Bumann et al. (2013), who note that financial liberalization may have a positive effect on growth, but it is not a very major one. It is thus not a sufficient condition for persistently strong economic growth.

4.7.3 Sub-sampling

The above results are further explored by considering several sub-samples. Earlier research suggests that there are several factors that can result in heterogeneity in the effects of capital account liberalization. First, the data are split on the basis of institutional quality, generating a sub-sample of countries with poor institutions and a sub-sample of countries with strong institutions.[34] Second, the data are split by the level of economic development, resulting in a sub-sample of poorly developed countries and a sub-sample of highly developed countries. Finally, another sub-sample is considered based on whether a country is making use of the IMFs credit facilities to deal with either financing problems or balance of payment issues. Due to the way the data are recorded, only countries that make use of these facilities are clearly labelled, while all other observations are listed with missing values. As there is no guarantee that there a missing value implies that no funding has been received, only one sub-sample is created here.[35]

Poor institutions may discourage foreign investors from sending capital over. If, for instance, property rights or the rule of law are not well established, then such capital would run the risk of being expropriated. Without such capital inflows, there is no reason to expect any new economic benefits to emerge.

Highly developed countries may experience smaller benefits from capital account liberalization

[33] Examples include France, Finland and Denmark, which were already highly developed before pursuing liberalization reforms, as captured by *ka_open*.

[34] Institutional quality has already been included in the PVAR model as an exogenous variable. In its current form, the PVAR model does not check whether there is an interaction between institutions and liberalization, however. Doing so is technically very challenging, compared to the sub-sampling approach used here. See e.g. Di Serio (2018) for an overview.

[35] As highlighted in section 4.5, the method used to capture IMF interventions here is crude and warrants more detailed future research.

than less developed countries. In high developed countries, there is less likely to be a shortage of domestic savings with which investments can be funded, whereas less developed countries do face such shortages. The influx of new capital would thus alleviate capital shortages in less developed countries, allowing for greater investment and higher growth than in highly developed countries. Additionally, highly developed countries tend to have relatively liberalized systems, and may be running into diminishing marginal returns of further liberalization.

Countries that make use of the IMF credit facilities have generally been required to undergo a variety of reforms, including capital account liberalization. The decision to open up the capital account is imposed on such countries rather than made entirely freely. Countries making use of IMF credit are particularly poorly performing countries. If capital account liberalization helps such countries improve their performance, then there is merit to the reforms being imposed. If capital account openness is unrelated to economic performance, then it is an inappropriate policy to pursue and push for.

Due to the large number of sets of OIRFs estimated, they are not fully displayed here, but left to the online appendix. The subsequent subsections do include, however, the OIRFs showing cumulative effects of liberalization on growth as a visual aid.

Institutional quality

The *polity2* measure for democratic institutions is used as a proxy here to split observations into a group consisting of countries with *very poor* institutions and *very good* institutions.[36] Countries featuring a *polity2* score lower than or equal to -5 are put in the group with poor institutions, while countries with a score greater or equal to 5 are in the strong institutions group.[37]

The estimated results using *ka_open* show that the effects of opening up the capital account differ substantially between these two groups. For countries featuring weak institutions, the cumulative effect on economic growth is approximately 3%, while for countries with strong institutions it is 1.5%. These results are visualised in figure 4.5. Furthermore, in countries with weak institutions, capital account openness results in no additional financial development, while in countries with strong institutions it has a positive impact on financial development. In the latter group, the cumulative effect is an additional credit provision of 0.5% of GDP. Finally, opening the capital ac-

[36]The *polity2* measure is found to be strongly correlated with a plethora of alternative measures of institutional quality (such a those provided by the Heritage Foundation). Its primary benefit is that it is very readily available, whereas other institutional data generally lacks observations. Its use here is thus parallel to its use in the PVAR model itself, as an exogenous measure of institutional quality, allowing one to form an overall picture of the world rather than merely the "history of the West".

[37]A split using a *polity2* value of 0 as a cut-off point was also considered, but this results in quite a few observations in the middle ground, with neither particularly weak nor strong institutions. Such observations would be quite arbitrarily and perhaps inaccurately split across the two groups.

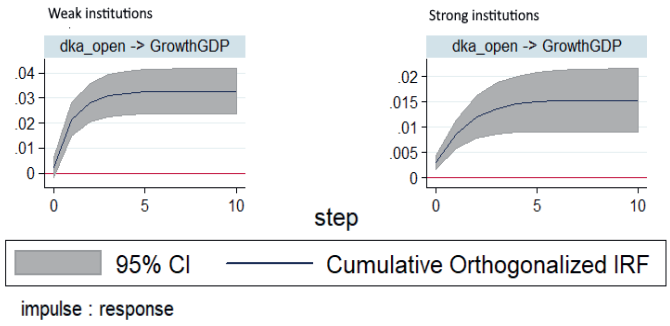

Figure 4.5: Cumulative Orthogonalized Impulse Response Functions depicting the cumulative effect of capital account liberalization (*ka_open*) on GDP growth. The left plot shows the results for countries with weak institutions, while the right plot shows the results for countries with strong institutions.

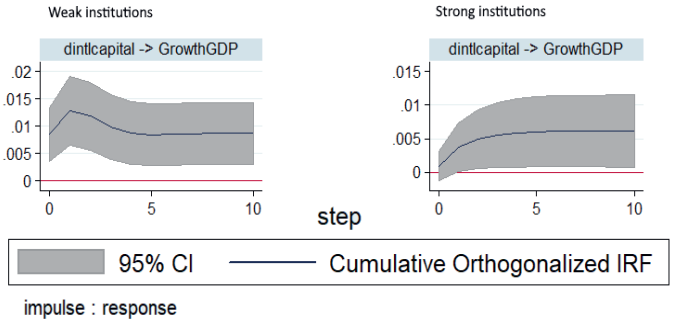

Figure 4.6: Cumulative Orthogonalized Impulse Response Functions depicting the cumulative effect of capital account liberalization (*intlcapital*) on GDP growth. The left plot shows the results for countries with weak institutions, while the right plot shows the results for countries with strong institutions.

count results in an increase in net FDI inflows of approximately 2% of GDP for countries with poor institutions, but has no significant impact on FDI of countries with strong institutions. Switching to *intlcapital* gives rise to an equivalent conclusion, albeit with smaller percentages. Growth is now cumulatively increased by just under 1% in countries with poor institutions, and just over 0.5% in countries with strong institutions. The significance of this gap is greatly reduced, hence its existence is not entirely robust to the choice of the capital account openness measure. This results is visualised in figure 4.6.

These results are quite striking. Strong institutions have often been deemed a necessity to ensure good economic performance, and a variety of authors have made the case that good institutions are a pre-requisite for benefiting from financial liberalization (see e.g. Kose et al. (2009)). Past

empirical studies have also found that strong institutions are a necessity for financial liberalization to pay off. Eichengreen et al. (2011), Klein and Olivei (2008) and Bekaert et al. (2005) empirically highlight the importance of institutional quality in generating any benefits from financial liberalization, but this same importance does not materialize in the current study.[38] The estimates found in the current study do not suggest that strong institutions are a necessary conditions for liberalization to pay off, as countries with weak institutions surprisingly outperform those with strong institutions. As was shown in the descriptive statistics, institutional quality is positively correlated with capital account openness. It may therefore be the case that countries with good institutional quality already have very open capital accounts, and that the room for further gains from opening up are limited. A follow-up test, where the sample is split up by the attained level of capital account openness reveals that there does not appear to be a pattern of diminishing returns. The solid performance of countries with weak institutions is therefore not driven by any diminishing returns. I thus find no evidence that solid institutions are a requirement for capital account liberalization to pay off.[39] The relevant hypothesis for the above test is:

Hypothesis 2: The effect of capital account liberalization on economic growth is positively related to the level of:

(a) Institutional quality

There is evidence indicating that countries with weak institutions outperform countries with strong institutions, hence rejecting the above hypothesis. Two conclusions can be drawn on the basis of these results, and either one or both may be true. The first is that past empirical studies have overestimated the need for high quality institutions. It may be that problems associated with weak institutions, such as corruption, simply do not hamper the effective allocation of funds coming in from abroad. A second, more conservative conclusion, is that the proxy used to capture institutional quality is too crude. More detailed measures of the rule of law, or the incidence of corruption may need to be included explicitly to accurately capture the effect of institutional quality. Such data has only limited availability at the time of writing. Given the surprising nature of the results, future studies may shed more light on which of these two conclusions is correct.

[38] This may be the result of how institutions are incorporated into the estimated model between the current paper and existing literature: Most existing literature includes it as a control variable, rather than making use of subsampling. As such, it estimates the average relation between institutions across all observations rather than looking at whether the effects do or do not exist across different sets of countries. I believe the latter approach to be more appropriate, as it better corresponds to testing the necessity of institutions rather than the correlation between institutions and growth.

[39] Note that this does not explicitly account for the increased risk of financial crises. It may be the case that the presence of weak institutions increase this risk more than strong institutions would. Accurately including volatility and crises in the estimation process goes beyond the scope of this paper.

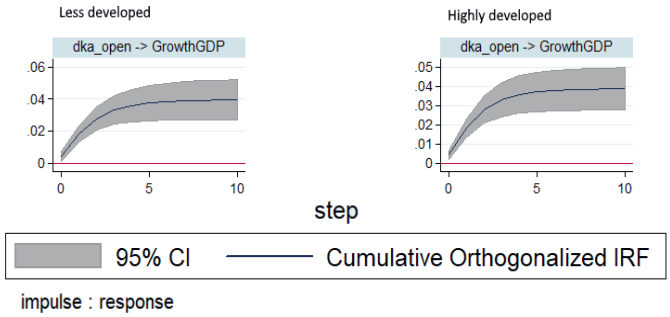

Figure 4.7: Cumulative Orthogonalized Impulse Response Functions depicting the cumulative effect of capital account liberalization (*ka_open*) on GDP growth. The left plot shows the results for less developed countries, while the right plot shows the results for highly developed countries.

Economic development

I construct two sub-samples on the basis of the level of real GDP per capita. Observations where GDP per capita is in the lowest quartile of the distribution fall under the less developed country sub-sample. Observations with GDP per capita in the upper quartile are placed in the highly developed country sub-sample.

The estimated effects of capital account openness on growth using *ka_open* indicate that there is no notable difference between less developed and highly developed countries. This result is visualised in figure 4.7. In both sub-samples, the estimated effects are positive, with the cumulative gain in GDP growth being close to 4% across all years. In the less developed countries, liberalization is associated with considerably higher FDI inflows, resulting in higher GDP growth. In highly developed countries, liberalization is associated with a reduction of financial development. Financial development is, however, negatively associated with GDP growth, hence its reduction is a positive outcome. This result is in line with the 'too much finance' literature; it may be the case that highly developed countries have too active domestic financial markets, which are adversely affecting growth rather than being a positive force. Using *intlcapital* results in a more conservative estimate of just under 1% cumulative growth gain, which is more comparable to the result of the full sample. This result is shown in figure 4.8. Growth after capital account liberalization is slightly higher in low-income countries than high-income ones, but also has a wider confidence band, indicating that the outcomes are less consistent. In contrast to the *ka_open* sample estimates, there is no sign of the 'too much finance' phenomenon.[40] The similarity in outcomes between less developed and highly developed is consistent across both capital account openness measures. However, the magnitude of the effect of liberalization on economic growth is once again sensitive to the choice of liberalization measure.

[40]Re-estimating the PVAR model using the *ka_open* measure, with observations restricted to the same time frame as the *intlcapital* sample does not unify the results between these two samples.

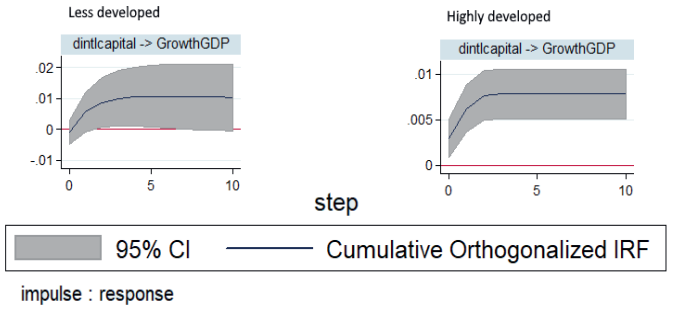

Figure 4.8: Cumulative Orthogonalized Impulse Response Functions depicting the cumulative effect of capital account liberalization (*intlcapital*) on GDP growth. The left plot shows the results for less developed countries, while the right plot shows the results for highly developed countries.

The lack of any major difference between less developed and highly developed countries is remarkable. Less developed countries are typically seen as facing greater capital constraints, and having more room for growth than highly developed countries, hence the lack of significantly bigger benefits is somewhat unexpected. Less developed countries often feature weak institutions, but the preceding stratification based on institutional quality reveals that this is not an aspect that diminishes the returns to capital account liberalization. Though a clear distinction between highly developed and less developed countries does not emerge from these results, the main takeaway is nevertheless that capital account liberalization Granger causes[41] a temporary increase in economic growth regardless of the development status.[42]

The relevant hypothesis tested in this subsection states that:

Hypothesis 2: The effect of capital account liberalization on economic growth is positively related to the level of:

(b) Economic development

In contrast to the ex ante expectations, less developed, (capital) poor countries experience the same effect on economic growth after capital account liberalization as highly developed, (capital) rich countries. The above hypothesis is therefore rejected. This result, too, clashes with some of the existing literature. Eichengreen et al. (2011) find that a high level of economic development is a pre-requisite for benefiting from financial liberalization. Similarly, Klein and Olivei (2008)

[41] Granger causality, of course, cannot guarantee a truly causal relation, but it does render it considerably more plausible and palatable.

[42] The comparison between the sets of OIRFs is primarily a visual one. I am not aware of any test allowing for a comparison between two curves estimated based on different samples, beyond perhaps a point-by-point comparison of each curve's coordinates. A visual inspection whether the curves fall within one another's confidence bands provides a more rapid conclusion.

note that their results showing a positive connection between financial liberalization and economic growth is driven entirely by the high-income countries in their sample, but that such a relation disappears when looking at low-income countries.[43] The current study indicates that high income is not a necessity. The neoclassical model states that the marginal returns to capital are high in (capital) poor, low-income countries but relatively low in (capital) rich, high-income countries. This implies that the former should outperform the latter in the wake of capital account liberalization. The above results suggest that this expected split between the two categories of countries is not present in the empirical data. It may be the case that low-income countries have features that prevent them from fully realizing the benefits of financial liberalization and outperforming high-income countries. For instance, in the Schumpeterian FEGMs, economic growth is driven by the existence of entrepreneurs pursuing productivity enhancements (such as improving physical capital quality and efficiency). The existence of such entrepreneurs may vary considerably between highly developed and less developed countries. Consequently, it may be that the source of growth in less developed countries stems from physical capital accumulation, whereas in highly developed countries it stems more from a better allocation of funds toward the right, productivity-pursuing entrepreneurs. Alternatively, it may be the case that high-income countries possess features that theory has not yet highlighted, which allow them to reap additional benefits from increased access and availability of financial capital. While theory has highlighted that institutional quality may play such a dividing role, the preceding results indicate otherwise, thus leaving a gap in existing theory.

IMF credit facility use

A final sub-sample is constructed on the basis of the use of IMF funding. Observations that denote the use of IMF credit from any of the IMF's facilities are filtered out to create this sub-sample.[44]

The resulting OIRFs indicate that countries making use of IMF credit on average receive the same benefits from undergoing capital account liberalization as estimated using the full sample. Should this sub-sample feature substantially different kinds of countries with very different results, a comparison with the full sample, which is primarily composed of country observations that have very likely not made use of IMF credit, should still reveal any clear differences. The corresponding hypothesis presented in section 4.3 states that:

Hypothesis 3: The effect of capital account liberalization is different for countries undergoing an

[43] There are major differences in the data covered by Klein and Olivei (2008), along with the methodology. In particular, these authors focus on the role of financial depth, where financial depth is only measured as an average across the early 1990s. This results in a regression model where the time dimension is not made use of, and where the modelled dynamics are considerably more limited.

[44] This sub-sample is also created using concessional and non-concessional loan data only, but this does not affect the ultimate results.

IMF intervention than those that do not undergo such intervention.

There is no evidence suggesting that countries undergoing capital account liberalization whilst undergoing an IMF intervention experience any different benefits compared to countries that are not making use of the IMF's lending facilities. This hypothesis is therefore rejected.

4.7.4 Robustness checks

Variable ordering for Cholesky decomposition

The ordering of the variables when generating the OIRFs determines the assumed contemporaneous causal structure of the error terms. In generating all preceding results, it is assumed that the ordering follows $\Delta ka_open \rightarrow$ FDI $\rightarrow$ FinDev $\rightarrow$ GDP Growth when using *ka_open* and $\Delta intlcapital$ $\rightarrow$ FDI $\rightarrow$ FinDev $\rightarrow$ GDP Growth when using *intlcapital*. While the order of these variables has no impact on the PVAR parameter estimates, it can affect the results obtained via OIRFs. As such, it is necessary to check whether these results are sensitive to changing the variable order. As there are two measures for capital account openness, and there are four endogenous variables, there are $2 * 4! = 48$ possible orders these variables can follow. The PVAR model and the corresponding OIRFs are re-estimated using every possible order, and then compared to see if any major differences arise. A comparison of this new, large set of OIRFs reveals that the preceding main conclusions remain unaffected by changing the variable order.[45]

Instrumental Variable sets

While weak instruments can cause biased and inconsistent estimates, excessively strong instruments can overfit endogenous variables and cause similarly biased and inconsistent estimates. Each of the preceding PVAR models makes use of the Hansen J-test to see whether the IVs are valid, but such tests may sometimes be inaccurate (Roodman, 2009b). Roodman warns that large sets of instruments may be prone to such overfitting, and suggests re-estimating models with smaller sets of IVs and observing whether the results change substantially. To account for such potential overfitting, all PVAR models are re-estimated using the first 2, 5 and 10 lags as instruments rather than the first 20. The results when using 10 lags as IVs remain largely unchanged. When only the first 5 lags are used, the confidence bands widen considerably, rendering some OIRFs insignificant. When using only the first 2 lags, most effects lose their statistical significance. The estimated average effects are unaffected, but the standard errors increase as the number of IVs is decreased to a very small number. The results are thus not immune to reductions in the number of IVs. This is not entirely unexpected. While the exogeneity assumption is easily satisfied for the IVs at hand, they

[45] The large set of OIRFs is available in the Supplementary Online Appendix.

are not very strongly correlated to the endogenous variables.[46] It is for this reason, too, that the estimation method allows for a very substantial number of moment conditions, so as to offset the downsides of relatively weak instruments.[47] Further research is needed to provide insights into what number of IVs is most appropriate, reliable and trustworthy in the estimation of PVAR models as found in this paper.

Alternative measures for financial development

The PVAR models from the preceding section employ two alternative measures for capital account openness, but there are also many candidates for measuring financial development. The choice for the proxy variable in the current study is to a large extent driven by data availability. This proxy variable, however, does not capture the full extent of financial development.

While data availability for alternative measures is limited, the PVAR models are nevertheless re-estimated using alternative measures for financial development, to get an impression of whether the preceding results are an artifact of the choice of the financial development proxy. These alternative measures are market capitalization of listed domestic firms (as a percentage of GDP), domestic credit to the private sector by banks (as a percentage of GDP) and the number of depositors with commercial banks (per 100,000).

For all three measures, combined with *ka_open*, the estimated effect of opening the capital account remains positive. When using the number of depositors with commercial banks as the proxy for financial development, the resulting estimate turns insignificantly different from zero, however. Using these three alternative measures for financial development with *intlcapital* provides to be more challenging. The use of market capitalization as the financial development proxy gives rise to insignificant results. The use of domestic credit to the private sector by banks produces equivalent results to those presented in the prior subsections of this paper. Using the number of depositors with commercial banks proves infeasible, as there are insufficient observations to run a PVAR model.

Overall, there are signs of some sensitivity to the choice of the proxy for financial development, as not all results remain statistically significant. While the proxy used throughout this paper is both appropriate and readily available, future research may need to explore alternative options at greater length, as there may be other appropriate, readily available candidates worth considering.

[46] This is unsurprising, as there is little reason to expect that, for instance, the level of economic growth in a given year would be particularly strongly related to *changes* in economic growth several years prior.

[47] Roodman (2009b) proposes the use of a collapsed moment matrix, where the exogenous properties of individual IVs can be combined together, resulting in far fewer moment conditions. Sigmund and Ferstl (2019) provide an R package that allows one to employ such alternative IV usage, but the use of this package proved too computationally cumbersome and is left for future research.

4.7.5 Summary of results

The primary finding is that financial liberalization in the form of capital account liberalization is positively associated with economic growth. On average, countries experience a 1-2% higher cumulative growth rate. The effect is short-lived, with the empirical results finding no indication or suggestion of long-run, steady-state growth being affected. Liberalization is thus estimated to permanently increase GDP per capita. Though PVAR models inherently cannot feature permanent effects of shocks, the estimated longevity of the effects on growth are nevertheless clearly brief. These results suggest that capital account liberalization is not a panacea or guarantee for high, long-term economic growth. There is some evidence in favour of a virtuous cycle between GDP growth and capital account liberalization. In this cycle, liberalization increases economic growth and economic growth encourages further liberalization. This cycle is sensitive to the choice of measure for capital account liberalization, however.

Overall, there is support for the core premise of the underlying growth theory. Capital account liberalization is indeed associated with higher economic growth, as both neoclassical growth theory and FEGMs predict. Neoclassical theory suggests that liberalization pushes economies toward a new steady state, but that growth is determined by other factors once the new steady state is reached. FEGMs, in contrast, provide mechanisms through which liberalization can have permanent growth effects. The findings of the current study are more in line with the neoclassical growth theory than with FEGMs, as growth effects are found to be only short-lived. A caveat in relating the findings to the FEGMs is that these theoretical models highlight many potential processes and channels that are not explicitly included in the estimation process of the current study. As such, it may be that a more detailed analysis including such mechanisms may highlight conditions under which growth effects indeed persist longer than the findings of the current study suggest.

In contrast to the existing theoretical and empirical literature, there is no evidence that strong institutions are a requirement for experiencing any benefits from capital account liberalization. Furthermore, economic development also does not appear to be a pre-requisite for capital account liberalization to stimulate economic growth. Countries undergoing capital account liberalization whilst making use of the IMF's lending facilities experience, on average, the same growth effects after such financial reforms as countries that are not making use of IMF funding. Other research has found that liberalization exposes countries to a higher risk of financial crises. Countries with repressed systems maintain more policy autonomy and are not exposed to excessive capital inflows fueling bubbles, nor sudden capital outflows. The estimated benefits of capital account liberalization are only small, while countries become more exposed to the risk of costly financial crises. These costs may outweigh the benefits, leaving countries with repressed systems better off, due to the absence of major downturns.

4.8 Conclusion

Though financial liberalization has been the subject of research for many decades, its effects on economic performance are still far from fully established. Basic theory primarily predicts positive results on the basis of such liberalization, as capital is expected to become more readily available and is employed more productively. Recent theoretical extensions and empirical research have made the relation between liberalization and economic performance ambiguous, however, with some literature pointing out several potential requirements in order for the benefits to materialize and some literature suggesting that there are no benefits to obtain in the first place.

This paper revisits the question on whether the connection expected from basic theory indeed appears in empirical data. The current study provides a methodological update, addresses prior endogeneity concerns, and includes potential feedback processes that may have rendered past empirical estimations less accurate. It narrows down the concept of financial liberalization, and studies it in the form of capital account liberalization. The findings in this paper suggest that capital account liberalization indeed generates additional economic growth, with modest reforms increasing economic growth by approximately 1-2% cumulatively, over a short time-span. The effects materialize within the first 5 years of the liberalization process, after which growth returns to its original pre-liberalization level. The paper finds no evidence that there are any requirements for the strength of institutions prior to undergoing a capital account liberalization process. Countries with weak institutions perform comparably to countries with strong institutions, and on average the former even slightly outperforms the latter. Similarly, (capital) poor countries perform similarly to (capital) rich countries after undergoing financial liberalization. Finally, usage of the IMF's lending facilities and the corresponding liberalization requirements that accompany such usage do not seem to influence the benefits generated by capital account liberalization. Existing literature has established that financially liberalized countries are more prone and more exposed to financial and economic crises, which can cause major damage to economic performance. The estimated economic growth benefits from financial liberalization in the form of capital account liberalization are quite small and short-lived. Capital account liberalization results in higher GDP per capita, even if growth is only temporarily boosted. The downsides associated with financial and economic crises are very large, potentially wiping away any benefits from liberalization. This raises the question if countries can expect to be better off by not pursuing extensive liberalization policies. While they may forfeit a temporary boost to output growth, the reduced incidence of economic and financial crises may generate more stable, consistent output growth, with fewer major downturns. The longevity of the higher GDP per capita generated by liberalization is highly conditional on whether countries are likely to face a crisis soon after liberalization, or whether such crises can be averted. Policy makers need to carefully assess these upsides and downsides, and capital account liberalization should not be pursued as a blanket policy.

Though this paper builds forth and attempts to improve upon past empirical studies, there are

still numerous limitations left to address in order to solidify the results. Firstly, the current study only includes a very limited role for international trade and trade liberalization. Including trade openness and trade liberalization in a more complete way poses considerable methodological challenges beyond the scope of this paper, but may have an appreciable impact on the estimated results. Secondly, the set of control variables is limited, and non-financial reforms, which may coincide with financial reforms, are not extensively included in the various models. Though the included non-financial variables are expected to capture several relevant reforms, there are still various nuances excluded, as data for such nuances is as of yet too limited. Thirdly, if the data become available, it may be worthwhile to explore the topic via bilateral liberalization measures, rather than multilateral ones, so as to better capture the precise extent of liberalization. Though it is methodologically challenging, the PVAR framework does allow for cross-sectional interdependence, which may shed new light on the conditions under which capital account liberalization generates the biggest benefits.

Chapter 5

Conclusion and final remarks

This dissertation is the compilation of three empirical studies. Chapter 2 focuses on changes in political leadership and democracy as a result of economic and financial crises. There is a lack of empirical research on this topic, and this chapter aims to fill part of the existing void. Chapter 3 examines whether changes in democracy have an impact on income inequality and income redistribution. Empirically, the connection between income inequality and democracy has been inconclusive, hence the main purpose of this study is to gather more empirical evidence. Chapter 4 tests whether capital account liberalization generates a boost to economic growth. For this topic, too, there is no clear consensus in the empirical literature. This study therefore also aims to document more evidence.

The following sections reiterate the main findings and conclusions of these three chapters. It then highlights several possible avenues for follow-up research, which could further solidify and clarify the presented results of each chapter. Casual readers, who have reached this concluding chapter straight after reading the introduction may wish to focus on only the final paragraph of each subsection.

5.1 Democratic Breakdowns, Transitions and Economic and Financial Crises

Chapter 2 collects data from 70 countries from 1810 onward to study whether changes in political leadership coincide with major economic downturns, as well as the coincidence between changes in democracy and major economic downturns. Changes in political leadership occur more frequently during times of crisis. Banking crises in particular are most strongly related with changes in political leadership, though there is also evidence that external debt crises result in leadership changes. Incumbent leaders are likely held accountable for their management of a crisis or for the conditions leading up to a crisis. A second finding is that the countries' level of democracy tends to be very persistent. Democratic countries tend to remain democratic, and non-democracies similarly tend

to remain non-democratic. The level of democracy nevertheless changes more frequently during times of crisis, with inflation crises having the strongest connections to such changes. During crises, democracies tend to move more toward authoritarianism, while non-democracies tend to partly democratize.[1] Even during times of crisis, the level of democracy remains very persistent, and the probability of seeing democracy change in any given year is still fairly small. Furthermore, changes in the level of democracy also tend to be quite small, with shifts between the extremes of the democratic spectrum being extremely uncommon. Crises weaken and partly undermine the extant regime, and give rise to democratic restructuring, but do not result in major, sweeping democratic changes.

The first implication of this study is that democracies are fragile to crises. Despite the built-in mechanism to choose the most effective political leadership, without altering the democratic framework, crises result in changes to the democratic framework itself. A second implication is that non-democracies tend to partly democratize during times of crisis. As the level of democracy tends to go hand in hand with various human rights, this highlights a potential positive outcome of otherwise economically disastrous periods of time.

There are several logical extensions to this study. It is not yet clear via which exact mechanism crises are connected to changes in political leadership or the level of democracy. It is unknown, for instance, whether direct government involvement in causing a crisis is of primary relevance, or whether it is instead the ineffective management and resolution of a crisis that matters most. Furthermore, the study does not assess whether the effect of crises depends on their severity. Finally, it may be the case that the risk of experiencing a crisis is related to the level of democracy. If so, then changes in leadership or democracy may have a bi-directional relationship to economic and financial crises.

5.2 The Relation between Democratic Changes and Income Redistribution

Chapter 3 studies whether there is an empirical connection between changes in democracy and changes in income inequality and income redistribution. Theory provides an ex ante expectation that countries undergoing democratization are likely to increase income redistribution, so that income inequality falls. Conversely, countries undergoing a democratic breakdown are likely to experience the opposite.

Chapter 3 studies periods of democratization and democratic breakdowns for 172 countries be-

[1] Note that changes in democracy can but do not have to coincide with changes in leadership. Many changes in political leadership occur without there being any change the level democracy.

tween 1960 and 2016. The method of choice in this chapter is Propensity Score Matching, which mimics randomised controlled trials to perform a macroeconomic quasi-experiment. It finds that countries do not experience any statistically significant change in income inequality following democratization, nor democratic breakdowns. Furthermore, there is no evidence that democratization or democratic breakdowns have any effect on income redistribution. The study distinguishes between a short-run and long-run time frame, but finds that a connection between democracy and income inequality never materializes. The magnitude of change in democracy is also found to have no impact on this result. The latter results may be less empirically robust, due to the rarity of major democratic changes, however.

The main implication is thus that changes in the level of democracy do not appear to have any appreciable impact on income inequality. Though highly democratic countries feature low levels of income inequality on average, it does not appear to be democracy itself that contributes to this outcome.

There are several points worth exploring further, so as to determine why there is so little evidence in favour of the theoretical framework presented by Meltzer and Richard (1981). This theory is based on the assumption that income inequality is perceived as unfair, and that the economically poor have an incentive to argue for redistribution. It may be that not every country sees inequality as unfair, or something to alter. Furthermore, it may be that inequality is misunderstood and underestimated by the poor, resulting in little to no redistribution being pursued. Finally, it may be that income inequality has a lower priority compared to other ideological goals, and that redistribution is only pursued when other goals have already been met. Data on the understanding of inequality, ideological differences and desirability of income redistribution are currently largely lacking. The current study is therefore not able to explicitly include such factors, but future research may be able to incorporate them.

5.3 The Impact of Capital Account Liberalization on Economic Growth

Chapter 4 switches its main focus from the institution of democracy to the institutions governing international finance. More specifically, it focuses on capital account liberalization and its effects on economic growth. The main hypothesis is that capital account liberalization should improve the efficiency and effectiveness of investment, that it reduces capital constraints allowing for more investment, and that it therefore increases economic growth. Existing literature highlights that capital account liberalization may, however, also expose countries to a greater risk of costly financial crises. The study adds to the existing literature by re-examining to what extent capital account liberalization has an impact on economic growth. In doing so, it helps generate a better picture of

whether the upsides of liberalization outweigh the downsides.

The methodology in Chapter 4 accounts for endogeneity more extensively than prior studies. It also considers potential indirect effects of capital account liberalization on economic growth via financial development and financial integration. The chapter presents impulse response functions to depict how the effects of capital account liberalization propagate over time.

The main results in this study are based on a sample of 182 countries covering the period 1970-2017. The results indicate that capital account liberalization indeed contributes to economic growth. A typical liberalization reform on average adds 1-2% of cumulative economic growth over the years. The majority of this growth occurs within the first 5 years after liberalization, after which economic growth returns to its pre-liberalization level. This effect does not appear to vary across democracies and non-democracies, nor across highly developed and less developed countries. Furthermore, there is no evidence that the use of IMF credit facilities has impacted the effects of capital account liberalization on economic growth. There is some evidence of a bi-directional relation between liberalization and economic growth, which suggests that countries may partly self-select into the liberalization process. This bi-directional connection is, however, sensitive to the choice of measure used for capital account liberalization, and warrants further exploration.

For this study, too, there are several avenues for follow-up research, which can shed more light on what factors augment or diminish the effects of capital account liberalization. It may be the case that capital account liberalization is paired with changes in openness to trade, both of which may ultimately impact economic growth. There may also be other, near simultaneous non-financial reforms that can impact economic growth. Readily available data on such reforms is largely limited, but may provide a better overview of under what conditions capital account liberalization generates the biggest benefits. Finally, it may be that liberalization policies and their outcomes are in part based on what major trading partners or surrounding countries are doing. For instance, a scenario where multiple neighbouring countries liberalize simultaneously could produce different results than a single country liberalizing on its own. Finally, liberalization is generally measured using multilateral indices, whereas bilateral measures may be more appropriate. It may be that capital account liberalization only involves opening up to part of the world, which is likely to produce different dynamics than full liberalization.

5.4 Main takeaways

The main results to take from the research in this thesis can be summarised by the following:

1. Political leadership changes more frequently during times of economic crisis.

2. Political regimes are undermined by economic and financial crises. Democracies tend to experience a (partial) breakdown. Non-democracies instead tend to democratize during such times.

3. Democratization has no significant impact on income redistribution or income inequality. Democratic breakdowns similarly have no measurable impact on income redistribution or inequality.

4. Capital account liberalization results in temporarily higher economic growth. The effect does not depend significantly on the level of democracy of a country, not its level of economic development.

Chapter 6

Valorization Addendum

This thesis not only contributes to academic understanding of the research topics, but also provides broader implications to society. The European Union (EU) explicitly makes international cooperation conditional on the maintenance of democracy.[1] Furthermore, the EU devotes substantial amount of resources to promoting democracy worldwide.[2] A better understanding of the emergence and consequences of democracy is thus of considerable interest to EU policy. Additionally, one of the core purposes of the EU is to foster stronger economic ties between its member states, with the goal to improve standards of living across Europe. One such tie between countries is the extent to which they are financially open to one another. A better understanding of whether financial openness indeed improves economic outcomes thus helps determine whether the trajectory followed by the EU has been effective, or whether it requires any adjustments.

This section briefly summarizes the main implications from each chapter for academic researchers, policy makers or society:

Democracies are fragile to economic and financial crises, and highly democratic countries tend to become less democratic during such crises. Democracy goes hand in hand with the provision and enforcement of human rights, hence a (partial) democratic collapse may weaken such human rights. Economic and financial crises are economically very costly in themselves, but a reduction in rights adds additional societal damage on top of it. In contrast, non-democracies tend to democratize partly during times of crisis. For academic researchers, Chapter 2 provides new insights into which factors drive regime changes. Prior research around democratization largely operated under the assumption that crises are relevant. Chapter 2 provides evidence that this is indeed the case. Furthermore, by distinguishing between different crisis types, it paves the way for further research, by establishing which crisis periods are most worthwhile studying in greater depth. For the EU, there are several implications. First, crises such as the recent Global Financial Crisis may pose a threat to democracy across the EU. A rapid, EU-wide resolution of such crises may be

[1] The Cotonou Agreement is an example hereof.

[2] The European Instrument for Democracy and Human Rights is a prime example of the EU's focus on democracy.

necessary not only to contain any economic damage, but to also prevent democratic decay within the union. Anti-EU sentiment across countries such as Hungary, Poland and the UK (culimating in Brexit) are likely in part driven by this late crisis. At the time of writing, the economic crisis induced by the COVID pandemic requires an EU-wide rapid, strong resolution so as to prevent additional undermining of democracy. As highlighted at the start of this section, the EU also has explicit policies promoting democracy around the world. Based on the results in this thesis, policy makers need to pay particular attention to countries undergoing economic crises. Among highly democratic countries, crises undermine EU efforts to promote democracy. Democracies may thus need more active EU participation to prevent democratic decay. Among weak democracies, economic crises may instead provide an opportunity for the EU to push for further democratization.

The results in Chapter 3 show that income inequality is unaffected by changes in the level of democracy. There is no evidence that democratization results in any income redistribution nor does it have any impact on income inequality. Democratic breakdowns are similarly not associated with any developments in income redistribution or income inequality. Changes in the level of democracy are thus not of paramount importance to the distribution of income or the distribution of standards of living. For academic researchers, it provides further evidence that existing models connecting democracy to income inequality make inaccurate predictions. The Sustainable Development Goals (SDGs) posed by the United Nations promote sharing prosperity in an equitable and sustainable way, so as to reduce poverty. The results in Chapter 3 highlight that this goal is unlikely to be reached by promoting the institution of democracy, but that it is better accomplished using different means instead. Policy makers attempting to implement and reach the SDGs related to inequality should thus not prioritise the process of democratization.

Capital account liberalization on average results in higher economic growth. The effect on growth is modest and it is quite short-lived, but nevertheless results in higher permanent GDP per capita. Capital account liberalization is known to expose countries to a higher risk of economic and financial crises. The effect on GDP per capita may thus be short-lived too, if a crisis ensues not long after liberalization. Policy makers have, in the past, pursued capital account liberalization as a way to achieve higher economic growth. The results in Chapter 4 show that such growth is quite limited, and that it is not a panacea guaranteeing more and more prosperity via ever-increasing GDP. Policy makers should thus consider capital account liberalization carefully and not pursue it as a blanket policy, as the costs caused by crises may outweigh the benefits generated via temporarily higher growth. For academic researchers, Chapter 4 provides new evidence on the magnitude and persistence of the effects of capital account liberalization. By explicitly accounting for potential feedback processes and including potential transmission mechanisms between capital account liberalization and economic growth, the results provide a more detailed understanding of the impact of liberalization policies than prior studies.

Hoofdstuk 7

Nederlandse Samenvatting

Dit proefschrift is een compilatie van drie empirische studies over de interactie tussen democratie, financiële regelgeving en economische prestaties. Dit korte hoofdstuk geeft een beknopt overzicht van de twee hoofdonderwerpen binnen dit proefschrift, het overkoepelende thema, de onderzoeksvragen en de resultaten. Met het oog op leesbaarheid probeert dit korte hoofdstuk technische details en jargon te vermijden.

Economen, sociologen en politicologen zijn al decennialang bezig met de vraag of democratie nuttig danwel noodzakelijk is voor het goed functioneren van economiën en maatschappijen. Ondanks de vele studies over democratie hebben de sociale wetenschappen hier nooit een consensus over bereikt. Zo is er bijvoorbeeld onenigheid over of democratie bevorderlijk is voor economische groei en ontwikkeling en of democratie voor meer inkomensgelijkheid zorgt. Tevens is het niet geheel duidelijk onder wat voor omstandigheden democratieën zich ontplooien of instorten, noch in hoeverre zulke democratische ontwikkelingen een economisch fundament hebben.

Financiële regelgeving heeft sinds de Grote Recessie van 2008/2009 aanzienlijk meer aandacht gekregen onder economische onderzoekers. De enorme, wereldwijde chaos en economische schade die deze bankencrisis heeft veroorzaakt heeft ervoor gezorgd dat de financiële wereld in een negatiever daglicht is komen te staan. Financiële integratie en ontwikkeling werden vroeger veelal gezien als een nuttige bron van economische groei en ontwikkeling. Financiële regelgeving werd hierdoor als een ongewenste beperking gezien, die de potentiele bijdragen van de financiële wereld teveel beperkte. Recent onderzoek geeft echter aan dat de deregulering van de financiële wereld wellicht te ver is doorgeschoten. Kapitaalmarkten zijn mogelijk te ver ontwikkeld en te zeer internationaal verstrengeld, tot het punt dat ze meer kwaad dan goed beginnen te veroorzaken.

Het overkoepelende thema tussen het onderwerp van democratische ontwikkelingen en financiële regelgeving is het concept van instituties. Instituties zijn, kort samengevat, de formele en informele regels die bepalen hoe individuën en organisaties met elkaar mogen omgaan. Democratische instituties bepalen bijvoorbeeld dat iedere volwassen persoon een gelijke stem en invloed heeft op het

bepalen van overheidsbeleid, in tegenstelling tot dictaturen waar slechts een handjevol mensen alle macht in handen heeft. Financiële regelgeving bepaalt bijvoorbeeld of kapitaal over grenzen mag worden verplaatst, of welke producten de bankenwereld mag verhandelen.

Dit proefschrift probeert binnen dit overkoepelende thema drie onderzoeksvragen te beantwoorden:

1. In hoeverre worden veranderingen in het niveau van democratie teweeggebracht door economische en financiële crises?
2. Zorgt democratisering voor een vermindering van inkomensongelijkheid?
3. Leidt een liberalisering van de kapitaalrekening tot hogere economische groei?

Hoofdstuk 2 onderzoekt de eerste vraag. Het hoofdstuk bestudeert de economische, democratische en politieke ontwikkelingen van 70 landen vanaf het jaar 1810 tot het jaar 2010. De studie maakt een onderscheid tussen 6 soorten crises: Hyperinflatie, bankcrises, buitenlandse schuldencrises, binnenlandse schuldencrises, beurscrashes en valuta crises. De resultaten van deze studie geven aan dat politieke leiders vaker worden vervangen ten tijde van een crisis. Van de 6 crisissoorten spelen bankencrises en buitenlandse schuldencrises de grootste rol hierin. Een tweede vondst binnen deze studie is dat het niveau van democratie maar zeer zelden verandert, maar dat zulke veranderingen wel vaker voorkomen tijdens crises. Periodes van hyperinflatie hebben de nauwste band met zulke democratische veranderingen. Onder democratische landen wordt het niveau van democratie ietwat verzwakt, terwijl niet-democratische landen juist deels democratischer worden. In het algemeen zijn de veranderingen van het democratische niveau vrij klein en is er bijvoorbeeld vrijwel nooit sprake van het volledig omgooien van het democratisch stelsel. Toch geven de resultaten aan dat democratieën kwetsbaar zijn voor economische en financiële crises. Aan de hand hiervan kan men in de context van crises als de recente Grote Recessie verwachten dat sommige EU landen minder democratisch uit de crisis tevoorschijn komen. Ook geeft dit aan dat sterk leiderschap en het beperken van de economische schade essentieel zijn bij de aanpak van de huidige COVID crisis, aangezien ook hier het democratisch stelsel mogelijk wordt ondermijnd.

Hoofdstuk 3 zoekt een antwoord op de tweede vraag. Theoretische economische modellen voorspellen dat democratisering tot meer inkomensherverdeling leidt, waardoor inkomensongelijkheid wordt verminderd. Dit komt omdat democratisering in het algemeen het machtsmonopolie van rijken vermindert en de politieke invloed van armere bevolkingsgroepen bevordert. Dit biedt zulke armeren de kans om bestaande, scheve inkomensverdelingen deels recht te trekken via, bijvoorbeeld, een progressief belastingsstelsel of een uitbreiding van de verzorgingsstaat. In landen die minder democratisch worden wordt dit proces omgedraaid. Hier verwacht men theoretisch gezien dat de inkomensongelijkheid dus juist groter wordt. Hoofdstuk 3 bestudeert 172 landen tussen 1960 en

2016, om te kijken of deze theoretische voorspellingen in het verleden inderdaad waar zijn gemaakt. Uit de resultaten blijkt dat democratisering zowel op de korte termijn als langere termijn geen statistisch significante invloed heeft op het niveau van inkomensongelijkheid, noch de mate waarin inkomensherverdeling plaatsvindt. Ook (gedeeltelijke) democratische instortingen blijken geen statistisch significante, meetbare effecten teweeg te brengen. Een expliciet onderdeel van de huidige Werelddoelen voor Duurzame Ontwikkeling is het verminderen van ongelijkheid tussen en binnen landen. Aan de hand van de resultaten van hoofdstuk 3 blijkt dat dit doel zeer onwaarschijnlijk kan worden behaald via beleiden die de democratie bevorderen, maar dat er andere kanalen moeten worden verkend.

Hoofdstuk 4 richt zich op het beantwoorden van de derde vraag. Het liberaliseren van de kapitaalrekening creëert de mogelijkheid om kapitaal te verplaatsen van plekken waar een overschot aan kapitaal is naar plekken waar daar een tekort aan is. Het openen van grenzen voor kapitaalstromen vergroot tevens de internationale concurrentie onder financiële instellingen, hetgeen ze dwingt om effectiever te werk te gaan. Liberalisatie van de kapitaalrekening biedt dus een betere toegang tot kapitaal voor investeringen en leidt tot financiële ontwikkeling. Zowel neoklassieke groeitheorie als financiele endogene groeimodellen stellen aan de hand hiervan dat de liberalisatie van kapitaalrekeningen bevorderlijk is voor economische groei. Toch hebben de vele financiële crises in Latijns Amerika sinds 1980, de Aziatische wisselkoerscrisis van 1997 en de recente Grote Recessie duidelijk gemaakt dat er ook nadelen vastzitten aan nauwe, international financiële integratie. Een open kapitaalrekening maakt immers niet alleen de instroom van kapitaal makkelijker, maar ook kapitaalvluchten kunnen makkelijker plaatsvinden, waardoor wisselkoerscrises en schuldencrises kunnen ontstaan. Hoofdstuk 4 kijkt naar hoeveel economische groei de liberalisatie van de kapitaalrekening gemiddeld oplevert, om een beter beeld te schetsen of de voordelen van liberalisatie groot genoeg zijn om voor de nadelen te compenseren. Aan de hand van een studie van 182 landen tussen 1970 en 2017 blijkt dat liberalisatie van de kapitaalrekening inderdaad tot een tijdelijk hogere economische groei leidt. Economische groei is gedurende de jaren na liberalisatie cumulatief 1-2% hoger dan voorheen, maar keert na ongeveer 5 jaar weer terug naar dezelfde waarde als voor liberalisatie. Er is geen bewijs dat dit effect afhankelijk is van het niveau van ontwikkeling van het liberaliserende land, noch is er een merkbaar verschil tussen democratische en niet-democratische landen. In het verleden is liberalisatie als een soort wondermiddel voor economische groei toegepast. Uit de resultaten van hoofdstuk 4 blijkt dat zulk beleid met scepsis en voorzichtigheid moet worden benaderd: Een gemiddeld 1-2% cumulatief hogere groei kan immers makkelijk weg worden gevaagd door een enkele financiële crisis.

Al met al zijn er vier kernpunten die men uit dit proefschrift kan halen:

1. Politiek leiderschap verandert vaker ten tijde van economische en financiële crises.
2. Democratieën zijn kwetsbaar voor crises en democratische instituties verzwakken tijdens crises. Niet-democratiën hebben, in tegenstelling, eerder de neiging om democratischer te worden.
3. Democratisering heeft geen significante invloed op inkomensongelijkheid noch op inkomensherverdeling. Ook (deelse) democratische instortingen hebben hier geen meetbare invloed op.
4. Liberalisatie van de kapitaalrekening leidt tot tijdelijk hogere economische groei. Dit effect is vrij klein, kortstondig en is gemiddeld hetzelfde tussen democratiën en niet-democratiën en tussen ontwikkelde landen en ontwikkelingslanden.

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Curriculum Vitae

Adam Kuczynski was born in Sittard, Netherlands on February 22 1990. He obtained a BSc in Economics in 2011 from Maastricht University, graduating cum laude. This study programme included a semester in the first half of 2011 at the National University of Singapore, in Singapore. The programme introduced Adam to conducting economic research via the Maastricht Research Based Learning programme (MaRBLE), in which he focused on empirically testing mainstream economic theory on economic growth. Now intrigued by a life as a researcher, he continued his studies at Maastricht University and obtained an MSc in Economic and Financial Research in 2013, graduating cum laude once again.

Adam was encouraged to stay at Maastricht University to pursue a PhD. He ultimately enrolled in a PhD programme in 2013, initially under the supervision of Prof. Dr. Bertrand Candelon - a role that was ultimately jointly taken over by Prof. Dr. Clemens Kool and Prof. Dr. Tom van Veen in 2019. Adam's primary research interests include institutional economics and international macrofinance. Throughout the PhD programme, Adam has taught courses on a wide variety of topics, including the management of financial crises, development economics, international trade, philosophy of science, and human resource policies. Regardless of the topic being taught, he has consistently been evaluated as an excellent tutor, resulting in two nominations for the Outstanding Tutor Award in 2016 and 2017.

During the PhD programme, Adam has been an active member of PhD Academy. This organization is devoted to bringing interdisciplinary PhD candidates together in both a social and academic environment. He acted as the Treasurer of this organization between 2017 and 2020. In addition to his research interests, Adam is an experienced classical pianist, an endurance running enthusiast, a fan of performing improvisation theatre and an avid SCUBA diver.

Ceterum autem censeo Carthaginem esse delendam.