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Economics Institute

Charles University in Prague

Empirical Essays on Crises, Reforms and Growth

Petar Stankov

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Dissertation Committee

Lubomír Lízal, CERGE-EI, Charles University

Evangelia Vourvachaki, CERGE-EI, Charles University

Peter Katuščák, CERGE-EI, Charles University

Levent Çelik, CERGE-EI, Charles University

Jan Kmenta, University of Michigan

Jan Švejnar, University of Michigan

Referees

Evan Kraft, American University, Washington, DC

Jan Babecký, Czech National Bank, Prague

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Introduction

This work addresses three policy-relevant empirical issues. First, how do banking crises affect financial reforms? It turns out that banking crises produce a variety of reform patterns in the financial sector over time. Second, do countries which reform their financial, product, and labor markets grow similarly? The results suggest that some countries benefit more from market-oriented reforms than others. Third, if some countries benefit more, could it be because various economies have markedly different firm-size distributions, and firms of different size grow differently after identical reforms? If firms of different size indeed grow differently after identical reforms, this could produce diverse growth outcomes across countries after similar reforms.

The first study has been motivated by the fact that a number of countries have gone through banking crises since the early 1970s. It links those episodes with the patterns of various financial reforms within those countries. As banking crises are endogenous, crisis exposures to major trading partners help identify the causality between crises and reforms. Consistent with the previous literature, the results of this work demonstrate that systemic banking crises reverse most financial reforms. However, they do so with various lags, whereas the impact of non-systemic crises is largely insignificant. The main results remain unaffected after numerous robustness checks. The main contribution of this work is to study financial reforms in a dynamic empirical

framework with endogenously determined banking crises. A rich set of policy implications is discussed which could help establish a growth-enhancing financial regulatory framework after banking crises.

The second study analyzes the influence of credit-, labor-, and product market deregulation policies on economic growth in more than 60 economies over a period of 40 years since 1970. By combining a difference-in-difference strategy with an IV approach to the endogeneity of the reform timing, this work finds that deregulation contributed to the per capita GDP levels of the early and consistent reformers relatively more than to the ones of the late reformers. The paper also finds a significant growth acceleration effect from market-oriented reforms over shorter periods of time. However, the growth acceleration effects dissipate over longer periods. A number of robustness checks support these conclusions.

The third study uses large firm-level data to search for the reasons similar market-oriented reforms can produce different growth outcomes across countries. It combines two observations. On the one hand, economies have markedly different firm size distributions. On the other hand, firms of different size grow differently after identical financial- and product-market liberalization reforms. Thus, identical reforms can produce different growth outcomes across countries. This result is reached by exploring firm-level data on sales and sales per worker across 135 developing and post-transition economies between 2000-2010. It helps explain the remarkable variation in the vast development literature studying the effects of various market-oriented reforms across countries and over time.

Introduction (Czech version)

Tato práce se zaměřuje na tři empirická témata relevantní pro veřejné politiky. Zaprvé, jak bankovní krize ovlivňují finanční reformy? Data ukazují, že bankovní krize produkují v průběhu času řadu reformních vzorců ve finančním sektoru. Za druhé, rostou země, které provedli reformu finančních trhů, trhů s produkty a trhů práce, podobně? Výsledky naznačují, že na některé země mají tržně orientované reformy pozitivnější vliv než na země jiné. Za třetí, pokud některé země opravdu těží víc z takovýchto reforem, může být důvodem to, že různé země mají výrazně odlišné rozložení firem z hlediska velikosti a že různě velké firmy rostou rozdílnou rychlostí po identických reformách? Pokud firmy různých velikostí opravdu rostou různou rychlostí po stejných reformách, mohlo by to vést k různým růstovým zkušenostem napříč zeměmi procházejícími obdobnými reformami.

První studie byla motivována skutečností, že řada zemí prošla od počátku sedmdesátých let minulého století bankovními krizemi. Tato práce spojuje tyto epizody se schématy různých finančních reforem v rámci těchto zemí. Protože bankovní krize jsou endogenní, vystavování se velkým obchodním partnerům během krize pomáhá identifikovat kauzalitu mezi krizemi a reformou. Výsledky této práce jsou konzistentní s předchozí literaturou a ukazují, že systémové bankovní krize zvrátí většinu finančních reforem. Děje se tak však se zpožděním různé délky, zatímco dopad nesystémových krizí

je do značné míry nevýznamný. Výsledky se nemění po mnoha zkouškách robustnosti. Práce diskutuje širokou škálu možných důsledků pro veřejné politiky, které by mohly vytvořit růst-posilující finanční regulační rámec po bankovních krizích.

Druhá studie analyzuje vliv deregulace úvěrového, pracovního a produktového trhu na hospodářský růst ve více než 60 ekonomikách po dobu 30 let. Studie pomocí kombinace rozdílových strategií a přístupem instrumentálních proměnných, tak aby adresovala problém endogenity načasování reforem, dochází k závěru, že deregulace přispěly k úrovni HDP na hlavu dřívějších reformátorů relativně více než k úrovním pozdějších reformátorů. Práce také poukazuje na signifikantní zrychlení ekonomického růstu v důsledku tržně-orientovaných reforem, které nicméně trvá po relativně kratší dobu. Tento efekt se však v delším časovém horizontu z dat vytrácí. Závěry práce jsou podpořeny několika zkouškami robustnosti.

Třetí studie používá data na úrovni jednotlivých firem, aby odpověděla na otázku proč podobné, tržně orientované reformy mohou vést k různému ekonomickému růstu v různých zemích. Za tímto účelem kombinuje dva postřehy. Zaprvé, mezi ekonomikami existují výrazné rozdíly v distribuci velikosti firem. Zadruhé, různě velké firmy rostou různou rychlostí po identických liberalizačních reformách finančních trhů a trhů s produkty. Z tohoto důvodu identické reformy můžou vést k různému ekonomickému růstu různých ekonomik. Toto je potvrzeno pomocí analýzy firemních dat o tržbách a tržbách na zaměstnance ze 135 rozvojových zemí a zemí po ekonomické transformaci. Tento výsledek pomáhá objasnit pozoruhodnou variaci v obsáhlé rozvojové literatuře, která se zabývá efektem různých tržně-orientovaných reforem v různých zemích a časových horizontech.

Chapter 1

Banking Crises and Reversals in Financial Reforms[1]

[1]An earlier version of this essay was published as a CERGE-EI Working Paper No. 474 in December 2012.

1.1 Introduction

Despite the rich history of both systemic and non-systemic banking crises in many countries, and the variety of policy responses to them, the financial reforms literature contains relatively little information on the specific *ex-post* financial reform patterns. It is still unclear which reform areas are more likely to be affected than others, how long it typically takes regulators to enact reforms in a given area, is the forcefulness of reforms related to the severity of crises, and whether a banking crisis concurrent with a recession induces faster reforms. To address those questions, economists need to look at many banking crises across a large number of countries over long periods of time. However, to date, the literature is scarce on panel data studies in this line of research.

One notable exception is the work by Abiad and Mody (2005). They study how banking crises affect the overall pattern of financial reforms across countries by using an ordered logit model. Implicitly, however, their model assumes banking crises are random events, which is arguably not the case. Banking crises are most likely determined endogenously and three channels for their incidence seem evident. First, Barth, Caprio, and Levine (2008), among others, conclude that banking system performance, hence its fragility, may be affected by banking regulations but leave empirical work in this direction for the future.[2] Demirgüç-Kunt and Detragiache (1998) also find that financial liberalization may positively influence the likelihood of a banking crisis, especially in countries with weaker banking supervision and judicial institutions. In a supporting argument, Demetriades and Hook Law (2006)

[2]In fact, Barth, Caprio, and Levine (2004) have already done some of this work on a cross-section of countries by using the data they collected in Barth, Caprio, and Levine (2001).

argue that financial development has larger effects on GDP per capita when the financial system is embedded within a sound institutional framework, while Rousseau and Wachtel (2011) state that the weakening of the financial development-growth link may also be a result of widespread financial liberalizations in the late 1980s and early 1990s in countries that lacked the legal or regulatory infrastructure to exploit financial development successfully. Therefore, any empirical study of financial reforms is prone to reverse causality issues between crises and reforms. Demetriades and Hussein (1996) are among the first to point this out. However, reverse causality is the first among many reasons to consider banking crises endogenous.

Second, it has been shown that banking crises in a given country i can occur through numerous endogenous channels on both the assets and the liabilities sides of the bank balance sheet. Crises occurring on both sides have been studied by Allen and Gale (1998, 2000). In their earlier paper, an economic downturn in the real sector reduces the returns on bank assets. As a result, depositors put pressure on the banking sector by liquidating bank liabilities. In their latter work, banks in region i liquidate claims on banks in region j when there is an excess demand for liquidity in region i. However, the liquidity may not be readily available in region j, which in turn causes banks in region j to contribute to the excess demand for liquidity, which drives contagion. Then, these two papers suggest that a banking crisis could not only originate in the real sector but it could also propagate across regions for reasons within both the financial and the real sector.

Third, the empirical literature adds cross-country trade and financial flows as contagion mechanisms. For example, Balakrishnan, Danninger, Elekdag, and Tytell (2011) suggest that deeper financial links are a key factor for the increased financial distress running from developed to developing

economies. Trade linkages are examined as an additional factor that may drive contagion in Eichengreen, Rose, and Wyplosz (1996) and in Gorodnichenko, Mendoza, and Tesar (2012). The work by Gorodnichenko et al. (2012) is one example of how trade linkages between the former Soviet Union and Finland might have caused the Finnish output collapse in the early 1990s which was followed by a banking crisis. At the same time, the financial reforms in Finland had little to do with the origin of its trade collapse in Russia. Therefore, an output collapse or a banking crisis in a trading partner could trigger a banking crisis in a given country, without necessarily being related to the financial reforms in that country. This intuition helps identify the causality running from banking crises to financial reforms.

The identification is done by constructing banking crisis exposures for each country and period of time. The crisis exposure reveals how a banking crisis in a given trading partner j affects the likelihood of a banking crisis in a given economy i, without directly affecting i's financial reforms. Thus, the paper identifies at least some part of the exogenous impact of banking crises on financial reforms and addresses one of the long-standing issues in the empirical literature of financial reforms: the implicit assumption of randomly occurring crises. This is the first contribution of this work.

Its second contribution is to acknowledge and incorporate the inherent dynamics of the reform process. The intuition supporting the inclusion of the reform dynamics is simple. First, if a country's financial system has not been liberalized at all, this may indicate high resistance to reform or a strong *status quo* bias, as in Abiad and Mody (2005). Thus, previous low levels of financial liberalization may also predict low levels in the current period. At the same time, however, high levels of financial liberalization in the past may mean that there is not much left to reform, even if the incumbent government

is reform-oriented. Hence, at high levels of financial liberalization we may see slow reforms as well. This is a path-dependent non-linear relationship which calls for inclusion of both linear and quadratic terms of lagged levels of reforms in any empirical model of reform dependence on banking crises.

However, financial reforms may occur and may also be delayed for reasons other than banking crises and reform dynamics. Past recessions and exchange rate fluctuations may well interfere with policy decisions on reforming the financial sector. Also, once countries become more open and gain from trade, they might be more likely to reduce their bias in favor of keeping the *status quo* and open up to financial liberalization, as in Rajan and Zingales (2003). In addition, the *status quo* bias against financial reforms may change at various stages of the business cycle, which would surface as a higher likelihood of opening up or re-regulating some parts of the financial system at various stages of the cycle.

There are also potential differences in how various countries respond to banking crises due to legal origin or geography, if they react at all. Morck and Yeung (2009) bring up legal origin, early land distribution, language, religion and culture as other possible fixed effects on a regulatory reform. Further, major events in a group of countries in a given period such as the economic transformation in Central and Eastern Europe in the early 1990s, the banking crises in Latin America, Asia and Eastern Europe in the late 1990s, and the current fiscal crisis in the Eurozone, may shape financial reforms as well. Those regional events which occur at a given point in time need to be taken into account in a study of any financial reform.

Based on the intuition above, the following section presents an empirical model to study financial reforms in a dynamic empirical framework with endogenously determined banking crises. The data and the results are pre-

sented next. Since some econometric concerns may arise over how the dynamic model was constructed, necessary robustness checks are presented after the discussion of the results. Those robustness checks validate the baseline results. The conclusions point to specific areas in which governments could focus financial reforms in the wake of banking crises.

1.2 Methodology

1.2.1 Baseline Model

To address the impact of a financial crisis on the ex-post financial reforms, I estimate the following model in differences:

$$R_{mit} = \beta_1 R_{mit-1} + \beta_2 R^2_{mit-1} + \sum_{s=0}^{s^*} \beta_s SBC_{it-s} + \tag{1.1}$$
$$+ \sum_{s=0}^{s^*} \beta_s NBC_{it-s} + \mathbf{Z}'_{it-1}\beta + f_i + f_r f_t + \varepsilon_{mit},$$

where R_{mit} is the reform measure m in country i in period t changing after a systemic banking crisis (SBC) or a non-systemic banking crisis (NBC) occurs in the same country in the current or previous two periods, and Z'_{it} is a vector of other controls. The measure R_{mit} is an index reflecting how the overall pattern of financial reforms or any of the specific financial reforms monitored by Abiad, Detragiache, and Tressel (2010), changes over time. The other controls include: a) lagged log-levels of per capita GDP and log-level of the exchange rate against the US dollar; b) the openness of the economy measured by the share of foreign trade in GDP; c) the reform gap: the difference between the highest level of the reform within the same region in year t and the country's level of reform in the manner of Abiad and Mody (2005), as well as an interaction of the reform gap with GDP and the exchange

rate;[3] and d) political system variables.

The optimal lag-length s^* was determined by using a procedure suggested in Babecký et al. (2013) and developed by Love and Zicchino (2006). Similarly to Babecký et al. (2013), I use a panel vector autoregression (PVAR) technique to generate impulse-response functions (IRFs) of each financial reform to an SBC or an NBC shock. The optimal lag s^* is then determined at the point at which an IRF of a particular reform to a shock reaches its maximum (in the case of a positive response) or minimum (in the case of a negative response). To illustrate the process of lag selection, Figure 1.1 presents the impulse-response pairs (R_{mit}; SBC) and (R_{mit}; NBC) for the overall reform. The lag selection process for the rest of the pairs is identical. For most reforms, including the overall reform pattern, the number of optimal lags after an SBC is 2. That is why I choose $s^* = 2$.

The panel OLS model above has two issues which may bias the results and possibly even produce inconsistent estimates. The first issue is the endogeneity of crises which, apart from being evident in the literature, is also noticeable in the IRFs of crises to reform shocks. The second issue is the serial correlation in the presence of reform dynamics. The first issue is addressed by using an instrumental variable (IV) approach, combined with the above fixed effects panel data estimations. The second issue is addressed by using a difference GMM model in the spirit of Arellano and Bond (1991), which leads to consistent estimates even in the presence of serial correlation (Cameron & Trivedi, 2005, p.764-765).

[3]Often, financial reforms within a country are a product of global or regional reform trends, even in the absence of local banking crises. If the regional leaders in reforms are thought of as the reform trend-setters, then the reform gap would capture not only some of the local reform idiosyncrasies but also the regional simultaneity of reforms which may or may not be due to a banking crisis.

1.2.2 Instrumental Variable Estimation

If a financial crisis is modeled as a purely random event occurring as a self-fulfilling prophecy, then the panel OLS approach to estimate the effect of a crisis would suffice for unbiased and consistent estimation. However, for reasons detailed above, a crisis is determined endogenously. Acknowledging the plethora of ways in which banking crises can spread across countries and over time, this work considers trade linkages to be a viable propagation mechanism of financial distress, as in Rose and Spiegel (2009) and Gorodnichenko et al. (2012). A crisis in country i will be more likely if it trades with country j, which happens to be in a crisis. If country j is in a crisis, it will likely demand less imports from country i. This will reduce exports from country i, which may induce a recession in an open economy and shrink assets in its banking sector, which in turn raises the likelihood of an asset crisis, with a certain lag. A crisis in country i will be all the more likely if more than one trading partner experiences an episode of financial distress at the same time, or if its export share to a country in crisis is large, or both. Based on this premise, I construct a *crisis exposure* variable for each country and year. In its simplest form, the crisis exposure is an export-weighted crisis occurrence in country i's trading partners at time t:

$$CrExp_{it} = \sum_{j} C_{jt} S_{ijt} \in [0; 1], \tag{1.2}$$

where $CrExp_{it}$ is the crisis exposure of country i in period t, C_{jt} is a dummy equal to 1 if a banking crisis occurs in country j in period t, and S_{ijt} is the share of i's exports to j in period t. Since C_{jt} is either 0 or 1, and $\sum_j S_{ijt} = 1$, then the crisis exposure varies between 0 and 1.

At first glance, the crisis exposure is prone to a weakness. Even if it identifies i's exposure to a shock coming from j, it *appears* to assume the

crisis in j to be exogenous. But the crisis in j is not exogenous, as j is exposed to other economies through its own trade.[4] However, note that the crisis exposure is constructed in a way that captures also j's exposure. Therefore, j's crisis is also identified, as well as its own partners' crises down to the ultimate originator.

Depending on the type of crisis occurring in country j, two instrumental variables can come from the crisis exposure variable simultaneously – a systemic banking crisis exposure, and a non-systemic banking crisis exposure. It is also important to note that a non-systemic crisis in a large trading partner may bring a disproportionately large effect in a small open economy. Therefore, both are used as instruments for the SBC and NBC in country i in the first stage of the 2SLS estimations.

1.2.3 Correcting for Serial Correlation

Standard panel data literature suggests that if the data contains a large time dimension, then fixed effects estimation may render consistent results even in a dynamic panel (Cameron & Trivedi, 2005, p.764). However, in some cases the linked data on banking crises and financial reforms contains just a few years of data. In fact, the maximum number of years in my sample is just below 30, which cannot be considered a large number. Therefore, the way to consistently estimate the parameters of interest in the presence of dynamics is to use a difference GMM method (Arellano & Bond, 1991). Apart from instrumenting with the lagged levels of the variables, the crisis exposures are kept as additional regressors in the first estimation stage. The

[4]It is also exposed through its financial linkages but longitudinal data on bilateral financial flows is still proprietary, and the Bank for International Settlements is yet to publish it: see the CGFS (2012, p.4-5) report.

first pass at estimating equation (1.1) is a one-step difference GMM with robust standard errors to both heteroskedasticity and serial correlation, in which the crisis exposures are treated as strictly exogenous.

As a standard procedure, the Sargan and Hansen tests of the overideintification restrictions are also done. As Baum, Schaffer, and Stillman (2003) note, the Sargan statistic is not valid in the presence of heteroskedasticity. Therefore, a significant difference between the Sargan test and the heteroskedasticity-robust Hansen test could be expected. However, even the robust Hansen test is prone to weaknesses in the presence of many instruments. Hence, a robustness check on the GMM method is required, which reduces the number of instruments significantly. The robustness checks on the GMM method are also discussed below.

1.3 Data

The data used here to feed the models above are a combination of four data sets. The first one is a data set constructed by Caprio and Klingebiel (2003). It features the timing of 117 episodes of systemic banking crises in 93 countries since the early 1970s and of 51 borderline systemic and non-systemic crises, thereby enabling this work to qualify which crises lead to the variety of financial reforms studied here.

The Caprio and Klingebiel (2003) data is supplemented by the newer Reinhart and Rogoff (2008) work, which dates further episodes of banking crises after 2002. In addition, the Reinhart and Rogoff data set eliminates some of the dating ambiguities in the former data set, especially the ones related to the end dates of some of the crises, and thus represents an important addition to it.

The third data set was assembled by Abiad et al. (2010). It has monitored seven financial reforms annually from 1973 till 2005 across 91 countries. The reforms include imposition of credit controls, interest rate controls, entry barriers, restrictions on private ownership and banking privatization, securities and banking supervision regulations, and capital account restrictions. Each particular financial reform is coded into a discrete index $i \in [0; 3]$.[5] In addition, Abiad et al. construct an overall index of financial reforms for each country and year, being equivalent to the sum of indices of each particular reform, and normalize it to 1. In each set of regressions – fixed effects, 2SLS, and difference GMM – I take the change in each of the normalized reform indices as the dependent variable. An increase in the reform index means a more liberalized financial system, with the exception of banking supervision reform, where stricter supervisory powers are associated with an increase in the index.

The fourth data set consists of the systemic and the non-systemic crises exposures for each country. To construct this data, I use the Caprio and Klingebiel (2003) crises data and interact each crisis episode in country j in year t with the shares of exports from country i to country j in year t. If there is no crisis in any country j in a given year, then the crisis exposure in country i is 0. If there is a crisis in country j, then the crisis exposure is the share of exports of i going to country j. A crisis exposure for country i increases with the number of trading partners in crisis, and with the share of exports to a given partner in crisis. To construct a panel of bilateral export shares, I need a longitudinal bilateral trade data. Such data are available

[5]For each of the 7 policy reforms, Abiad et al. code the current situation as 0 if the policy is most restrictive, and 3 if the policy is most liberalized. I normalize these indices to 1.

for 1970-2000 in Feenstra, Lipsey, Deng, Ma, and Mo (2005). An alternative source of bilateral trade data for 1948-2000 is Gleditsch (2002). Despite having a longer time coverage, the Gleditsch (2002) data has an identical matchable span to the Feenstra et al. data. Therefore, I use the Feenstra et al. data only.

The additional controls are taken from the Penn World Table 7.0. produced by Heston, Summers, and Aten (2011) and from the Database of Political Institutions prepared by Beck, Clarke, Groff, Keefer, and Walsh (2001).[6] The GDP is the log-level of per-capita GDP; the exchange rate (XR) is the log of the exchange rate against the US dollar and its increase means an exchange rate depreciation; the openness is the share of foreign trade in GDP. The model is then estimated after differencing all the variables, including the interaction terms. The results from these estimations are presented below.

1.4 Results

Table 1.1 presents the results from estimating equation (1.1) in differences by fixed effect panel data OLS with clustered standard errors. The table reveals several policy response patterns to financial crises, taken from the experience of more than 70 countries, spanning roughly 30 years. Column (1) demonstrates the effect of banking crises on the overall pattern of financial reforms. The expected significant non-linearities in the reform dynamics are indeed present. They are indicated by a negative and significant coefficient on $Reform^2_{t-1}$. The sign also contributes evidence to an inverted U-shape of overall reform dynamics, which was found to be significant by Abiad and Mody (2005) and later by Campos and Coricelli (2012). This means countries

[6]The most recent update of the Beck et al. (2001) database was in December, 2010.

which reversed their financial liberalization in the past are less likely to reform and that those who reformed most in the previous period are also less likely to undertake further reforms.

The inverted U-shape of reforms is also consistent with recent findings in the literature about a more nuanced relationship between finance and growth. Arcand, Berkes, and Panizza (2012) suggest that financial depth starts having a negative effect on output growth when credit to the private sector reaches 100% of GDP. Cecchetti, Mohanty, and Zampolli (2011) and Cecchetti and Kharroubi (2012) confirm the existence of debt thresholds for the government, the private sector and the household sector, beyond which debt can be damaging for growth, while Rousseau and Wachtel (2002) conclude that there is an inflation threshold for the finance-growth relationship beyond which finance is no longer supporting growth.

The overall response pattern is also affected by the severity of the crisis. Whereas non-systemic banking crises do not exert significant influence on the overall financial reforms, systemic banking crises reverse reforms, although with a certain lag of about two years. Given the complexity of changing financial regulations, and the likelihood of a strong lobbying process affecting the financial regulatory process, it is well within expectations that financial reforms will be delayed after systemic banking crises. An example of an overall lag is the adoption of the Dodd-Frank act, which was passed about two years after the collapse of Lehman Brothers in 2008 and introduced a swathe of new financial regulations in the entire U.S. financial industry.[7]

Similar to the overall reform patterns, credit controls are one of the areas of financial regulation in which an inverted U-shape of reform dynamics is observed. This is evident in column (2) of Table 1.1. Higher government

[7]See Krainer (2012) for a broad review of the Dodd-Frank Act.

intervention in the allocation of credit, indicated by higher required reserves and more directed credit to given industries, is also evident after systemic banking crises. However, both interest rate controls and entry barriers in the financial industry seem unaffected by either systemic or non-systemic banking crises, by recessions, by reform dynamics or by a *status quo* bias. This is evident in columns (3) and (4) of Table 1.1. There, most of the parameter estimates are insignificant. The one notable exception is the significantly higher government intervention related to setting the market interest rates after an exchange rate appreciation. Intuitively, if exchange rate appreciation constrains local production by making it more expensive internationally, then intervening in the credit market by lowering deposit or lending rates would help restore competitiveness. It should be noted, though, that the effect is significant only at the 10% level, and disappears in the difference GMM estimations presented in Table 1.3.

The results in column (4) of Table 1.3 also demonstrate that systemic banking crises lead to tightening of the entry regulations in the banking industry. However, the more stringent entry policies are implemented with a sizable time lag, and the effect is significant only at the 10% level. On the one hand, this reform is rational. Limiting the number of participants in the sector, especially in combination with improved supervision on the incumbent banks, which is also evident in column (5), may impose higher costs on future risk taking, thereby reducing the probability of future crises, as implied by Thakor (2012). On the other hand, abundant theory and evidence suggests that limiting entry into the banking sector is also associated with higher loan interest rates and lower deposit rates, which hampers investment.[8]

[8]For a theoretical argument, see Besanko and Thakor (1992). Evidence for both deposit and loan interest rates is available for Turkey (Denizer, 1997), Portugal (de Pinho, 2000), Philippines (Unite & Sullivan, 2003), China (Fu & Heffernan, 2009), Kyrgyzstan (Brown,

It may be the case that banking supervision was improved in both Europe and the US after the latest financial crisis. The results in column (5) of both Table 1.1 and Table 1.2 reveal, however, that banking crises between the early 1970s and early 2000s did little to improve banking supervision. When the full set of available instruments for a financial crisis and for regulatory dynamics is taken into account in Table 1.3, systemic banking crises clearly bring more government, and other independent regulatory intervention in the financial sector supervision. Still, the effect is significant only at the 10% level. This extended role of the regulators may include but is not limited to the adoption of Basel capital requirement rules, establishing a financial regulatory body which is independent of the incumbent government or chief executive, and a more comprehensive supervisory coverage, including a more pronounced role of macroprudential supervision, which is increasingly necessary in the aftermath of the Great Recession.

After systemic crises, governments intervene in the financial sector through another tool: ownership. Column (6) in all three tables demonstrates that the state increases its ownership in the banking sector immediately after or even during the crisis itself. This is hardly surprising given the ubiquitous bail-outs during a systemic crisis. Demirgüç-Kunt and Servén (2010, p.98) describe this trend as a "very common [way] of dealing with systemic banking crises" and discuss some pros and (more extensively) cons of increased government ownership in the banking sector. Unlike systemic banking crises, however, the results here suggest that non-systemic crises rarely induce governments to step in to prevent bank failure. In a more normative context, it should be also noted that if indeed the debt and inflation thresholds found

Maurer, Pak, & Tynaev, 2009), the European Union (Corvoisier & Gropp, 2002), and for a wide cross-section of countries (Demirgüç-Kunt, Laeven, & Levine, 2004).

in the literature[9] are working, government intervention should be used with more caution, especially at high levels of debt. This is so because despite the short-term gains from preventing bank failures, the government could actually help break the positive financial development-growth link if goes significantly above the critical thresholds after crises.

Governments also introduce more restrictions on capital inflows and outflows after systemic banking crises. The significant estimates in column (7) of all three tables on SBC_{t-s} show that systemic crises induce governments to impose stronger capital restrictions. Those might involve introducing a special exchange rate regime, e.g. a currency board, limiting the amount of claims that foreign banks can have on local ones, or enacting restrictions on capital outflows. The results in Table 1.3 also suggest that governments impose capital flows restrictions with a significant time lag.

This lag implies that capital controls may be adopted for all the wrong reasons: rather than containing a looming exchange rate crisis and limiting the risk of a subsequent banking crisis, capital controls are sometimes imposed long after the peak of the crisis. This implementation lag may limit the effectiveness of the policy and may also limit capital inflows in the aftermath of a crisis when they are needed most. Demirgüç-Kunt and Servén (2010) provide an excellent review of the drawbacks of using extensive capital account restrictions to deter a crisis.

The last dimension of financial reforms that can be analyzed with the data from Abiad et al. (2010) is the securities markets policies, regulations and governing institutions. Those policies and regulations demonstrate the willingness of the incumbent government to actively support the development

[9]See Cecchetti et al. (2011), Cecchetti and Kharroubi (2012) and Rousseau and Wachtel (2002).

of securities markets within a given country. An example of such government support could be establishing a bonds market with various maturities on it, setting up a securities and exchange supervisory body, enacting bonds, stocks and derivatives trade laws, and allowing foreign entry into the securities markets. The results in column (8) demonstrate that, as with most financial policies, securities markets policies experience reform reversals after systemic crises. Those reversals may stall the development of a securities market or introduce more limitations on foreign participation in the stock market.

It has been shown that systemic banking crises significantly influence financial regulations, and do so more strongly than non-systemic crises. In addition, this study has found an inherent financial regulatory dynamic adjustment process, in which the degree of current reforms is affected by how much was reformed in the immediate past, with the majority of the reforms exhibiting an inverted U-shape. This regulatory dynamic process implies that countries are gradually moving towards two plausible regulatory equilibria: a fully liberalized financial system or a fully repressed financial system, with neither system consisting of zero or of an infinite burden of financial regulations. Naturally, other factors also play a significant role in establishing the new financial regulatory realm after banking crises. Their impact can be seen in all tables.

One of the additional factors affecting financial reforms after banking crises is the business cycle. When the economy is in a recession, governments respond to it is by implementing financial liberalization reforms. This overall pattern is indicated in Column (1) of Table 1.3, and is intuitive if governments are assumed to be rationally targeting financial development and growth. At a deeper level, three particular reform areas are affected most by a recession. They include liberalization of credit controls, improving banking supervision

and reducing the ownership control over the banking sector.

First, a rational government would reduce credit controls in a recession by limiting the direct allocation of resources to favored sectors, and the monetary authorities would reduce the required reserves in the banking system to support credit activity. Second, improving banking supervision after recessions also makes sense – it could limit the riskier banking activities that likely caused the recession in the first place. Third, governments often reduce their ownership in the banking sector after a recession which may happen for two reasons. On the one hand, a recession makes losses in the banking sector more likely. If the government anticipates the losses, then it is rational to reduce its ownership in the sector for sure cash now instead of waiting for lackluster dividend prospects to materialize. On the other hand, privatizing some part of the banking system can spark competition in the sector, which can drive down interest rates and catalyze private activity. In addition, more competition in the banking industry is found to enhance stability (Schaeck, Cihak, & Wolfe, 2009).

Stimulating private activity through more competition in the financial industry, however, has a downside. Allen and Gale (2004) argue that competition in the financial sector is sometimes at odds with financial stability – an argument which perhaps goes as far back as Keeley (1990). At the same time, Beck (2008) presents considerable differences across studies examining the competition-stability link, and concludes that their results are ambiguous. In addition, he also finds that in cases where loose competition has increased fragility, this has been mostly the consequence of regulatory and supervisory failures, a finding supported by Tarr (2010) in the case of the latest US financial crisis.

Apart from the GDP dynamics, regional competition for capital inflows

and policy learning also play a role in shaping financial reforms. These competition and policy learning effects, which Abiad and Mody (2005) introduced into the financial reforms literature, is evident from three variables: the reform gap, and the interaction of the gap with the GDP and with the exchange rate. The higher the gap between the regional reform leaders and a given country, the more the country is lagging behind the regional leaders in financial reforms. Therefore, closing the gap also positions the country more favorably for attracting foreign investment. Based on the evidence in Abiad and Mody (2005, p.80), one would expect the gap to be significant in shaping overall reform patterns, as well as many specific financial reforms. However, interestingly, the results in Table 1.3 demonstrate that reducing the reform gap does not play a significant role in shaping overall reform patterns.[10] Zooming in on particular reforms, the reform gap affects only two of them: credit controls, and securities market policies and regulations. In those two reforms, reducing the gap increases the likelihood of pursuing further financial liberalization. This learning effect is significant at the 5% level for credit controls, and at 1% level for the securities policies.

In the reform of credit controls and in the reform of securities markets, the gap also plays a different role at various stages of the business cycle. Countries closer to the regional reform leaders in terms of financial liberalization tend to shed their credit controls more in recessions, and pursue more favorable policies to develop the securities markets than countries lagging behind with liberalization. This is indicated by the positive and signif-

[10]In fact, in one of the robustness checks on the overall reform pattern, the reform gap becomes significant at the 5% level when the political factors are taken into account. However, the gap has the unexpected positive sign which demonstrates that, rather than building up a reform momentum, closing in on the regional leaders reduces the overall reform drive.

icant parameter estimates on the interaction term between the reform gap and the GDP dynamics. The positive estimates imply that governments do learn to pursue growth-enhancing policies in recessions, particularly related to developing their financial markets and to enhancing competition in the real sector by reducing direct allocation of resources to favored industries. In times of economic growth, however, rather than pursuing further liberalization, governments closer to the reform frontier seem to extend favors for some industries more than the lagging countries do. This is perhaps only natural, since governments are also expected to have higher revenues in the good times. Hence, they have a stronger ability to allocate resources to particular industries during the growth phase.

In addition, an exchange rate appreciation would encourage countries closer to the regional reform leaders to pursue a more extensive capital account liberalization than the backward countries. This is seen from the positive and significant sign on $Gap^{*}XR_{t-1}$, where reducing the gap is seen as closing in on the regional reform leaders, whereas reducing the XR represents an exchange rate appreciation. To interpret this finding, we need to consider a dynamic trade-off between long-term benefits and short-term costs for the local economy. On the one hand, an exchange rate appreciation lowers the international competitiveness of the domestic firms in the short run and creates an incentive for the central bank to sterilize the appreciation, or for the government to impose capital inflow restrictions. On the other hand, attracting new foreign capital and encouraging greenfield investment could boost potential GDP more than slowing an exchange rate appreciation.

Further research would determine whether that is indeed the proper trade-off to consider in the context of capital control liberalization. Further research is also needed to find out why the reform gap is insignificant for other financial

reforms, why systemic banking crises affect different financial reforms with a different lag, and why interest rate controls do not depend on systemic crises, although it is quite intuitive to expect a more pronounced government intervention in directing pricing in the deposits and lending markets.[11]

1.5 Robustness Checks

1.5.1 Robustness of the GMM Results

Although the GMM estimations in this work deliver new insights into the policy-making process after banking crises, they also require implicit assumptions when employing any given version of the GMM method.[12] Therefore, it would be useful to know if the GMM estimation results remain robust when some of the main traits of the model here are altered. The baseline specification included a one-step robust difference GMM with a full set of instruments in which the crisis exposures are treated as strictly exogenous. The robustness checks are done along the following lines: 1) a *two-step* robust difference GMM with full set of instruments; 2) a two-step robust *system* GMM with full set of instruments; 3) a one-step robust difference GMM with a *collapsed* set of instruments; and 4) a one-step robust difference GMM with a collapsed set of instruments in which the crisis exposures are treated as possibly *endogenous* rather than strictly exogenous.

The first robustness check is driven by the expected increase in efficiency that a two-step estimation creates, at least in theory. If indeed the two-step

[11] In fact, when the political factors are taken into account in one of the robustness checks, systemic banking crises become significant at the 10% level, while non-systemic crises retain their insignificance.

[12] Roodman (2009) is a useful source on both the strengths and the pitfalls of the GMM method.

estimation is more efficient, then the significance of the baseline results here is not artificially inflated. Alternatively, if the two-step GMM estimations are less significant than the one-step estimations, then the reason is perhaps the existence of a small sample bias of the two-step GMM discussed in Cameron and Trivedi (2005, p.177). The second robustness check is needed to see if there is an additional gain from using the system GMM rather than the original Arellano-Bond type regression. The third robustness check is needed because both the difference and the system GMM create many instruments and could deliver Sargan/Hansen P-values that are suspiciously high. Thus, limiting the number of instruments may also increase the information value of the validity tests. Finally, endogenizing the crisis exposures is intuitive. If a banking crisis in a given economy influences the risk of a crisis in another economy, then that risk would feed back into the first economy, especially if there is a large trade and financial exposure between the two. If that is indeed the case, then the crisis exposures can no longer be treated as strictly exogenous. Therefore, I endogenize them in the last robustness check on the GMM method.

The results from the first and second robustness checks yield lower significance of the parameter estimates. This refers back to the possible small sample bias of the two-step GMM. A sample of approximately 1600 observations is not particularly small per se. However, the number of clusters is only 76, which cannot be considered a large number. Therefore, a small sample bias may well be among the reasons for the lower significance of crises for financial reforms.

The third robustness check confirms the magnitude and the significance of the baseline results. In this robustness check, the number of instruments is collapsed to about 300 from about 1400, with minor variations in the

number of instruments across models. Collapsing the number of instruments is expected to weaken the robust Hansen overidentification test. However, the Hansen p-value remains unchanged in all cases, which suggests that the instruments remain valid. Further, the still implausibly high Hansen p-value calls for a further reduction of the number of instruments by removing some of the lags. I continue to collapse the number of instruments but further limit the number of lags to 4 to accommodate most electoral cycles. This leads to a Hansen p-value of 0.387 for the overall reform model, and to similar p-values for the other reforms, with the number of instruments down to 61, which is less than the number of clusters. At the same time, the magnitude and the significance of the results remain almost exactly the same. Thus, the main results remain robust to the drastic reduction of the number of instruments, while the Hansen J-test acquires plausible values and increases the credibility of the results.

The final robustness check is to endogenize the crisis exposure variables. This corroborates the baseline results. Specifically, the reform dynamics play an identical role as before, and banking crises exert a very similar influence on the reform process, with some of the reforms affected more by the crises than the main results suggest. This final robustness check of the GMM method supports the conclusion that the main results are rather conservative and that banking crises may exert an even stronger role on various financial reforms than previously thought.

For completeness, in one of the robustness checks I also include the political orientation of the incumbent government and of the chief executive, political system dummies, and whether the government holds a majority in both chambers of parliament and if it is in office during the first year of its mandate. The results remained almost identical, and some of the main

variables gained significance, while most of the political variables were found insignificant, consistent with the previous results in the literature.

1.5.2 Dating of Crises

Even a superficial look at the Caprio and Klingebiel (2003) data would suffice to understand that the data needs a considerable judgement on the end dates of the crises. Initially, the data by Reinhart and Rogoff (2008) seems sufficient to resolve some of the dating ambiguities. However, Babecký et al. (2012, p.11) point out a considerable remaining disagreement over the duration and the end dates of many systemic and non-systemic banking crises. Therefore, I do a robustness check on that front. I replace the crisis incidences from the Caprio and Klingebiel (2003) and the Reinhart and Rogoff (2008) data with the latest data by Laeven and Valencia (2013). The robustness checks are done by performing identical estimations to the ones in Table 1.1, Table 1.2, and Table 1.3, and are presented in Table 1.8, Table 1.9, and Table 1.10.

The results come even stronger with the Laeven and Valencia (2013) data. In the Panel OLS estimations, 14 coefficients gained significance, while only 4 lost significance. Similarly, in the GMM estimations, 14 coefficients gained significance, while 5 lost it. The evidence in the 2SLS estimations is less strong, with 4 coefficients gaining significance, and 10 others losing it. Despite the considerable disagreement over the dating of the crises, the evidence here points to a more pronounced influence of both systemic and non-systemic crises on financial reforms than the main results demonstrate. That is why I consider the core results a rather conservative estimate of the effects of banking crises on financial reforms.

1.5.3 Recession Exposures

One of the possible drawbacks of the crisis exposure instruments is that they depend crucially on banking crisis occurrences in trading partners. Although crises in trading partners are arguably successful in predicting a crisis in the home country, they are hardly the only driver of those crises. An additional factor, which is perhaps not less important, is a recession in a trading partner.

A recession in a trading partner brings a negative export demand shock to the home country. In turn, this may deliver negative shocks on both the assets and the liabilities side of the home country's banking system. Either way, after a recession in a trading partner the likelihood of a crisis in the home country increases.

At the same time, a recession in a trading partner is not necessarily related to the home country's financial reform pattern. Therefore, it is related to the occurrence of a crisis in the home country but is not directly associated with its financial reforms. This logic leads me to construct an additional instrument for the incidence of crises: the recession exposure.

For each country, the recession exposures are constructed identically to the crisis exposures. The difference between them is that the recession dummies in a trading partner substitute for the banking crisis dummies. Therefore, a recession exposure of country i in time t is i's export-weighted sum of recession occurrences in all trading partners j in time t. The recessions data is taken from the Penn World Table 7.1., and the annual data on bilateral trade for 1970–2000 is taken from Feenstra et al. (2005).

The results from including the recession exposures at the first stage of the 2SLS and difference GMM estimations are presented in Table 1.11 and Table 1.12. The 2SLS estimates experience minor changes relative to the baseline results. Specifically, 5 parameters gain significance, while 6 of them lose it. At

the same time, signs are the same, while magnitudes remain approximately the same. The GMM estimates are virtually unchanged. No estimates gain or lose any significance and the magnitudes are almost identical to the baseline results. Therefore, it can be concluded that the recession exposures do not change significantly the way crises affect financial reforms. The rest of the conclusions are presented below.

1.6 Conclusion

This paper uses the rich history of systemic and non-systemic banking crises since the early 1970s to identify the patterns of overall financial reforms, as well as reforms in seven broad areas: credit controls, interest rate controls, entry barriers, banking supervision, state ownership in the banking sector, capital controls and securities markets policies. To arrive at arguably efficient and consistent estimates, I use fixed effects panel OLS, as well as 2SLS and GMM estimations. By constructing a crisis exposure for each country and year, this work adopts a more realistic propagation mechanism of crises across countries than previous literature on financial reforms. The crisis exposure is at the heart of identifying the causal effect of banking crises on financial reforms. Thus, this work analyzes financial reforms in a dynamic empirical framework with endogenous financial crises, which is its core methodological contribution to the literature.

The results demonstrate that systemic banking crises reverse the overall pattern of financial reforms. They also reverse most of the other particular financial reforms, although with a varying reaction lag. For example, governments allocate favors and impose more entry barriers in the financial industry, which may reduce competition and ultimately impede growth of

the incumbent firms. Further, systemic crises induce more state ownership in the banking sector. This is also intuitive given the importance of not letting systemically important financial institutions fail. However, in the more recent environment of aversion to fiscal expansion, whether there are other potentially more efficient mechanisms to save or dismantle those institutions could be investigated.

Systemic crises also lead to more capital inflow and outflow controls. This may be an efficient way to stem a looming crisis, but the evidence in this work points to the fact that more often than not governments implement capital account restrictions as a reaction to a crisis, rather than as a means to prevent it. In addition, systemic crises slow down the creation and development of securities markets. Finally, systemic banking crises end up in improved banking supervision, which is perhaps a natural policy reaction to a crisis occurring in the banking sector and need not be considered a policy reversal.

Non-systemic banking crises, however, exert a much weaker influence on financial policies and regulations. Whenever some evidence of a policy reaction emerges, it is only marginally significant.

Whereas some financial crises reverse reforms, recessions tend to induce financial liberalizations. After recessions, governments reduce their direct allocation of resources to particular industries, and reduce their ownership in the banking sector. Further, if a recession occurs, the countries closer to the regional reform leaders create a growth-enhancing financial regulatory framework faster. This is especially valid for credit controls and for securities markets policies and reforms. A recession, however, makes banking supervision less independent from the incumbent government.

Exchange rate movements rarely play a significant role in shaping most financial reforms, except for capital controls. Further, in times of exchange

rate appreciation, the countries which are closer to the regional reform leaders reduce their capital controls relatively more than the backward countries.

Finally, the results here suggest financial reforms tend to move to one of two states: a fully liberalized financial system or a fully repressed financial system. This is indicated by the inverted U-shape of the reform dynamics, and remains robust to various tweaks in the GMM method. The results remain robust to changing the source of crises data as well.

Naturally, this study has its limitations. Particularly, considering only seven areas of financial regulation and supervision in a myriad of proposed policy measures within each country is a low level of specificity. The paper also cannot say if financial reforms are moving towards a given regulatory optimum after crises. Perhaps, as the results in the other two essays suggest, the optimal reforms would be different across countries and would ultimately be determined by the within-country political economy. Until better panel data sets are available to measure reforms and crisis exposures – especially longitudinal bilateral financial flows data – this is as far as this research can go. Therefore, this work identifies the regulatory policy patterns after crises rather than entering the debate on the optimal regulatory measures to deal with the consequences of a banking crisis. This debate is bound to liven up in the aftermath of the Great Recession.

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Table 1.1: Crises and Financial Reforms: Panel OLS Estimations

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	.115	-.006	-.083	-.053	-.133***	-.047	.027	-.095
	(.227)	(.067)	(.114)	(.077)	(.042)	(.063)	(.100)	(.058)
$Reform^2_{t-1}$	-.248***	-.142***	-.032	-.073	-.027	-.085	-.144*	-.016
	(.072)	(.053)	(.099)	(.055)	(.039)	(.052)	(.079)	(.044)
SBC_t	-.005	.013	-.014	-.007	-.004	-.033**	.027	-.015
	(.006)	(.012)	(.016)	(.015)	(.011)	(.013)	(.020)	(.010)
SBC_{t-1}	-.006	.010	-.008	.009	-.010	-.035**	.006	-.006
	(.006)	(.014)	(.019)	(.015)	(.011)	(.017)	(.016)	(.010)
SBC_{t-2}	-.022***	-.036*	-.033	-.019	.016	-.011	-.050***	-.011
	(.006)	(.018)	(.022)	(.018)	(.011)	(.015)	(.019)	(.014)
NBC_t	-.002	.001	-.022	.024*	-.003	-.020	.005	.008
	(.005)	(.017)	(.020)	(.012)	(.012)	(.013)	(.012)	(.012)
NBC_{t-1}	-.010	-.007	-.018	.011	.016	-.054**	-.017	-.001
	(.008)	(.013)	(.021)	(.021)	(.025)	(.021)	(.012)	(.008)
NBC_{t-2}	-.005	-.002	-.000	.007	-.000	-.016	-.021	.002
	(.008)	(.017)	(.015)	(.010)	(.016)	(.015)	(.029)	(.019)
$GDP/c._{t-1}$	-.065	-.103	-.100	-.032	-.096	-.098	-.053	-.057
	(.041)	(.072)	(.138)	(.084)	(.061)	(.078)	(.086)	(.071)
XR_{t-1}	.002	-.006	.038*	.001	-.003	-.019*	.003	-.004
	(.006)	(.010)	(.023)	(.009)	(.004)	(.011)	(.016)	(.011)
$Openness_{t-1}$	.000	-.000	.001	.001	.000	.000	.000	-.001
	(.000)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Gap_{t-1}	.000	.451	-1.108	-.620	.187	-.713	-.303	-.196
	(.)	(.600)	(.670)	(.674)	(.393)	(.474)	(.650)	(.360)
Gap^*GDP_{t-1}	-.015	-.076	.120	.038	-.022	.057	.019	.000
	(.028)	(.070)	(.073)	(.078)	(.047)	(.051)	(.074)	(.042)
Gap^*XR_{t-1}	-.004	.005	-.025	-.001	.001	.003	.009	.004
	(.007)	(.006)	(.021)	(.007)	(.003)	(.007)	(.008)	(.007)
Const.	.002	.032	.075	.014	.003	-.017	.059	.010
	(.015)	(.021)	(.069)	(.026)	(.010)	(.032)	(.039)	(.013)
N	1589	1589	1589	1589	1589	1589	1589	1589
adj. R^2	.116	.049	.052	.040	.059	.061	.048	.052
No. countries	76	76	76	76	76	76	76	76

Notes: The table presents estimates from equation (1.1) by fixed-effects OLS, as explained in the text. The time period covered is 1973–2000. Standard errors are clustered by country, and are presented in parentheses. All estimations include country and region-time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises. The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.2: Crises and Financial Reforms: 2SLS Panel Estimations

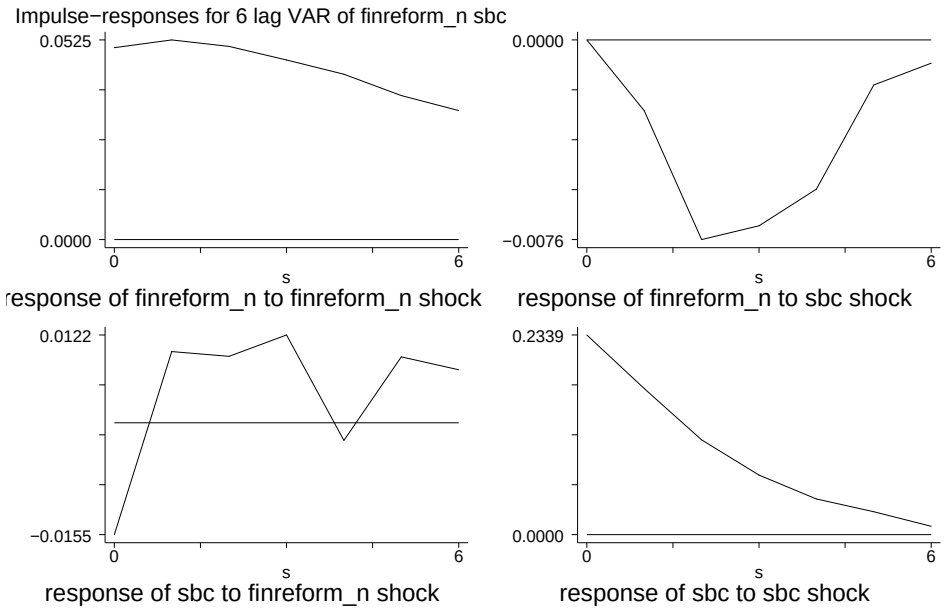
	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	.264***	.001	-.040	-.010	-.143***	-.035	.016	-.070
	(.074)	(.062)	(.096)	(.060)	(.047)	(.065)	(.079)	(.056)
$Reform^2_{t-1}$	-.218***	-.145***	-.048	-.095**	-.004	-.084	-.136*	-.029
	(.072)	(.053)	(.080)	(.046)	(.058)	(.060)	(.072)	(.042)
SBC_t	-.041**	-.019	-.035	-.024	.014	-.074*	-.078*	-.061**
	(.016)	(.039)	(.040)	(.035)	(.029)	(.043)	(.046)	(.028)
SBC_{t-1}	-.016	.033	-.080	.042	.014	-.041	-.044	-.025
	(.018)	(.054)	(.052)	(.034)	(.033)	(.048)	(.046)	(.025)
SBC_{t-2}	-.014	.071*	-.069*	.015	.001	-.040	-.028	-.044
	(.014)	(.039)	(.037)	(.035)	(.035)	(.033)	(.045)	(.032)
NBC_t	.027	.140**	.159**	.013	-.064	-.006	.013	-.063
	(.025)	(.067)	(.078)	(.062)	(.062)	(.057)	(.080)	(.050)
NBC_{t-1}	.051**	.121**	.137	-.031	.043	.012	.106	.018
	(.026)	(.060)	(.085)	(.070)	(.056)	(.073)	(.067)	(.046)
NBC_{t-2}	.008	.028	.027	.069	.101*	-.068	.039	-.035
	(.025)	(.064)	(.077)	(.053)	(.054)	(.067)	(.076)	(.050)
$GDP/c._{t-1}$	-.061*	.004	-.143	.101	-.060	-.087	.002	-.072
	(.035)	(.094)	(.119)	(.079)	(.058)	(.080)	(.094)	(.068)
XR_{t-1}	.007	-.001	.058***	.001	-.009*	-.008	.009	.003
	(.005)	(.009)	(.022)	(.008)	(.005)	(.011)	(.012)	(.008)
$Openness_{t-1}$	.000	-.000	.001	.001**	.000	.000	.000	-.001
	(.000)	(.001)	(.001)	(.001)	(.000)	(.001)	(.001)	(.001)
Gap_{t-1}	.052	.430	-.496	-.236	.084	-.329	.247	.115
	(.200)	(.522)	(.553)	(.460)	(.301)	(.411)	(.427)	(.308)
$Gap * GDP_{t-1}$	-.006	-.078	.056	-.002	-.011	.019	-.048	-.033
	(.025)	(.062)	(.064)	(.056)	(.037)	(.048)	(.051)	(.037)
$Gap * XR_{t-1}$	-.001	.005	-.022	-.002	.001	.002	.013*	.005
	(.005)	(.008)	(.022)	(.006)	(.003)	(.005)	(.007)	(.006)
N	1589	1589	1589	1589	1589	1589	1589	1589
No. countries	76	76	76	76	76	76	76	76
Hansen P	.010	.915	.900	.958	.845	.983	.304	.710

Notes: The table presents estimates of equation (1.1) by panel 2SLS. The time period covered is 1973–2000. Standard errors are robust and are presented in parentheses. All estimations include country and region-time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises. The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

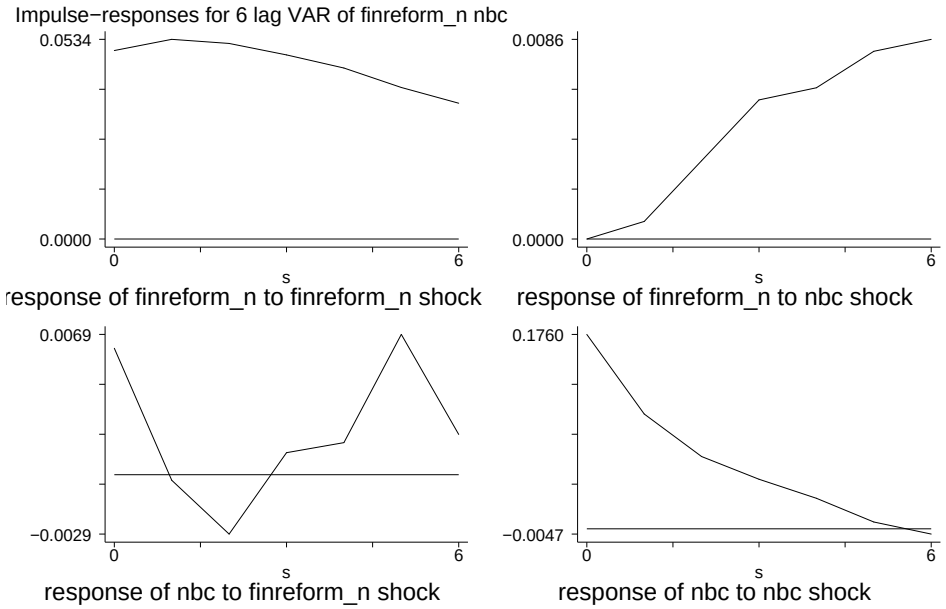
Table 1.3: Crises and Financial Reforms: Difference GMM

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	.973***	.943***	1.013***	.886***	.769***	.887***	.787***	.756***
	(.035)	(.058)	(.082)	(.049)	(.038)	(.045)	(.073)	(.040)
$Reform^2_{t-1}$	-.180***	-.153***	-.240***	-.108***	-.001	-.084*	-.059	.013
	(.033)	(.050)	(.078)	(.036)	(.032)	(.044)	(.062)	(.033)
SBC_t	-.013**	.001	-.011	-.009	-.003	-.041**	.003	-.020**
	(.006)	(.012)	(.018)	(.015)	(.010)	(.016)	(.018)	(.009)
SBC_{t-1}	-.003	.002	.001	.018	-.014	-.012	-.017	.009
	(.008)	(.019)	(.020)	(.019)	(.014)	(.019)	(.019)	(.011)
SBC_{t-2}	-.009	-.030**	-.006	-.025*	.019*	.019	-.029**	-.006
	(.006)	(.013)	(.019)	(.013)	(.010)	(.014)	(.013)	(.012)
NBC_t	.002	.011	-.009	.023*	.006	-.013	.018	.004
	(.005)	(.016)	(.020)	(.013)	(.013)	(.013)	(.013)	(.012)
NBC_{t-1}	-.004	.001	.003	-.009	.027	-.033	-.006	-.002
	(.008)	(.014)	(.026)	(.017)	(.025)	(.021)	(.013)	(.008)
NBC_{t-2}	.010*	.013	.001	-.007	.022	.033	.016	.005
	(.006)	(.014)	(.020)	(.017)	(.017)	(.021)	(.017)	(.011)
Gap_{t-1}	.100	-.435**	-.347	.053	.122	-.100	.029	-.564***
	(.103)	(.206)	(.311)	(.178)	(.157)	(.174)	(.216)	(.163)
Gap^*GDP_{t-1}	-.016	.040*	.042	-.016	-.021	-.002	-.012	.059***
	(.012)	(.023)	(.035)	(.020)	(.019)	(.021)	(.026)	(.019)
Gap^*XR_{t-1}	-.001	.002	-.009	.001	.000	.004	.011***	.004
	(.003)	(.004)	(.013)	(.004)	(.002)	(.003)	(.003)	(.004)
$GDP/c._{t-1}$	-.027**	-.105***	-.043	-.032	.060**	-.140***	.040	-.015
	(.012)	(.030)	(.057)	(.030)	(.028)	(.037)	(.045)	(.027)
XR_{t-1}	.001	-.000	.006	.001	.001	.000	.001	.004
	(.001)	(.002)	(.004)	(.002)	(.001)	(.002)	(.002)	(.002)
$Openness_{t-1}$	.000	-.001**	.000	-.000**	.000	.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
N	1589	1589	1589	1589	1589	1589	1589	1589
No. countries	76	76	76	76	76	76	76	76
No. instr.	1409	1408	1402	1404	1372	1391	1406	1408
Hansen P	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Sargan P	.000	.004	.001	.000	.000	.000	.000	.000

Notes: The table presents estimates from equation (1.1) by Arellano-Bond (1991) one-step difference GMM with full set of instruments. The time period covered is 1973–2000. Robust standard errors are presented in parentheses. All estimations include time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises. The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$



(a) Impulse-Responses of the (Overall Reform; SBC) Pair



(b) Impulse-Responses of the (Overall Reform; NBC) Pair

Figure 1.1: Impulse-Response Functions of Financial Reforms to Crisis Shocks

1.8 Online Appendix

Table 1.4: Crises and Financial Reforms: Two-Step Robust Difference GMM

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	.513	.678	.391	.737*	.248	.949***	.471*	.658
	(.328)	(.522)	(.489)	(.400)	(.356)	(.159)	(.282)	(.445)
$Reform^2_{t-1}$	.092	-.154	.160	-.111	.249	-.086	.088	-.145
	(.265)	(.450)	(.440)	(.247)	(.329)	(.188)	(.396)	(.402)
SBC_t	-.004	.037	.090	-.071	-.062	-.020	-.027	-.018
	(.028)	(.058)	(.131)	(.050)	(.054)	(.052)	(.066)	(.037)
SBC_{t-1}	-.011	-.064	.007	-.014	-.068	.020	.020	-.037
	(.029)	(.068)	(.089)	(.062)	(.057)	(.055)	(.083)	(.049)
SBC_{t-2}	-.036	-.049	-.014	-.147*	.052	.051	-.038	.024
	(.028)	(.092)	(.113)	(.081)	(.055)	(.061)	(.085)	(.066)
NBC_t	-.008	.036	.051	.165	.019	-.060	.031	.081
	(.026)	(.059)	(.143)	(.115)	(.057)	(.051)	(.077)	(.074)
NBC_{t-1}	-.009	.179*	.040	.264**	.059	-.098	-.103	-.048
	(.053)	(.098)	(.160)	(.109)	(.089)	(.077)	(.125)	(.109)
NBC_{t-2}	-.062	-.073	.140	-.080*	-.075	.033	-.013	-.100
	(.049)	(.126)	(.131)	(.048)	(.090)	(.063)	(.097)	(.068)
Gap_{t-1}	-1.665*	.461	-4.286**	.930	-1.661	-.754	-1.236	-.432
	(.957)	(1.794)	(1.807)	(1.930)	(1.957)	(1.970)	(2.018)	(2.013)
Gap^*GDP_{t-1}	.218*	-.065	.586***	-.080	.211	.066	.164	.022
	(.118)	(.213)	(.215)	(.230)	(.245)	(.241)	(.251)	(.244)
Gap^*XR_{t-1}	.009	.003	.015	.098	-.051	.062	.091	-.001
	(.028)	(.013)	(.054)	(.072)	(.081)	(.040)	(.074)	(.029)
$GDP/c._{t-1}$	-.196*	.028	-.269	-.598*	.149	-.099	.270	-.145
	(.114)	(.331)	(.329)	(.310)	(.293)	(.248)	(.407)	(.319)
XR_{t-1}	.000	-.014	.009	-.065	.046	-.003	-.031	.026
	(.018)	(.024)	(.047)	(.056)	(.051)	(.021)	(.054)	(.035)
$Openness_{t-1}$	.000	-.005	.001	.001	.000	.002	-.000	.002
	(.001)	(.003)	(.003)	(.002)	(.001)	(.002)	(.002)	(.002)
N	1589	1589	1589	1589	1589	1589	1589	1589
No. countries	76	76	76	76	76	76	76	76
No. instr.	1409	1408	1402	1404	1372	1391	1406	1408
Hansen P	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Sargan P	.000	.004	.001	.000	.000	.000	.000	.000

The table presents estimates from equation (1.1) by a two-step robust difference GMM with full set of instruments. The time period covered is 1973–2000. Robust standard errors are presented in parentheses. All estimations include time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises. The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.5: Crises and Financial Reforms: Two-Step Robust System GMM

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	1.156***	1.382*	.287	.985**	.765**	.843**	.351	.542
	(.422)	(.761)	(.559)	(.412)	(.322)	(.342)	(.350)	(.390)
$Reform^2_{t-1}$	-.586*	-.538	.249	-.172	-.012	.029	.143	-.036
	(.339)	(.619)	(.461)	(.287)	(.361)	(.422)	(.311)	(.319)
SBC_t	-.010	.017	-.097	-.004	.005	-.025	.015	-.006
	(.029)	(.060)	(.099)	(.060)	(.060)	(.050)	(.066)	(.042)
SBC_{t-1}	.010	.025	.073	.046	-.023	-.018	.053	-.044
	(.023)	(.087)	(.086)	(.060)	(.066)	(.064)	(.117)	(.055)
SBC_{t-2}	.003	-.052	.054	-.091	.047	-.018	-.050	.018
	(.029)	(.072)	(.122)	(.081)	(.069)	(.061)	(.085)	(.054)
NBC_t	-.006	.066	.044	.050	.044	-.032	-.011	.039
	(.036)	(.050)	(.095)	(.149)	(.080)	(.055)	(.126)	(.081)
NBC_{t-1}	.015	.186*	-.010	.069	.023	.009	-.043	.044
	(.044)	(.097)	(.175)	(.158)	(.163)	(.073)	(.174)	(.119)
NBC_{t-2}	.007	.048	.126	.067	-.027	-.025	-.057	-.100
	(.034)	(.073)	(.110)	(.097)	(.087)	(.048)	(.106)	(.065)
Gap_{t-1}	.694	.696	-1.326	-.332	-.399	.485	-2.031	-.735
	(1.158)	(2.071)	(2.891)	(1.509)	(1.718)	(1.302)	(2.295)	(1.471)
Gap^*GDP_{t-1}	-.082	-.093	.145	.062	.049	-.041	.250	.084
	(.135)	(.248)	(.354)	(.200)	(.218)	(.165)	(.295)	(.183)
Gap^*XR_{t-1}	-.007	-.005	-.038	.028	.003	.035	.059	.029
	(.027)	(.011)	(.088)	(.056)	(.063)	(.045)	(.065)	(.055)
$GDP/c._{t-1}$	-.027	-.090	-.466	-.328	-.102	.034	-.005	-.087
	(.205)	(.294)	(.421)	(.337)	(.292)	(.083)	(.419)	(.284)
XR_{t-1}	.008	-.014	-.006	-.010	-.002	.013	-.029	-.000
	(.019)	(.019)	(.048)	(.037)	(.031)	(.026)	(.047)	(.035)
$Openness_{t-1}$	.000	-.000	.003	.002	.001	-.001	-.001	-.001
	(.002)	(.003)	(.004)	(.003)	(.002)	(.002)	(.002)	(.002)
N	1673	1673	1673	1673	1673	1673	1673	1673
No. countries	76	76	76	76	76	76	76	76
No. instr.	1493	1492	1486	1488	1456	1475	149	1492
Hansen P	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Sargan P	.000	.007	.006	.001	.000	.000	.001	.001

The table presents estimates from equation (1.1) by two-step system GMM with full set of instruments. The time period covered is 1973–2000. Robust standard errors are presented in parentheses. All estimations include time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises. The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.6: Crises and Financial Reforms: Difference GMM with a Collapsed Number of Instruments

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	1.018***	1.056***	1.257***	.951***	.892***	1.012***	.756***	.702***
	(.050)	(.115)	(.158)	(.077)	(.068)	(.111)	(.111)	(.094)
$Reform^2_{t-1}$	-.171***	-.258***	-.427***	-.152**	-.122*	-.266**	.004	.047
	(.048)	(.092)	(.148)	(.059)	(.064)	(.107)	(.108)	(.085)
SBC_t	-.015*	.007	-.037	-.001	.043*	-.071***	-.002	-.020
	(.009)	(.030)	(.024)	(.020)	(.022)	(.024)	(.027)	(.015)
SBC_{t-1}	.002	.014	.013	-.004	-.055**	.008	-.013	.006
	(.008)	(.026)	(.023)	(.019)	(.023)	(.024)	(.021)	(.015)
SBC_{t-2}	-.005	-.015	-.000	-.031**	.021*	.011	-.040**	-.011
	(.007)	(.015)	(.022)	(.014)	(.012)	(.015)	(.016)	(.013)
NBC_t	.003	.027	-.059	.024	.001	-.011	-.021	.031
	(.015)	(.055)	(.043)	(.030)	(.039)	(.026)	(.032)	(.025)
NBC_{t-1}	-.005	-.025	.050	-.012	.033	-.031	.013	-.024
	(.013)	(.035)	(.043)	(.022)	(.035)	(.029)	(.025)	(.017)
NBC_{t-2}	.012*	.006	.020	-.013	.024	.037*	.007	-.003
	(.006)	(.014)	(.024)	(.016)	(.018)	(.019)	(.018)	(.011)
Gap_{t-1}	.075	-.971**	.408	.631	.553	.345	-.446	-.651*
	(.160)	(.415)	(.520)	(.400)	(.340)	(.393)	(.524)	(.387)
Gap^*GDP_{t-1}	-.008	.104**	-.041	-.081*	-.059	-.046	.031	.061
	(.019)	(.047)	(.057)	(.044)	(.039)	(.045)	(.058)	(.044)
Gap^*XR_{t-1}	-.001	-.003	-.016	.008	.005	.005	.006	-.002
	(.005)	(.008)	(.018)	(.008)	(.005)	(.006)	(.008)	(.006)
$GDP/c._{t-1}$	.026	-.006	.172	-.052	.182**	-.026	-.018	-.020
	(.048)	(.082)	(.171)	(.101)	(.081)	(.110)	(.130)	(.108)
XR_{t-1}	.004*	.003	.015	-.004	.003	.004	.002	.005
	(.002)	(.004)	(.009)	(.005)	(.003)	(.004)	(.006)	(.005)
$Openness_{t-1}$	.001	-.001	.002	-.001	.001	-.002*	-.001	-.002*
	(.000)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
N	1589	1589	1589	1589	1589	1589	1589	1589
No. countries	76	76	76	76	76	76	76	76
No. instr.	289	289	289	289	28	289	289	289
Hansen P	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Sargan P	.000	.285	.715	.158	.000	.000	.001	.290

The table presents estimates from equation (1.1) by one-step robust difference GMM with a collapsed number of instruments. The time period covered is 1973–2000. Robust standard errors are presented in parentheses. All estimations include time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises. The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.7: Crises and Financial Reforms: Difference GMM with Endogenized Crisis Exposures

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	1.000***	1.025***	1.260***	.953***	.855***	1.004***	.770***	.696***
	(.046)	(.111)	(.155)	(.076)	(.068)	(.103)	(.104)	(.084)
$Reform^2_{t-1}$	-.174***	-.260***	-.436***	-.149**	-.123*	-.249**	-.022	.038
	(.042)	(.089)	(.144)	(.058)	(.066)	(.101)	(.098)	(.076)
SBC_t	-.018**	.006	-.030	-.002	.038*	-.069***	-.007	-.026*
	(.008)	(.023)	(.024)	(.018)	(.020)	(.022)	(.024)	(.014)
SBC_{t-1}	.002	.012	.010	.005	-.051**	.007	-.014	.012
	(.008)	(.021)	(.022)	(.018)	(.021)	(.021)	(.019)	(.014)
SBC_{t-2}	-.006	-.021	-.000	-.028*	.021*	.011	-.040**	-.010
	(.007)	(.015)	(.021)	(.014)	(.011)	(.015)	(.015)	(.013)
NBC_t	.008	.024	-.064*	.029	.019	.003	-.008	.026
	(.014)	(.052)	(.037)	(.033)	(.039)	(.028)	(.032)	(.024)
NBC_{t-1}	-.007	-.021	.054	-.017	.019	-.039	.007	-.019
	(.012)	(.034)	(.042)	(.023)	(.033)	(.029)	(.024)	(.016)
NBC_{t-2}	.012*	.006	.020	-.012	.023	.038**	.012	.001
	(.006)	(.014)	(.023)	(.016)	(.017)	(.019)	(.017)	(.011)
Gap_{t-1}	.067	-.970**	.454	.580	.711**	.192	-.353	-.650*
	(.155)	(.398)	(.526)	(.358)	(.321)	(.375)	(.482)	(.385)
Gap^*GDP_{t-1}	-.010	.098**	-.048	-.072*	-.078**	-.031	.022	.062
	(.018)	(.044)	(.057)	(.038)	(.037)	(.044)	(.054)	(.043)
Gap^*XR_{t-1}	-.001	-.002	-.019	.005	.006	.004	.009	-.002
	(.005)	(.008)	(.018)	(.006)	(.005)	(.006)	(.008)	(.005)
$GDP/c._{t-1}$	.007	-.050	.184	.012	.160**	-.059	-.008	.009
	(.039)	(.083)	(.151)	(.084)	(.080)	(.085)	(.097)	(.103)
XR_{t-1}	.003*	.001	.016**	-.001	.001	.003	.001	.006
	(.002)	(.004)	(.008)	(.004)	(.003)	(.003)	(.004)	(.005)
$Openness_{t-1}$	.000	-.001	.002	-.001	.001	-.001	-.001	-.002*
	(.000)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
N	1589	1589	1589	1589	1589	1589	1589	1589
No. countries	76	76	76	76	76	76	76	76
No. instr.	335	335	335	335	326	335	335	335
Hansen P	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Sargan P	.000	.076	.883	.054	.000	.000	.002	.150

The table presents estimates from equation (1.1) by one-step robust difference GMM with a collapsed number of instruments and endogenized crisis exposures. The time period covered is 1973–2000. Robust standard errors are presented in parentheses. All estimations include time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises. The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.8: Crises and Financial Reforms: Panel OLS Estimations with the Laeven-Valencia Data

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	.167	-.003	-.082	-.050	-.135***	-.074	.021	-.098*
	(.226)	(.068)	(.114)	(.077)	(.043)	(.065)	(.098)	(.058)
$Reform^2_{t-1}$	-.219***	-.143**	-.033	-.078	-.027	-.052	-.144*	-.013
	(.072)	(.055)	(.099)	(.056)	(.039)	(.052)	(.076)	(.043)
SBC_t	-.011*	-.012	.002	-.024**	.005	-.037***	-.003	-.007
	(.006)	(.015)	(.019)	(.011)	(.012)	(.013)	(.019)	(.012)
SBC_{t-1}	-.003	.008	.022	-.005	.011	-.025	-.022	-.007
	(.007)	(.011)	(.019)	(.016)	(.016)	(.025)	(.019)	(.011)
SBC_{t-2}	-.020***	-.012	-.023	-.027*	-.002	-.033**	-.043**	.000
	(.006)	(.013)	(.018)	(.016)	(.010)	(.013)	(.020)	(.012)
NBC_t	-.014	.068	-.025	-.030*	-.024	-.081	-.012	.002
	(.017)	(.042)	(.025)	(.015)	(.024)	(.058)	(.028)	(.013)
NBC_{t-1}	-.039**	-.002	-.047	-.040**	-.088***	-.111	-.037	.040
	(.019)	(.042)	(.040)	(.020)	(.022)	(.071)	(.035)	(.037)
NBC_{t-2}	-.036***	-.023	-.032	-.014	-.086*	-.060**	-.066*	.023
	(.011)	(.023)	(.028)	(.019)	(.045)	(.026)	(.039)	(.017)
$GDP/c._{t-1}$	-.064	-.089	-.092	-.037	-.101*	-.105	-.050	-.050
	(.039)	(.072)	(.134)	(.083)	(.060)	(.082)	(.085)	(.070)
XR_{t-1}	.003	-.005	.038	.002	-.002	-.017*	.004	-.005
	(.006)	(.011)	(.023)	(.009)	(.004)	(.010)	(.016)	(.010)
$Openness_{t-1}$	.000	-.000	.001	.001	-.000	.000	.000	-.001
	(.000)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Gap_{t-1}	.000	.481	-1.152*	-.642	.128	-.915*	-.251	-.189
	(.)	(.583)	(.678)	(.669)	(.390)	(.494)	(.664)	(.350)
Gap^*GDP_{t-1}	-.006	-.080	.125*	.040	-.015	.081	.011	-.000
	(.028)	(.068)	(.074)	(.077)	(.046)	(.052)	(.075)	(.040)
Gap^*XR_{t-1}	-.003	.005	-.025	-.000	-.000	.002	.010	.004
	(.007)	(.007)	(.020)	(.007)	(.003)	(.006)	(.008)	(.007)
Const.	.036***	.040*	.082	.003	.009	.023	.055	.049*
	(.012)	(.021)	(.066)	(.021)	(.013)	(.020)	(.039)	(.027)
N	1589	1589	1589	1589	1589	1589	1589	1589
adj. R^2	.118	.046	.052	.042	.061	.058	.043	.051
No. countries	76	76	76	76	76	76	76	76

Notes: The table presents estimates from equation (1.1) by fixed-effects OLS, as explained in the text. The time period covered is 1973–2000. Standard errors are clustered by country, and are presented in parentheses. All estimations include country and region-time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises, as in Laeven and Valencia (2013). The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.9: Crises and Financial Reforms: 2SLS Panel Estimations with the Laeven-Valencia Data

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	.246***	.067	.016	-.022	-.119***	-.008	-.008	-.071
	(.076)	(.071)	(.112)	(.061)	(.042)	(.102)	(.077)	(.061)
$Reform^2_{t-1}$	-.233***	-.215***	-.114	-.087*	-.019	-.082	-.114	-.001
	(.072)	(.076)	(.101)	(.045)	(.043)	(.088)	(.071)	(.048)
SBC_t	-.051**	-.079	-.080	.018	.027	-.151***	-.078	.030
	(.023)	(.052)	(.070)	(.043)	(.037)	(.058)	(.058)	(.037)
SBC_{t-1}	-.015	.006	-.058	-.007	.020	-.109*	-.002	-.094**
	(.023)	(.058)	(.066)	(.050)	(.036)	(.063)	(.057)	(.039)
SBC_{t-2}	-.016	.031	-.079	-.006	-.045	-.023	.018	.041
	(.022)	(.048)	(.064)	(.051)	(.041)	(.059)	(.055)	(.045)
NBC_t	.128	.100	.940**	-.175	-.092	.068	-.034	.088
	(.122)	(.269)	(.473)	(.273)	(.225)	(.299)	(.344)	(.221)
NBC_{t-1}	.235	.701	.214	.380	.080	.369	-.178	.253
	(.163)	(.476)	(.386)	(.342)	(.219)	(.442)	(.403)	(.237)
NBC_{t-2}	.016	.414	.019	-.294	-.133	.514	-.297	-.177
	(.110)	(.311)	(.327)	(.293)	(.195)	(.376)	(.294)	(.223)
$GDP/c._{t-1}$	-.049	-.004	-.095	.052	-.123**	-.070	.048	-.066
	(.039)	(.091)	(.129)	(.079)	(.057)	(.089)	(.094)	(.069)
XR_{t-1}	.005	-.010	.061**	-.003	-.011*	-.011	.013	.000
	(.007)	(.014)	(.027)	(.010)	(.006)	(.013)	(.013)	(.009)
$Openness_{t-1}$	.000	-.000	.000	.001	-.000	.001	.000	-.001
	(.000)	(.001)	(.001)	(.001)	(.000)	(.001)	(.001)	(.001)
Gap_{t-1}	-.060	.058	-.676	-.141	.086	-.488	.066	.288
	(.197)	(.451)	(.550)	(.454)	(.294)	(.403)	(.409)	(.316)
Gap^*GDP_{t-1}	.004	-.036	.072	-.008	-.008	.035	-.027	-.048
	(.025)	(.055)	(.064)	(.055)	(.036)	(.046)	(.049)	(.038)
Gap^*XR_{t-1}	-.002	.003	-.028	.000	.003	.002	.012*	.007
	(.005)	(.009)	(.022)	(.006)	(.003)	(.006)	(.007)	(.006)
N	1589	1589	1589	1589	1589	1589	1589	1589
No. countries	76	76	76	76	76	76	76	76
Hansen P	.060	.695	.905	.988	.755	.990	.357	.867

Notes: The table presents estimates of equation (1.1) by 2SLS. The time period covered is 1973–2000. Standard errors are robust and are presented in parentheses. All estimations include country and region-time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises, as in Laeven and Valencia (2013). The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.10: Crises and Financial Reforms: Difference GMM with the Laeven-Valencia Data

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	.957***	.943***	1.015***	.892***	.796***	.867***	.776***	.748***
	(.035)	(.056)	(.081)	(.049)	(.036)	(.048)	(.075)	(.040)
$Reform^2_{t-1}$	-.177***	-.152***	-.244***	-.117***	-.027	-.072	-.050	.018
	(.033)	(.049)	(.076)	(.036)	(.029)	(.045)	(.064)	(.033)
SBC_t	-.022***	-.031*	-.013	-.024**	-.001	-.057***	-.021	-.007
	(.006)	(.016)	(.017)	(.011)	(.013)	(.014)	(.020)	(.013)
SBC_{t-1}	.004	.025	.019	.006	.006	.009	-.018	-.018
	(.007)	(.017)	(.020)	(.016)	(.016)	(.024)	(.020)	(.014)
SBC_{t-2}	-.017***	-.024*	-.037**	-.021*	-.014	-.014	-.015	.004
	(.006)	(.013)	(.018)	(.012)	(.012)	(.016)	(.018)	(.012)
NBC_t	-.010	.078	-.004	-.007	-.019*	-.102**	-.021	.022**
	(.019)	(.061)	(.011)	(.016)	(.010)	(.050)	(.019)	(.010)
NBC_{t-1}	-.024***	-.046*	-.022	-.001	-.041	-.040***	-.048***	.029
	(.006)	(.025)	(.014)	(.008)	(.044)	(.013)	(.014)	(.033)
NBC_{t-2}	-.016*	-.033***	-.037*	-.000	.048	.031	-.094***	-.043
	(.009)	(.012)	(.020)	(.017)	(.036)	(.046)	(.029)	(.029)
Gap_{t-1}	.095	-.417**	-.347	.083	.116	-.134	.046	-.565***
	(.094)	(.204)	(.304)	(.175)	(.161)	(.168)	(.217)	(.163)
Gap^*GDP_{t-1}	-.017	.038	.043	-.019	-.020	.002	-.015	.059***
	(.011)	(.023)	(.034)	(.019)	(.019)	(.020)	(.026)	(.019)
Gap^*XR_{t-1}	-.000	.002	-.009	.001	.000	.003	.012***	.005
	(.003)	(.004)	(.013)	(.004)	(.002)	(.003)	(.003)	(.004)
$GDP/c._{t-1}$	-.027**	-.099***	-.045	-.032	.052*	-.149***	.052	-.014
	(.012)	(.029)	(.059)	(.030)	(.029)	(.039)	(.045)	(.027)
XR_{t-1}	.002*	-.000	.006	.001	.000	.001	.002	.003
	(.001)	(.002)	(.005)	(.002)	(.001)	(.002)	(.003)	(.002)
$Openness_{t-1}$	.000	-.001**	.000	-.001**	.000	.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
N	1589	1589	1589	1589	1589	1589	1589	1589
No. countries	76	76	76	76	76	76	76	76
No. instr.	1402	1401	1394	1396	1364	1382	1399	1402
Hansen P	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Sargan P	.000	.003	.001	.000	.000	.000	.000	.000

The table presents estimates from equation (1.1) by Arellano-Bond (1991) one-step difference GMM with full set of instruments. The time period covered is 1973–2000. Robust standard errors are presented in parentheses. All estimations include time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises, as in Laeven and Valencia (2013). The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.11: Crises and Financial Reforms: 2SLS Panel Estimations with Recession Exposures

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	.214***	-.017	-.056	-.016	-.131***	-.044	.001	-.068
	(.073)	(.065)	(.101)	(.060)	(.050)	(.072)	(.079)	(.051)
$Reform^2_{t-1}$	-.185***	-.124**	-.038	-.090**	-.017	-.091	-.120*	-.026
	(.072)	(.056)	(.084)	(.045)	(.061)	(.066)	(.072)	(.040)
SBC_t	-.033**	-.016	-.025	-.004	.006	-.063	-.053	-.044
	(.016)	(.040)	(.037)	(.034)	(.030)	(.044)	(.045)	(.028)
SBC_{t-1}	-.010	.044	-.086	.052	.022	-.044	-.053	-.024
	(.019)	(.057)	(.053)	(.034)	(.035)	(.054)	(.047)	(.025)
SBC_{t-2}	-.004	.073**	-.039	-.013	-.020	-.050	-.034	-.059*
	(.015)	(.037)	(.037)	(.029)	(.035)	(.035)	(.048)	(.031)
NBC_t	.035	.140**	.174**	.019	-.069	-.006	.047	-.051
	(.026)	(.071)	(.073)	(.056)	(.067)	(.064)	(.078)	(.049)
NBC_{t-1}	.061**	.160***	.105	.015	.031	-.003	.144**	.017
	(.027)	(.059)	(.081)	(.059)	(.058)	(.074)	(.073)	(.046)
NBC_{t-2}	.024	.043	.011	.050	.092	-.090	.068	-.030
	(.026)	(.060)	(.078)	(.052)	(.059)	(.072)	(.078)	(.053)
$GDP/c._{t-1}$	-.014	.446*	.036	.474**	-.052	.215	.139	-.022
	(.099)	(.229)	(.270)	(.211)	(.144)	(.196)	(.275)	(.160)
XR_{t-1}	.007	.014	.062***	.013	-.009	.002	.016	.004
	(.006)	(.011)	(.024)	(.010)	(.006)	(.013)	(.014)	(.010)
$Openness_{t-1}$	.000*	-.000	.001	.002***	.000	.001	.001	-.000
	(.000)	(.001)	(.001)	(.001)	(.000)	(.001)	(.001)	(.000)
Gap_{t-1}	.079	.928	-.215	.315	.023	.078	.423	.099
	(.221)	(.598)	(.554)	(.458)	(.320)	(.483)	(.494)	(.366)
$Gap * GDP_{t-1}$	-.011	-.134*	.024	-.063	-.004	-.028	-.067	-.030
	(.029)	(.071)	(.065)	(.057)	(.040)	(.057)	(.060)	(.044)
$Gap * XR_{t-1}$	-.001	.002	-.022	-.005	.001	-.001	.012	.004
	(.005)	(.009)	(.022)	(.006)	(.004)	(.005)	(.008)	(.006)
N	1571	1571	1571	1571	1571	1571	1571	1571
No. countries	76	76	76	76	76	76	76	76
Hansen P	.032	.940	.906	.977	.809	.940	.382	.835

Notes: Instruments include the recession exposures. The table presents estimates of equation (1.1) by panel 2SLS. The time period covered is 1973–2000. Standard errors are robust and are presented in parentheses. All estimations include country and region-time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises. The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.12: Crises and Financial Reforms: Difference GMM with Recession Exposures

	Overall	CrC	IRC	EB	BS	Pr	CaC	SM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{t-1}$	.966***	.941***	1.003***	.888***	.771***	.887***	.784***	.747***
	(.035)	(.058)	(.082)	(.049)	(.039)	(.045)	(.073)	(.039)
$Reform^2_{t-1}$	-.175***	-.154***	-.235***	-.112***	-.006	-.084*	-.058	.019
	(.033)	(.051)	(.078)	(.036)	(.033)	(.044)	(.062)	(.033)
SBC_t	-.013**	.001	-.011	-.009	-.003	-.041**	.003	-.020**
	(.006)	(.012)	(.018)	(.015)	(.010)	(.016)	(.018)	(.009)
SBC_{t-1}	-.003	.002	.000	.018	-.014	-.012	-.018	.009
	(.008)	(.019)	(.020)	(.019)	(.014)	(.019)	(.019)	(.011)
SBC_{t-2}	-.009	-.030**	-.007	-.025*	.019*	.018	-.029**	-.006
	(.006)	(.013)	(.019)	(.013)	(.010)	(.014)	(.013)	(.012)
NBC_t	.002	.011	-.011	.025*	.005	-.014	.014	.003
	(.005)	(.016)	(.020)	(.014)	(.013)	(.013)	(.013)	(.012)
NBC_{t-1}	-.004	.000	.002	-.009	.029	-.034	-.004	-.002
	(.008)	(.015)	(.027)	(.017)	(.025)	(.021)	(.014)	(.008)
NBC_{t-2}	.011*	.012	-.001	-.006	.023	.035	.015	.004
	(.006)	(.014)	(.021)	(.017)	(.017)	(.022)	(.017)	(.011)
Gap_{t-1}	.090	-.426**	-.374	.065	.143	-.101	.022	-.585***
	(.103)	(.208)	(.314)	(.179)	(.158)	(.175)	(.219)	(.164)
$Gap * GDP_{t-1}$	-.015	.039*	.046	-.017	-.024	-.001	-.011	.061***
	(.012)	(.023)	(.035)	(.020)	(.019)	(.021)	(.026)	(.019)
$Gap * XR_{t-1}$	-.000	.002	-.009	.000	-.000	.004	.011***	.005
	(.003)	(.004)	(.013)	(.004)	(.002)	(.003)	(.004)	(.004)
$GDP/c._{t-1}$	-.027**	-.104***	-.045	-.032	.062**	-.140***	.040	-.015
	(.012)	(.030)	(.057)	(.031)	(.028)	(.038)	(.045)	(.027)
XR_{t-1}	.001	-.000	.005	.001	.001	.000	.001	.003
	(.001)	(.002)	(.004)	(.002)	(.001)	(.002)	(.002)	(.002)
$Openness_{t-1}$	.000	-.000**	.000	-.000**	.000	.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
N	1574	1574	1574	1574	1574	1574	1574	1574
No. countries	76	76	76	76	76	76	76	76
No. instr.	1400	1399	1393	1395	1363	1382	1397	1396
Hansen P	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000

Notes: Instruments include the recession exposures. The table presents estimates from equation (1.1) by Arellano-Bond (1991) one-step difference GMM with full set of instruments. The time period covered is 1973–2000. Robust standard errors are presented in parentheses. All estimations include time fixed effects. Dependent variables are: an overall index of financial reforms, as well as specific reforms, including credit controls (CrC), interest rate controls (IRC), entry barriers and pro-competition measures in the banking system (EB), banking supervision (BS), banking privatization (Pr), capital controls (CaC), policies on the securities markets (SM). The variables $Reform_{t-1}$ and $Reform^2_{t-1}$ represent the lags of the respective dependent variables. SBC and NBC stand for systemic and non-systemic banking crises. The rest of the explanatory variables are detailed in the methodology section. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 1.13: Episodes of Banking Crises

Year	Systemic Crises	Non-Systemic Crises
1974	-	UK
1975	-	UK
1976	Chile	UK
1977	Israel, Spain	-
1978	Israel, Spain	Germany, South Africa, Venezuela
1979	Israel, Spain	Germany
1980	Argentina, Israel, Spain	-
1981	Argentina, Chile, Colombia, Ecuador, Israel, Mexico, Spain	-
1982	Argentina, Chile, Colombia, Ecuador, Ghana, Israel, Mexico, Spain, Turkey	Hong Kong
1983	Chile, Colombia, Ghana, Israel, Morocco, Peru, Spain, Thailand	Canada, Hong Kong, Taiwan
1984	Chile, Colombia, Ghana, Peru, Spain, Thailand, Turkey	Canada, Hong Kong, Taiwan, UK, US
1985	Chile, Colombia, Ghana, Peru, Spain, Thailand, Turkey	Canada, Hong Kong, US, Venezuela
1986	Bolivia, Chile, Colombia, Ghana, Peru, Thailand	Hong Kong, US, Venezuela
1987	Bangladesh, Bolivia, Colombia, Costa Rica, Ghana, Norway, Peru, Philippines, Tanzania, Thailand	Denmark, New Zealand, US

Continued on next page

Table 1.13 – continued from previous page

Year	Systemic Crises	Non-Systemic Crises
1988	Bolivia, Burkina Faso, Costa Rica, Ghana, Madagascar, Nepal, Nicaragua, Norway, Senegal, Tanzania	Denmark, New Zealand, US
1989	Argentina, Burkina Faso, El Salvador, Ghana, Nicaragua, Norway, Senegal, Sri Lanka, Tanzania	Australia, Denmark, Jordan, New Zealand, South Africa, US
1990	Algeria, Argentina, Brazil, Burkina Faso, Nicaragua, Norway, Senegal, Sri Lanka, Tanzania	Australia, Denmark, Guatemala, Italy, Jordan, New Zealand, US
1991	Algeria, Burkina Faso, Finland, Hungary, Nicaragua, Nigeria, Norway, Poland, Senegal, Sri Lanka	Australia, Denmark, Greece, Guatemala, Italy, Tunisia, UK, US
1992	Albania, Algeria, Burkina Faso, Estonia, Finland, Hungary, Japan, Mozambique, Nicaragua, Nigeria, Norway, Poland, Sri Lanka, Sweden	Australia, Denmark, Greece, Italy, Tunisia
1993	Burkina Faso, Estonia, Finland, Hungary, Japan, Mozambique, Nicaragua, Nigeria, Norway, Poland, Sri Lanka, Sweden	Greece, India, Italy, Tunisia, Venezuela
1994	Bolivia, Brazil, Burkina Faso, Ecuador, Estonia, Finland, Hungary, Jamaica, Japan, Kyrgyz Republic, Mexico, Mozambique, Nigeria, Sweden, Uganda	Costa Rica, Ethiopia, France, Greece, India, Italy, Tunisia, Turkey

Continued on next page

Table 1.13 – continued from previous page

Year	Systemic Crises	Non-Systemic Crises
1995	Argentina, Bolivia, Brazil, Bulgaria, Ecuador, Estonia, Hungary, Jamaica, Japan, Kyrgyz Republic, Mexico, Mozambique, Nicaragua, Nigeria, Paraguay, Uganda	Costa Rica, Ethiopia, France, Greece, India, Italy, Taiwan, Tunisia, UK
1996	Brazil, Bulgaria, Jamaica, Japan, Mexico, Nicaragua, Paraguay, Uganda	Costa Rica, Dominican Republic, India
1997	Brazil, Bulgaria, China, Colombia, El Salvador, Jamaica, Japan, South Korea, Mexico, Paraguay, Romania, Taiwan, Thailand, Ukraine, Vietnam	Costa Rica, Nigeria
1998	Brazil, China, Colombia, Ecuador, El Salvador, Jamaica, South Korea, Paraguay, Philippines, Romania, Taiwan, Thailand, Ukraine, Vietnam	Estonia, Hong Kong
1999	Bolivia, Brazil, China, Ecuador, Jamaica, South Korea, Paraguay, Peru, Philippines, Thailand	-
2000	Jamaica, South Korea, Philippines, Thailand, Turkey, Vietnam	-

Notes: The crises episodes, as well as their classification into systemic- or non-systemic banking crises, are taken from Caprio and Klingebiel (2003). Whenever an ambiguity arises with respect to the end date of a crisis, the newer work by Reinhart and Rogoff (2008) is used.

Chapter 2

Deregulation, Economic Growth and Growth Acceleration[1]

[1]An earlier version of this essay was published as CERGE-EI Working Paper No. 424 in October 2010.

2.1 Introduction

After the oil shock of 1973, the developed economies experienced a dramatic decline in their economic growth (Nordhaus, 1980; Sachs, 1982) and labor productivity growth (Baily, 1981). Since the mid-1970s, the productivity decline triggered a wide range of policy responses, including economic deregulation.[2] Deregulation reforms were initiated in the US (Morgan, 2004; Winston, 1998), followed by the UK and other developed economies in the early 1980s (Matthews, Minford, Nickell, & Helpman, 1987; Pera, 1989) and were imitated by the new democracies and many developing countries in the 1990s with an extensive set of labor-, capital-, and product-market reforms. The process continued throughout the early years of the 21st century (Wölfl, Wanner, Kozluk, & Nicoletti, 2009) until the recent global economic and financial crisis undermined the efforts to relax economic regulations.

The differences in the deregulation reform timing across countries point to a natural question: Did the early reformers – those countries reforming extensively in the 1970s and the 1980s – benefit more than the late reformers in improving their living standards and in accelerating economic growth? If they did, then the economies that innovated with deregulation enjoyed growth, while those who imitated best-practice institutions did not always benefit from deregulation, as some evidence suggests (Rodrik, 2008). Answering this question is important at least for two additional reasons. On the one hand, a substantial bulk of the literature uses the time variation of various indices of regulation to gauge deregulation reforms. However, using those directly in a regression equation is problematic because equal changes in the indices represent unequal policy changes across countries. This work

[2]Following Winston (1993) the economic deregulation may be interpreted as the state's withdrawal of its legal powers to direct pricing, entry, and exit within an industry.

proposes a way out from this measurement problem by using the time variation of the reforms across countries and over time. Using the variation of the indices over time allows for the setting up of a difference-in-difference estimation which gets around the direct use of reform indices.

On the other hand, few papers account for where the time variation in the indices comes from in the first place, and if they do, their instruments are rarely time-varying. This paper uses two time varying indicators for each country which are arguably both strong and valid in predicting the timing of the deregulation reform. These indicators are a country's energy independence and its natural resource rents. I find that the more energy independent the country is, and the more natural resources it has, the later it deregulates its product-, labor- and credit markets.

By combining how the reform timing affects living standards and growth with the political economy of reform timing, this paper addresses simultaneously two of the long-standing problems in the empirical analysis of deregulation reforms. At the same time, the work supports previous evidence of a positive impact of reforms on growth. The results also demonstrate important differences in the reform outcomes across countries. The benefits from deregulation were unequally spread, and the timing of the reform played an important role in reaping those benefits. Specifically, while early reformers enjoyed higher living standards, it is the late reformers' growth that accelerated most, especially after a credit market deregulation reform. Thus, despite the evidence that most reforms do not produce growth accelerations (Hausmann, Pritchett, & Rodrik, 2005), credit market reforms seem to be an exception. Therefore, they require special attention, especially when the need for faster recovery is coupled with a widespread political drive to re-regulate the financial sector.

The paper delivers two main messages. First, deregulation contributed to growth but its impact was different across countries, and the deregulation reform timing can at least partly explain the cross-country differences in the outcomes of similar reforms. Second, a large-scale financial re-regulation could backfire with substantial negative dynamic effects on growth acceleration, which may delay a desired recovery in the aftermath of the Great Recession.

2.2 Literature Review

The political economy behind the large-scale deregulation reforms initiated in the late 1970s is two-fold. On the one hand, deregulation reduces the rents that regulation creates for workers, and for incumbent producers and service providers. This view has gained a widespread popularity among academics and policymakers alike ever since the seminal works by Stigler (1971), Posner (1974) and Peltzman (1976) contributed to the understanding of the political economy of regulation. On the other hand, deregulation allows the newly created competition on the product-, labor- and capital markets to determine the winner of those rent transfers. Thus, by spurring productivity and efficiency gains (Winston, 1993), economic deregulation ultimately contributes to the overall increase in economic growth. The additional growth is brought primarily through increased employment and real wages (Blanchard & Giavazzi, 2003), which affects both production and consumption and through increased investment (Alesina, Ardagna, Nicoletti, & Schiantarelli, 2005).

However, a more recent take on the efficiency gains from deregulation in the developing world provides a word of caution. The key contention in this newer line of literature is that deregulation reforms influence different economies differently, depending on their position on the technology ladder

and on the quality of their institutions. For example, Açemoglu, Aghion, and Zilibotti (2006) claim that certain restrictions on competition may benefit the technologically backward countries, while Estache and Wren-Lewis (2009) find that the optimal regulatory policies in developed and developing countries are different because of differences in the overall institutional quality in those countries. In addition, Aghion, Alesina, and Trebbi (2007) use industry-level data to demonstrate that within each economy, institutional reforms influence different industries differently, and more specifically, industries closer to the technology frontier would be affected more by deregulation and would innovate more than the backward industries in order to prevent entry. As a result, countries closer to the technology frontier would benefit more from deregulation. The alleged benefits of economic deregulation in many industries prompted a debate on the growth effects from specific types of reforms, such as capital-, labor-, and product-market deregulation.

Although various authors interpret the scope of **product-market regulation (PMR)** reforms differently,[3] most agree that PMR reforms include deregulation of at least pricing and entry. As the literature on entry regulation suggests, stricter and more costly procedures to set up a firm are associated with lower GDP *levels* (Djankov, La Porta, Lopez-de-Silanes, & Shleifer, 2002). As it is the case with other empirical studies on deregulation

[3]For example, Wölfl et al. (2009, p.11-12) include direct control of pricing behavior of private firms, administrative burdens on the setting up of a corporation and a sole proprietorship, barriers to trade and foreign investment, among other reforms; Gwartney, Hall, and Lawson (2012) study price controls, start-up regulations, licensing restrictions, administrative requirements for businesses, bureaucracy costs and other business regulations; the World Bank Doing Business reports consider an extensive set of business regulations in ten different areas, including starting and closing a business; while Kaufmann, Kraay, and Mastruzzi (2010, p.4) aggregate data on the product market, the financial market, and the international trade and investment regulations from various underlying sources such as the Economist Intelligence Unit and the Heritage Foundation which capture the general "ability of the government to formulate and implement sound policies and regulations that permit and promote private sector development."

reforms, it is tempting to interpret this finding as a policy recipe for growth because it implies that entry deregulation causes economic growth. Yet it is obvious that such an interpretation is superficial at least because to be able to derive growth effects of a given regulatory reform, one needs to focus on the effects of the *changes* in regulations over time rather than their levels.[4]

The conclusions of Djankov et al. (2002) bring about many extensions.[5] For example, by using firm-level data Scarpetta, Hemmings, Tressel, and Woo (2002) find that PMR hampers both total factor productivity and entry in OECD countries, while Alesina et al. (2005) build upon those findings to emphasize a positive *causal* relationship between deregulation and investment in seven OECD industries. Further, Barseghyan (2008) supports the causal relationship with an IV estimation on a sample of between 50 to 95 countries.

However, there are papers that do not find enough evidence that institutional reforms, including deregulation, matter for economic performance. Commander and Svejnar (2011) use firm-level data from the Central and Eastern European countries and the former USSR to find that regulatory constraints do not affect firm performance. In addition, Babecký and Campos (2011) summarize results from 46 studies in a detailed meta-analysis of the impact of reforms on economic growth. They conclude that the institutional impact on growth performance shows "remarkable variation" both in terms of sign and significance, and Babecký and Havránek (2013) reinforce the point by expanding the list of papers in a similar meta-study. A similar, if not even stronger, difference in opinion is found in the debates on the

[4]Campos and Coricelli (2002) were among the first to suggest that reform changes rather than their levels might be more appropriate to include in an empirical analysis of the impact of institutions on growth.

[5]Djankov (2008) reports that 195 academic articles emerged as a result of this paper and the subsequent work of the Doing Business team at the World Bank.

growth impact of labor- and credit-market regulation reforms.

Similarly to PMR, **labor-market regulations (LMR)** also affect growth factors. Yet, the literature on the effects of LMR on wages, working hours, and labor productivity is also not unanimous. For example, severance payments are found to have no effect on wages because firms make workers pre-pay them back through the labor contracts (Lazear, 1990; Leonardi & Pica, 2007). On the other hand, van der Wiel concludes that mandatory notice worker protection increases wages (van der Wiel, 2010).

The debate on how labor regulations affect labor productivity and employment is also inconclusive. For example, MacLeod and Nakavachara (2007) test whether more stringent labor regulations reduce employee turnover and lead to a more productive employee-employer relationship for some types of occupation, especially the high-skilled ones. Acharya, Baghai, and Subramanian (2010) find support for the hypothesis that stricter labor dismissal laws encourage innovation within firms, and therefore, could potentially promote labor productivity and economic growth. The intuition is that labor laws provide high-skilled innovative staff with a certain degree of insurance in case of a short-term failure to innovate.

These results contradict the traditional argument that labor regulations impose costs to firms and thus reduce labor force participation, employment (Botero, Djankov, La Porta, Lopez-de-Silanes, & Shleifer, 2004), and investment as well as value added per worker (Cingano, Leonardi, Messina, & Pica, 2009). Autor, Kerr, and Kugler (2007) present evidence that imposing employment protection laws increases employment but reduces productivity in the US. Further, Bassanini, Nunziata, and Venn (2009) extend the latter evidence of a reduction in labor productivity as a result of more worker-friendly labor laws for a sample of OECD countries. By analyzing firm-level data

from Italy, Boeri and Garibaldi (2007) are in line with the traditional view on labor regulations summarized in Cingano et al. (2009) and find strong support for the conclusion that employment protection reduces productivity.

The long-run effects from labor regulations on economic growth are further analyzed in Deakin and Sarkar (2008). Their conclusions support the "indeterminacy hypothesis". That is, the effects from labor laws on growth would ultimately depend on context-specific factors, and therefore, finding any evidence supporting or confronting the notion that regulation hampers growth is, perhaps, always tentative and should be interpreted with caution.

The discussion above confirms that whether labor market reforms have an impact on economic growth is inconclusive (Freeman, 2009).[6] The same can be said for the reform impact of **credit market regulation (CMR)**.

In an excellent review of the state of the debate, Demirgüç-Kunt and Levine (2008) present the reasons why credit market deregulation may lead to growth. They claim that financial deregulation, such as equity market liberalization and allowing foreign bank competition, may spur growth by improving the allocation of capital and reducing its cost, thereby increasing overall efficiency. In a similar spirit, Bekaert, Harvey, and Lundblad (2005) find that liberalizing the equity market leads to a 1 percentage point increase in annual economic growth, while Demirgüç-Kunt et al. (2004, p.593) con-

[6]In a separate line of literature, labor regulation has an inconclusive effect on income inequality. For example, Rosenbloom and Sundstrom (2009) argue that labor market institutions have a significant impact on the income distribution. They support the conclusions by Fortin and Lemieux (1997) who find that labor market reforms increase wage inequality in the US, and side with the cross-country evidence by Freeman (2007) who finds that more regulated labor markets exhibit lower income inequality. However, in a more recent take at the issue, Scheve and Stasavage (2009) disagree with this argument, presenting a time-series evidence encompassing most of the 20th century. They find that income inequality in 13 industrialized countries was shrinking even before the stringent labor market institutions like the centralized wage bargaining were introduced. Thus, they conclude, there is little support for the cross-sectional evidence that labor market institutions like centralized wage bargaining influenced significantly the income distribution.

clude that "tighter regulations on bank entry and bank activities boost the cost of financial intermediation," which ultimately hampers growth.

The positive association between banking liberalization and economic growth is also found in earlier studies.[7] Levine (1998, p.598) uses legal origin as an instrumental variable for banking development on cross-country data to arrive at a "statistically significant and economically large relationship between the exogenous component of banking development and the rate of economic growth." Earlier, Jayaratne and Strahan (1996) apply a difference-in-difference strategy on US data to find that both output and per capita income rise after the relaxation of the intra-state bank branching restrictions.

The recent literature on the causes and consequences of the financial crisis of 2007-2008 revives a lively debate about the impact of credit market liberalization. For example, Gorton (2008) identifies the innovations in the financial industry as standing at the heart of the sub-prime crisis of 2007-2008. Yet, as early as 1995, Edwards and Mishkin (1995) concluded that financial innovations expose the banking industry to the same risks which were effectively regulated in the past. Their result suggests that it is not financial innovation which leads to the crisis.

However, Diamond and Rajan (2009) claim that the US financial sector mis-allocated resources to the real estate sector by issuing new financial instruments, while Stiglitz (2010) develops a model in which full financial liberalization may be welfare-decreasing. This conclusion is also in line with the threshold finance literature reviewed briefly in the previous chapter. In brief, the literature reveals thresholds beyond which finance no longer plays the expected positive role for growth and could contribute to financial fragility.[8] Interestingly enough, increased financial fragility is sometimes associated with

[7] See Levine (2005a) for an extensive review.

[8] See Arcand et al. (2012), Cecchetti et al. (2011) and Cecchetti and Kharroubi (2012).

higher macro growth. Rancière, Tornell, and Westermann (2008) suggest a positive effect of systemic risk on growth, especially in countries lacking a proper institutional and regulatory environment. In their work, systemic risk encourages investment which leads to higher growth but also to more frequent crises.

In addition, Calomiris (2009, p.62) concludes that banking regulations have "...always been the key additional necessary condition to producing a propensity for banking distress" and in a more recent essay Levine (2010, p.3) maintains that "...financial regulations and policies created incentives for excessive risk and the financial regulatory apparatus maintained these policies even as information became available about the growing fragility of the financial system."[9,10] However, Tarr (2010) argues that CMR is unrelated to the financial crisis of 2007-2009. Instead, political failures stand at its core.

This work extends the literature in the nexus of deregulation and growth fields in two ways. First, it approaches the measurement of various deregulation reforms in a similar fashion to Estevadeordal and Taylor (2008) who transform the traditionally used reform indices into dummy variables, thereby allowing for a difference-in-difference estimation. The advantage of this approach lies in using the reform indices to construct policy treatment and control groups rather than using the indices directly to infer the effect of a unit change of a reform index.

Second, and perhaps even more importantly than dealing with the measurement issue, few empirical papers which clarify the impact of deregulation

[9]For similar conclusions on the role of financial liberalization in previous crises, most notably the Japanese banking crisis, see Hoshi and Kashyap (1999).

[10]The political science literature builds upon the critique of CMR. For example, Satyanath and Berger (2007) claim that the growth effect of capital control liberalization would critically depend on the degree of democracy, implying that the growth effect hinges on local country-specific political factors which in turn renders the reform outcomes ex-ante unknown.

on growth account for where the time variation in the indices comes from. For example, Alesina et al. (2005) use lagged values of PMR indices as instruments for current regulation for OECD countries only. Further, they study the impact of the reform timing without taking into account the origin of the reforms. Barseghyan (2008) uses a number of instruments for entry regulations and property rights, such as geographical latitude, legal origins, settler mortality, and indigenous population density as early as the 16th century for a large sample of countries. However, as Barseghyan's instruments do not vary over time, they can explain only the cross-sectional variation in the reform data. As a result, the studies using those instruments fail to explain the time variation in deregulation reforms.

In contrast, I explore energy independence and natural resource rents across countries which also vary over time to predict the timing of the deregulation reforms, and only then study the impact of those reforms on growth. Beck and Laeven (2006) apply similar logic to a broad aggregate index of institutional reforms in 24 transition economies. Both the energy independence and rents indicators, as well as the empirical strategy, are detailed below.

2.3 Empirical Strategy

The literature review points to two methodological issues that need to be addressed in the analysis of any institutional impact on growth: the measurement of reforms and the endogeneity of the reform timing. In this section, I present a possible approach to deal with both at the same time in the context of deregulation. The benchmark model addresses primarily the measurement issue, while the 2SLS model extends it and addresses the endogeneity issue.

2.3.1 Benchmark Model

Much in the spirit of Estevadeordal and Taylor (2008), I define reformers between 1970 and 1990 as countries with an above-median (above-mean) increase in the Economic Freedom of the World (EFW) index of regulation between 1970 and 1990 and non-reformers otherwise. Identically, reformers between 1990 and 2010 are defined as countries with an above-median (above-mean) increase in the EFW index of regulation between 1990 and 2010 and non-reformers otherwise. Thus, four distinct groups of countries emerge: 1) non-reformers in the first period becoming reformers in the second period (late reformers); 2) reformers in the first period becoming non-reformers in the second period (early reformers); 3) reformers in both periods ("marathon" or consistent reformers); and 4) non-reformers in both periods. The first three groups are the policy treatment groups in all baseline estimations, while non-reformers are the control group. Table 2.1 lists the countries in the 1970-2010 sample.

Although the data split may seem arbitrary, it is justified for several reasons. First, the data are such that they allow for two equally long periods to be constructed in both deregulation reforms and growth performance. Second, 1990 marks an important change in economic history with the start of many market-oriented reforms across a wide range of economies. As the data description demonstrates, the reforms before 1990 were rather sporadic, while after 1990 they were widespread but varying in their magnitude, which presents a suitable opportunity for a difference-in-difference study. Third, splitting the data into smaller periods would undermine capturing some effects that materialize over longer periods of time within each economy; it would also present a challenge in capturing a policy variation in deregulation within a decade or within a shorter span, as many economies might not

reform at all within shorter periods of time. Finally, the 1990 threshold is not new to the literature on the impact of deregulation on growth: Alesina et al. (2005) also use it. Therefore, splitting the data into two relatively long 20-year periods is suitable for this empirical work.

To address how the timing of the reform affected living standards and growth, I estimate the following benchmark model:

$$\Delta y_{it} = \beta_1 + \beta_2 \mathrm{ER}_{it} + \beta_3 \mathrm{LR}_{it} + \beta_4 \mathrm{MR}_{it} + \mathbf{X}_{it}^{'}\beta + \Delta\varepsilon_{it}, \tag{2.1}$$

where Δy_{it} is either the difference in the average log-GDP per capita for country i in period t, $\Delta \text{Avg.}\log(GDP)_{it}$, to measure the effect of the reform on the living standards, or the difference in the compound growth rates between the two periods, denoted by Δg_{it}, to measure the growth acceleration effect;[11] ER_{it} is a dummy variable equal to 1 if the country was an early reformer and to 0 otherwise; LR_{it} is a dummy variable equal to 1 for the late reformers and to 0 otherwise; MR_{it} are the countries that were reformers in both periods; $\mathbf{X}_{it}^{'}$ is a vector of country characteristics, such as: i) the initial level of log-GDP per capita in 1970 to control for growth convergence and initial conditions, and ii) other institutional reform covariates such as barriers to trade; and $\Delta\varepsilon_{it}$ is an error term about which I assume, at least for now, that the standard linear regression assumptions are satisfied. Finally, note that with only two reform periods – before and after 1990 – the t-dimension collapses to 1, which effectively means performing a cross-sectional estimation on differenced data.

[11]The compound growth rate for country i within each 20-year period t is measured as $g_{it} = [(x_n/x_0)^{1/20} - 1]^*100$, where x_0 is the initial level of per capita real GDP, while x_n is the terminal level. Thus, the compound growth rate measures the growth rate of the economy as if it was growing with the same rate throughout the period. I do not use the least squares growth rate because its estimation requires a sufficiently large number of observations over time.

2.3.2 2SLS Estimation

The above benchmark estimation does not account for the selection process into the various treatment and control groups. To do that, the following local average treatment effect (LATE) model is estimated:[12]

$$\Delta y_{it} = X_{it}^{'}\beta + D_{it}^{'}\alpha + \Delta\varepsilon_{it}, \tag{2.2}$$

where $X_{it}^{'}$ is the vector of the observed explanatory variables described above, and D_{it} is a vector of treatment indicators (ER, LR and MR) that depend on the instrumental variables, z_{it}, in a way that $D_{it}^{*} = \gamma_0 + \gamma_1 z_{it} + u_i$ is a latent variable with its observable counterpart D_{it} generated by:

$$D_{it} = \begin{cases} 0 & \text{if } D_{it}^{*} \leq 0, \\ 1 & \text{if } D_{it}^{*} > 0. \end{cases} \tag{2.3}$$

Equation (2.3) means that the reform participation decision D_{it} is driven by some unobservable factors D_{it}^{*} that in turn depend on some predetermined country characteristics z_{it} which I assume exogenous. These characteristics are the instrumental variables which vary over time and can arguably predict the selection into early, late, and marathon reformers. The instruments, z_{it}, are the energy independence of a country i in period t, and the natural resource rents of the same country in the same period. In line with the political economy literature, the more energy abundant the country is and the more natural resources it possesses, the less incentives its policy makers have to deregulate at any point in time. Therefore, the more energy independent the country is, and the more rents it has from natural resources, the lower the probability of reforming early. At the same time, however, changes in the resource abundance may also influence political decisions to reform or

[12]The model is detailed in Cameron and Trivedi (2005, pp.883-884).

to reverse reforms at any point in time. Therefore, the energy independence instrument is constructed as follows:

$$z_{it} = \frac{P_{it} - C_{it}}{C_{it}}, z_{it} \in [-1; \infty), \tag{2.4}$$

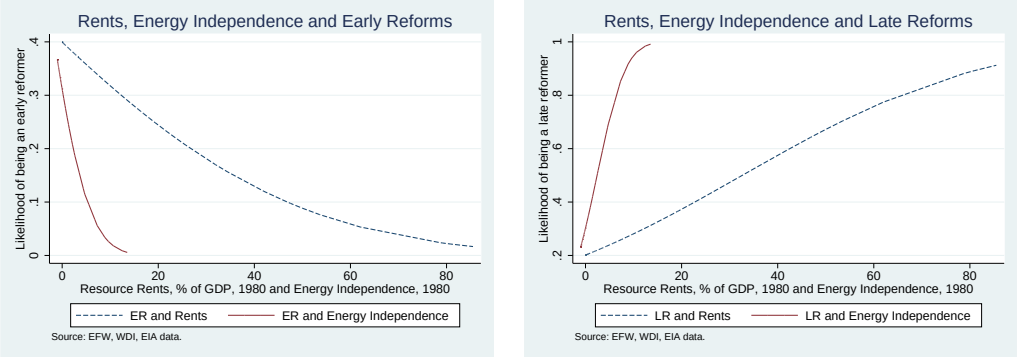
where P_{it} is the production, and C_{it} is the consumption of energy in a given year between 1980 and 2009.[13] The variable z_{it} also means that the more production of energy there is in the country, the more energy-independent the country becomes. For example, if $z_{it} = 9$, then the country produces 10 times more energy than it consumes.[14]

Apart from the energy independence, the time variation in reforms may also come from the natural resource abundance a country enjoys. The resource abundance is measured with the natural resource rents as a share of GDP. For a given natural resource, its rent is the difference between its market value and the cost of obtaining it. Then, the sum of the rents for all of a country's resources is the total indicator of natural resource rents. Natural resources for which panel data is readily available include coal, forests, minerals, natural gas, and oil.

The relationship between the natural resource rents, energy independence and the likelihood of market-oriented reforms is presented in Figure 2.1. In line with the political economy expectations, panel 2.1(a) indicates that the more energy-independent the country is, i.e., the higher the share of its consumption which could be satisfied from local production, the lower the probability is of the country being an early reformer. In addition, panel 2.1(b) demonstrates that energy-rich countries actually have a higher probability of

[13]Effectively, this means I have a data point for y_i and the timing of the reform (ER_i, LR_i or MR_i) and 30 possible instruments.

[14]As it includes diverse sub-indicators such as petroleum, natural gas, coal, hydroelectric power, nuclear electric power, solar, wind, and waste electric power, the energy-independence indicator is measured in the generic British Thermal Units (BTU).



(a) (b)

Figure 2.1: Rents, Energy Independence and Likelihood of Reforms

reforming late. Identical conclusions are reached when the share of natural resource rents in GDP is used to predict the timing of reforms.

The natural resource underpinning of market-oriented reforms is supported also by other empirical findings. For example, Levine (2005b) finds sufficient credibility in the idea that endowments create elites who subsequently shape the property rights of a country in their own favor of in favor of a strong private sector. Further, Beck and Laeven (2006) isolate the exogenous component of the institutional variation in the transition economies by using their natural resource abundance to study the institutional impact on economic growth. In addition, Mulligan and Tsui (2008) develop a theoretical argument justifying why resource abundant countries tend to be non-democratic, and in a more recent paper, Tsui (2011) finds empirical support for the model and concludes that oil discoveries significantly reduce the likelihood of democratic reforms. These findings support the validity of using the path of resource independence over time as a predictor of the timing of market-oriented reforms.

There is one major concern when using the time variation in rents and energy independence as instruments for the timing of reforms: its correlation

with living standards and growth. It is certainly true that energy production and consumption is correlated with both GDP levels and growth within a period. Within a short given period, higher energy production and consumption and higher resource rents raise GDP and GDP growth. At the same time, higher rents and energy independence make politicians postpone reforms. This turns richer and faster growing countries – especially those who made their fortunes after discovering natural resources – into candidates for being late reformers. Therefore, if there is a positive correlation between resources and GDP, and between resources and reforming late or never, then there should be a positive correlation between being a non-reformer, a late reformer and GDP. Then, the estimates of being an early or a marathon reformer would actually be biased downwards in both OLS and the 2SLS estimations. Further, notice that both the reform variables and GDP capture 20-year periods, while the instruments are annual observations of rents and energy independence. Thus, although certainly possible, any short-term correlation between rents, energy independence and GDP is limited within a small segment of the reform timeline. Therefore, the biases resulting from those correlations should not significantly affect the main results, especially when those results capture the effects from reforms over long periods.

At the same time, the validity of the instrument is justified by the emerging evidence that rents and energy independence have only a *short-term direct impact* on economic growth, if it has any impact at all (Alexeev & Conrad, 2009, 2011; Aliyev, 2011), which justifies using this instrument over longer periods. Otherwise, applying it as an IV for reforms would not be a valid empirical approach over short periods of time.

2.3.3 Data

Deregulation reforms

The explanatory variables on the changes of the index of regulation and other reforms are taken from the Gwartney et al. (2012) index of Economic Freedom of the World (EFW) data, which traces the economic policy development in 144 countries back to 1970 in the following relevant policy areas: 1) Size of Government: Expenditures, Taxes, and Enterprises; 2) Legal Structure and Security of Property Rights; 3) Freedom to Trade Internationally; and 4) Regulation of Credit, Labor, and Business. Those indices are transformed into reform variables, as outlined in the empirical model description. The main explanatory variable is taken from the changes in the index of Regulation of Credit, Labor, and Business.

Country-level economic growth

One of the most comprehensive sources of country-level GDP and growth data is the 7.1 version of the Penn World Table (PWT) by Heston, Summers, and Aten (2012). My main dependent variables are the GDP per capita and the GDP per worker which are the RGDPCH and the RGDPWOK variables in the PWT. For every country in the sample, the dependent variables are constructed as follows: take the average log-level of GDP per capita for the first period (1971-1990) and difference it from the log-level of GDP per capita for the second period (1991-2010). Thus, we have a data point for every country, which indicates the difference in the average log-GDP between the two periods.

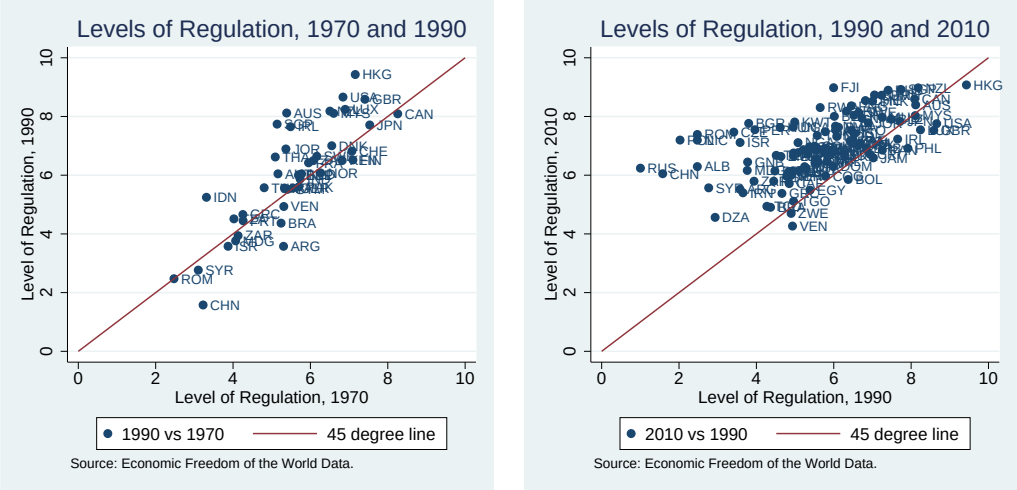
Further, the geometrically averaged compound growth rate is measured within each 20-year period and for each country. The difference of the two compound growth rates suggests of a growth acceleration or deceleration

after 1990. This difference is the second dependent variable used in this work. The match between non-missing GDP levels and growth rates and the overall EFW index of regulation over the 1970-2010 period is for 39 countries only, which is the size of the baseline sample. As there is more abundant data on credit market regulation (CMR) reforms, the match with the EFW index of CMR over the 1970-2010 period is for 65 countries. As this is admittedly a rather small sample, it is further extended in ways explained below in the robustness checks section.

As a supplementary dataset on growth performance and its factors, I use the World Development Indicators (WDI) database, which is arguably at least as precise as the PWT in its ability to measure economic growth and its factors in a large panel of countries. It contains information on GDP, GDP per capita and GDP per worker from 214 countries and territories since 1960. As there are some differences across datasets in the way the growth series are constructed, which might affect the results of the empirical estimations,[15] using both the PWT and the WDI data sets makes it possible to check if the results hinge on the data source. The natural resources rents data set is also obtained from the WDI database. It spans across the entire 40-year period of reforms and growth for 131 countries.

Finally, the data on energy production and consumption, which are needed to construct the energy independence indicator, are taken from the Energy Information Administration (EIA) of the US government. The database contains annual observations for 193 countries and territories between 1980 and 2009, the majority of which are also present in the PWT and the EFW data sets.

[15]See, for example, Hanousek, Hajkova, and Filer (2008) for a study of how the choice of data might affect the results of cross-country growth regressions.



(a) (b)

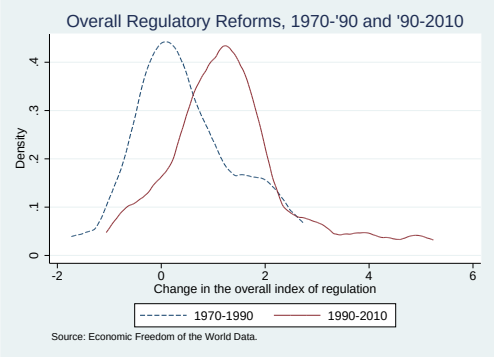
Figure 2.2: Overall Deregulation Reforms, 1970-2010

Deregulation and economic growth trends since 1970

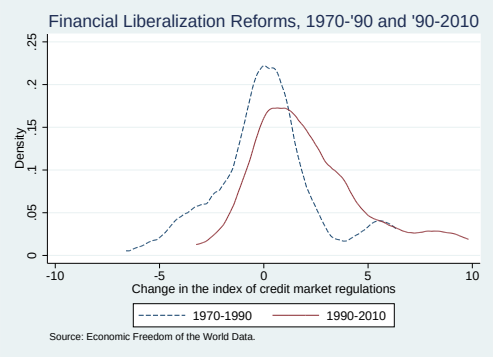
This section illustrates graphically how the deregulation policies developed from 1970 to 2010. The fairly long period of conducting those policies avoids the risk of having almost no policy change within a shorter span.

Figure 2.2(a) relates the index of overall regulation in 1970 with the same index 20 years later, and Figure 2.2(b) characterizes the relation for the period that followed. A variation in the data in both directions is observed for both periods. Most of the countries are lined around the 45-degree line in the first period. As a higher index of regulation in the data means less restrictive regulations, the data demonstrates that most countries did not deregulate extensively between 1970 and 1990. However, in the second period, most of the countries stand above the 45-degree line, which indicates improvement over their 1990 standpoint.

The overall trend in deregulation and in the credit market regulation policies, is also clearly demonstrated in Figure 2.3 where the shift in the



(a) Overall Reforms



(b) Credit Market Reforms

Figure 2.3: Distribution of Deregulation Reforms, 1970-2010

distributions of deregulation policies can be observed. Clearly, there is a marked difference before and after 1990. Not only did the majority of countries improve their regulatory environment, which is visible from the shift of the distribution to the right, but also far more countries were adopting more radical market-oriented reforms, which is obvious from the increased variance of the distribution. The next section illustrates whether those extensive reforms in the 1990s brought an increase in the living standards and growth of the reforming economies.

2.4 Results

The results from OLS and 2SLS estimations of the benchmark equation (2.1) in which the reform dummies are predetermined by the country's own energy independence are presented in Table 2.2. The table is divided into two main sections, identifying the two main discrimination criteria between reformers and non-reformers: the median and the mean criterion. In the first four models, the median criterion is being used, while the mean criterion is applied in the latter four models. Within each criterion, four estimations are

carried out, two for each level of aggregation of the reform variable. In the first two models, OLS and 2SLS estimations are presented with the underlying explanatory variable being the overall index of regulation for the given economy. In the second pair of models, the underlying explanatory variable is the sub-index of financial regulation only.

Table 2.2 demonstrates clearly that late reformers (LR), or those countries that lagged behind in their deregulation reform in the 1970s and in the 1980s but accelerated the reform in the 1990s and in the early years of the 21st century, had lower per capita GDP levels than the early reformers (ER) and those countries that reformed extensively in both periods – the "marathon" reformers (MR). Model (1) in Table 2.2 produces an expected result: ERs increased their per capita GDP about 40% points more than the LRs, and the MRs increased their living standards by about 20% points. This means deregulating early and continuously is also associated with significantly higher living standards. The result is obtained when controlling for other institutional variables, such as removing trade barriers, and for initial per capita GDP levels.

In Model (2) I control for the same variables. At the same time I estimate the model by 2SLS and instrument with energy independence in each year between 1980 and 2009. The results not only retain their sign but also increase both their magnitude and their significance. This confirms that early and marathon reformers become considerably richer while reforming their overall labor-, business- and financial regulations. The instruments pass the Hansen over-identification J-test, which is a good signal about their validity. The Angrist-Pischke first stage F-test (APF), however, points to a plausible weakness of the instruments.

The estimates above reveal the effect of an overall deregulation reform.

Thus, the above results are somewhat loose and difficult to interpret. The variation in the overall reform variable admittedly captures a wide range of reforms simultaneously, thereby limiting the chances for formulating specific deregulation policy implications.

In models (3) and (4), I replace the overall reform variable with an identically constructed variable, tracking down only one of the three reforms constituting the overall reform: the deregulation on the credit market. Although the results are not as strong as before in terms of magnitude, the sign remains indicative of the inherent difference between the three types of reformers: The levels of per capita GDP of the early reformers and marathon reformers were significantly higher than those of the late reformers. The same result holds when the reform is instrumented with energy independence in the 2SLS model.

The results above do not change if a different criterion for defining the reformers and non-reformers is applied within each 20-year period. When I use the mean of reforms across all countries instead of the median to distinguish the timing of the reform, the ERs still appear better-off than both the LRs and the non-reformers. The significance is lost for the MRs though which might indicate that the results are sensitive to how the reformers are defined. Just as before, the APF tests point to a possible weakness of the instruments.

Using the compound GDP/c. growth as the explained variable brings additional information on the growth effect from the deregulation reforms since 1975. Table 2.3 presents the results obtained from the compound GDP/c. growth regressions.

While in the previous estimations it was evident that the one-shot growth effect was different for the various types of reformers, the effect on growth

acceleration is far less obvious. There is no significant difference between the various types of reformers in models (1) and (2), which indicates that an overall deregulation and liberalization of the labor-, product- and credit markets may not cause growth acceleration over a 20-year period. This is evident from the insignificant estimates on both the overall reform variables and the credit market reform variables (ER, LR, MR and CMER, CMLR, CMMR, respectively). However, there appears to be a large positive and significant acceleration effect from trade liberalization alone, which adds evidence to the gains from trade liberalization literature.

It would be naïve to treat the above results as unbiased and consistent without questioning a few important features of the model and the data. First, the model uses too few observations. Although data limitations are a natural weakness of models which go as far back as the 1970s, the number of observations could be increased significantly. Second, the 2SLS estimations do not use the rents from natural resources and resort to instrumenting reforms with the energy dependence alone.

Third, the results above also ignore important time-invariant country characteristics which might affect both living standards and growth prospects of any of the countries in the sample. Also, 1990 draws a meaningful division line between early and late reformers due to the fact that the bulk of the reforms were done after 1990 for most of the countries. However, imposing 1990 on all countries at the same time kills a lot of cross-country heterogeneity in reform patterns. Therefore, an interesting remaining question is how does a country's own deregulation reform pattern – not the relative reform pattern to the other countries in the distribution of reformers – influence the growth outcomes. To address these issues, panel data methods could significantly help.

Fourth, the above results are produced with the PWT data on GDP and GDP growth. It would be interesting to see if the results hinge on the choice of dependent variables data source. Those weaknesses of the model are addressed in the next section.

2.5 Robustness Checks

To address the weaknesses of the baseline model, the following modifications are applied. First, the period starting in 1970 is shortened by 5 years. Starting in 1975 allows for more observations to enter into consideration. This is so, because not all countries were observed in 1970 but were observed in 1975. Using 1970 instead of 1975 excludes them for the entire period through 2010 because the reform variables cannot be created without knowing the initial level of regulation. By using the more abundant data which starts in 1975, the sample size is raised from 39 to 66 observations of overall reforms, and from 65 to 89 observations of CMR reforms. Table 2.4 lists the countries in the 1975-2010 sample.

Second, while the initial starting point of the reforms is kept at 1975 to use the newly available observations, energy independence is dropped and natural resource rents are used now to predict the timing of reforms in the first stage of the 2SLS estimations. This allows for checking if the results crucially depend on the choice of instruments.

Third, four sets of panel data estimations were conducted for both the GDP and the growth rate of country i in period t. Initially, I use the overall deregulation reform, the trade reform and the property rights reform indices I_{it} directly.[16] Then, in the remaining three sets of estimations, I construct

[16]I did not use the property rights indices in the baseline model because it would have limited the number of observations even further.

reform variables in the following way: a) a country is a reformer if $I_{it} > I_{it-1}$, and a non-reformer otherwise; b) a country is a reformer if $I_{it} > \text{Median}(I_i)$, and a non-reformer otherwise; c) a country is a reformer if $I_{it} > \text{Mean}(I_i)$, and a non-reformer otherwise. The advantage of those four methods of constructing a reform variable is that all of them avoid relating a country to the entire distribution of reforms within a period. Rather, a country is considered a reformer based on its own merit, and its own path of reforms. In case a), a country is a reformer in a given area if the reform index I in that area was higher than in the previous period. In case b), the median is taken over the entire time span of reforms in a given area for that country. When the country goes over the median of its own reforms, it becomes a reformer. In case c), the median is substituted with the mean of reforms.

Fourth, the PWT is admittedly one of the most comprehensive sources of country level panel data. However, one of the criticisms aimed at the different versions of the PWT dataset is that they lead to a systematic variability of the levels and the growth estimates.[17] Therefore, in the final robustness check, I repeat the baseline exercise but use the WDI data instead of the PWT.

2.5.1 Shortening the Time Span

The results from repeating the baseline model on a shorter time span are given in Table 2.5. Table 2.5 repeats the main message from Table 2.2. Early and Marathon reformers differ significantly from the rest of the reformers, and enjoy higher living standards. In addition, the growth acceleration in Table 2.6 of the ERs and MRs was not significantly higher than the one of

[17]See for example, Johnson, Larson, Papageorgiou, and Subramanian (2012) about the differences within the PWT versions and Hanousek et al. (2008) about the differences stemming from using different sources such as PWT and the World Development Indicators (WDI).

the non-reformers, which is still a robust result.

However, two of the results seem unintuitive and hard to reconcile at first glance. First, why do the overall early and marathon reformers have higher living standards but insignificantly higher growth acceleration? After all, the higher living standards have to come from a growth process. Second, why do late CMR reformers have lower GDP levels but higher growth acceleration? When reconciling the finding that there is a significant level effect from the overall deregulation reform without an apparent acceleration effect, we have to bear in mind that all the results are relative to the non-reformers. The fact that there is no overall acceleration effect means that the ERs and MRs are growing at similar rates as the non-reformers. However, a growth of 2% from a base of 100 produces a different GDP level than that of 2% from a base of 50. Therefore, ERs and MRs have indeed become relatively richer than before, while growing at the same rate as the non-reformers. This shows up as a significant level effect without a significant growth acceleration effect.

Similar logic applies to the second unintuitive result. The LRs were poorer in the first period, while in the second they grew much faster than the non-reformers. However, they could not catch up by 2010. This surfaces as a positive growth acceleration effect without the apparent and intuitive level effect. Thus, it appears that a large catch up process exists for those economies that reformed extensively in the 1990s. This can lead to dramatic differences in GDP per capita levels, when we take into account the long-term dynamic gains from such a large annual margin in favor of the late reformers. Yet, the results here should once again be interpreted with caution, since the first stage APF test points to rather weak instruments, despite their validity indicated by the Hansen J test.

2.5.2 Substituting Rents for Energy Independence

Table 2.7 presents the results from the 2SLS estimates of the baseline equation when rents substitute for the energy independence. The message the table delivers is that, again, ERs and MRs have higher living standards while they do not necessarily grow faster than the LRs and the non-reformers. The results are somewhat stronger in terms of instruments testing as well. While the Hansen test cannot reject the validity of the instruments in most cases, the APF test is higher than 10 in more cases than before.

2.5.3 Panel Data Estimation

Tables 2.8 through 2.15 present the results from panel data versions of the baseline model. The initial panel OLS model is $y_{it} = \beta_1 + \beta_2 \mathrm{y}_{it-1} + \beta_3(\mathrm{CM})\mathrm{R}_{it} + \beta_4 \mathrm{PR}_{it} + \beta_5 \mathrm{T}_{it} + \mathbf{X'}_{it}\beta + \varepsilon_{it}$, where y_{it} is either the Log(GDP) per capita of country i in period t, or its growth rate over the same period; $(CM)R$ is either the index of credit market regulation (CMR) or the index of overall regulation (R); and PR and T are country-specific indices of property rights and trade policies, respectively. The other controls in the X' vector are the sum of exports and imports in GDP, log of population and log of the country's exchange rate to the US dollar, respectivey.

Before running the models, the Harris-Tzavalis panel data unit root test was conducted. The Harris-Tzavalis test is the appropriate test to use with the current data due to the proximity of its underlying assumptions to the traits of the data: large N, and fixed T periods. The test detects a unit root for the entire panel of the $Log(GDP/c.)_{it}$ series, while it strongly rejects the non-stationarity of the $\Delta Log(GDP/c.)_{it}$ data. Due to the non-stationarity of most of the underlying data, especially of the per capita GDP series, the models are estimated in differences, and presented in Tables 2.8 through 2.15.

In each table, two sets of three estimations each were done. The first set of three models corresponds to the overall effect from deregulation, while the second set of models corresponds to the effects from liberalizing credit markets. Within each set of three models, the following estimations were conducted: i) Panel OLS with country and time fixed effects (FEs) and clustered standard errors (SEs); ii) Panel 2SLS with country and time FEs and clustered SEs, where energy independence instrumented for the change in the reform index; iii) Panel 2SLS with country and time FEs and clustered SEs, where rents instrumented for the change in the reform index.

Table 2.8 uses the reform indices *directly*. In Table 2.9 the indices are transformed into reform variables by using the country's *own path* of reforms for each of the reforms studied. Specifically, a country i is considered a reformer in a given area in time t if the index of reforms I in that particular reform area is higher in time t than in $t-1$. In Table 2.10 the indices are transformed into reform variables by using the country's *own median* of reforms over the entire period, while Table 2.11 uses the country's *own mean* of reforms over the entire period. When a given reform index passes above its own median/mean, then the country is considered a reformer in that particular area. Tables 2.8 through 2.11 use the $Log(GDP/c.)_{it}$ as the dependent variable, while Tables 2.12 through 2.15 repeat the above work for the compound growth rate g_{it}. To accommodate the data availability in the EFW reform indices, the time interval in each panel data estimation is set at 5 years.

Most panel data estimations point to a recurrent message: both overall and CMR reforms are associated with higher living standards. Unlike the baseline models, however, the panel data estimates add another conclusion: both the overall and the CMR reforms can accelerate growth. This result

could be driven by either the more appropriate panel data methods or by a natural trait of growth accelerations: that they are only temporary, as Hausmann et al. (2005) suggest. Thus, it is only natural that a reform could not produce a growth acceleration effect over a 20-year period, since accelerations happen over shorter horizons. This work adds to their evidence by slicing the data into 5-year periods and concluding that both overall and CMR reforms could produce growth accelerations.

In line with the recent evidence of a non-linear relationship between financial development and growth (Arcand et al., 2012; Cecchetti & Kharroubi, 2012), and the underlying non-linearity of the effects of reforms on growth, I add a variable that could potentially capture this non-linearity in the final panel data estimations: the reform gap. Similarly to Chapter 1, the reform gap in a given area – overall reform or CMR – for country i in period t is defined as $Gap_{it} = Max\{I_{rt}\} - I_{it}$, i.e., the contemporaneous difference between the maximum level of an overall or a financial reform in region r and the level of overall or financial liberalization reform, respectively, within the given economy i. Both the levels and the squared terms of the reform gap are included in the panel estimations. It is interesting to note that including the gap renders some of the baseline reform effects insignificant but whenever that happens, the gap itself and the squared term of the gap become significant. The signs of both are negative, which suggests that closing in on the reform leaders in the region may bring the desired growth effects only up to a certain point, after which the positive effects of reforming start fading. This results could be treated as an additional evidence in favor of the recently discussed non-linear relationship between finance and growth (Arcand et al., 2012; Cecchetti & Kharroubi, 2012; Cecchetti et al., 2011).

2.5.4 Transition Bias

Table 2.1 and Table 2.4 allocate the countries across the various types of reformers between 1970–2010 and 1975–2010, respectively. They indicate that 6 transition countries are present in the sample: Bulgaria, Czech Republic, Hungary, Poland, Romania and Russia. All of them are either late or marathon reformers in both the overall reform trends and the credit market liberalization. At the same time, those countries experienced severe recessions after 1990 which may lead to the expectation that the results presented here are biased. This check is intended to investigate if the benchmark results are driven by the presence of transition economies.

By construction, the main results could not have possibly been driven by the presence of transition economies. This is so because despite the availability of data on both the GDP per capita and the overall and credit market liberalization indices, data on trade indices for all transition countries is missing. This means FTER, FTLR and FTMR are also missing for the transition countries. Therefore, the benchmark model is estimated without the transition countries in the first place.

To make sure transition countries are not causing any biases, a dummy for transition countries was interacted with a time dummy, and included in the panel model. The panel model was estimated in differences so including a dummy for the entire region in the presence of country fixed effects would hardly make sense. However, there were factors driving the transition period which affected the entire region at approximately the same time, so a region-time dummy would make more sense. Yet, as the results remain virtually identical, I conclude that the benchmark results are not driven by a transition bias.

2.5.5 Using the WDI Data

Instead of the PWT data, the last set of robustness checks uses the WDI data to measure both GDP and growth rates, and repeat the baseline model. As expected, the results from this check do not significantly differ from the baseline results. They are robust both in terms of magnitude and significance.

The results from the four broad sets of robustness checks here demonstrate that there is indeed a robust positive relationship between being early and a marathon reformer and the levels of per capita GDP. In addition, reforming credit markets late is associated with lower levels of per capita GDP but with a faster catch up process emanated in a significant growth acceleration effect for the late reformers. The panel data methods and using another major data source on the dependent variable corroborates the baseline findings. The results in this section are sufficient to conclude this work about the effect of deregulation on economic growth.

2.6 Conclusion

The effects from deregulation on living standards and on growth vary across economies and across the timing of the deregulation reform. The countries that lagged behind in their deregulation reform in the 1970s and the 1980s but accelerated the reform in the 1990s and early in the new century had lower per capita GDP levels than the early reformers and those countries that reformed extensively in both periods – the "marathon" reformers. This means deregulating early and continuously is also associated with higher living standards. However, when it comes to growth acceleration, there is no significant difference between the various types of overall deregulation reformers. This result suggests that an overall reform does not necessarily

cause growth accelerations over long periods of time.

In order to analyze the impact of a more specific reform, I consider the impact of deregulation on the credit markets alone. Although the results are not as strong as before in terms of magnitude, the sign remains indicative of the inherent difference between the early and the late reformers: late credit market deregulation is also associated with being poorer.

There appears to be a large positive and significant effect on both living standards and on growth rates from both the overall and the credit market deregulation. This result surfaced from the robustness checks in which the data was sliced into shorter 5-year time periods, and panel data methods were applied. The results also lead to the conclusion that growth accelerations could indeed be only temporary events, as the literature has suggested. The positive effect from reforms is also supported by the other robustness checks.

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Table 2.1: List of Reformers and Types of Reforms, 1970-2010

	Median Criterion	Mean Criterion
	Overall deregulation reforms	
ER	Australia, France, Greece, Hong Kong, Indonesia, Ireland, Jordan, Luxembourg, Malaysia, New Zealand, Singapore, South Africa, Spain, Thailand, United Kingdom, United States	Australia, Hong Kong, Indonesia, Ireland, Jordan, Luxembourg, Malaysia, New Zealand, Singapore, Thailand, United Kingdom, United States
LR	Argentina, China, Congo (Kinshasa), Finland, Germany, Israel, Italy, Madagascar, Netherlands, Norway, Portugal, Romania, Switzerland, Syria	Argentina, Belgium, China, Congo (Kinshasa), Denmark, Finland, Germany, Israel, Italy, Madagascar, Netherlands, Portugal, Romania, Switzerland, Syria
MR	Austria, Belgium, Denmark, Sweden, Tanzania, Tunisia	Austria, Sweden, Tanzania, Tunisia
NR	Brazil, Canada, Guatemala, India, Japan, Kenya, Pakistan, Venezuela	Brazil, Canada, France, Greece, Guatemala, India, Japan, Kenya, Norway, Pakistan, South Africa, Spain, Venezuela
	Credit market liberalization reforms	
ER	Australia, Bolivia, Central African Republic, Congo (Brazzaville), Cyprus, Denmark, France, Greece, Indonesia, Ireland, Jamaica, Japan, Malaysia, Mauritius, Morocco, Netherlands, New Zealand, Philippines, Portugal, Singapore, Spain, Sweden, Thailand, Tunisia, United Kingdom, United States	Australia, Austria, Bolivia, Central African Republic, Congo (Brazzaville), Cyprus, Denmark, France, Gabon, Greece, India, Indonesia, Ireland, Jamaica, Japan, Malaysia, Mauritius, Morocco, Netherlands, New Zealand, Norway, Philippines, Portugal, Singapore, Spain, Sweden, Thailand, Tunisia, United Kingdom, United States
LR	Argentina, Brazil, China, Congo (Kinshasa), Czech Republic, Fiji, Ghana, Guyana, Haiti, Iran, Italy, Jordan, Kuwait, Madagascar, Malawi, Niger, Pakistan, Peru, Romania, Sierra Leone, Syria, Trinidad and Tobago, Turkey, Uganda, Zambia	Argentina, Brazil, Burundi, China, Congo (Kinshasa), Czech Republic, Fiji, Ghana, Guyana, Haiti, Iran, Jordan, Kuwait, Madagascar, Malawi, Niger, Pakistan, Peru, Romania, Sierra Leone, Syria, Turkey, Uganda, Zambia
MR	Algeria, Austria, Bangladesh, Benin, Bulgaria, Burundi, Cameroon, Ecuador, Gabon, Hungary, India, Israel, Malta, Mexico, Nepal, Nigeria, Norway, Poland, Russia, Rwanda, Tanzania	Algeria, Bangladesh, Benin, Bulgaria, Cameroon, Ecuador, Hungary, Israel, Malta, Mexico, Nepal, Nigeria, Poland, Russia, Rwanda, Tanzania
NR	Bahamas, Bahrain, Barbados, Belgium, Canada, Costa Rica, Dominican Republic, El Salvador, Finland, Germany, Guatemala, Honduras, Hong Kong, Iceland, Kenya, South Korea, Luxembourg, Mali, Panama, South Africa, Switzerland, Venezuela	Bahamas, Bahrain, Barbados, Belgium, Canada, Costa Rica, Dominican Republic, El Salvador, Finland, Germany, Guatemala, Honduras, Hong Kong, Iceland, Italy, Kenya, South Korea, Luxembourg, Mali, Panama, South Africa, Switzerland, Trinidad and Tobago, Venezuela

Notes: The table lists the countries in the 1970-2010 sample, and classifies them into early- (ER), late- (LR), marathon- (MR) and non-reformers (NR), according to two possible criteria: the median and the mean of the reforms distribution. Two reform areas are considered: overall deregulation, and credit market liberalization.

Table 2.2: Deregulation and Average Levels of GDP/c.: 1970-2010

	Median criterion				Mean criterion			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
ER	.397***	.426***			.456***	.496***		
	(.135)	(.140)			(.133)	(.142)		
LR	-.002	-.030			-.047	.006		
	(.131)	(.150)			(.105)	(.126)		
MR	.188*	.267**			.178	.215		
	(.105)	(.120)			(.122)	(.131)		
FTER	.095	.092	.034	-.122	-.097	-.164	.010	-.127
	(.095)	(.113)	(.113)	(.182)	(.105)	(.113)	(.118)	(.178)
FTLR	.221	.240	.126	.225	.041	.040	.110	.240
	(.329)	(.312)	(.107)	(.228)	(.240)	(.233)	(.107)	(.217)
FTMR	.326	.308	.137	.218	.412	.554*	.178	.278
	(.345)	(.303)	(.146)	(.252)	(.290)	(.292)	(.161)	(.250)
RGDPc'70	.110	.112	.046	.054	.109	.118	.034	.032
	(.120)	(.108)	(.050)	(.055)	(.110)	(.102)	(.049)	(.051)
CMER			.144	.351			.125	.272
			(.117)	(.220)			(.110)	(.231)
CMLR			-.291**	-.411*			-.349**	-.484**
			(.142)	(.217)			(.147)	(.227)
CMMR			-.060	-.099			-.083	-.200
			(.141)	(.203)			(.158)	(.228)
Const.	-.895	-.928	-.125	-.210	-.720	-.806	.000	-.012
	(1.180)	(1.062)	(.473)	(.600)	(1.038)	(.957)	(.474)	(.557)
Obs.	39	39	65	65	39	39	65	65
Adj. R^2	.184		.145		.325		.150	
Hansen J		.347		.245		.553		.182
APF-ER		9.289		6.015		16.177		4.945
APF-LR		2.379		6.422		8.306		9.691
APF-MR		4.891		13.716		15.175		7.521

Notes: The estimated OLS equation is $\Delta\text{Avg.}\log(GDP)_{it} = \beta_1 + \beta_2 \text{ER}_{it} + \beta_3 \text{LR}_{it} + \beta_4 \text{MR}_{it} + \beta_5 X_{it} + \Delta\varepsilon_{it}$, where $\Delta\text{Avg.}\log(GDP)_{it}$ is the difference in the average log-levels of per capita GDP. The variables ER, LR and MR are the Early-, Late-, and Marathon reformers, respectively, as defined in the text. The variables CM-ER, CM-LR and CM-MR are the early-, late-, and marathon reformers in the credit market regulation as defined in the text. The variables FT-ER, FT-LR and FT-MR are the early-, late-, and marathon reformers in the freedom to trade internationally as defined in the text. RGDPc'70 is the log of real GDP per capita in 1970. Robust standard errors are presented in parentheses. Hansen J is the P-value on the Hansen overidentification test. APF- is the value of the Angrist-Pischke first stage F-test on the endogenous variables of interest. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.3: Deregulation and Compound Economic Growth: 1970-2010

	Median criterion				Mean criterion			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
ER	.276	.103			-.321	-.417		
	(.871)	(.761)			(.659)	(.651)		
LR	.238	.163			-.208	-.547		
	(.775)	(.646)			(.517)	(.563)		
MR	.618	.308			.481	.417		
	(.827)	(.750)			(.660)	(.572)		
FTER	-.340	-.259	-.553	.621	.003	.141	-.094	1.473**
	(.517)	(.510)	(.549)	(.787)	(.460)	(.502)	(.502)	(.715)
FTLR	.712	.753	.441	2.285**	.920	1.055	.622	2.018*
	(.926)	(.866)	(.801)	(1.132)	(.727)	(.677)	(.688)	(1.038)
FTMR	1.206	.860	.838	2.550**	.704	-.881	.895	2.482**
	(1.053)	(.987)	(.757)	(1.215)	(.863)	(1.906)	(.764)	(1.241)
RGDPc'70	.181	.176	.065	.361	.219	.186	.152	.392
	(.191)	(.175)	(.225)	(.237)	(.180)	(.170)	(.220)	(.244)
CMER			-.237	.123			-.376	-.428
			(.541)	(1.129)			(.516)	(1.095)
CMLR			.934	1.325			1.316	1.645
			(.811)	(.975)			(.812)	(1.067)
CMMR			.064	.379			.428	.752
			(.789)	(.945)			(1.027)	(1.085)
Const.	-2.521	-2.365	-.977	-4.844*	-2.656	-2.258	-1.961	-5.080*
	(2.235)	(2.061)	(2.228)	(2.807)	(2.015)	(1.882)	(2.176)	(2.769)
Obs.	39	39	65	65	39	39	65	65
Adj. R^2	-.097		.067		-.091		.087	
Hansen J		.264		.174		.340		.249
APF-ER		9.289		6.015		16.177		4.945
APF-LR		2.379		6.422		8.306		9.691
APF-MR		4.891		13.716		15.175		7.521

Notes: The estimated OLS equation is $\Delta g_{it} = \beta_1 + \beta_2 \text{ER}_{it} + \beta_3 \text{LR}_{it} + \beta_4 \text{MR}_{it} + \beta_5 X_{it} + \Delta\varepsilon_{it}$, where Δg_{it} is the difference in the compound growth rate between the two periods. The variables ER, LR and MR are the Early-, Late-, and Marathon reformers, respectively, as defined in the text. The variables CM-ER, CM-LR and CM-MR are the early-, late-, and marathon reformers in the credit market regulation as defined in the text. The variables FT-ER, FT-LR and FT-MR are the early-, late-, and marathon reformers in the freedom to trade internationally as defined in the text. RGDPc'70 is the log of real GDP per capita in 1970. Robust standard errors are presented in parentheses. Hansen J is the P-value on the Hansen overidentification test. APF- is the value of the Angrist-Pischke first stage F-test on the endogenous variables of interest. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.4: List of Reformers and Types of Reforms, 1975-2010

	Median Criterion	Mean Criterion
	Overall deregulation reforms	
ER	Australia, Bangladesh, Chile, Greece, Hong Kong, Iceland, India, Indonesia, Ireland, Jordan, Kenya, South Korea, Luxembourg, Malaysia, Mauritius, New Zealand, Pakistan, Philippines, Singapore, South Africa, Thailand, Turkey, United Kingdom	Australia, Bangladesh, Chile, Greece, Hong Kong, Iceland, India, Indonesia, Ireland, Jordan, Kenya, South Korea, Luxembourg, Malaysia, Mauritius, New Zealand, Pakistan, Philippines, Singapore, Thailand, Turkey, United Kingdom
LR	Argentina, Bahamas, Bahrain, Burundi, China, Congo (Kinshasa), Finland, Germany, Ghana, Israel, Kuwait, Mali, Nigeria, Norway, Portugal, Romania, Russia, Sierra Leone, Syria, Uganda, Zambia	Argentina, Bahamas, Bahrain, Burundi, China, Congo (Kinshasa), Finland, Germany, Ghana, Israel, Kuwait, Nigeria, Portugal, Romania, Russia, Syria, Uganda, Zambia
MR	Austria, Belgium, Denmark, Fiji, Italy, Mexico, Netherlands, Rwanda, Sweden, Switzerland, Tanzania, Tunisia	Austria, Belgium, Denmark, Fiji, Italy, Mexico, Netherlands, Rwanda, Sweden, Switzerland, Tanzania, Tunisia
NR	Barbados, Brazil, Canada, Costa Rica, Cyprus, France, Guatemala, Japan, Malawi, Niger, Panama, Spain, Trinidad and Tobago, United States, Venezuela	Barbados, Brazil, Canada, Costa Rica, Cyprus, France, Guatemala, Japan, Malawi, Mali, Niger, Norway, Panama, Sierra Leone, South Africa, Spain, Trinidad and Tobago, United States, Venezuela
	Credit market liberalization reforms	
ER	Australia, Bahamas, Belgium, Bolivia, Canada, Central African Republic, Chad, Chile, Congo (Brazzaville), Egypt, Greece, Iceland, Indonesia, Ireland, Japan, Kenya, South Korea, Malaysia, Mauritius, Morocco, Netherlands, New Zealand, Oman, Panama, Philippines, Portugal, Sri Lanka, Sweden, Switzerland, Togo, Tunisia, United Kingdom, Uruguay, Zimbabwe	Australia, Belgium, Bolivia, Central African Republic, Chad, Chile, Congo (Brazzaville), Egypt, Gabon, Greece, Iceland, India, Indonesia, Ireland, Japan, Kenya, South Korea, Malaysia, Mauritius, Netherlands, New Zealand, Oman, Philippines, Portugal, Sri Lanka, Sweden, Switzerland, Togo, Tunisia, United Kingdom, Uruguay, Zimbabwe
LR	Algeria, Argentina, Botswana, Brazil, Burundi, China, Congo (Kinshasa), Czech Republic, Ecuador, Ghana, Haiti, Iran, Jordan, Kuwait, Madagascar, Malta, Mexico, Niger, Norway, Papua New Guinea, Peru, Poland, Romania, Russia, Sierra Leone, Syria, Trinidad and Tobago, Turkey, Uganda, Zambia	Algeria, Argentina, Botswana, Brazil, Burundi, China, Congo (Kinshasa), Czech Republic, Ecuador, Ghana, Guyana, Haiti, Iran, Israel, Jordan, Kuwait, Madagascar, Malawi, Malta, Mexico, Niger, Peru, Poland, Romania, Russia, Sierra Leone, Syria, Turkey, Uganda, Zambia
MR	Austria, Bangladesh, Benin, Bulgaria, Cameroon, Cote d'Ivoire, Fiji, Gabon, Guyana, Hungary, India, Israel, Italy, Malawi, Nepal, Nigeria, Pakistan, Rwanda, Senegal, Tanzania	Bangladesh, Benin, Bulgaria, Cameroon, Cote d'Ivoire, Fiji, Hungary, Nepal, Nigeria, Pakistan, Rwanda, Senegal, Tanzania
NR	Bahrain, Barbados, Belize, Colombia, Costa Rica, Cyprus, Denmark, Dominican Republic, El Salvador, Finland, France, Germany, Guatemala, Honduras, Hong Kong, Jamaica, Luxembourg, Mali, Singapore, South Africa, Spain, Thailand, United Arab Emirates, United States, Venezuela	Austria, Bahamas, Bahrain, Barbados, Belize, Canada, Colombia, Costa Rica, Cyprus, Denmark, Dominican Republic, El Salvador, Finland, France, Germany, Guatemala, Honduras, Hong Kong, Italy, Jamaica, Luxembourg, Mali, Morocco, Norway, Panama, Papua New Guinea, Singapore, South Africa, Spain, Thailand, Trinidad and Tobago, United Arab Emirates, United States, Venezuela

Notes: The table lists the countries in the 1975-2010 sample, and classifies them into early- (ER), late- (LR), marathon- (MR) and non-reformers (NR), according to two possible criteria: the median and the mean of the reforms distribution. Two reform areas are considered: overall deregulation, and credit market liberalization.

Table 2.5: Deregulation and Average Levels of GDP/c.: 1975-2010

	Median criterion				Mean criterion			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
ER	.340***	.459***			.346***	.430***		
	(.084)	(.111)			(.082)	(.128)		
LR	.008	.055			-.025	-.153		
	(.090)	(.148)			(.104)	(.201)		
MR	.079	.257**			.062	.167		
	(.063)	(.117)			(.061)	(.119)		
FTER	-.007	-.038	-.030	-.094	-.028	-.023	.012	-.053
	(.079)	(.136)	(.080)	(.162)	(.072)	(.128)	(.082)	(.155)
FTLR	.038	.084	.086	.123	.001	.084	.078	.094
	(.107)	(.151)	(.096)	(.196)	(.096)	(.159)	(.088)	(.176)
FTMR	.047	.059	.155	-.050	.033	.191	.189*	.009
	(.122)	(.162)	(.107)	(.176)	(.110)	(.225)	(.102)	(.194)
RGDPc'75	.084**	.091**	.059*	.045	.078**	.088**	.039	.017
	(.039)	(.037)	(.033)	(.041)	(.035)	(.035)	(.031)	(.042)
CMER			.075	.049			.073	-.055
			(.079)	(.170)			(.080)	(.143)
CMLR			-.290***	-.276**			-.317***	-.375**
			(.096)	(.140)			(.090)	(.147)
CMMR			-.098	-.171			-.145	-.284
			(.094)	(.190)			(.105)	(.230)
Const.	-.582	-.725*	-.231	-.046	-.503	-.649*	-.069	.228
	(.376)	(.372)	(.317)	(.457)	(.342)	(.371)	(.293)	(.453)
Obs.	66	66	89	89	66	66	89	89
Adj. R^2	.257		.178		.288		.195	
Hansen J		.385		.428		.512		.516
APF-ER		4.521		3.312		3.827		7.320
APF-LR		5.319		12.488		12.334		8.601
APF-MR		8.479		9.705		11.629		25.924

Notes: The estimated OLS equation is $\Delta\text{Avg. log}(GDP)_{it} = \beta_1 + \beta_2 \text{ER}_{it} + \beta_3 \text{LR}_{it} + \beta_4 \text{MR}_{it} + \beta_5 X_{it} + \Delta\varepsilon_{it}$, where $\Delta\text{Avg. log}(GDP)_{it}$ is the difference in the average log-levels of per capita GDP. The variables ER, LR and MR are the Early-, Late-, and Marathon reformers, respectively, as defined in the text. The variables CM-ER, CM-LR and CM-MR are the early-, late-, and marathon reformers in the credit market regulation as defined in the text. The variables FT-ER, FT-LR and FT-MR are the early-, late-, and marathon reformers in the freedom to trade internationally as defined in the text. RGDPc'75 is the real GDP per capita in 1975. Robust standard errors are presented in parentheses. Hansen J is the P-value on the Hansen overidentification test. APF- is the value of the Angrist-Pischke first stage F-test on the endogenous variables of interest. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.6: Deregulation and Compound Economic Growth: 1975-2010

	Median criterion				Mean criterion			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
ER	-1.132	-2.364**			-1.008	-2.000*		
	(.848)	(1.162)			(.779)	(1.118)		
LR	-.275	-.978			.293	-.260		
	(1.060)	(1.422)			(1.093)	(1.063)		
MR	-.487	-1.494			-.295	-.897		
	(.750)	(1.056)			(.653)	(.799)		
FTER	-.393	.216	-.238	.472	-.138	.286	-.073	1.071
	(.524)	(1.165)	(.545)	(1.418)	(.505)	(1.013)	(.528)	(1.272)
FTLR	.403	2.546**	.293	1.102	.903	2.930**	.801	2.383
	(1.158)	(1.274)	(.792)	(1.432)	(1.179)	(1.330)	(.847)	(1.690)
FTMR	.977	2.168	.469	1.803	1.372	2.325	.912	2.444*
	(.841)	(1.549)	(.762)	(1.340)	(.898)	(1.557)	(.821)	(1.371)
RGDPc'75	-.263	-.064	.018	.271	-.228	-.024	.111	.489
	(.246)	(.253)	(.269)	(.335)	(.270)	(.256)	(.279)	(.343)
CMER			.073	1.235			.264	1.597
			(.592)	(1.201)			(.552)	(1.111)
CMLR			1.864**	4.053***			1.679*	3.308***
			(.917)	(1.182)			(.877)	(1.218)
CMMR			1.218	1.703			1.242	2.141
			(.753)	(1.624)			(.861)	(1.882)
Const.	2.852	1.037	-.513	-4.353	2.112	.170	-1.490	-6.646*
	(2.246)	(2.551)	(2.625)	(3.466)	(2.466)	(2.744)	(2.690)	(3.664)
Obs.	66	66	89	89	66	66	89	89
Adj. R^2	.024		.072		.055		.060	
Hansen J		.391		.586		.439		.495
APF-ER		4.521		3.312		3.827		7.320
APF-LR		5.319		12.488		12.334		8.601
APF-MR		8.479		9.705		11.629		25.924

Notes: The estimated OLS equation is $\Delta g_{it} = \beta_1 + \beta_2 \text{ER}_{it} + \beta_3 \text{LR}_{it} + \beta_4 \text{MR}_{it} + \beta_5 X_{it} + \Delta\varepsilon_{it}$, where Δg_{it} is the difference in the compound growth rate between the two periods. The variables ER, LR and MR are the Early-, Late-, and Marathon reformers, respectively, as defined in the text. The variables CM-ER, CM-LR and CM-MR are the early-, late-, and marathon reformers in the credit market regulation as defined in the text. The variables FT-ER, FT-LR and FT-MR are the early-, late-, and marathon reformers in the freedom to trade internationally as defined in the text. RGDPc'75 is the real GDP per capita in 1975. Robust standard errors are presented in parentheses. Hansen J is the P-value on the Hansen overidentification test. APF- is the value of the Angrist-Pischke first stage F-test on the endogenous variables of interest. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.7: Deregulation and Growth, 1975-2010: Using Rents

	Level effect				Acceleration effect			
	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS	(7) 2SLS	(8) 2SLS
ER	.498***		.474***		-1.916		-1.645	
	(.126)		(.119)		(1.546)		(1.264)	
LR	.047		-.054		.022		.829	
	(.123)		(.148)		(1.798)		(1.610)	
MR	.320*		.230		1.048		1.223	
	(.189)		(.163)		(1.517)		(1.173)	
FTER	-.049	-.088	-.072	.134	-.926	-1.876	-.355	-.665
	(.149)	(.234)	(.125)	(.191)	(1.190)	(2.114)	(1.033)	(1.401)
FTLR	.121	.184	.021	.283*	2.498*	.854	3.123**	1.904
	(.159)	(.158)	(.132)	(.150)	(1.488)	(1.446)	(1.469)	(1.377)
FTMR	.111	.147	.093	.471**	1.407	.618	1.998	1.028
	(.144)	(.172)	(.142)	(.183)	(1.395)	(1.717)	(1.373)	(1.419)
RGDPc'75	.105**	.078*	.088***	.058	-.092	.393	-.065	.347
	(.041)	(.045)	(.034)	(.037)	(.279)	(.410)	(.282)	(.369)
CMER		.230		.184		2.404		1.046
		(.207)		(.164)		(1.578)		(1.392)
CMLR		-.162		-.377***		3.649***		2.545**
		(.125)		(.137)		(1.208)		(1.281)
CMMR		-.139		-.290*		3.492*		2.194
		(.237)		(.171)		(2.101)		(1.735)
Const.	-.876**	-.467	-.649*	-.370	1.064	-4.893	.118	-4.167
	(.413)	(.456)	(.350)	(.371)	(2.881)	(4.150)	(2.809)	(3.912)
Obs.	63	84	63	84	63	84	63	84
Hansen J	.423	.070	.292	.176	.400	.651	.261	.332
APF-ER	21.265	5.337	5.345	6.594	21.265	5.337	5.345	6.594
APF-LR	26.397	3.475	24.571	1.240	26.397	3.475	24.571	1.240
APF-MR	2.588	3.543	2.545	28.718	2.588	3.543	2.545	28.718

Notes: The estimated OLS equation is $\Delta y_{it} = \beta_1 + \beta_2 \text{ER}_{it} + \beta_3 \text{LR}_{it} + \beta_4 \text{MR}_{it} + \beta_5 X_{it} + \Delta\varepsilon_{it}$, where Δy_{it} is the difference in either log(GDP) or the growth rate between the two periods. The variables ER, LR and MR are the Early-, Late-, and Marathon reformers, respectively, as defined in the text. The variables CM-ER, CM-LR and CM-MR are the early-, late-, and marathon reformers in the credit market regulation as defined in the text. The variables FT-ER, FT-LR and FT-MR are the early-, late-, and marathon reformers in the freedom to trade internationally as defined in the text. RGDPc'75 is the real GDP per capita in 1975. Robust standard errors are presented in parentheses. Hansen J is the P-value on the Hansen overidentification test. APF- is the value of the Angrist-Pischke first stage F-test on the endogenous variables of interest. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.8: Reforms and GDP, 1970-2010: Direct Use of Reform Indices

	Overall Deregulation			Credit Market Liberalization		
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
$\Delta L(GDPc)_{t-1}$	-0.04	0.01	-0.00	-0.03	-0.07	-0.07
	(0.04)	(0.07)	(0.05)	(0.04)	(0.06)	(0.05)
ΔR	0.02***	0.10	0.08*			
	(0.01)	(0.10)	(0.05)			
ΔPR	0.01***	0.01	0.01**	0.01**	0.01*	0.01**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
ΔT	0.01**	0.00	0.01	0.01**	0.02*	0.02***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\Delta Open$	0.00	-0.00	0.00	0.00	0.00	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\Delta L(Pop)$	-0.25	0.06	-0.09	-0.10	-0.14	-0.15
	(0.44)	(0.62)	(0.51)	(0.44)	(0.55)	(0.46)
$\Delta L(XRat)$	-0.04***	-0.03***	-0.03***	-0.03***	-0.04***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
ΔCMR				0.02***	-0.02	-0.01
				(0.00)	(0.04)	(0.03)
Const.	0.12***			0.05		
	(0.03)			(0.04)		
Obs.	687	545	615	704	549	630
No. countries	139	121	115	139	121	115
Adj. R^2	0.169			0.212		
Hansen J		0.47	0.07		0.52	0.05

Notes: The estimated Panel OLS equation is $\Delta Log(GDPc)_{it} = \beta_2 \Delta \text{L(GDPc)}_{it-1} + \beta_3 \Delta \text{(CM)R}_{it} + \beta_4 \Delta \text{PR}_{it} + \beta_5 \Delta \text{T}_{it} + \Delta \mathbf{X'}_{it}\beta + \Delta\varepsilon_{it}$. L(GDPc)$_{t-1}$ is the lagged Log(GDP) per capita; R, CMR, PR and T are country-specific indices of overall regulation, credit market regulation, property rights and trade policies, respectively. The variables Open, L(Pop) and L(XRat) are the sum of exports and imports in GDP, log of population and log of the country's exchange rate to the US dollar, respectively. Clustered standard errors are presented for the OLS and 2SLS estimations in parentheses. Hansen J is the P-value on the Hansen overidentification J test. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.9: Reforms and GDP, 1970-2010: A Country Reformed if $R_{it} > R_{it-1}$

	Overall Deregulation			Credit Market Liberalization		
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
$\Delta L(GDPc)_{t-1}$	-0.05	0.09	-0.05	-0.05	-0.03	-0.05
	(0.04)	(0.16)	(0.05)	(0.04)	(0.05)	(0.05)
ΔR	0.02**	0.70***	0.06			
	(0.01)	(0.20)	(0.12)			
ΔPR	0.02**	0.04	0.02**	0.02**	0.01	0.02**
	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
ΔT	0.04***	-0.04	0.03*	0.04***	0.03*	0.03**
	(0.01)	(0.04)	(0.02)	(0.01)	(0.02)	(0.01)
$\Delta Open$	0.00	-0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\Delta L(Pop)$	-0.08	0.96	-0.06	-0.08	0.09	-0.05
	(0.45)	(0.81)	(0.50)	(0.45)	(0.56)	(0.47)
$\Delta L(XRat)$	-0.03***	-0.02	-0.03***	-0.03***	-0.03***	-0.03***
	(0.00)	(0.02)	(0.01)	(0.00)	(0.01)	(0.00)
ΔCMR				0.03***	0.08	0.06
				(0.01)	(0.06)	(0.06)
Const.	0.02			0.01		
	(0.04)			(0.04)		
Obs.	687	545	615	704	549	630
No. countries	139	121	115	139	121	115
Adj. R^2	0.189			0.202		
Hansen J		0.49	0.05		0.40	0.09

Notes: The estimated Panel OLS equation is $\Delta Log(GDPc)_{it} = \beta_2 \Delta \text{L(GDPc)}_{it-1} + \beta_3 \Delta \text{(CM)R}_{it} + \beta_4 \Delta \text{PR}_{it} + \beta_5 \Delta \text{T}_{it} + \Delta \mathbf{X'}_{it}\beta + \Delta\varepsilon_{it}$. L(GDPc)$_{t-1}$ is the lagged Log(GDP) per capita; R, CMR, PR and T are dummies = 1 if $I_{it} > (I_{it-1})$, where I_{it} are country-specific indices of overall regulation, credit market regulation, property rights and trade policies, respectively. The variables Open, L(Pop) and L(XRat) are the sum of exports and imports in GDP, log of population and log of the country's exchange rate to the US dollar, respectively. Clustered standard errors are presented for the OLS and 2SLS estimations in parentheses. Hansen J is the P-value on the Hansen overidentification J test. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.10: Reforms and GDP, 1970-2010: A Country Reformed if $R_{it} >$ Median(R_i)

	Overall Deregulation			Credit Market Liberalization		
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
$\Delta L(GDPc)_{t-1}$	-0.08**	-0.11*	-0.03	-0.08**	-0.10	-0.07
	(0.04)	(0.07)	(0.09)	(0.04)	(0.07)	(0.05)
ΔR	0.02	0.23	-0.28			
	(0.01)	(0.16)	(0.25)			
ΔPR	0.01	0.02	0.01	0.01	0.01	0.02*
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)
ΔT	0.04***	0.03	0.08**	0.04***	0.01	0.05*
	(0.01)	(0.03)	(0.04)	(0.01)	(0.03)	(0.03)
$\Delta Open$	0.00	-0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\Delta L(Pop)$	0.26	0.13	0.30	0.23	-0.02	0.25
	(0.32)	(0.37)	(0.45)	(0.31)	(0.39)	(0.35)
$\Delta L(XRat)$	-0.03***	-0.02**	-0.04***	-0.02***	-0.00	-0.03**
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
ΔCMR				0.04***	0.41***	-0.06
				(0.01)	(0.12)	(0.22)
Const.	0.04			0.02		
	(0.03)			(0.03)		
Obs.	782	600	715	793	600	723
No. countries	142	139	132	142	139	132
Adj. R^2	0.151			0.163		
Hansen J		0.35	0.27		0.22	0.14

Notes: The estimated Panel OLS equation is $\Delta Log(GDPc)_{it} = \beta_2 \Delta \text{L(GDPc)}_{it-1} + \beta_3 \Delta \text{(CM)R}_{it} + \beta_4 \Delta \text{PR}_{it} + \beta_5 \Delta \text{T}_{it} + \Delta \mathbf{X'}_{it} \beta + \Delta \varepsilon_{it}$. L(GDPc)$_{t-1}$ is the lagged Log(GDP) per capita; R, CMR, PR and T are dummies = 1 if $I_{it} >$ Median(I_i), where I_t are country-specific indices of overall regulation, credit market regulation, property rights and trade policies, respectively. The variables Open, L(Pop) and L(XRat) are the sum of exports and imports in GDP, log of population and log of the country's exchange rate to the US dollar, respectively. Clustered standard errors are presented for the OLS and 2SLS estimations in parentheses. Hansen J is the P-value on the Hansen overidentification J test. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.11: Reforms and GDP, 1970-2010: A Country Reformed if $R_{it} >$ Mean(R_i)

	Overall Deregulation			Credit Market Liberalization		
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
$\Delta L(GDPc)_{t-1}$	-0.08**	-0.06	-0.08*	-0.09**	-0.19*	-0.01
	(0.04)	(0.06)	(0.04)	(0.04)	(0.11)	(0.09)
ΔR	0.03**	0.32***	-0.08			
	(0.01)	(0.11)	(0.17)			
ΔPR	0.02*	0.02	0.02**	0.02**	0.00	0.03**
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
ΔT	0.04***	-0.01	0.05	0.03***	-0.03	0.07**
	(0.01)	(0.02)	(0.03)	(0.01)	(0.05)	(0.03)
$\Delta Open$	0.00	-0.00	0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\Delta L(Pop)$	0.23	0.24	0.24	0.22	-0.03	0.29
	(0.32)	(0.33)	(0.37)	(0.31)	(0.38)	(0.42)
$\Delta L(XRat)$	-0.03***	-0.02***	-0.03***	-0.02***	-0.01	-0.04***
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
ΔCMR				0.04***	0.45*	-0.23
				(0.01)	(0.26)	(0.20)
Const.	0.03			0.02		
	(0.03)			(0.03)		
Obs.	782	600	715	793	600	723
No. countries	142	139	132	142	139	132
Adj. R^2	0.149			0.160		
Hansen J		0.11	0.14		0.25	0.23

Notes: The estimated Panel OLS equation is $\Delta Log(GDPc)_{it} = \beta_2 \Delta \text{L(GDPc)}_{it-1} + \beta_3 \Delta \text{(CM)R}_{it} + \beta_4 \Delta \text{PR}_{it} + \beta_5 \Delta \text{T}_{it} + \Delta \mathbf{X'}_{it} \beta + \Delta \varepsilon_{it}$. L(GDPc)$_{t-1}$ is the lagged Log(GDP) per capita; R, CMR, PR and T are dummies = 1 if $I_{it} >$ Mean(I_i), where I_t are country-specific indices of overall regulation, credit market regulation, property rights and trade policies, respectively. The variables Open, L(Pop) and L(XRat) are the sum of exports and imports in GDP, log of population and log of the country's exchange rate to the US dollar, respectively. Clustered standard errors are presented for the OLS and 2SLS estimations in parentheses. Hansen J is the P-value on the Hansen overidentification J test. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.12: Reforms and Growth, 1970-2010: Direct Use of Reform Indices

	Overall Deregulation			Credit Market Liberalization		
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
Δg_{it-1}	-0.43***	-0.36***	-0.41***	-0.43***	-0.42***	-0.40***
	(0.03)	(0.08)	(0.04)	(0.03)	(0.05)	(0.04)
ΔR	0.79***	7.12**	-0.17			
	(0.21)	(2.86)	(2.00)			
ΔPR	0.28**	0.12	0.28**	0.25*	0.15	0.30**
	(0.13)	(0.33)	(0.13)	(0.13)	(0.18)	(0.14)
ΔT	0.38***	-0.43	0.50*	0.35**	0.12	0.64**
	(0.13)	(0.44)	(0.29)	(0.14)	(0.39)	(0.26)
$\Delta Open$	0.01	-0.03	0.01	0.01	0.00	0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
$\Delta L(Pop)$	-9.48	6.46	-2.30	-4.72	2.47	-3.32
	(12.70)	(19.81)	(13.58)	(13.61)	(17.05)	(13.01)
$\Delta L(XRat)$	-0.52***	-0.03	-0.48***	-0.41***	-0.31	-0.61***
	(0.12)	(0.30)	(0.16)	(0.10)	(0.28)	(0.19)
ΔCMR				0.43***	1.37	-0.67
				(0.12)	(1.44)	(0.84)
Const.	0.56			0.19		
	(0.93)			(1.30)		
Obs.	634	535	565	645	539	574
No. countries	139	118	111	139	118	111
Adj. R^2	0.298			0.344		
Hansen J		0.42	0.10		0.33	0.12

Notes: The estimated Panel OLS equation is $\Delta g_{it} = \beta_2 \Delta g_{it-1} + \beta_3 \Delta(\text{CM})\text{R}_{it} + \beta_4 \Delta \text{PR}_{it} + \beta_5 \Delta \text{T}_{it} + \Delta \mathbf{X'}_{it}\beta + \Delta\varepsilon_{it}$. g_{it} is the country-specific compound growth rate.; R, CMR, PR and T are country-specific indices of overall regulation, credit market regulation, property rights and trade policies, respectively. The variables Open, L(Pop) and L(XRat) are the sum of exports and imports in GDP, log of population and log of the country's exchange rate to the US dollar, respectively. Clustered standard errors are presented for the OLS and 2SLS estimations in parentheses. Hansen J is the P-value on the Hansen overidentification J test. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.13: Reforms and Growth, 1970-2010: A Country Reformed if $R_t > R_{t-1}$

	Overall Deregulation			Credit Market Liberalization		
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
Δg_{it-1}	-0.43***	-0.47***	-0.41***	-0.43***	-0.42***	-0.42***
	(0.03)	(0.12)	(0.04)	(0.03)	(0.05)	(0.03)
ΔR	0.57*	20.54***	-1.56			
	(0.33)	(4.95)	(6.57)			
ΔPR	0.04	0.81	-0.08	-0.02	-0.33	0.01
	(0.24)	(1.02)	(0.55)	(0.26)	(0.34)	(0.26)
ΔT	0.95***	-1.39	1.22*	0.78**	0.55	0.79
	(0.34)	(1.15)	(0.69)	(0.32)	(0.56)	(0.50)
$\Delta Open$	0.01	-0.02	0.01	0.02*	0.00	0.01
	(0.01)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)
$\Delta L(Pop)$	-4.86	26.35	-2.77	-9.77	4.56	-0.77
	(13.91)	(24.94)	(13.42)	(12.54)	(16.51)	(14.14)
$\Delta L(XRat)$	-0.43***	-0.20	-0.47**	-0.49***	-0.33*	-0.33*
	(0.11)	(0.46)	(0.20)	(0.11)	(0.18)	(0.17)
ΔCMR				1.03***	4.05	2.11
				(0.31)	(2.75)	(2.85)
Const.	0.18			-0.15		
	(1.32)			(0.92)		
Obs.	634	535	565	645	539	574
No. countries	139	118	111	139	118	111
Adj. R^2	0.314			0.288		
Hansen J		0.60	0.07		0.35	0.10

Notes: The estimated Panel OLS equation is $\Delta g_{it} = \beta_2 \Delta g_{it-1} + \beta_3 \Delta(\text{CM})\text{R}_{it} + \beta_4 \Delta \text{PR}_{it} + \beta_5 \Delta \text{T}_{it} + \Delta \mathbf{X'}_{it}\beta + \Delta \varepsilon_{it}$. g_{it} is the country-specific compound growth rate.; R, CMR, PR and T are dummies = 1 if $I_t > I_{t-1}$, where I_t are country-specific indices of overall regulation, credit market regulation, property rights and trade policies, respectively. The variables Open, L(Pop) and L(XRat) are the sum of exports and imports in GDP, log of population and log of the country's exchange rate to the US dollar, respectively. Clustered standard errors are presented for the OLS and 2SLS estimations in parentheses. Hansen J is the P-value on the Hansen overidentification J test. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.14: Reforms and Growth, 1970-2010: A Country Reformed if $R_{it} >$ Median(R_i)

	Overall Deregulation			Credit Market Liberalization		
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
Δg_{it-1}	-0.44***	-0.47***	-0.39***	-0.45***	-0.50***	-0.44***
	(0.03)	(0.06)	(0.07)	(0.03)	(0.07)	(0.10)
ΔR	0.24	7.48	-10.85			
	(0.34)	(8.03)	(8.35)			
ΔPR	0.02	0.22	-0.13	0.02	0.03	-0.01
	(0.28)	(0.53)	(0.54)	(0.29)	(0.54)	(0.40)
ΔT	1.07***	0.68	2.28**	1.13***	0.32	0.91
	(0.29)	(0.68)	(1.14)	(0.32)	(0.67)	(1.39)
$\Delta Open$	0.00	-0.01	0.04	0.01	0.00	0.00
	(0.01)	(0.03)	(0.03)	(0.01)	(0.02)	(0.01)
$\Delta L(Pop)$	2.89	-0.12	7.20	2.87	-3.88	3.67
	(9.95)	(12.29)	(14.27)	(9.12)	(12.58)	(11.38)
$\Delta L(XRat)$	-0.38***	-0.06	-1.03**	-0.34***	0.27	-0.29
	(0.11)	(0.42)	(0.49)	(0.10)	(0.36)	(1.08)
ΔCMR				0.95***	10.52**	1.99
				(0.30)	(4.55)	(17.41)
Const.	-1.32			-1.45*		
	(0.80)			(0.82)		
Obs.	686	577	617	691	577	620
No. countries	142	130	122	142	130	122
Adj. R^2	0.313			0.303		
Hansen J		0.43	0.83		0.51	0.14

Notes: The estimated Panel OLS equation is $\Delta g_{it} = \beta_2 \Delta g_{it-1} + \beta_3 \Delta(\text{CM})\text{R}_{it} + \beta_4 \Delta \text{PR}_{it} + \beta_5 \Delta \text{T}_{it} + \Delta \mathbf{X'}_{it}\beta + \Delta\varepsilon_{it}$. g_{it} is the country-specific compound growth rate.; R, CMR, PR and T are dummies = 1 if $I_{it} >$ Median(I_i), where I_t are country-specific indices of overall regulation, credit market regulation, property rights and trade policies, respectively. The variables Open, L(Pop) and L(XRat) are the sum of exports and imports in GDP, log of population and log of the country's exchange rate to the US dollar, respectively. Clustered standard errors are presented for the OLS and 2SLS estimations in parentheses. Hansen J is the P-value on the Hansen overidentification J test. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.15: Reforms and Growth, 1970-2010: A Country Reformed if $R_{it} >$ Mean(R_i)

	Overall Deregulation			Credit Market Liberalization		
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
Δg_{it-1}	-0.43***	-0.42***	-0.45***	-0.45***	-0.55***	-0.34***
	(0.03)	(0.04)	(0.05)	(0.03)	(0.10)	(0.08)
ΔR	0.73**	6.84	-7.84			
	(0.36)	(5.86)	(7.86)			
ΔPR	0.05	0.08	0.11	0.04	-0.27	0.44
	(0.28)	(0.39)	(0.39)	(0.29)	(0.58)	(0.50)
ΔT	0.87***	0.02	2.08*	1.03***	-1.14	1.91**
	(0.31)	(0.79)	(1.21)	(0.33)	(1.01)	(0.87)
$\Delta Open$	0.00	-0.02	0.03	0.01	-0.02	0.02
	(0.01)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)
$\Delta L(Pop)$	2.18	2.83	3.32	2.05	-6.63	7.50
	(9.88)	(9.84)	(13.58)	(9.14)	(12.51)	(12.55)
$\Delta L(XRat)$	-0.35***	-0.18	-0.68**	-0.33***	0.26	-0.78***
	(0.11)	(0.24)	(0.31)	(0.10)	(0.39)	(0.28)
ΔCMR				0.93***	12.86**	-7.95
				(0.33)	(6.32)	(5.56)
Const.	-1.58*			-1.35		
	(0.84)			(0.84)		
Obs.	686	577	617	691	577	620
No. countries	142	130	122	142	130	122
Adj. R^2	0.313			0.298		
Hansen J		0.49	0.41		0.52	0.52

Notes: The estimated Panel OLS equation is $\Delta g_{it} = \beta_2 \Delta g_{it-1} + \beta_3 \Delta(\text{CM})\text{R}_{it} + \beta_4 \Delta \text{PR}_{it} + \beta_5 \Delta \text{T}_{it} + \Delta \mathbf{X'}_{it}\beta + \Delta\varepsilon_{it}$. g_{it} is the country-specific compound growth rate.; R, CMR, PR and T are dummies = 1 if $I_{it} >$ Mean(I_i), where I_t are country-specific indices of overall regulation, credit market regulation, property rights and trade policies, respectively. The variables Open, L(Pop) and L(XRat) are the sum of exports and imports in GDP, log of population and log of the country's exchange rate to the US dollar, respectively. Clustered standard errors are presented for the OLS and 2SLS estimations in parentheses. Hansen J is the P-value on the Hansen overidentification J test. Data source: PWT 7.1 data; EFW 2012 index; US EIA data. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Chapter 3

Firm Size, Market Liberalization and Growth[1]

[1]An earlier version of this essay was published as a CERGE-EI Working Paper No. 485 in April 2013.

3.1 Introduction

Suppose an identical market-oriented reform is adopted simultaneously across a number of countries. Will the reformers be affected identically? This paper argues they will not, and looks for the reasons behind an eventual outcome divergence. The explanation offered here, and the main hypothesis of this work, is that economic liberalization – i.e., the state's withdrawal from its legal powers to direct pricing, entry and exit on a given market (Winston, 1993) – affects firms of different size differently. Then, if two countries go through identical reforms but their firm size distributions are *ex-ante* different, the two economies will react differently to the reform. Naturally, the argument extends to more than two economies and to more than one liberalization reform. It also produces a variety of reform outcomes across countries and possibly over time.

Previous work has shown that, indeed, different economies may benefit differently from an identical reform. For example, Aghion et al. (2007) use industry-level data to demonstrate that entry liberalization affects different industries differently. More specifically, industries closer to the technology frontier would be affected more by entry liberalization and would innovate more than backward industries in order to prevent entry. Thus, countries closer to the world technology frontier benefit more from a liberalization reform because they have a higher share of innovating industries. As a result, those countries also grow faster after a reform.

By using firm-level data and linking it with country-level reforms, I find that although firms closer to the technology frontier do innovate more, they do not do so as a result of market-oriented reforms. This finding motivates me to argue that the literature has largely ignored one of the important and at the same time intuitive determinants of reform outcome divergence across

countries. In this work, I hypothesize that it is the firm size, among other factors, which drives the different impact of identical liberalization reforms on firm growth across countries. I test this hypothesis by using data on sales and sales per worker of more than 110,000 firm-level observations in 135 developing and post-transition economies, collected between 2000 and 2011. Firm sales and sales per worker are conditioned on country data on credit market liberalization reforms, on an overall economic liberalization reform, as well as on other aggregate and firm-level observables.

The advantage of having firm-level data in this study is that reform impact is studied at a level at which it allegedly matters most for growth, and where the growth decisions are actually taken: the firm. This work finds sufficient evidence to conclude that the cross-country variation in firm size distributions before the reform takes place is one of the drivers behind growth divergence across countries after similar market-oriented reforms.

The next sections illustrate how the literature around this problem evolved, including why it could be assumed that the firm-size distribution (FSD) is exogenous to policy changes in the short run.

3.2 Literature Review

3.2.1 Overall Impact of Market Liberalization on Growth

Since George Stigler and his coworkers pioneered the rigorous study on the effects of various regulations in the 1960s,[2] a vast literature emerged on how product-, labor- and capital-market liberalization affect entry, exit, employment, investment and productivity, among other determinants of economic growth. The literature moved from studying specific regulations (e.g. price or quantity) within a specific industry (e.g. trucking or airlines) in the 1980s

[2]See Stigler (1988, p.116-118) for a brief history of that work.

to broader studies of how regulation affects growth, growth factors or living standards across countries. Examples of the latter include Djankov et al. (2002) and Botero et al. (2004) on regulations of entry and labor, respectively. Along similar lines, Bertrand and Kramarz (2002) investigate the significant negative effects of hampering entry liberalization on job creation, while Alesina et al. (2005) establish a positive relationship between product market liberalization and investment in seven big OECD industries.

The work by Alesina et al. (2005) was extended by using firm-level data from both developed and developing economies, which include both small and large firms. In three studies Ardagna and Lusardi (2008, 2009a, 2009b) find that more cumbersome entry and labor regulations discourage firm entry, and that the effects are unequal across a number of individual firm characteristics. Klapper, Laeven, and Rajan (2006) also show that entry rates by firms are significantly affected by entry regulations, and further conclude that stricter entry regulations result in larger entering firms but also slower firm growth afterwards. In effect, aggregate growth slows down because of slower firm growth.

More recently, empirical works rely on firm-level data, in which micro and small firms represent the sample majority, thereby making the results more credible. Commander and Svejnar (2011) link firm performance from the Business Environment and Enterprise Performance (BEEPS) data with a wide range of institutional constraints on firm growth. Contrary to previous empirical findings, they do not support the hypothesis that institutional constraints matter for firm performance in Central and Eastern Europe (CEE) and the former Soviet Union, and find that country fixed effects are perhaps the main determinant of firm performance in the region. Commander and Nikoloski (2010) use more countries than Commander and Svejnar and

also find that the relationship between institutions, as measured in the Doing Business Database, and firm performance, is not robust across countries. Specifically, firms in countries belonging to different income groups are affected differently by reforms, with the reforms having the expected positive sign only in high- and upper-middle income groups.

Although Commander and Nikoloski (2010) control for firm size, they do not use firm size as a factor which, if combined with the effect of the reform, could determine differences in reform outcomes across countries. There is an emerging body of empirical evidence of differences in the responses of small and large firms to various types of liberalization reforms.

3.2.2 The Effect of Economic Liberalization across Firms of Different Size

Studies in various lines of empirical literature on liberalization – especially trade and financial liberalization – document a differential effect of reforms on firms of different size. The differential impact of the trade liberalization between Turkey and the EU on small and large firms is studied by Erzan and Filiztekin (1997). Their conclusion is that small firms' value-added growth decreased after the introduction of the Customs Union (CU) with the European Union, while the impact on large firms was mostly insignificant.

The reason for different reform outcomes for small and large firms is often described in the IO literature as "compliance asymmetries." In particular, Millimet (2003) argues that smaller firms are disadvantaged in their resources to investigate and challenge legislative changes. Therefore, economic liberalization may have disproportionate effects on firms of different size. Moreover, large firms spread the fixed compliance costs attributed to a given regulation over a larger output which gives them a cost advantage.

The finance literature also analyses the difference between the effects of financial regulation on the costs of small and large firms. For example, Franks, Schaefer, and Staunton (1997) find that the ratio between the direct and indirect compliance costs of financial regulations tends to decrease with size. Consequently, larger firms are also less affected by financial regulations. Contrary to this conclusion, Bena and Jurajda (2007) find little evidence of a differential effect of financial development across firm size, conditional on the firms reaching a certain minimum size (in their data it is 100 employees and 20 million Euro of total assets).

Aghion et al. (2007) provide a strong intuition as to why identical reforms may exert a different effect across different economies. The core of their argument is that firms closer to the technology frontier would benefit from the easing of industry entry more than the backward firms because they innovate more to deter entry, and find industry-level evidence for this differential impact. In a supporting study, Bourlès, Cette, Lopez, Mairesse, and Nicoletti (2010) find that industries closer to the technology frontier would benefit more from liberalizing product market regulations, thus extending the argumentation in Aghion et al. (2007).

However, micro-level evidence presented further in this work suggests that it is not necessarily the position on the technology ladder that determines the different reaction of firms to liberalization reform. Rather, it is the size of the firm. Therefore, it can be argued that if the firm-size distributions across two economies are different, then an identical reform may have different growth impacts because firms of different size react differently to liberalization. The next section illustrates the differences observed in the firm-size distributions (FSDs) across countries and argues why those differences matter for delivering different reform outcomes across countries.

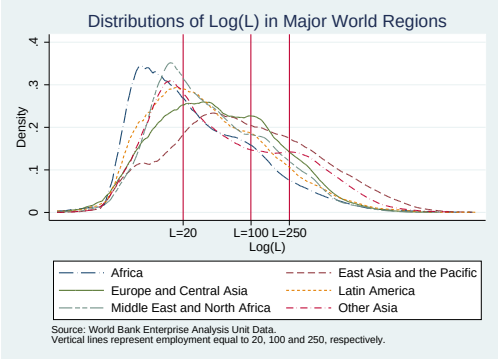
3.3 Firm-size Distributions across Countries

Establishing any evidence of a differential effect of an identical reform across countries hinges on several important questions. First, are there significant differences in the firm-size distributions (FSDs) across countries? If the FSDs are the same, then the reform outcomes across countries would hardly be significantly different, even if small and large firms are found to grow differently after the reform. Second, do reforms influence those distributions? If FSDs are influenced by the reforms over short periods of time, then the FSDs themselves would be endogenous to the liberalization reforms. Therefore, it is important to know whether one can take the FSDs as exogenous at least in a cross-sectional setting. Third, are the cross-country growth differences affected by the differences in the FSDs? If they are, then a reform could not only have a different effect on firms of different size but it could also bring aggregate reform implications across countries. This part of the paper addresses each of these questions.

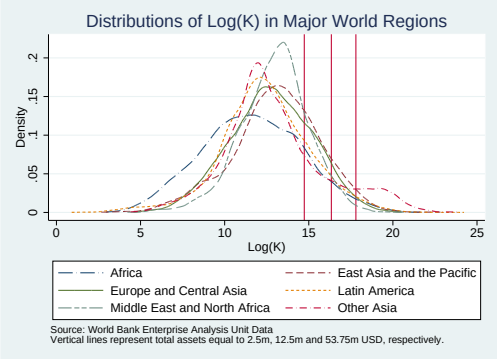
Over recent decades there have been substantial efforts to explain the statistical regularities behind FSDs both within and across countries, and over time. Gabaix (2009) reviews the evidence that FSDs in developed countries are found to have a Zipf distribution, at least in their upper tails.[3] However, in some developed countries such as Japan (Kaizoji, Iyetomi, & Ikeda, 2005), and most notably in the developing world, this regularity in FSDs is harder to observe, as the data presented here and additional evidence suggests.[4] In addition, looking at the figures below, it is obvious that

[3]Following Gabaix (2009), the Zipf distribution in firm size essentially means that the probability of a firm size S being greater than x is inversely proportional to x. More formally, $P(S > x) \simeq kx^{-\alpha}$, and in the particular case of Zipf distribution, $\alpha \simeq 1$.

[4]For some differences in the FSDs between the developed and the developing world, see Alfaro, Charlton, and Kanczuk (2008).



(a) Log(L)



(b) Log(K)

Figure 3.1: Firm-Size Distributions of Employment and Assets

there are marked differences in FSDs across major regions of the world, especially in the small-firm segments of the distributions.[5] Those differences are also observed within each of those regions and may be explained by several theoretical and empirical arguments.

First, many young firms operate in the small-firm segment. Those firms' growth is more volatile (Alexander, 1949; Samuels & Smyth, 1968). They grow faster as well but are also more likely to fail (Dunne, Roberts, & Samuelson, 1989; Jovanovic, 1982; Mansfield, 1962; Mata, 1994). The snapshots of FSDs in Figure 3.1(a) and Figure 3.1(b) capture marked differences in the FSDs across major world regions exactly in the small firms segment [below 20 employees in Figure 3.1(a) and below USD 2.5m in assets in Figure 3.1(b)].

Second, trade theory produces a well-known proposition that different countries specialize in different industries.[6] If there is a different evolution of FSDs across industries, then the within-country industry specialization would give rise to divergent evolutions of FSDs across countries depending on their industrial structure.

[5]The first and the last percentiles of each tail are removed.
[6]See Heckscher-Ohlin and Rybczynski theorems.

Third, significant differences in FSDs across industries within a period (Rossi-Hansberg & Wright, 2007) and different evolutions of FSDs across industries have been documented (Lotti & Santarelli, 2004). Lotti and Santarelli (2004) study FSDs of new entrants in several industries and find they vary across their minimum-efficient scale and technological requirements. Technology is also found to be an important factor generating differences in FSDs across industries by Marsili (2005). These facts might explain the differences in FSDs at a point in time across countries.

However, despite the marked cross-country differences in FSDs, and despite the documented underlying evolutionary process towards an equilibrium FSD *within an industry* (Hashemi, 2000, 2003), the within-country distributions are relatively stable, as found by Cabral and Mata (2003) and Henly and Sánchez (2009). Cabral and Mata (2003) also note that the FSD of a given cohort of firms changes slowly over time, while Henly and Sánchez (2009) add that the within-industry FSD changes over long periods of time and the within-country FSD stays unchanged. Doi and Cowling (1998) assert that in some countries (e.g., Japan) the share of output and employment across size classes is relatively constant over long periods of time, while in others (e.g., the UK) they change only slowly in favor of smaller firms. Axtell (2001) also concludes that FSDs are stable over time, at the same time being robust to the employed definition of firm size. Then, it can be assumed that FSDs are stable over relatively short periods of time, such as the one examined in this work, and are not affected by economic liberalization reforms in the short-run.

Naturally, the above exogeneity assumption does not mean the within-country and within-industry FSDs evolve independently as mere statistical

regularities.[7] After all, the differences in FSDs across countries came from an underlying difference in some fundamental factor. Lucas (1978) argues that FSD is underlined by a distribution of managerial talent. Thus, different countries end up having different FSDs depending on the international allocation of talent. At the same time, countries with lower quality of institutions and enforcement of property rights have a different allocation of talent into productive and rent-seeking occupations (Murphy, Shleifer, & Vishny, 1991). Thus, it is tempting to explain the observed cross-country differences in FSDs with different underlying institutions and property rights systems.

Finally, there are emerging implications in the FSD literature that FSDs are correlated with cross-country income differences (Alfaro et al., 2008; Gabaix, 2009). This evidence contributes to the understanding that FSDs are an important determinant of cross-country differences in the growth effects of reforms.

In a nutshell, both the firm-level data used here and the size distribution literature point to significant differences in FSDs across countries. However, policies seem to do little to affect the evolution of FSDs over short periods of time within a country. Rather, FSDs are more likely to be driven by within-industry product life cycles that have more to do with fundamentals such as preferences and factor endowments that affect industry specialization than with policies. Thus, it is legitimate to assume both the FSD within a country and the cross-country differences in FSDs as given, at least in a short panel, and especially in a cross-sectional data setting. However, the variation in the FSDs also affects the cross-country income differences. Thus, it is very intuitive to hypothesize that an identical policy would have a different impact across countries based on its different effect on small and large firms. The

[7]See Sutton (1997, 2007) for extensive discussions on FSD evolution.

empirical strategy to test this hypothesis is presented below.

3.4 Empirical Strategy

3.4.1 Confronting Previous Evidence

Does economic liberalization influence firms of different size differently? I answer the question by considering the papers of Aghion et al. (2007) and Bourlès et al. (2010) as a starting point, and contribute to their works in several ways.

First, instead of using industry-level data, this work uses data with more than 110,000 firm-level observations, which spans a richer set of industries than the manufacturing data in Aghion et al. (2007) or in Bourlès et al. (2010). Besides manufacturing, the data set used here includes trade and other services, although it covers an admittedly lower number of countries than Aghion et al. (2007). The main advantage of the data set here is that it is able to reveal the actual decisions about innovation and growth at the firm level.

Second, I abstain from the definition of distance to the technological frontier in Aghion et al. (2007), which is more relevant at the industry level. Instead, I assume that firms have a good knowledge of the level of technology of their main competitors and of their own technology, and are able to compare them. This also assumes firms optimize based on the decisions of their nearest rival. If this reasoning is legitimate, three possibilities arise. Specifically, the firm can have a more advanced, a similar or an inferior technology to its closest rival. After classifying firms into these three broad categories, I estimate the following probit model:

$$P(y_i = 1|X_i) = \Phi(\beta_0 + \beta_1 ADV_i + \beta_2 LAG_i + \beta_4 ADV_i R_j +$$

$$+\beta_5 LAG_i R_j + \beta_6 R_j + \mathbf{Z}_i^{'}\beta + f_s + \varepsilon_i), \qquad (3.1)$$

where $P(y_i = 1|X_i)$ is the probability of obtaining an ISO certification or of introducing a significant innovation in the firm's product line after economic liberalization. I further condition the firm's behavior on its relative position on the technology ladder: ADV_i and LAG_i are dummy variables indicating that a given firm has a superior (advanced, ADV) or inferior (lagging, LAG) technology compared to its main rivals; R_j is a measure of how liberalized economic policies in country j are, as measured by Worldwide Governance Indicators (WGI), by the Economic Freedom of the World (EFW) data, and by Heritage Foundation Data (HFD);[8] $ADV_i R_j$ and $LAG_i R_j$ are interactions between the technological standpoint of the firm and the liberalization variable to indicate the impact of liberalization on each step of the technology ladder relative to the firms that have about the same technology as their main competitor. Finally, the vector $\mathbf{Z}_i^{'}$ includes other firm-level controls relevant for the innovation process such as the age of the firm, the experience of top management, sales in the previous period and the size of the firm; f_s are time-invariant sector effects; ε_i is an error term that I assume to be uncorrelated with the explanatory variables and the Φ function has a normal distribution so that the parameter estimates in the above equation represent the direction of the impact of being a technologically advanced or inferior firm to the probability of innovation after the reform takes place.

By applying this methodology, this work answers the following question: Do technologically advanced firms innovate more after an economic liberalization reform? If indeed technologically advanced firms innovate more after a reform, then the theory by Aghion et al. (2007) would be supported by stronger firm-level evidence and by an empirical strategy that uses a direct

[8]See the data description for further details on these.

comparison with the distance to the frontier from a firm's point of view. However, if advanced firms do not innovate more after a reform, then perhaps an alternative explanation would be needed on why different firms react differently to economic liberalization.

The firm-level evidence in favor of the above theory is mixed at best. It is presented in Table 3.1. Indeed, consistent with Aghion et al. (2007) and with Bourlès et al. (2010), technologically advanced firms innovate more, and backward firms innovate less than firms whose technology is about the same as the technology of their main competitors. However, the interaction between the level of technology and the reform is rarely significant, and if it is, its significance is not robust across different data sets measuring economic liberalization. Therefore, there is not enough support at the firm level for the evidence that the distance to the technological frontier drives the differential impact of economic reforms across countries, and perhaps a new explanation is in order. The new explanation is based on the hypothesis that small and large firms react differently to reforms. The methods to test this hypothesis are presented below.

3.4.2 Estimation Strategy

To test the hypothesis that firms of different size grow differently after economic liberalization, I estimate the following baseline model for the growth of firm i in country k at time t:

$$\begin{aligned}\log Y_{ikt} &= \alpha_0 + \alpha_1 \log Y_{ikt-1} + \alpha_2 \log K_{ikt} + \alpha_3 \log L_{ikt} + \\ &+ \alpha_4 CMR_{kt} S_{ikt} + \alpha_5 R_{kt} S_{ikt} + \alpha_6 RoL_{kt} S_{ikt} + \\ &+ \alpha_7 T_{kt} S_{ikt} + \mathbf{Z}'_{ikt}\alpha + f_{st} + f_{kt} + \varepsilon_{ikt},\end{aligned}$$

where $\log Y_{ikt}$ stands for either sales, $\log SAL_{ikt}$, or the sales per worker, $\log SPW_{ikt}$, of firm i in country k in period t. In addition, $\log K_{ikt}$ and

$\log L_{ikt}$ are the value of total assets and the labor costs, respectively, to estimate the impact of the main factors of production;[9] CMR_{kt}, R_{kt}, RoL_{kt} and T_{kt} are the indices of credit market regulation, overall regulation, the rule of law, and international trade policies, respectively, for country k in period t, taken from EFW indices;[10] S_{ikt} is the size of the firm measured by either the log-number of employees or by the log-value of assets; $\mathbf{Z}'_{ikt}$ is a vector of firm observables, including whether the firm has obtained an ISO certification, to capture some differences in the growth of firms with different levels of technology and more sophisticated management procedures, legal structure, age of the firm and top manager experience.

Further, in order to capture common but temporary shocks to firm performance within an industry or a country, the model includes industry-specific and country-specific dummies for each of the years available in the sample. The interactions of the country dummies with the year dummies would also capture the overall reform processes happening in the country. That is why the model does not include liberalization indices as distinct explanatory variables – they are captured by the country-year dummies. Finally, ε_{ikt} is the error term about which it is assumed, at least for now, to be distributed normally with a zero mean, and to satisfy classic linear regression assumptions.

As the reform indices vary only on the country level, firm-level variation is introduced by interacting the indices with the log-number of employees or the log-value of assets of the firm. The interaction captures how differently small and large firms grow after financial liberalization, after overall eco-

[9]When sales per worker is the main explained variable, $\log K_{ikt}$ and $\log L_{ikt}$ are transformed into capital per worker by dividing total assets by the number of employees.

[10]An increase in the CMR_{kt} index means financial liberalization, an increase in the R_{kt} index means overall economic liberalization on labor, product, and credit markets, an increase in the RoL_{kt} index means strengthening the rule of law, and an increase in T_{kt} means trade liberalization.

nomic liberalization reforms and after strengthening the rule of law. Thus, the interaction terms $CMR_{kt}S_{ikt}$, $R_{kt}S_{ikt}$, $RoL_{kt}S_{ikt}$ and $T_{kt}S_{ikt}$ address the main question of this work, and $\alpha_i, i \in [4;7]$ are the parameters of primary interest. If significant, they would demonstrate that firms of different sizes react differently to reforms. If the estimates are positive, then larger firms grow more than smaller firms after a given reform.

If we take the above equation as it is, we will have to assume, at least implicitly, that K_{ikt}, L_{ikt} and the interaction terms are exogenous variables, which would be a strong assumption. For various reasons, all of the right hand-side variables in the above equation, except perhaps the size variable S_{ikt}, are endogenous.[11] Therefore, both the identification and estimation of their parameters would require constructing a system of equations in which the endogenous variables in the baseline equation are being explained by some other factors outside of the baseline equation rather than being assumed as "weakly exogenous." This system is as follows:

$$\begin{aligned}
\log Y_{ikt} &= \alpha_0 + \alpha_1 \log Y_{ikt-1} + \alpha_2 \log K_{ikt} + \alpha_3 \log L_{ikt} + \\
&\quad + \alpha_4 CMR_{kt}S_{ikt} + \alpha_5 R_{kt}S_{ikt} + \alpha_6 RoL_{kt}S_{ikt} + \\
&\quad + \alpha_7 T_{kt}S_{ikt} + \mathbf{Z}'_{ikt}\alpha + f_{st} + f_{kt} + \varepsilon_{1ikt} \qquad (3.2) \\
\log Y_{ikt-1} &= \rho_0 + \rho_1 \log Y_{ikt-2} + \rho_2 \log K_{ikt-1} + \rho_3 \log L_{ikt-1} + \\
&\quad + \rho_4 CMR_{kt-1}S_{ikt-1} + \rho_5 R_{kt-1}S_{ikt-1} + \rho_6 RoL_{kt-1}S_{ikt-1} +
\end{aligned}$$

[11] Naturally, the size S_{ikt} is also endogenous. For the purposes of this work however, I take it as exogenous. The literature review demonstrates that the size distribution of firms is changing only slowly, and within a cross-section of data can be taken as independent from the policy changes. Then, if a given reform is enacted in some countries, it will be the initial size distribution variation that would determine the differences in the reaction of the economy, while the second-order effects of the liberalization reform, which run through the within-country changes of the size distribution, would appear only after a slow adjustment process. Then, this longer-term margin of adjustment is irrelevant in a cross-section of firms. Yet, I acknowledge the need to address the issue of endogenous firm-size adjustment by using a longer panel of firms.

$$+\rho_7 T_{kt-1} S_{ikt-1} + \mathbf{Z}'_{ikt-1}\rho + f_{st-1} + f_{kt-1} + \varepsilon_{2ikt-1} \tag{3.3}$$

$$\begin{aligned}\log K_{ikt} &= \beta_0 + \beta_1 \log K_{ikt-1} + \beta_2 \log r_{ikt} + \beta_3 \log r_{ikt-1} + \\ &+\beta_4 \log Y_{ikt-1} + \beta_5 R_{kt} + \beta_6 R_{kt-1} + \\ &+\beta_7 RoL_{kt} + \beta_8 RoL_{kt-1} + \varepsilon_{3ikt}\end{aligned} \tag{3.4}$$

$$\begin{aligned}\log L_{ikt} &= \gamma_0 + \gamma_1 \log L_{ikt-1} + \gamma_2 \log w_{ikt} + \gamma_3 \log w_{ikt-1} + \\ &+\gamma_4 \log Y_{ikt-1} + \gamma_5 R_{kt} + \gamma_6 R_{kt-1} + \\ &+\gamma_7 RoL_{kt} + \gamma_8 RoL_{kt-1} + \varepsilon_{4ikt}\end{aligned} \tag{3.5}$$

$$\begin{aligned}CMR_{kt}S_{ikt} &= \delta_0 + \delta_1 CMR_{kt-1}S_{ikt-1} + \delta_2 CMR_{kt-2}S_{ikt-2} + \\ &+\delta_3 C_{kt} + \varepsilon_{5ikt}\end{aligned} \tag{3.6}$$

$$\begin{aligned}R_{kt}S_{ikt} &= \eta_0 + \eta_1 R_{kt-1}S_{ikt-1} + \eta_2 R_{kt-2}S_{ikt-2} + \\ &+\eta_3 C_{kt} + \varepsilon_{6ikt}\end{aligned} \tag{3.7}$$

$$\begin{aligned}RoL_{kt}S_{ikt} &= \theta_0 + \theta_1 RoL_{kt-1}S_{ikt-1} + \theta_2 RoL_{kt-2}S_{ikt-2} + \\ &+\theta_3 C_{kt} + \varepsilon_{7ikt}\end{aligned} \tag{3.8}$$

$$\begin{aligned}T_{kt}S_{ikt} &= \mu_0 + \mu_1 T_{kt-1}S_{ikt-1} + \mu_2 T_{kt-2}S_{ikt-2} + \\ &+\mu_3 C_{kt} + \varepsilon_{8ikt},\end{aligned} \tag{3.9}$$

where the demand for production factors depends on present and lagged values of the exogenously determined factor prices, on the levels of the employed factors and on the output in previous periods, and on the policy determinants of the firm growth; the endogenous interaction terms depend on the past levels thereof, as well as on some country characteristic C_{kt}.

The reasons for building such a system are based on theory and intuition. First of all, basic economic intuition suggests that labor and capital demand would depend on prices. In addition, the input prices from the previous periods are included because the change in relative prices between labor and capital in the past may also influence the factor demand decisions in

the current period. Further, the past values of the inputs are included as exogenous variables. It is not unreasonable to assume that if the firm overshot its labor demand in the last period, it may downsize in the current period, or if the managers of the firm had too few fixed assets in the last period, they may want to invest more this period. Also, if a firm had a good year, it may wish to expand by buying more capital and labor services the following year. This is the intuition to include also the previous values of sales or sales per worker in the factor demand decisions.

Finally, the decisions of the government on how much to liberalize depend on how much regulation there is in the first place. For example, if a country has liberalized extensively in the past periods and now the level of the overall regulatory burden is low, it may not need to reform much further. Also, the decision on how much to liberalize depends on some purely country-specific characteristic such as the political orientation of the incumbent government, the legal origin, the history of regional conflicts, or the resource endowments.

This system has its limitations as well. Its design is intended to capture a rather short-term effect of reforms on the growth of firms, or, alternatively, use a cross-country variation in reforms to answer an inherently dynamic question. Also, some reforms take much longer to affect hiring and investment decisions. Therefore, the system may miss any reform benefits for the firm that materialize over a longer term. A much longer panel of firms may address the longer-term effect of reforms more properly. In this case, it is data limitations affecting the decision to include only one lag of reforms: there is only one lag in the data spanning over 3 years for all firms. To capture any reform effect over the growth of firms within that period, I also estimate the above system in differences. The results are much stronger than estimating equation (3.2) in levels, and are discussed below. However, the

cost of differencing is a massive loss of observations as fewer firms have lagged data on sales, assets, labor costs and number of workers.

Differencing the firm-level performance indicators has another advantage over estimating the model in levels. When the model is run in levels, non-stationary variables like sales and sales per worker remain on both sides of the equation. This could lead to potentially spurious results indicated by a very high R^2 and a high statistical significance of most estimates. If the underlying data was a long panel, the solution would be to run panel unit root tests, log-difference the data, and then estimate the model in differences with individual fixed effects. With the current data, however, it is not possible to credibly do the panel unit root testing because the maximum number of lags in the data is just one, and most of the firms are observed in one period only which further impedes testing. Therefore, checking for potential stationarity in the data had to be done in an indirect way. To do that, I ran the model in levels, and inquired if some of the typical issues with the estimation results in the presence of unit roots have surfaced. Indeed, the high significance of most parameters, as well as the very high R^2 in the level estimation, flag the above concerns. That is why my preferred estimation is the one in differences.

Since the primary interest of this work is in the best possible estimation of equation (3.2), constructing the above system has the sole purpose of identifying $\alpha_i, i \in [4; 7]$, by finding possible instruments for the endogenous interaction terms. In the 2SLS estimations, the exogenous variables in the rest of the system of equations are used as instruments, where the crucial role is played by the lagged values of the interaction term. I estimate equation (3.2) by both OLS and 2SLS, in both levels and differences. The results from estimating the model in levels are presented in Table 3.2 and Table 3.3, and in differences – in Table 3.4 and Table 3.5. A more detailed description of

the data which feeds the model is given below.

3.5 Data

3.5.1 Country-level Data on Reforms

There is more than one source of country-level data on the variables used in equation (3.1). One of the widely used data sets is Worldwide Governance Indicators (WGI) for 1996-2010, constructed by Kaufmann et al. (2010). The WGI dataset is constructed biannually for 1996-2002 and annually since 2003 in 6 areas: Voice and Accountability, Political Stability, Government Effectiveness, Regulatory Quality, Rule of Law and Control of Corruption. For the purposes of this work, the most relevant indicator of economic liberalization is the regulatory quality. The other data set used here is the Heritage Foundation Data (HFD) reported in Miller, Holmes, and Feulner (2013). It contains information on 10 broad reform areas across 181 countries. Among those reform areas are business freedom, investment freedom and labor freedom. I average those three freedoms to arrive at an index of overall liberalization that I further use in equation (3.1). The final data set I use in equation (3.1) and in equation (3.2) is the Economic Freedom of the World (EFW) data set.

The EFW data set, constructed by Gwartney et al. (2012), was used as the main source of economic liberalization data. The EFW data contain information on both the overall country patterns of economic and property rights reforms but also on more specific patterns of credit market liberalization. The database contains annual indices of economic freedom in 5 areas: Size of Government, Legal Structure and Security of Property Rights, Access to Sound Money, Freedom to Trade Internationally and the Regulation of Credit, Labor, and Business. The last area in the database is the most

relevant to the estimation of equation (3.2).

A positive feature of the EFW database is that it dates back well before the firm performance measures were obtained. Thus, I can construct instruments for both the overall and the specific market liberalization reforms, and for the rule of law. Those instruments are the indices of CMR_{kt}, R_{kt}, RoL_{kt} and T_{kt} in 1990 and in 1995, interacted with the size of the firm 3 years before the dependent variable was measured. Thus, endogeneity issues behind the interaction terms are allegedly mitigated.

3.5.2 Firm-level Data

The Enterprise Surveys (ES) firm-level data are collected by the Enterprise Analysis Unit (EAU) of the World Bank in various periods. The data set encompasses two broad periods: 2000-2005 and 2005-2011 in various countries. The first data set has more than 53,000 firm-level observations across more than 90 countries and the second one has more than 60,000 firm-level observations from more than 70 countries. Both data sets consist of a wide range of firm-level performance indicators. I stack them together so that I have a large cross-country data set spanning from 2000 to 2011 that I can further merge with the country-level data. Further, to reduce the number of empty industry-country cells, I drop any industry with less than 1000 observations, and any country with less than 100 observations.

The EAU data is perhaps the largest publicly available firm-level data set with relevance to the main hypothesis of this work. The results from testing it are presented below.

3.6 Results

By using industry-level data, Aghion et al. (2007) and later Bourlès et al. (2010) reveal some reasons why product market liberalization reforms might benefit advanced economies – or those economies with a higher share of advanced firms – more than economies with a higher share of backward firms. However, it was shown in Table 3.1 that firm-level evidence in support of their theory is weak. Therefore, a new hypothesis may explain why some economies benefit from liberalization reforms while others do not. I hypothesize that firms of different sizes react differently to deregulation. Thus, based on the notable differences in the size distribution of firms across countries, various economies would react differently to identical economic liberalization reforms.

To test the hypothesis, I use both OLS and 2SLS estimation of equation (3.2) in which the instruments for the endogenous variables are found in the rest of the system of equations. The results from these estimations are presented in Tables 3.2 and 3.3.

Tables 3.2 and 3.3 present the estimates of equation (3.2) by OLS and 2SLS. Within each table, two sets of estimations are conducted. The first set uses the number of employees as a proxy for firm size, whereas the second set of estimations uses the value of assets as a proxy for size. Within each set of estimations, four columns are presented. The first two columns present the estimates from equation (3.2) without the country-year effects and the second two columns present the estimates with the country-year effects. The reason to present both estimates was that time-varying and time-invariant country characteristics may turn out to be among the crucial determinants of the variation in the responses to reforms within each country, as already suggested by Commander and Svejnar (2011).

Tables 3.2 and 3.3 present evidence that liberalization reforms have dif-

ferent impacts on both sales and sales per worker of firms of different size. Table 3.2 demonstrates that credit market liberalization helps increase the sales per worker of larger firms more than the sales per worker of smaller firms. This result supports the different impact of financial regulations on small and large firms discussed by Franks et al. (1997). However, reforming product and labor markets affects smaller firms more. This is indicated by the negative sign on some of the estimates of R^*Size. Strengthening the rule of law and trade liberalization also seems to improve the sales per worker of larger firms more. This is indicated by the parameter estimates on the interaction terms. Interestingly, the result concerning trade liberalization does not hold across different measures of firm size, that is, when size is changed from number of employees to value of assets. Further, firm controls such as managerial experience and age of the firm do not appear to increase sales per worker, conditioned on the other controls. In addition, including both time-varying and time-invariant country effects in the estimated equation does not change the above result.

The above evidence suggests that larger firms benefit more from liberalizing credit markets, from strengthening the rule of law and from trade liberalization. At the same time, smaller firms benefit more from an overall reform that, apart from credit market liberalization, includes also labor- and product-market reforms.

There is a reason the results here are presented both with and without country fixed and time-varying effects. The reason is that the current literature seems to be still looking for conclusive evidence on the effects of various reforms on economic growth. Cross-country studies *á la* Djankov et al. (2002) and Botero et al. (2004) imply a positive impact of reforms. However, firm-level studies, e.g., Commander and Svejnar (2011), offer a more

nuanced explanation of the growth impact of market-oriented reforms. The results here offer one of the possible explanations for the dissent analyzed well in Babecký and Campos (2011).

Babecký and Campos (2011) and later Babecký and Havránek (2013), however, also find that external liberalization had a robust positive effect on growth in the transition economies. One of the reasons for this robust effect was perhaps the fact that the initial phase of transition period was characterized by higher share of large firms than the subsequent phases. The results here point to the fact that those firms benefit more from liberalizing credit markets and trade. Consequently, the results here offer an intuitive explanation of why those CEE countries which liberalized their external sectors earlier also benefited more from the initial reforms in their external sectors.

The result that small and large firms grow differently after similar reforms is much stronger in Table 3.4 and Table 3.5. At the cost of a massive loss of observations, the estimations presented in those two tables gain insight into the *growth* of sales per worker and the *growth* of sales of firms of different size after various market-oriented reforms. The baseline equation is now estimated in differences. It tells a much more consistent story on the growth impact of various reforms across firms of different size. The messages from Table 3.2 still stand.

Without going into too much detail, bigger firms grow more than smaller firms after liberalizing credit markets, after improving the rule of law, and after trade liberalization. Unlike bigger firms, their smaller competitors benefit more from reforms in labor and product markets. The results are also robust to including country effects. With or without the country effects, the main result emerging from this analysis stands: The growth of firms of different size after market liberalization and property rights reforms is different.

As a result, aggregate growth would also be affected by the within-country firm size distribution. Given the cross-country differences in the FSDs, it is intuitive why some countries benefit from market-oriented reforms, while others do not.

3.7 Robustness Checks and Tests of Instruments

The results above would have causal interpretations only if the the error terms are uncorrelated with the explanatory variables, and if the instruments in the 2SLS estimations are valid and strong. To ensure that some unobserved firm-level effect is not driving the growth of the firms instead of the included explanatory variables, I employ two separate procedures. First, I store the residuals from each estimation and then regress the residuals on the observed firm-level explanatory variables. In all of those estimations of the error term, I find that the included explanatory variables have no effect on the unobservable firm-level effects. These conclusions are also supported by the residual plots against the observables included.

Second, I do a RESET test. The test rejects the hypothesis that there are no omitted variables in almost all models. These omitted variables could be either the power terms of the included explanatory variables or the firm fixed effects. Re-running the model and repeating the RESET test with the squared and higher-power terms still leads to a detection of omitted variables and the magnitude of the F-test does not decrease, so the issue is not mitigated by the additional variables. Given the cross-sectional data, I have no way of controlling for the firm-specific fixed effects that I suspect are causing the specification issue.

Therefore, I presume that the unexplained parts of the variations in sales and sales per worker are driven by either the firm fixed effects or some random

factor that is not causing an omitted variable bias (OVB). Moreover, the explanatory power of most models is large enough so I expect any OVB to be relatively small. Despite the small OVB, the core message still persists across all models.

However, the OVB is not the ultimate concern with these estimations. An additional issue arises with the Hansen J-test because it rejects the null of the validity of instruments in some of the estimations. This could be because the instruments are invalid or because of misspecification (Cameron & Trivedi, 2005, p.277). In either way, the significant Hansen J-test calls for caution in interpreting the 2SLS estimates. The positive news about the instruments is that they are strong. This is indicated by the Angrist-Pischke first-stage F-test (APF) whose value is more than 10 in most cases and which is more conservative than the standard first-stage F-test.

Assuming the Hansen test does not undermine the main message of the paper, I perform several robustness checks. First, I add more instruments. I interact the values of the reform variables in the year 2000 with the size of the firm, and add the resulting variable to the list of instruments. Unlike 1995 and 1990 that were used to interact with size so far, the year 2000 is closer to the sample. I expect the inclusion of this instrument to add strength to the instruments. The results are presented in Table 3.6 and are robust to the ones presented in the main tables. I repeat the estimations with the higher number of instruments for sales and get similar conclusions.

Second, instead of using the EFW indices of reforms throughout this study, I plug the Abiad et al. (2010) overall index of financial reforms for the CMR index in the main estimations. The goal is to see if the results are robust to a certain change in the data source of reforms. The results are presented in Table 3.7 and are roughly robust, with minor exceptions. An important

exception is that firm sales per worker do not behave consistently better for larger firms after a CMR reform, or consistently better for smaller firms after an overall reform. The results are robust in another way though: firms of different size do not grow identically after various market liberalization reforms.

Third, because of multicollinearity concerns over the correlations of CMR and the overall index of regulation R, I drop R from the main estimations, and stick with the CMR index offered by Abiad et al. (2010). The downside of this approach is that it introduces an OVB. Still, the results are roughly consistent. Overall, the robustness checks confirm the broad conclusions of this study.

Fourth, I investigate if the results are driven by the Great Recession. The reason to expect an impact of the Recession on the results of the paper is that many firms ceased their existence in 2008-2011. Perhaps the effect was more pronounced for the smaller firms. This could have affected the firm-size distribution in that period and, hence, the results of the paper. Since the data features about 6200 firms monitored after 2007, it is natural to see if the results with and without those firms are similar. There is one caveat to dropping data after 2007, however: No data on the lagged values of assets was collected after 2004. Effectively, this means that by construction the results will remain virtually identical even after the drop. Therefore, I have to resort to an alternative method to analyze if the Great Recession alters the results.

To check if the Recession alters the results significantly, I drop the lagged values of assets as an explanatory variable from all models. The advantage is that I can utilize the data after 2004 which does not have records of the lagged values of assets but has all the remaining explanatory variables. Admittedly,

this produces biased estimates because of an omitted relevant variable. Yet, the results will be consistently biased in the same direction because the same variable is missing from all models. That is why the omitted variable bias is of lesser concern here. The more important concern is how the Great Recession changes the results, if it changes them at all. The results after dropping the value of assets are presented in Table 3.8 and Table 3.9.

It turns out the Recession changes somewhat the main results and adds an additional angle on the performance differences after a reform is enacted between small and large firms. When the estimations are done on the restricted sample before 2008, the results come out a notch stronger than the ones obtained from the full sample but not for all reforms. This means the performance differences of small and large firms after market oriented reforms were slightly more evident during the boom years before the Great Recession. With a bit of an overreach, this conclusion could serve as a guidance to further pursuits of a more general effect of crises on the impact of reforms across firms of different size. Specifically, one could expect the different effect of market oriented reforms across firms of different size to be stronger in good times, and weaker in times of recessions. This particular avenue for research, however, is left for the future. The other conclusions from this work are presented below.

3.8 Conclusion

By using firm-level data from a large number of developing and post-transition countries, it was shown that firms of different size grow differently after similar reforms. This could bring sizable aggregate implications for cross-country differences in the outcomes of many market-oriented reforms. Those differences could be determined, among other factors, by the notable variation in

FSDs across countries.

In a policy context, the success of reforms depends on the share of firms with relative gains after the reform. If an economy has a larger share of smaller firms, then liberalizing product and labor markets would benefit this economy more than an economy populated by larger firms. Bigger firms seem to grow slower after those reforms. However, improving property rights, liberalizing trade and liberalizing the financial system would make an economy with a higher share of large firms grow faster than the economy populated by small firms.

The results here also partly explain why a given set of reforms might affect a number of countries differently, despite the similarity in those reforms. For example, a rich history of similar market-oriented reforms in Central and Eastern Europe has led to remarkably different reform outcomes. Offering an explanation for this and other growth divergences that occurred after a similar set of reforms could be considered the main contribution of this work to the development literature.

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Table 3.1: Liberalization, Technology and Innovation

	WGI				EFW				HFD			
	ISO Cert		Signif Innov		ISO Cert		Signif Innov		ISO Cert		Signif Innov	
	(1) OLS	(2) Probit	(3) OLS	(4) Probit	(5) OLS	(6) Probit	(7) OLS	(8) Probit	(9) OLS	(10) Probit	(11) OLS	(12) Probit
ADV	0.063***	0.216***	0.062***	0.282***	-0.013	0.196	-0.131	-0.015	0.002	0.218	0.281***	0.807*
	(0.014)	(0.056)	(0.013)	(0.060)	(0.125)	(0.422)	(0.102)	(0.449)	(0.102)	(0.373)	(0.104)	(0.436)
LAG	-0.059***	-0.310***	-0.031**	-0.097	0.003	0.012	-0.111	-0.499	0.029	-0.068	-0.284***	-0.804**
	(0.012)	(0.077)	(0.016)	(0.060)	(0.101)	(0.573)	(0.145)	(0.447)	(0.078)	(0.431)	(0.108)	(0.396)
ADV*R	0.064*	0.098	-0.030	0.076	0.013	0.006	0.034*	0.052	0.001	0.000	-0.004**	-0.010
	(0.033)	(0.129)	(0.030)	(0.134)	(0.022)	(0.073)	(0.018)	(0.077)	(0.002)	(0.006)	(0.002)	(0.008)
LAG*R	-0.077***	-0.391**	0.047	0.108	-0.012	-0.066	0.009	0.052	-0.002	-0.005	0.004**	0.013*
	(0.028)	(0.164)	(0.036)	(0.129)	(0.018)	(0.103)	(0.026)	(0.080)	(0.001)	(0.008)	(0.002)	(0.007)
R	0.151***	0.731***	0.112***	0.401***	0.034***	0.139***	-0.063***	-0.236***	0.004***	0.020***	0.007***	0.024***
	(0.018)	(0.089)	(0.020)	(0.075)	(0.011)	(0.042)	(0.012)	(0.038)	(0.001)	(0.004)	(0.001)	(0.004)
Exp.	0.003***	0.009***	-0.000	-0.002	0.002***	0.009***	0.001	0.003	0.002***	0.009***	-0.001	-0.003
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Age	0.002***	0.005***	0.000	0.000	0.002***	0.006***	0.000	0.001	0.002***	0.005***	-0.000	-0.000
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Sal_t	0.006	0.029	0.023**	0.084**	0.005	0.031	0.022**	0.087**	0.004	0.028	0.020**	0.073**
	(0.007)	(0.033)	(0.009)	(0.035)	(0.007)	(0.032)	(0.010)	(0.037)	(0.007)	(0.033)	(0.009)	(0.035)
Sal_{t-3}	0.013**	0.053	-0.035***	-0.133***	0.006	0.011	-0.045***	-0.177***	0.010	0.029	-0.033***	-0.125***
	(0.007)	(0.033)	(0.009)	(0.035)	(0.007)	(0.032)	(0.009)	(0.036)	(0.007)	(0.032)	(0.009)	(0.036)
Size	0.096***	0.484***	0.093***	0.362***	0.125***	0.582***	0.117***	0.463***	0.115***	0.547***	0.100***	0.389***
	(0.009)	(0.042)	(0.011)	(0.042)	(0.008)	(0.041)	(0.010)	(0.040)	(0.008)	(0.041)	(0.010)	(0.040)
Const.	-0.262***	-3.102***	0.732***	0.609***	-0.439***	-3.670***	1.110***	2.066***	-0.493***	-4.082***	0.347***	-0.743***
	(0.017)	(0.114)	(0.021)	(0.074)	(0.063)	(0.267)	(0.066)	(0.230)	(0.055)	(0.269)	(0.066)	(0.251)
Obs.	4569	4562	4547	4547	4569	4562	4547	4547	4569	4562	4547	4547
Adj. R^2	0.174		0.075		0.153		0.073		0.156		0.084	
Ps. R^2		0.199		0.080		0.172		0.079		0.177		0.085
F	14.672	.	3.914	.	1.095	.	0.878	.	1.989	.	15.342	.
p	0.000	0.007	0.048	0.842	0.295	0.517	0.349	0.994	0.158	0.558	0.000	0.014

Notes: The table presents results from OLS and probit estimations of obtaining ISO quality certification and of significant innovation of a product or service in the last 3 years on the levels of technology, on the interactions of the technology with the overall liberalization variable R as well as on other firm controls. The data are taken from the Enterprise Analysis Unit at the World Bank, from the Worldwide Governance Indicators (WGI), Economic Freedom of the World (EFW) Indices and Heritage Foundation (HFD) data. The table also presents the results from testing $ADV*R = LAG*R$. Robust standard errors are presented in parentheses. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.2: Reforms and Log(SPW) across Firms of Different Size

	Size: Log(No. of employees)				Size: Log(Value of assets)			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Log(SPW)_{t-1}	.71***	.98***	.62***	.38**	.71***	.04	.62***	.78***
	(.01)	(.06)	(.01)	(.18)	(.01)	(.11)	(.01)	(.10)
Log(KPW)	.15***	.01	.11***	.19***	.11***	.41***	.09***	.18***
	(.01)	(.03)	(.01)	(.05)	(.01)	(.05)	(.01)	(.04)
CMR*Size	.00*	.00	.01	.02	-.00	.01	.00	.05***
	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)	(.00)	(.01)
R*Size	-.01	-.01	-.01	-.01	-.00	-.02*	-.02**	-.11***
	(.01)	(.01)	(.01)	(.03)	(.00)	(.01)	(.01)	(.02)
RoL*Size	.02***	.01	-.01*	-.01	.01***	.02***	.01**	.02***
	(.00)	(.01)	(.01)	(.02)	(.00)	(.00)	(.00)	(.01)
T*Size	-.01***	.01*	.01**	.00	-.00	.02***	.01***	.02***
	(.00)	(.00)	(.01)	(.02)	(.00)	(.00)	(.00)	(.01)
Mgr. Exp.	.00	-.00	-.00	-.00	.00	.00	-.00	.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Firm Age	.00	-.00	.00	-.00	.00	-.00	.00	-.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Const.	.79***	-.39	2.45***	4.27***	.85***	4.02***	2.47***	2.48***
	(.11)	(.39)	(.13)	(1.38)	(.11)	(.41)	(.14)	(.36)
C'try Eff's	No	No	Yes	Yes	No	No	Yes	Yes
Obs.	16686	14383	16686	14383	16686	3133	16686	3133
Adj. R^2	.825	.795	.845	.832	.826	.679	.845	.839
Hansen J		.02		.03		.55		.03
APF CMRS		110.9		15.46		451.1		42.40
APF RS		68.59		11.82		162.5		16.05
APF RLS		333.1		47.77		4158		109.1
APF TS		399.8		46.69		342.3		90.40

Notes: The table presents results from OLS and 2SLS estimations of Log(Sales per worker) on lagged Log(SPW), Log(Capital per worker) and other observables from the firm-level data of the Enterprise Analysis Unit at the World Bank, and reform data, measured with Economic Freedom of the World (EFW) Indices, as well as on their interaction with firm size measured by either Log(No. of employees) or Log(Value of assets). All estimations include the age of the firm, its legal status, an indicator of a quality certificate and industry-year effects. Some estimations include country-year effects. The Hansen J-test and the first-stage Angrist-Pischke F-tests are given for each of the endogenous variables. Robust standard errors are in parentheses. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.3: Reforms and Log(Sales) across Firms of Different Size

	Size: Log(No. of employees)				Size: Log(Value of assets)			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Log(Sal)_{t-1}	.60***	.89***	.59***	.50**	.61***	.35***	.61***	.23***
	(.01)	(.31)	(.01)	(.25)	(.01)	(.07)	(.01)	(.09)
Log(K)	.09***	.01	.08***	.10*	.09***	.34***	.19***	.70***
	(.01)	(.08)	(.01)	(.05)	(.01)	(.07)	(.03)	(.16)
Log(L)	.28***	.14	.23***	.32***	.30***	.21***	.29***	.29***
	(.01)	(.19)	(.01)	(.11)	(.01)	(.03)	(.01)	(.04)
CMR*Size	-.00	-.00	.00	.01	-.00	.01	-.00	.02
	(.00)	(.02)	(.01)	(.02)	(.00)	(.00)	(.00)	(.02)
R*Size	-.00	.01	-.01	-.02	.00	-.02	-.01	-.04
	(.00)	(.03)	(.01)	(.03)	(.00)	(.01)	(.01)	(.03)
RoL.*Size	.02***	.00	-.00	.01	.00***	.01***	.00	.02
	(.00)	(.01)	(.00)	(.02)	(.00)	(.00)	(.00)	(.01)
T*Size	.00	-.01	.03***	.01	-.00***	.01	-.01	-.04*
	(.00)	(.01)	(.01)	(.01)	(.00)	(.01)	(.00)	(.02)
Mgr. Exp.	.00	-.00	-.00	-.00	.00	.00***	-.00	.00**
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Firm Age	-.00***	-.00***	-.00***	-.00**	-.00***	-.00*	-.00***	-.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Const.	3.09***	.00	1.16***	1.41	2.97***	1.68***	3.03***	1.88***
	(.16)	(.82)	(.14)	(1.12)	(.16)	(.22)	(.20)	(.25)
C'try Eff's	No	No	Yes	Yes	No	No	Yes	Yes
Obs.	17207	13713	17207	13713	17207	3286	17207	3286
Adj. R^2	.927	.912	.932	.932	.927	.932	.930	.921
Hansen J		.65		.04		.00		.46
APF CMRS		9.80		25.16		4512		213.0
APF RS		8.32		13.10		1703		84.76
APF RLS		536.8		29.66		32359		577.4
APF TS		106.8		53.60		43461		2166

Notes: The table presents results from OLS and 2SLS estimations of Log(Sales) on lagged Log(SAL), Log(Capital), Log(Labor costs) and other observables from the firm-level data of the Enterprise Analysis Unit at the World Bank, and on reform data, measured with Economic Freedom of the World (EFW) Indices, as well as on their interaction with the firm size measured by either Log(No. of employees) or Log(Value of assets). All estimations include the age of the firm, its legal status, an indicator of a quality certificate and industry-year effects. Some estimations include country-year effects. The Hansen J-test and the first-stage Angrist-Pischke F-tests are given for each of the endogenous variables. Robust standard errors are in parentheses. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.4: Reforms and Δ Log(SPW) across Firms of Different Size

	Size: Log(No. of employees)				Size: Log(Value of assets)			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Δ LogKPW	.46***	.44***	.44***	.43***	.45***	.44***	.45***	.43***
	(.04)	(.04)	(.04)	(.04)	(.04)	(.05)	(.04)	(.05)
CMR*Size	.03***	.05***	.04***	.04**	.01***	.02***	.02***	.05***
	(.01)	(.01)	(.01)	(.02)	(.00)	(.00)	(.01)	(.01)
R*Size	-.06***	-.08***	-.11***	-.09***	-.02**	-.03***	-.06***	-.11***
	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.02)	(.02)
RoL*Size	.02***	.02***	.03***	.02	.00***	.01***	.01**	.02**
	(.00)	(.00)	(.01)	(.01)	(.00)	(.00)	(.01)	(.01)
T*Size	-.00	.01	.02***	.03***	-.00	.00	.02**	.02***
	(.01)	(.01)	(.01)	(.01)	(.00)	(.00)	(.01)	(.01)
Mgr. Exp.	.00	.00	.00	.00	.00	.00	.00	.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Firm Age	-.00*	-.00***	-.00*	-.00***	-.00	-.00**	-.00	-.00**
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Const.	.51***	.47***	.66***	.56***	.53***	.55***	1.13***	1.41***
	(.14)	(.15)	(.15)	(.16)	(.17)	(.17)	(.26)	(.28)
C'try Eff's	No	No	Yes	Yes	No	No	Yes	Yes
Obs.	3840	3133	3840	3133	3840	3133	3840	3133
Adj. R^2	.275	.294	.283	.305	.274	.292	.283	.304
Hansen J		.00		.00		.00		.00
APF CMRS		106.2		49.77		573.3		63.05
APF RS		32.84		16.97		217.3		33.84
APF RLS		1637		144.4		9861		178.0
APF TS		284.8		104.3		185.6		268.2

Notes: The table presents results from OLS and 2SLS estimations of the change in Log(Sales per worker) on the change in Log(Capital per worker) and other observables from the firm-level data of the Enterprise Analysis Unit at the World Bank and reform data, measured with Economic Freedom of the World (EFW) Indices, as well as on their interaction with the firm size measured by either Log(No. of employees) or Log(Value of assets). All estimations include the age of the firm, its legal status, an indicator of a quality certificate and industry-year effects. Some estimations include country-year effects. The Hansen J-test and the first-stage Angrist-Pischke F-tests are given for each of the endogenous variables. Robust standard errors are in parentheses. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.5: Reforms and Δ Log(Sales) across Firms of Different Size

	Size: Log(No. of employees)				Size: Log(Value of assets)			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Δ Log(K)	.36***	.37***	.34***	.35***	.35***	.36***	.34***	.35***
	(.04)	(.05)	(.04)	(.04)	(.04)	(.05)	(.04)	(.05)
Δ Log(L)	.30***	.31***	.31***	.32***	.31***	.32***	.32***	.33***
	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)
CMR*Size	.04***	.05***	.05***	.04**	.01***	.02***	.03***	.05***
	(.01)	(.01)	(.01)	(.02)	(.00)	(.00)	(.01)	(.01)
R*Size	-.06***	-.08***	-.12***	-.10***	-.02**	-.03***	-.06***	-.11***
	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.02)	(.02)
RoL.*Size	.02***	.02***	.03***	.02	.01***	.01***	.01***	.03***
	(.00)	(.00)	(.01)	(.01)	(.00)	(.00)	(.00)	(.01)
T*Size	.00	.01	.03***	.03***	.00	.00	.02**	.02***
	(.01)	(.01)	(.01)	(.01)	(.00)	(.00)	(.01)	(.01)
Mgr. Exp.	.00	.00	.00	.00	.00	.00	.00	.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Firm Age	-.00***	-.00***	-.00***	-.00***	-.00***	-.00***	-.00***	-.00***
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Const.	.50***	.57***	.67***	.67***	.46***	.60***	1.08***	1.52***
	(.14)	(.14)	(.15)	(.16)	(.16)	(.17)	(.25)	(.28)
C'try Eff's	No	No	Yes	Yes	No	No	Yes	Yes
Obs.	3840	3133	3840	3133	3840	3133	3840	3133
Adj. R^2	.285	.298	.295	.309	.283	.295	.293	.306
Hansen J		.00		.00		.00		.00
APF CMRS		2585		1029.4		9110		627.5
APF RS		2603		1155		9884		567.3
APF RLS		14492		1673.4		27712		692.4
APF TS		94734		58129		167689		28479

Notes: The table presents results from OLS and 2SLS estimations of the change in Log(Sales) on the change in Log(Capital), the change in Log(No. employess) and other observables from the firm-level data of the Enterprise Analysis Unit at the World Bank and on reform data, measured with Economic Freedom of the World (EFW) Indices, as well as on their interaction with the firm size measured by either Log(No. of employees) or Log(Value of assets). All estimations include the age of the firm, its legal status, an indicator of a quality certificate and industry-year effects. Some estimations include country-year effects. The Hansen J-test and the first-stage Angrist-Pischke F-tests are given for each of the endogenous variables. Robust standard errors are in parentheses. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.6: Reforms and Log(SPW) across Firms of Different Size: Robustness with More IVs

	Size: Log(No. of employees)				Size: Log(Value of assets)			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Log(SPW)_{t-1}	.71***	.96***	.62***	.66***	.71***	.04	.62***	.78***
	(.01)	(.03)	(.01)	(.13)	(.01)	(.11)	(.01)	(.10)
Log(KPW)	.15***	.02	.11***	.10**	.11***	.41***	.09***	.18***
	(.01)	(.02)	(.01)	(.04)	(.01)	(.05)	(.01)	(.04)
CMR*Size	.00*	-.01	.01	.01	-.00	.01	.00	.05***
	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)	(.00)	(.01)
R*Size	-.01	.01	-.01	-.02	-.00	-.02*	-.02**	-.11***
	(.01)	(.01)	(.01)	(.03)	(.00)	(.01)	(.01)	(.02)
RoL*Size	.02***	.01*	-.01*	-.01	.01***	.02***	.01**	.02***
	(.00)	(.00)	(.01)	(.01)	(.00)	(.00)	(.00)	(.01)
T*Size	-.01***	.01**	.01**	.02*	-.00	.02***	.01***	.02***
	(.00)	(.00)	(.01)	(.01)	(.00)	(.00)	(.00)	(.01)
Mgr. Exp.	.00	-.00	-.00	-.00	.00	.00	-.00	.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Firm Age	.00	-.00	.00	-.00	.00	-.00	.00	-.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Const.	.79***	-.27	2.45***	2.17**	.85***	4.02***	2.47***	2.48***
	(.11)	(.20)	(.13)	(1.05)	(.11)	(.41)	(.14)	(.36)
C'try Eff's	No	No	Yes	Yes	No	No	Yes	Yes
Obs.	16686	14383	16686	14383	16686	3133	16686	3133
Adj. R^2	.825	.800	.845	.851	.826	.679	.845	.839
Hansen J		.00		.00		.55		.03
APF CMRS		104.0		14.00		451.1		42.40
APF RS		59.84		9.42		162.5		16.05
APF RLS		37.02		56.75		4158		109.1
APF TS		391.3		59.27		342.3		90.40

Notes: The table presents results from OLS and 2SLS estimations of Log(Sales per worker) on lagged Log(SPW), Log(Capital per worker) and other observables from the firm-level data of the Enterprise Analysis Unit at the World Bank and reform data, measured with Economic Freedom of the World (EFW) Indices, as well as on their interaction with the firm size measured by either Log(No. of employees) or Log(Value of assets). All estimations include the age of the firm, its legal status, an indicator of a quality certificate and industry-year effects. Some estimations include country-year effects. The Hansen J-test and the first-stage Angrist-Pischke F-tests are given for each of the endogenous variables. Robust standard errors are in parentheses. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.7: Reforms and Log(SPW) across Firms of Different Size: Robustness for CMR definition

	Size: Log(No. of employees)				Size: Log(Value of assets)			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Log(SPW)_{t-1}	.68***	1.18***	.65***	.36	.68***	.05	.65***	.84***
	(.02)	(.06)	(.02)	(.33)	(.02)	(.12)	(.02)	(.10)
Log(KPW)	.24***	-.06	.19***	.32**	.19***	.41***	.18***	.16***
	(.02)	(.04)	(.01)	(.15)	(.02)	(.06)	(.02)	(.04)
CMR*Size	-.00	-.03***	.00	.00	-.00***	.01	-.00	.08***
	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)	(.01)	(.02)
R*Size	.03***	.07***	-.01	-.04*	.01***	-.02*	.00	-.10***
	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)	(.01)	(.02)
RoL*Size	.01***	-.01	.01	.00	.00***	.02***	.01	.01
	(.00)	(.01)	(.01)	(.01)	(.00)	(.00)	(.01)	(.01)
T*Size	-.03***	-.01***	.01	.04***	-.00***	.01***	-.00	-.01
	(.00)	(.00)	(.01)	(.01)	(.00)	(.00)	(.01)	(.01)
Mgr. Exp.	-.00***	-.00**	.00	.00	-.00**	.00	.00	.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Firm Age	-.00***	-.00**	-.00	-.00*	-.00***	-.00	-.00	-.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Const.	.07	-1.78***	1.41***	2.96	-.01	3.95***	1.57***	2.36***
	(.12)	(.32)	(.18)	(2.03)	(.12)	(.42)	(.21)	(.37)
C'try Eff's	No	No	Yes	Yes	No	No	Yes	Yes
Obs.	5309	4600	5309	4600	5309	3069	5309	3069
Adj. R^2	.893	.828	.901	.886	.894	.687	.901	.830
Hansen J		.00		.13		.28		.03
APF CMRS		687.7		61.10		239.6		33.65
APF RS		394.3		45.85		209.1		3.30
APF RLS		1248		162.5		4571		229.6
APF TS		1144		65.63		551.7		111.2

Notes: The table presents results from OLS and 2SLS estimations of Log(Sales per worker) on lagged Log(SPW), Log(Capital per worker) and other observables from the firm-level data of the Enterprise Analysis Unit at the World Bank and reform data, measured with Economic Freedom of the World (EFW) Indices, as well as on their interaction with the firm size measured by either Log(No. of employees) or Log(Value of assets). All estimations include the age of the firm, its legal status, an indicator of a quality certificate and industry-year effects. Some estimations include country-year effects. The Hansen J-test and the first-stage Angrist-Pischke F-tests are given for each of the endogenous variables. Robust standard errors are in parentheses. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.8: Reforms and ΔLog(SPW) across Firms of Different Size: Is the Great Recession Affecting the Results?

	Full Sample				Sample Before 2008			
	Size:Log(L)		Size:Log(K)		Size:Log(L)		Size:Log(K)	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
CMR*Size	.02***	.04**	.01*	.02	.02***	.02	.01***	.04***
	(.01)	(.02)	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)
RoL*Size	.00	.03	.00	.03**	.00	-.01	.01*	.04***
	(.01)	(.02)	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)
T*Size	.00	-.01	-.00	-.05***	.01	-.02	.00	-.04***
	(.01)	(.01)	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)
Mgr. Exp.	.00	.00	.00	.00	.00	.00	.00	.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Firm Age	.00**	-.00	.00*	.00	.00	-.00	.00	.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Const.	.20*	-.26*	.21*	-.19*	.02	-.46***	-.15	-.57***
	(.11)	(.14)	(.12)	(.11)	(.14)	(.13)	(.17)	(.20)
C'try Eff's	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	16686	14383	16686	14383	12705	11458	12705	11458
Adj. R^2	.081	.079	.080	.078	.102	.099	.102	.098
Hansen J		.00		.00		.00		.00
APF CMRS		15.05		21.26		19.07		27.86
APF RS		9.72		25.46		1.75		32.74
APF RLS		46.47		159.37		54.09		189.07
APF TS		52.92		191.13		43.60		167.45

Notes: The table presents results from OLS and 2SLS estimations of the difference in Log(Sales per worker) on observables from the firm-level data of the Enterprise Analysis Unit at the World Bank, and on reform data, measured with The Economic Freedom of the World (EFW) Indices, as well as on their interaction with the firm size measured by either Log(No. of employees) or Log(Value of assets). All estimations include the age of the firm, its legal status, an indicator of a quality certificate, and industry-year effects. All estimations include country-year effects. The Hansen J-test and the first-stage Angrist-Pischke F-tests are given for each of the endogenous variables. Robust standard errors are in parentheses. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.9: Reforms and ΔLog(Sales) across Firms of Different Size: Is the Great Recession Affecting the Results?

	Full Sample				Sample Before 2008			
	Size:Log(L)		Size:Log(K)		Size:Log(L)		Size:Log(K)	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Δ Log(L)	.37***	.36***	.38***	.36***	.35***	.35***	.36***	.35***
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
CMR*Size	.02***	.07***	.01*	.03***	.03***	.03**	.01***	.04***
	(.01)	(.02)	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)
R*Size	-.04***	-.12***	-.01*	-.03	-.05***	-.05*	-.03***	-.04**
	(.02)	(.03)	(.01)	(.02)	(.01)	(.03)	(.01)	(.02)
RoL.*Size	.00	.07***	.00	.02*	-.00	.02	.01**	.02**
	(.01)	(.02)	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)
T*Size	.01*	-.02	.00	-.03***	.02***	-.01	.01*	-.03**
	(.01)	(.01)	(.00)	(.01)	(.01)	(.02)	(.00)	(.01)
Mgr. Exp.	-.00	-.00	-.00	-.00	-.00	-.00	.00	-.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Firm Age	-.00***	-.00***	-.00***	-.00***	-.00***	-.00***	-.00***	-.00***
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Const.	.08	.09	.23**	.20	-.02	-.05	.04	.49***
	(.11)	(.09)	(.12)	(.22)	(.13)	(.10)	(.17)	(.13)
C'try Eff's	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	16686	14383	16686	14383	12705	11458	12705	11458
Adj. R^2	.150	.149	.150	.149	.178	.184	.177	.176
Hansen J		.00		.00		.00		.00
APF CMRS		3.70		22.30		38.19		28.97
APF RS		27.19		26.50		29.08		33.50
APF RLS		156.88		16.85		199.09		187.82
APF TS		169.19		19.77		131.85		168.41

Notes: The table presents results from OLS and 2SLS estimations of the change in Log(Sales) on the change in Log(No. employees) and other observables from the firm-level data of the Enterprise Analysis Unit at the World Bank, and on reform data, measured with The Economic Freedom of the World (EFW) Indices, as well as on their interaction with the firm size measured by either Log(No. of employees) or Log(Value of assets). All estimations include the age of the firm, its legal status, an indicator of a quality certificate, and industry-year effects. All estimations include country-year effects. The Hansen J-test and the first-stage Angrist-Pischke F-tests are given for each of the endogenous variables. Robust standard errors are in parentheses. Symbols: * $p < .10$, ** $p < .05$, *** $p < .01$

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