TRADE LIBERALIZATION
AND THE POLITICS OF FINANCIAL DEVELOPMENT

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Abstract

A well developed financial system enhances competition in the industrial sector by allowing easier entry. The impact varies across industries, however. For some, small changes in financial development quickly induce entry and dissipate incumbents' rents, generating strong incentives to oppose improvement of the financial system. In other sectors incumbents may even benefit from increased availability of external funds. The relative strength of promoters and opponents determines the political equilibrium level of financial system development. This may be perturbed by the effect of trade liberalization in the strength of each group. Using a sample of 41 trade liberalizers we conduct an event study and show that the change in the strength of promoters vis-à-vis opponents is a very good predictor of subsequent financial development. The result is not driven by changes in demand for external funds, or by the success of the trade policy. The relationship is mediated by policy reforms, the kind that induces competition in the financial sector, in particular. Real effects follow not so much from capital deepening but mainly through improved allocation. The effect is stronger in countries with high levels of governance, suggesting that incumbents resort to this costly but more subtle way of restricting entry where is difficult to obtain more blatant forms of anti-competitive measures from politicians.

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1. Introduction

It has been extensively documented that the level of financial development, whatever measured, varies greatly across countries (LaPorta et al. (1997, 1998)). This does not imply that the ranking of countries does not vary through time, though. The rank in terms of level of private credit to GDP in the early 1970s explains *only* 51% of the cross-country variance in the rank 25 years later[2]. More than half the countries moved out of their position decile, with two out of seven countries moving out of their quartile. For instance, from having one of the least developed financial systems in the world, Bolivia became roughly the median country, with a level of private credit to GDP of 0.48 (as compared to 0.05 initially). Costa Rica, initially at the 55 percentile (private credit of 0.19), by the end of the 1990s had a level of financial depth comparable only to the African (0.14).

Our theories of financial development need to explain at the same time the relatively high persistence in the indicators of financial depth and these non-trivial changes in the ranking across countries at different moments in time. Existing theories -that rely on stable and largely predetermined institutional features- successful as they are in explaining the cross-section variation, are challenged when applied to the time series dimension of the data. Moreover, if deep institutional factors are behind financial development the prospect for solving the problem is grim. Both Bolivia and Costa Rica inherited the same legal origin (in the line of LaPorta et al. (1997, 1998)), were colonized following a similar pattern (Acemoglu and Johnson (2003)), share a common religion (Stulz and Williamson (2001)), and are probably not very different in terms of social capital endowment (Guiso et al. (2004)) and many other institutional features. The difference in outcomes does not seem to be driven by the demand side either. Not only was Bolivia initially somewhat poorer than Costa Rica, but it also grew one and a half percentage points slower during the period. Moreover, both countries' manufacturing sector composition was such that their aggregate demand for external finance was within half a standard deviation between one another and not significantly different from the mean (Rajan and Zingales (1998)). Nor the difference seems to be related to the balance between bank and market-based systems, since neither of them have a particularly well developed stock exchange (see Allen and Gale (1999) for a discussion on the issue).

Rajan and Zingales (2003) propose a political economy view to understand the u-shape pattern of financial development during the 20th Century. The political economy approach seems sensible. On the one hand policies matter: financial system depth is not driven solely by differences in the general level of economic development –a proxy for the demand of financial services- but also by differences in the rules pertaining to financial systems and their enforcement (La Porta et al (1997)). It is also interesting to note that financial sector regulations are very much ad-hoc and typically form a self-contained body of rules

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[2] The figures mentioned in this introduction come from a sample of the 73 countries for which we have data on both financial system and trade indicators since 1970.

and enforcement reasonably distinct from other economic institutions. On the other hand financial development seems to exert a first order, positive impact on economic outcomes (King and Levine (1993), Demirguc-Kunk and Maksimovic (1998), Rajan and Zingales (1998), Jayaratne and Strahan (1996), among others). More to the point, it is being documented recently that not everybody is equally affected (Rajan and Zingales (1998), Kroszner and Strahan (1999), Braun (2002), Raddatz (2003), Braun and Larrain (2004)). This literature is advancing our knowledge of the mechanism through which financial development has an effect on real outcomes. Putting all together suggests that distinct policies affecting the development of financial markets are likely to have important distributive consequences. This should be fertile ground for finding political economy explanations[3].

The political economy game we have in mind builds on the premise that a well developed financial system enhances competition in the industrial sector by allowing easier entry. There is some previous evidence on this mechanism. Rajan and Zingales (1998) show that most of the difference in growth between more and less external finance dependent industries across countries sorted by financial development comes from differences in the growth of the number of firms as opposed to the growth of the typical establishment. Cetorelli (2001, 2003) Cetorelli and Strahan (2003) show that lower degrees of banking competition are associated with larger firms across countries, across US States, and following the passage of the Second European Banking Directive. Here we further document this fact by showing that both aggregate manufacturing sector price-cost margins and average firm size –which we take as measures of incumbents' rents or the inverse of the degree of competition in industry- are significantly negatively correlated with financial development across countries. This complements the literature that looks for a mechanism for financial development to affect real outcomes, and also early results on the interrelation between financial constraints and product market competition (Chevalier and Scharfstein (1995, 1996), Phillips (1995)).

We show, however, that there is important heterogeneity on the impact of financial development on these measures across industries. In each industry, incumbents weight the benefits of easier access to external finance with the costs of increased competition. For some industries, such as beverages, leather, and food, margins fall more sharply and the number of firms rises more rapidly with financial development. This suggests that the costs of entry probably outweigh the benefits of easier access to external funds. Incumbents in these industries would probably oppose policies meant to improve the financial system. Incumbents in sectors such as pharmaceuticals, printing, and furniture, where margins are either little affected or actually increase are more likely to favor them. We split the industries in two

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[3] This approach, which has a long tradition in the analysis of regulatory reform dating at least since the seminal work of Stigler (1971), has only recently being applied to the regulation of financial markets. See Kroszner (1998) for a discussion of the issues when the framework is applied to banking and financial regulatory reform across countries.

equal-sized groups along this dimension and call them the *promoters* and *opponents* of financial development.

The relative strength of each group determines the equilibrium level of financial system sophistication. Absent significant perturbations to this political economy equilibrium we do not expect significant changes in financial development. There has been, however, a significant change during this period which is that an important number of countries opened their borders for trade in goods. As stressed by Rajan and Zingales (2003), trade can have profound effects on the politics of financial development. The essence of the argument is that trade liberalization decreases incumbents' rents and consequently both their ability and willingness to oppose financial sector development[4]. Morck et al. (1998)'s evidence on the fall in stock price of Canadian firms controlled by old money families following the unexpected enactment of free trade with the U.S. is consistent with the general argument. However, Siegel (2003) finds that increasing openness in Korea actually strengthened the country's most politically connected firms. We show here that on average the countries that liberalized trade during the last three decades of the 20th Century gained around 4 spots in the ranking of private credit to GDP. The figure is neither significant nor economically noteworthy. Both Bolivia and Costa Rica, for instance, liberalized trade at about the same time (1985 and 1986, respectively). While the first climbed many spots, the second was advantaged by around one third of the countries. The average effect was positive but small. However, the fact that trade liberalization does not have much of an effect *on average* does not imply that it does not matter. The sign of the effect is most likely to be country-specific as the conflicting evidence of Morck et al. (1998) and Siegel (2003) suggest. Trade liberalization is a perturbation to the relatively high persistence of private credit. Among the countries that did not liberalize trade since the early 1970s the initial position in the ranking explains 67% of the variation in the final position. For those that liberalized, however, the dependence on history is completely broken -the initial ranking accounts for less than 5% of the final one. We, therefore, provide a way to reconcile the evidence showing that deep institutional factors determine to a great extent the cross-country level of financial development with the view that openness to trade changes things.

Relative prices are profoundly and permanently affected by trade liberalization. Since the change in prices is a function of both world prices, comparative advantages, and the initial structure, the effect of trade liberalization across sectors varies from country to country. In Bolivia the average price-cost margin of the promoters increased by 4.6 percentage points relative to that of the opponents. We label this figure the *change in the relative strength of promoters*. In Costa Rica, on the contrary, opening up for trade brought about a decline in the strength of promoters of measure -5.8. In the five years preceding

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[4] Rajan and Zingales (2003) point out that the effect on financial development is likely to be stronger when the opposition of financial sector incumbents is muted by free flow of capital.

liberalization Bolivia and Costa Rica's private credit to GDP was on average 0.12 and 0.18 respectively. After five years had passed since liberalization and up to the tenth year, the average figure was 0.37 for Bolivia and 0.12 for Costa Rica.

We repeat this event study for a sample of 41 countries that liberalized trade and find that the change in the relative strength of promoters induced by trade liberalization is a very good predictor of subsequent financial development. The political economy variable alone explains around one fourth of the change in private credit over GDP not accounted by its initial level and the fixed liberalization-year effects we also include in the specification. The result is robust to a battery of tests that includes controlling for demand-side determinants of financial development, different strategies to classify promoters and opponents, the use of different event study windows, separating early and late liberalizers, excluding potentially influential industries, and several other changes to the specification. Bolivia and Costa Rica are good examples of countries in the first and fourth quartiles in terms of change in promoters' strength. The economic magnitude of the effect when comparing these two groups directly is similar. The typical country in the first quartile experiences a relative strengthening of promoters of 5 percentage points and an increase in private credit of 14 points of GDP (from 0.31 to 0.41). In the average country of the fourth quartile opponents are strengthened by 5 points and private credit increases just 3 points (from 0.21 to 0.24).

We adopt a *de-facto* approach throughout the paper with the rationale of testing the political economy view in the most general way. We avoid imposing our preconceptions about what factors determine whether an industry will tend to oppose or favor policies that develop the financial system. We do not impose structure on the effect of trade openness across industries, either. Both the classification and the effect of trade come directly from the data. Importantly, we do not focus a-priori on particular policies, but just measure the final effect on financial development. This stands in contrast to the approach of the recent literature on corporate governance which emphasizes this one determinant[5]. It also differentiates this paper from the literature following Kroszner and Strahan (1999) that, although based on similar arguments about the differential effect across groups and the event study methodology, tend to focus on particular policies and case studies (interstate bank branching deregulation in this case). Our data have much richer variation in terms of number of countries, time period, and the within country dimension.

Of course, we try to address the shortcoming of the de-facto approach. Despite not being able to directly show the groups influencing the government as in case studies (notably Kroszner and Strahan

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[5] Some relevant papers in this literature include: Bolton and Rosenthal (2002), Biais and Recasens (2001), Ang and Boyer (1999), Pagano and Volpin (2000), Pistor (1999), Holmstrom and Kaplan (2001), Hellwig (2000), Biais and Perotti (2001), Johnson and Shleifer (2000).

(1999)), we recognize that policy reform is at the heart of the mechanism we propose. With the caveat of limited data availability, we show evidence suggesting that the relation between the change in relative strength and subsequent financial development is indeed mediated by policy adjustments made in the five-year period following trade liberalization. Policies that induce competition in the financial sector are particularly important. Moreover, we check that the real consequences of financial development are present, working not so much through increased investment but through improvement in the quality of capital allocation (as measured in the way of Wurgler (2000)). The question of why would incumbents resort to financial underdevelopment to protect rents instead of using more direct forms of entry barriers is addressed. We show that our political economy story is especially strong in countries with relatively high levels of governance (as captured by the degree of rule of law, corruption, etc.). We interpret this as suggesting that incumbents resort to this costly but more subtle way of restricting entry when the degree of governance would make it difficult to obtain more blatant forms of anti-competitive measures from politicians.

Wouldn't the agents involved anticipate the financial effects of trade liberalization and internalize them in their decision to open up? The two reforms are not independent, and the relationship between them is not obvious (see, for instance, the arguments and results by Aizenman (2004), and Aizenman and Noy (2004)[6]). We certainly do not pretend ours to be the whole story. We deal with this issue in a number of ways both theoretical and empirical. On the theoretical side it might not be obvious a-priori the exact effect of trade liberalization across sectors. Empirically we include only countries that liberalized trade so that the decision to open up has no effect on the coefficient for the political economy variable. Also, we include liberalization-year fixed effects to control for the timing of the decision. Finally, we recognize that many countries liberalize trade not as a consequence only of politics but also due to factors external to the country, so that the decision is not entirely endogenous. For many trade liberalization occurred as a result of IMF intervention. Since the IMF emphasis on financial market policies is relatively new, splitting the sample between early and late liberalizers, and showing a similar effect across them suggests that this reforms interdependence does not bias our results.

The paper is also related to the literature on the real effects of trade liberalization started by Sachs and Warner (1995) when showing the impact on growth. When showing that relative prices change distinctively across countries following liberalization we propose paying attention not just to the average effect of trade but also at the heterogeneity of it. This has direct implications in terms of the political economy of trade and relates to empirical work on the matter (see Rodrick (1996) for a survey). We also delve into the relationship between finance and trade, and so relate to Beck (2003), Svaleryd and Vlachos

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[6] These papers suggest that trade liberalization and capital account liberalization are endogenous. Here we focus on the development of the domestic financial sector instead.

(2004). Finally, we show that particular policies adopted as a result of our political economy mechanism are indeed related to outcomes. In this sense the paper adds an additional layer of exogeneity to the literature on the effects of financial liberalization (Williamson and Mahar (1998), Abiad and Mody (2003)).

The rest of the paper is structured as follows. Section 2 explains our empirical methodology, including measurement issues and a discussion of the assumptions that are implicit in our approach. Section 3 presents the main result of the paper, the robustness tests, and the additional results regarding the mechanism underlying our political economy story. Section 4 concludes.

2. Methodology and data

Our empirical approach consists of testing whether a shock to the ability of different parties to influence politics in favor or against financial development affects the subsequent development of the financial sector. Our hypothesis is that when trade liberalization strengthens (in relative terms) those parties that favor financial development it will be followed by an increase in the depth of financial markets. We conduct an event study for 41 liberalizing countries and then explain the cross-sectional effect on financial development by running the following regression:

$$\Delta FD_c = \alpha_0 + \beta \times \Delta (STRENGTH\ PROMOTERS)_c + X_c \gamma + \varepsilon_c \qquad (1.1)$$

where ΔFD_c is a measure of the change in financial development and $\Delta (STRENGTH\ PROMOTERS)$ is a measure of the change in the relative strength of the parties that favor financial development (the *promoters*), both computed around the trade liberalization episode. X_c is a general set of possible controls, and ε_c is the error term, which may include several components. Our hypothesis implies that the coefficient β should be significantly positive.

In the rest of this section we explain how we identify the parties that favor and oppose financial development, justify our views of trade liberalization as a shock to the political economy equilibrium, and explain the measurement of the impact of trade liberalization on the strength of the different parties. The details of the specification will be discussed in section 3.2. The different data sources used at each step are discussed along the way.

2.1 Winners and losers from financial development

There are many dimensions along which the development of the financial system can have an asymmetric effect across groups. Although we could, in principle, identify winners and losers along each

dimension, in our setting the basic source of conflict across groups comes from the effect that financial development has on the product market. The idea that finance has an effect on how firms conduct business is not new (see, for instance, Chevalier (1995), Phillips (1995)). Rajan and Zingales (2003), in particular, provide the basic mechanism on which we build here. They argue that a more developed financial system reduces the correlation between credit allocation and a borrower's collateral and reputation, which facilitates the entry of new firms, increasing the degree of competition and reducing the rents of incumbents.

Figure 1 suggests that the mechanism is indeed empirically relevant. Panel A plots, for a sample of countries, the average ratio of *Private Credit to GDP* during the 1980s and 1990s versus the average *Price-Cost Margin* in manufactures during the same period. The correlation is strong and significant (-0.27, significant at the 0.2% level). The Price-Cost Margin (henceforth PCM) -computed using industry data from the Unido (2002) dataset- is defined as follows:

$$PCM = \frac{Value\ of\ Sales - Payroll - Cost\ of\ Materials}{Value\ of\ Sales}.$$

PCM is essentially a measure of the profitability of incumbents, the flow accrued to the owners of capital. One can think of a number of refinements to this indicator –that would take into account the amount of capital invested and indirect taxes, in particular. Our choice is dictated primarily by simplicity and data availability. Since, as will be made clear below, we will not be using the level of PCM but will just rely on its within-country, cross-industry variation, the simplification is unlikely to be of first-order importance. The methodology implies that the fact that some industries have higher margins everywhere due to larger capital requirements or taxes (tobacco and oil, for instance), or that some countries exhibit higher margins across the board (perhaps due to a lower level of competition or higher regulatory requirements) will have no impact at all in our measurement.

We are not the first to use PCM to proxy for the degree of product market competition. The measure has been shown to be strongly positively correlated with measures of concentration across industries (see for example Domowitz et al. (1986), Collins and Preston (1969), Clarke et al (1984), and Encaoua and Jacquemin (1980)). This correlation is shown in Panel B, which plots the average PCM across U.S. manufacturing industries against the C-4 concentration ratio (the sales of the four largest firms in an industry to total industry sales)[7].

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[7] The concentration data was obtained from the U.S. Bureau of the Census. For the vast majority of countries in our sample data are not available to construct such a measure.

As a robustness check we consider an alternative measure of how incumbents are differently affected by financial development based on quantity instead of price indicators. In particular, we measure the extent to which average firm size across industries is related to private credit. The ranking of industries along this dimension is quite similar to the one using the PCM measure. The correlation of the two variables is 0.58, significant at 1% levels. The results turn out to be basically same.

Although incumbents seem to be, on average, worse off relative to potential entrants with an increase in financial development, the effect can vary significantly across industries. Industries where incumbents rents are (relatively) more affected by the development of financial markets are probably more willing to organize and spend resources to maintain policies that keep the financial system underdeveloped than those industries whose rents are unaffected by financial conditions.

To identify the relative *promoters* and *opponents* of financial development we look at the effect of financial development on the PCM of 28 different three digit ISIC industries across countries by estimating the parameters of the following regression:

$$PCM_{ic} = \alpha_0 + \alpha_i + \alpha_c + \sum_i \eta_i \times FD_c + \varepsilon_{ic}, \qquad (1.2)$$

where PCM_{ic} is the PCM of industry i in country c, α_0 is a constant, α_i and α_c are industry and country fixed effects respectively, FD_c is the financial development of country c measured as the ratio of *Private Credit to GDP* (obtained from World Development Indicators 2003), and the η_i measure the relative effect of financial development on industry i's PCM. Both the PCM and private credit correspond to the averages for the period 1980-2000.

The relationship between incumbent rents and financial development is, of course, quite complex. A number of industry characteristics are likely to be involved. Since there is not much previous research upon which to rely here, it is difficult to come up with good proxies for some potentially important ones (such as the importance of innovation or the minimum efficient scale). These characteristics may also interact in a complex and non-monotonic way to determine the total effect of financial development on margins. Our approach reflects these problems and takes an agnostic position regarding which industries we expect to be relatively more and less affected. We just let the data speak. Figure 2, which shows the relationship between private credit and margins for two different industries (Beverages and Machinery), suggests that the effect is indeed heterogeneous.

Two comments regarding the specification are in place before showing the results. First, the specification in equation (1.2) suffers from reverse causality. The reason is that when rents are high

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incumbents have more resources to persuade politicians to keep in place legislation that restricts the development of financial markets. We address this problem by instrumenting the measure of financial development. The instrument we use is a country's legal origin, as it is standard in the law and finance literature (La Porta et al. (1997), Beck et al. (2000)). Second, because of the multicollinearity induced by the "dummy problem" we can only identify relative effects. So the η_i coefficient capture the impact of financial development on industry i PCM relative to an arbitrary benchmark industry.

The relative effects of financial development on the margins of different manufacturing industries (the η coefficients), obtained from the estimation of equation (1.2), are presented in Table 1. Column one reports the estimated effects, and column two the standard deviations. The demeaned values of the effects are reported in column three. A simple inspection of the table shows that there is indeed significant variation on the estimated effects across industries. A Wald's test, reported at the bottom of the table, strongly rejects the hypothesis that all the effects are equal. This dispersion can be observed in Figure 3a. The figure plots the η coefficients of each industry against its private-credit-weighted average PCM.[8] Besides presenting the dispersion of the coefficients, the figure shows that the relationship is not materially affected by a few outliers. The relationship between the PCM and the size measure is depicted in Figure 3b.

We use the η coefficients to distinguish between those industries that favor (in relative terms) policies conducive to the development of the financial system (henceforth the *"Promoters"*) and those industries that oppose these policies (henceforth the *"Opponents"*). We identify the promoters (opponents) with those industries with a η coefficient above (below) the median. This criteria has the advantage of simplicity and of taking into account the natural clustering observed in η.

2.2 Measuring the impact of trade liberalization

We estimate the impact of trade liberalization on margins across industries in each country using an event-study approach. That is, we consider trade liberalization as a discrete event that occurred at a specific time for each country. The date of trade liberalization was obtained from Wacziarg and Horn Welch (2003) that updated the dates originally estimated by Sachs and Warner (1995).[9] A straightforward argument against this approach is that trade liberalization is a gradual process instead of a one time event.

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[8] The private-credit-weighted margin of an industry is computed as:

$$PCM_i^{PCW} = \frac{\sum_c PCM_{i,c} \times (PRIVATE\ CREDIT)_c}{\sum_c (PRIVATE\ CREDIT)_c} \ .$$

[9] The sample of countries used in the study and the corresponding dates of trade liberalization are reported below in Table 3.

Although there is always some degree of gradualism in the implementation of reforms, an important aspect of trade liberalization is the removal of tariffs and quantitative restrictions that can have an immediate impact on the volume of commerce of a country. This can be seen in Figure 4a, which plots the average volume of trade as a fraction of GDP around the time of liberalization.[10] The figure shows that our liberalization dates do indeed capture a discrete break in the trend of the volume of trade for the typical country.

Besides changing the volume of commerce, trade liberalization significantly affects margins, both in absolute and relative terms. This is especially important for our analysis because, as long as the ability or willingness of parties to influence policies depends on their rents, it justifies our use of trade liberalization as a shock to the political economy equilibrium. The absolute effect of liberalization on margins is shown in Figure 4b, which plots the evolution of the average PCM around the time of trade liberalization. The large decline in margins following the liberalization event is apparent.

Of course, the political economy equilibrium is determined by the *relative* strength of the parties, so a common decline in margins across industries may not be sufficient to trigger a change in the equilibrium. To analyze the potential effect of trade liberalization on this equilibrium we define the *relative* strength of promoters and opponents of financial development as follows:

$$STRENGTH\ PROMOTERS = PCM^{PROM} - PCM^{OPP} = \sum_{i \in Promoters} share_{i,P} PCM_i - \sum_{j \in Opponents} share_{j,o} PCM_j \text{ w}$$

here PCM^{PROM} and PCM^{OPP} are the average PCM of promoters and opponents of financial development respectively (as identified in section 2.1); and, for each industry i that belongs to the group of promoters (*P*), $share_{i,P}$ is the share of that industry's value added in the total value added of that group ($share_{j,o}$ is defined analogously among the industries that belong to the group of opponents).

Figure 4c, illustrates how trade liberalization perturbs the equilibrium between promoters and opponents. The figure shows the median absolute deviation of the residuals of a regression of relative strength on its lagged value around the liberalization date.[11] If changes in the relative strength across countries were just random, the median absolute deviation of the residuals would be stable around the event. On the contrary, we observe a spike around the time of the event that signals that trade

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[10] Volume of trade corresponds to the sum of exports and imports as a fraction of GDP, and was obtained from the World Bank World Development Indicators 2003.

[11] The reason to use the median absolute deviation instead of the standard deviation or the R^2 is that because of the small number of countries for which we can perform the exercise (average number of countries in a given event time is around 25) the last two measures are too sensitive to outliers. A robust measure of R^2 obtained from a trimmed regression (not reported) gives similar results.

liberalization has a significant heterogeneous effect on the relative strength of promoters across countries. This heterogeneity is critical for the ability of our political economy mechanism to explain the variability of the development of financial markets post-liberalization. Notice also that by year t+5 this relationship as well as the level of aggregate margins seem to stabilize.

Based on the measure described above, the effect of trade liberalization on the relative strength of promoters was computed for each country in the following manner:

$$\Delta(STRENGTHPROMOTERS) = \Delta PCM^{PROM} - \Delta PCM^{OPP}$$
$$= \sum_{i \in Promoters} share_{i,P} \Delta PCM_i - \sum_{j \in Opponents} share_{j,o} \Delta PCM_j \quad (1.3)$$

where the shares are computed as above, except that they correspond to the average value in the five year window before liberalization,[12] and the ΔPCM_k $(k = i, j)$ correspond to

$$\Delta PCM_k = \sum_{t=\tau}^{\tau+5} \frac{1}{6} PCM_k - \sum_{t=\tau-5}^{\tau-1} \frac{1}{5} PCM_k, \quad (1.4)$$

so they are the change in average PCM of an industry in a five year window around the liberalization date τ. This measure of changes in average PCM at the country-industry level also allows us to look further into the heterogeneity of the effect of the event on margins of different industries. Within a country, the variation of the effect of liberalization on margins across industries (as measured by the standard deviation) is about seven percentage points. Within an industry, the variation of the effect across countries is also around seven percentage points. This variability of the effect on margins across both countries and industries ensures the power of the test described at the beginning of section 2.

3. Results

In this section we report the changes in financial development associated with trade liberalization. We show that the cross-country variation in financial development can be explained in part with our measure of the change in the political economy equilibrium. We check the robustness of the result, provide details of the mechanism, and use the liberalization experiment within the political economy framework to further explore the real effects of financial development.

3.1 Trade and financial development

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[12] By using the average shares before liberalization we are assuming that the liberalization has no effect on sectoral shares in the five year window, which is indeed the case. Results obtained using different shares before and after are analogous.

We are certainly not the first to consider the relationship between trade openness and financial development. Rajan and Zingales (2003) show that the degree of world openness to trade and bank and stock market development both exhibit a U-shaped form in the 20th Century. Stulz and Williamson (2001) find that in a cross-section of countries trade openness mitigates the influence of structural factors on financial development-enhancing policies. We present additional evidence of this relationship here.

In Table 2 we provide summary statistics for a sample of 73 countries for which we have complete data for both trade openness and private credit to GDP during the 1970s, 80s and 90s. We split the data in two groups based on whether the country liberalized trade during the period or not, and compute a ranking based on private credit taking the average value in the 1970-74 and 1995-99 periods. The first panel reports statistics for the whole sample.

The first thing to notice is that the countries that liberalized trade (first panel) advanced on average just 3.9 positions in the ranking of 73 countries. Aside from this figure not being economically noteworthy, it is not statistically significant either. The median change in the ranking is even smaller (1 position). The data are not particularly supportive of the view that opening up for trade triggers financial development automatically. Notice also that the ranking of private credit is highly persistent in time. When considering liberalizers and non-liberalizers together (third panel) the rank correlation of the measures in the early 1970s and the late 1990s is 0.68. The countries' initial position then explains more than 50% of their position more than a quarter century later. This persistence is more suggestive of deep, slow moving institutional factors being at the core of financial development.

Consider, however, what happens when we compare the persistence across the liberalizers and non-liberalizers groups. While for the countries that did not open up for trade during the period the initial position in the ranking explains 2/3 of the variation in the final position, for the liberalizers it explains less than 5% and the correlation is not even statistically significant. Figure 5 makes the point graphically by plotting the relationship between initial and final rank for the two groups separately. Countries above (below) the upper (lower) straight line in each figure gained (lost) at least as many positions as to outpace (lag behind) 20% of the countries in their respective group. While over 60% of the liberalizers lie outside the region, only 23% of the non-liberalizers do so.

The data show that trade liberalization is a perturbation to the high persistence of private credit. There is then a way to reconcile the institutional view with the idea that opening the economy to trade changes matters. In the next section we show how one can use trade liberalization events and exploit the cross-country variation in its effect on financial development to understand the relationship. As it will be the case, our political economy story will prove to be critical.

3.2 Trade liberalization and the political economy determinants of financial development

The main result

Using the measure of the change in the relative strength of the promoters of financial development defined in section 2.2, introducing some specific controls, and specifying the form of the error term, the benchmark specification described in equation (1.1) becomes

$$\Delta FD_c = D_{event}\delta + \alpha \times FD_{0,c} + \beta \times \Delta(\Delta PCM^{PROM} - \Delta PCM^{OPP})_c + \varepsilon_c + \mu_{event}, \qquad (1.5)$$

where ΔFD is the change in the ratio of bank credit to the private sector to GDP computed as the difference between the average ratio between t-5 and t-1, and the average ratio between t+5 and t+10 (everything in event time); [13] FD_0 is average private credit between t-5 and t-1; D_{event} is a set of indicator variables for the year of trade liberalization; $(\Delta PCM^{PROM} - \Delta PCM^{OPP})$, the measure of the change in the relative strength of promoters, is defined as in equation (1.3); finally ε_c and μ_{event} are country and event error components. α, β, and δ are the parameters to be estimated. The coefficient of interest is β, which according to our hypothesis should be positive and statistically significant.

Table A1 shows the basic characteristics of the sample. There are 41 countries, 6 developed (Australia, Ireland, Korea, Japan, New Zealand, and Singapore) and 35 developing. Latin America is the largest group with 17 countries, followed by East Asia Pacific and Sub-Saharan Africa with 6 each. The sample also includes three transition economies (Hungary, Poland, and Romania). Eight countries liberalized in 1991, four in 1996 and 1986 each, and three in 1990, 1989 and 1985. The mean (median) value of the change in private credit is 8% (6%) with a standard deviation of 19%.[14] The positive sign confirms previous cross-country evidence on the positive relation between trade openness and financial development. This is reassuring given the difficulty in interpreting cross-country relationships. Here fixed-country characteristics are implicitly controlled for, and although complete exogeneity cannot be claimed, the dependent variable at least follows in time the change in openness.

Note, however, that although significantly positive there is important cross-country variation in the change in private credit that follows trade liberalization. A 95% confidence interval places the effect between 2.4 and 13.4 points of GDP. Moreover, for 12 of the 41 countries in the sample private credit actually decreases. This cross-country variation is what we seek to explain based on political economy

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[13] Notice that we do not include the years immediately after the event (τ to $\tau + 5$) to compute the level of post-event financial development because we assume that the political economy mechanism operates with some delay. Nevertheless, as it will be shown later, this assumption can be significantly relaxed.

[14] When considering all the 67 countries that had liberalized trade by 1995 and not just the 41 included in our sample there is still a significant, although smaller, positive sign.

considerations. The dispersion of private credit right before trade liberalization is, in comparison, quite small: 17% over a mean of 24%. The correlation between initial credit and its change is negative (-0.23) but not significant. The statistical moments suggest that, although significantly different from zero, there is ample variation across countries in the change in private credit that cannot be explained simply by initial conditions. The political economy variable is centered on a mean (median) of zero (-1%) with a standard deviation of 4%.

Our sample only includes countries that did liberalize trade. Sample selection (i.e. the fact that some countries choose to open up for trade while others do not) potentially has an effect on the size of the constant in (1.5), however it plays no role in the identification of the coefficient for political economy variable. This is an important advantage of our methodology since we do need not worry about the interaction between the decisions to liberalize trade and the financial system. It is easy to imagine how these two interact; policy changes such as these are not typically accidental nor do they come alone. They are usually part of a broader transformation that drives these and other policy reforms. Chile's reforms in the 1970s, Latin America's changes, and Eastern Europe's process in the beginning of the 1990s, are vivid examples of this link.

This is not to say that we can safely treat trade and financial liberalization as independent, or more precisely assume that the former only has an effect on the latter through the political economy channel we propose and otherwise has no direct impact on it. There are other possibilities. One is related to the timing of liberalization. For instance, some countries may have liberalized trade when the rest of the world was more open to the flow of both goods and capital, which might translate to higher impact on trade volume and capital flows-induced deepening of bank credit. This could show up in our exercise through the political economy variable if relative world prices across sectors were themselves a function of global trade time-varying characteristics. These could be long-term shifts such as the decline of textiles. But they could also be related to the world economic cycle and the diverse cyclical properties of industries around the cycle (in terms of durability, for instance). Cyclical shifts would be more troubling since one would not expect the political economy equilibrium to change very much when impacted by non-persistent, short-term shocks. All these would map into our measure of change in margins and, provided that it affects the two groups of sectors in a systematically different way, ultimately bias our results. To address the issue of timing in a very general way, specification (1.5) includes trade liberalization year fixed effects.

Lastly, we allow for heteroskedasticity and the possibility of errors to be clustered around liberalization dates.

The first column in Table 3 shows the basic result of this paper. The coefficient of the change in the strength of the pro-financial development group is positive and highly statistically significant. The initial level of private credit to GDP turns out not to be significantly associated with subsequent change in the variable after the event. Figure 6a plots the partial relationship between the change in private credit and the value of the political economy variable derived from the regression above. The figure makes more apparent the sense in which the mean change in financial development after liberalizations is not a very useful statistic in waging the relationship between one another. It also shows that there is no noticeable clustering around geographical or economic dimensions[15]. This suggests the need for a non-evident additional variable to explain the cross-sectional heterogeneity. The political economy variable measuring the change in relative strength between those that promote and those that oppose financial development does a good job in explaining it. That variable alone explains around one fourth of the variation in the dependent variable not accounted for the initial level of financial development and the fixed effects. Furthermore, the relation does not seem to be driven by a few influential outliers but rather to be a robust pattern in the data.

Figure 6b shows the time pattern of private credit around the liberalization event. The figure plots average private credit to GDP against event time separately for the group of countries that score above and below the median in the political economy variable[16]. Before trade liberalization the two groups are remarkably similar both in terms of the level of bank credit (around 25% of GDP) and its evolution. Shortly after liberalization, though, the group of countries for which the shock advances the political prospects of improving the financial system shows rapidly increasing private credit, ending up at around 45% of GDP or almost twice the value before the event. In contrast, in the countries where conditions for developing the financial system do not improve as much, private credit shows on average no significant change, ending up at roughly the same level as before. The post-liberalization difference between the two groups is quite large, comparable to the distance between Denmark and Ecuador or Chile and Libya in the 1990s.

Since the experiences of Bolivia and Costa Rica roughly match that of the average country in each group, we use them to further quantify the economic size of the effect. These two countries liberalized trade at almost the same time (1985 and 1986, respectively) and so probably faced similar external conditions for their financial development. In Bolivia the pro-financial development group turned out to be strengthened by trade liberalization increasing margins by 2.1 points while the opposing group

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[15] Variables capturing geographical or economic proximity when included are almost always insignificant and they never affect the coefficient of the political economy variable in a material way (see some of them in the remaining of Table 2).

[16] This figure only considers the countries for which we have complete private credit data coverage for the +/- 10 years window around the trade liberalization event.

was weakened losing 2.5 points in margin. Just the opposite happened in Costa Rica where the promoters lost 0.3 points and opponents gained 5.6. In relative terms in Bolivia the promoters were strengthened by a measure of 4.6 margin points while in Costa Rica they were weakened by 5.9 points. Despite having similar initial financial depth (0.12 and 0.18 respectively), following trade liberalization Bolivia trebled Costa Rica's level (0.37 vs. 0.12).

Interesting is also the case of Poland and Hungary. Glaeser et al (2001) document how different approaches to securities market regulation yielded startlingly different results in terms of the development of a market for equity. Although here we do not address the stock market development –which may be subject to a singular political economy mechanism-, we complement the general argument by proposing a rationale for why the two countries adopted different policies. While, as a consequence of the opening up for goods trade, in Poland the promoters of financial development were relatively strengthened (by 1.4 points), in Hungary they were weakened (by 2.3 points). Hungary started the process with a relatively developed banking system (a ratio of private credit to GDP of 0.38, not very different to that of Mexico in the 1990s), but ended up with roughly the same level as Poland (0.25) which had started with almost no banking system to speak of (0.04).

Demand vs. Supply

Of course, the result in the first column -although indicative- does not necessarily imply that financial development was formerly constrained by poor policy. Demand considerations are a real possibility. In fact, whether the level of financial development responds primarily to demand or to supply factors has been the main issue in this literature at least since the pioneering efforts of Goldsmith (1969). In our context, trade liberalization, and more generally the reform process, can shift the investment possibility frontier and thus alter the demand for funds. This would introduce omitted variable bias if the change in demand for funds happened to be correlated with the political economy variable. It is not obvious why this would be so, but it is always a possibility.

The following columns of Table 3 try to address the issue by adding controls thought to be associated to investment possibilities and the demand for funds. Neither the effect of liberalization on GDP growth nor its effect on the change in the investment rate seems to be driving our result (see columns two and three). When introduced in the regression they do not enter significantly nor do they materially or significantly affect the size of the coefficient for the political economy variable.

It might still be the case that growth or investment take time to become visible or that they are just poor measures of the change in investment possibilities. Instead of trying to measure how the frontier shifts one can assume that countries were initially close to the frontier and that this shifts out to achieve a

common level for all countries that liberalize (a level given by common world factors). If this is so a country's initial position can be used as a measure of the distance to frontier or new investment possibilities. We approximate each country's initial position with the average GDP per capita in the 5-year period preceding trade liberalization. Alternatively, one can interpret this variable as measuring the ability of the country to finance new investment with internal rather than external funds if the stock of firms' retained earnings is increasing with average country income. Again, adding this variable has no effect on our results. The positive (though insignificant) coefficient for initial income is inconsistent with either of the interpretations since both imply a negative effect.

Of course, when we include initial per capita GDP we also mean to rule out simple explanations of why some countries developed financially following trade liberalization and others don't.

By changing the relative desirability to invest across sectors trade liberalization can have an effect on the aggregate demand for external funds. If margins increase more in sectors with higher demand for external funds aggregate demand would increase and therefore it would not be surprising to find that the stock of credit increases. This would not require arguing for supply-side constraints and political economy effects. The worry here is that we are just measuring external finance dependence and calling it the effect of financial development on industry rents. This is not the case as seen in column five where we add the trade liberalization-induced change in margins of highly externally dependent industries (ranking higher than median in Rajan and Zingales (1998)'s measure for all firms) relative to less dependent ones as an explanatory variable. The variable enters insignificantly and does not affect the estimates for the political economy coefficient. Its sign is consistent with a different political economy effect where financial underdevelopment constrains potential entrants relatively more than it does incumbents in sectors with high need for funds[17]. When incumbents in dependent sectors are relatively strengthened by trade openness, their views regarding financial development have a better chance of becoming actual policies and outcomes.

The identification of promoters and opponents to financial development was based on a price measure because this is a direct indicator of the existence of rents in each sector. Rents are then taken as *prima-face* evidence of the existence of barriers to entry. One can alternatively rely on quantity measures that, although indirect, also suggest the existence of some sort of entry barriers. We compute the change in the strength of promoters to financial development substituting the effect of financial development on margins with the effect of financial development on average firm size across industries. The average firm

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[17] There is indeed a positive, though small, correlation between external finance dependence and the effect of financial development on margins.

size measure has been used before in similar contexts[18]. The idea here is that if entry is restricted by poor financial development, growth in the industry will be more tightly associated to growth in the size of the typical incumbent rather than in the number of firms. The way average firm size correlates with financial development gives us a measure of the importance of financial underdevelopment as an entry barrier across industries. The ranking of industries along this dimension turns out to be almost identical to the one based on margins, and therefore to yield very similar results in terms of its power to explain trade liberalization-induced financial development (see lower panel).

3.3 Further robustness

In Table 4 we check the robustness of the results to a number of potentially important issues involved in the experimental design.

We begin by considering a smaller window for computing the impact of trade liberalization on margins. Instead of considering 5 years before and 5 years after the event to compute the change, we use 3 years[19]. Our concern here is that if financial development is quick to respond after the event, our measure of final margins might by then be affected by this change. Since financial development is associated with larger margins for promoter sectors this would introduce an upward bias in the political economy measure for the countries that ultimately develop their financial systems more. Our result would be the product of a mechanical relationship. We already shown evidence that the impact on margins occurs within a small window of at most 3 years around the event. We provide a more direct check here. As can be seen in the first column in the upper panel, and despite losing a few observations because of data availability, the results are robust to this change. We experimented with window sizes between +/-2 and +/-7 years and the results were never materially affected (not reported).

The following column shows that when the dependent variable is measured as the change in the average level of private credit the three years preceding the event and the three years following it, the coefficient for the political economy variable is much smaller and not statistically significant. Then, there is no indication that financial development responds any differently across countries based on the political economy measure in the immediacy of the event, all the difference comes much after that. This speed of response of financial development is more consistent with an indirect rather than a direct effect of trade liberalization on financial development. It would have been worrisome to find that trade liberalization is followed by an almost immediate change in the level of financial development since that would require the change in political forces to translate into policies and policies to impact outcomes in too short of a

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[18] Among others, by Rajan and Zingales (1998) and Cetorelli (2001, 2003) and Cetorelli and Strahan (2003).

[19] To be consistent, we also measure the initial and final level of private credit over windows of three years, both before the event and 5 years after the event.

period (on average 1.5 years), something difficult to imagine in our political economy context. In the next section we show how financial policy reforms that occur during the first five years following liberalization explain what subsequently happens with private credit. All this argues that the mechanical relationship is not a problem in practice.

In the next column of Table 4 we try out measuring the final level of private credit not after the fifth year following trade liberalization but after 10 years have elapsed. The idea is to see whether there is truly a difference in private credit that is maintained in time or just a credit boom that follows trade liberalization. The distinction is important since previous research has shown that changes in private credit are associated with subsequent growth in the long-run but not in the short-run (Loayza and Ranciere (2001)), suggesting that only permanent differences in private credit can be identified as financial development. Since the coefficient for the political economy variable is not significantly different to that in our benchmark the results support the view that the event induces a permanent change in the level of financial development across countries[20]. We think this pattern is also more consistent with structural policy-induced changes than with transient demand effects.

Our computation of the effect of financial development on margins controls for both industry and country fixed effects. As long as taxes –or other regulations- are either country or industry specific, the measure adequately reflects how the relative flow to capital in each sector is affected by financial development. This is unlikely to be the case for two particular sectors: tobacco and petroleum refineries in which taxes probably increase with a country's income. If so, the positive correlation between financial development and income per capita may be mapping into too large of an effect for both these sectors[21]. In column (4) we drop both industries from all computations. The results do not appear to be sensitive to this change.

Wouldn't the agents involved anticipate the financial effects of trade liberalization and internalize them in their decision to open up? Said differently, why would some countries open up for trade knowing that this would unleash political economy forces leading to financial underdevelopment? The first thing to keep in mind here is that trade liberalization has been shown to have a positive effect (for the effect on growth, see Sachs and Warner (1995)), so that it may be worthwhile to open up even at the cost of having a relatively less well developed financial system. Second, our result shows that *on average* trade liberalization is associated with 8 points of GDP higher private credit; only a few countries actually decrease their degree of development in absolute terms. It might not be obvious a-priori the exact effect of

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[20] This is still true when one computes the benchmark using only the countries included in this last sample, and when computing the final level of private credit 15 years later (neither of these are reported).

[21] Still, one has to take into account that in computing such effect we instrumented financial development with legal origin, a variable largely orthogonal to GDP per capita.

trade liberalization across sectors (see, for instance, the argument put forward by Hausmann and Rodrik (2002)). In expectation the effect on subsequent financial development is in fact an additional benefit.

Having included only countries that actually liberalized and also controlled for the timing of the decision, the issue speaks to the reasons for liberalizing trade and how these can interact with that of developing the financial system. Trade and financial liberalization do not necessarily come isolated but may be part of a reform process that includes both. To introduce bias in our estimation one needs to argue that the reason why some countries adopt them together and others don't is correlated with the political economy variable.

The degree of bundling of policies can be a function of local and external forces. In terms of the former, the outcomes of initial reforms may matter a lot in securing political support for the next round and keeping the reforming momentum (see, for the case of mass privatization, the formalization by Roland and Verdier (1994), and the vivid account of the Russian experience in Boycko et al (1995)). In our case it might be that the countries we see developing their financial systems faster do so not because the political balance between promoting and opposing incumbents change, but simply because the first round of reforms (trade liberalization) worked well and the liberalization process gained further political support. If the success of trade liberalization is for some reason correlated with our variable we would have omitted variable bias. We check this by introducing the effect of trade liberalization on the volume of trade, which we compute in the same way we do the effect on private credit. The results are shown in column one of the lower panel. The correlation between this variable and the political economy one is negative and non-significant, and when included in the regression appears insignificantly negative leaving the results mostly unaltered. Measuring success of trade liberalization with its effect on GDP growth or the change in the investment rate yields the same conclusion (see the previous table where these variables were used to measure demand for external funds).

Now from an ex-ante point of view, countries may liberalize trade when the external conditions are most favorable, for instance after a period of high terms of trade. High terms of trade could be positively correlated with the political economy variable if they were associated more tightly with the prices of opponent industries. If terms of trade mean-revert, this can map in a larger decline of margins for opponents and therefore a relative strengthening of promoters of financial development. The correlation between the change in the relative strength of promoters and the initial terms of trade (i.e. the average for the 5 years preceding trade liberalization) is positive, although small and not significant. When included as an independent variable in the benchmark regression (column two), the initial level of terms of trade does not enter significantly. Similar results were obtained when measuring the ex-ante incentives to liberalize with the growth rate of trading partners (column three). We therefore find no

evidence that countries that improved their financial systems further did so because they were better able to align the politics of trade and financial development.

Cross-industry incentives can also be at the hart of the reasons to liberalize trade. For each group (promoters and opponents of financial development) we compute the average margin for those industries in the US in the period right before trade liberalization. We interpret the margin in the US as the (normalized) international price of output and take the difference of it across groups as an indicator of the relative incentive of promoters to liberalize trade. It may be that those countries that developed the financial system further just happened to be those in which promoters of trade and finance coincided, and not necessarily those in which promoters of financial development were strengthened by trade liberalization[22]. This is not the case: this variable is virtually uncorrelated with our basic one and does not enter significantly in the regression (column 4).

The politics of trade liberalization are, at least in this sample, not tightly intertwined with the specific political economy mechanism of financial development discussed here. The fact that almost all countries decided to liberalize in a relatively short period of time and that this time happened to coincide with the emergence of a strong international political agenda towards free trade points to the view that the trade process was more the result of external forces, and largely independent to the financial development ones.

Still, these external forces can also be related to the bundling of reforms. One case would be that of a country subject to the structural reforms conditionality of IMF programs. The lack of variation in IMF involvement across countries in our sample (since 1970 the Fund has had programs outstanding in all but 4 of the developing countries and these countries represent 35 out of our 41 cases) means that not considering this does not bias the estimation of our political economy effect[23].Now, the IMF did not begin seriously considering financial sector reforms in the conditions until the late 1980s: *"Until the mid 1980s, structural reforms in (International Monetary) Fund-supported programs were typically confined to the exchange and trade system... While in the late 1980s, programs began to cover an increasing variety of structural measures..." (IMF (2001)).* Taking advantage of this fact in columns five and six we estimate the benchmark regression separately for the sample of countries that liberalized before and after 1990. The sample countries are evenly split along this dimension. Both the coefficient estimates and overall fit of the regression are very similar for those countries more likely to have been pushed to bundle both

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[22] The implications of this related mechanism in terms of the decision to open up for trade or not are quite interesting and merit further research. They are nevertheless out of our scope. Since we only have countries that did liberalize, we just need to worry about this reason being correlated with our explanatory variable.
[23] Since compliance with structural conditions is relatively low (see IMF (2001)), the Fund's involvement could still affect our results through variation in the compliance with the conditions if it was positively correlated with our political economy variable.

reforms and those where financial sector reforms were probably not a condition. Therefore, if IMF involvement explains openness, it does not seem to explain the fact that some countries develop their financial systems and others don't.

We do not have data on the exact conditions imposed by each IMF program in each country. However, we can also measure the likelihood that the IMF involvement implied the commitment to all-encompassing reforms (as opposed to just trade-related ones) with the ratio of funds disbursed to GDP in the period preceding trade liberalization. The variable enters positively (although not significant) in the regression. The coefficient of the political economy variable remains unchanged suggesting that, even if the extent of IMF intervention can explain in part the extent of reforms, this is not what the political variable is picking up.

3.4 More on the mechanism

Are the differences in financial depth related to policy changes?

Up to now we have taken as given that financial development occurs as a result of policy improvement. We provided some evidence against it being associated just to demand factors, favoring a supply-side explanation. Policy is not the only factor constraining the supply of funds, though. Recent research has pointed out the importance of deep structural reasons for the heterogeneity in financial development across countries, among them legal structure (La Porta et al (1997)), social capital (Guiso et al (2004)), culture and religion (Stulz and Williamson (2003), and institutional development (Acemoglu et al (2004)). Of course, these slow-moving factors are not likely to explain time series changes in financial depth, at least not within the time interval we consider here. Still, we have not yet provided evidence that differential policy changes are behind the disparity of outcomes. This is important because policies are at the core of the political economy story. It will also allow a better understanding of the kind of reforms involved. The issue is, then, whether our political economy variable can explain subsequent financial development through policy reforms.

Data on actual policy change is not easy to find and when available faces a number of shortcomings related to sample and policy selection, and the amount of subjectivity involved. We use recent data from Abiad and Mody (2003). The authors extend the work of Williamson and Mahar (1998) and others, and construct indices for the degree of liberalization in 6 different policy dimensions related to the functioning of the financial system between 1973 and 1996. The dimensions considered are directed credit/reserve requirements, interest rate controls, entry barriers/pro-competition measures, regulation/securities markets, privatization, and international capital flows. Each index goes from 0 to 3, with 3 indicating complete liberalization.

This exploration comes with two costs. First, the sample is significantly reduced from 41 to 15 countries. Fortunately this restricted sample is not peculiar. Regarding the main result of this paper (Table 3, column one) there is no important difference between the behavior of these countries and that of the rest of the sample[24]. Something worth keeping in mind when assessing the results below is that the precision of the estimates is, of course, reduced. Secondly, due to the smaller number of observations we have to drop the event time fixed effects to perform a meaningful estimation. Again, this is not of great concern since most of these liberalizations happen to be naturally clustered around a few years (6 in 1991, 3 in 1986), meaning that most of the variation exploited is actually within liberalization years as before.

With these caveats in mind, we review the results in Table 5. The specification is the same as in (1.5) with the exception that the change in each of the measures of financial policy liberalization is also included. This change is measured as the difference in the average of the indicator between t-5 and t-1 and t+6 to t+10. The first column presents our benchmark regression in this reduced sample of countries. Although borderline insignificant (p-value 0.12), the coefficient for the political economy variable is remarkably similar to the one obtained before. In the following columns each policy indicator is added to the regression separately. If the effect of the change in the strength of promoters on financial development is working through policy changes in the five years following trade liberalization, the coefficient of the political economy variable should decrease importantly when we include the policy variable, and the coefficient of the policy variable should enter positively. This would be consistent with the joint hypothesis that the particular policy affects subsequent financial development and that the predictive power of the political economy variable comes precisely from that relationship. Putting this together with the fact that the change in the strength of promoters comes from an independent event (the trade liberalization), and that it precedes the policy change in time, that result would be suggestive of the mechanism proposed. The alternative is that either the policy is not important or that the effect that the political economy variable is picking is not related to that particular policy.

As can be seen from columns two to seven, the policy variable always enters positively, meaning that these policies do have an effect on financial development. However, the coefficient is significant only in the case of the lifting of entry barriers to the banking sector and the regulation of securities markets. The coefficient of the political economy variable drops significantly only in the case of entry barriers. Put together, and bearing the small number of observations and the difficulty of measuring policy in mind, the results are consistent with the idea that the change in the strength of promoters is translated into the lifting of entry barriers to the banking sector, which in turn generate a deepening of the financial system.

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[24] The inclusion in (1.5) of an indicator variable capturing whether the country belongs or not to the restricted sample, both alone and interacted with the other explanatory variables, is never statistically significant or economically relevant.

The results obtained above adding each policy to the benchmark regression separately may be difficult to interpret because policy changes are likely to be correlated. However, we do not have enough degrees of freedom to include all the policy variables together and run a horse race. Instead, we create an index of policy liberalization. As the previous results suggest that all policies are not equally relevant in explaining financial development, we construct the index by giving more weight to those that are more strongly associated with financial development. We estimate these weights by regressing the change in private credit to GDP against the change in all the policy variables for all the years and countries in the 1973-1996 panel considered by Abiad and Mody (2003). Changes are measured as the difference between averages in t-5 to t-1 and t+6 to t+10. The regression controls for time effects and the initial level of financial development. Consistently with the results above, the index turns out to give higher weight to entry liberalization, securities market regulation, and international financial integration. When the change in this index is included (column 8) the coefficient of the political economy variable drops importantly and the policy variable enters significantly. This suggests that an important part of what the change in the strength of promoters is capturing is indeed related to actual changes in the kind of policies that have an important effect on financial development.

These results point towards the view that incumbents in the financial sector are important players in the political economy game (as hypothesized by, among others, Rajan and Zingales (2003)). Our view is that competition in the financial sector brings innovation in information and risk management and increased participation, all of which echo higher financial depth. Financial sector incumbents enjoy the quiet life and rents associated with low financial development. It is not obvious, however, that their rents would diminish when credit and interest rate restrictions are lifted or when the securities market is better regulated. It is likely, then, their opposition to these reforms will be much weaker than towards increasing competition in the industry. It follows that no particularly strong change in the politics of financial development among non-financial incumbents might be needed for the former policies to be enacted. Kroszner and Strahan (1999) document how the incentives for financial sector incumbents to restrict competition are indeed related to policy outcomes in the case of the relaxation of U.S. bank branching restrictions. These results add to previous literature trying to determine the effect of policy on financial repression, in particular on financial development. It does so by adding an additional layer of exogeneity to the policy variables. Not only pro-competition policies precede financial sector development in time (see for instance Caprio et al (2001), Williamson and Mahar (1998), Fanelli and Medhora eds. (1998)), but they still do so when these policy changes can be motivated by a shift in the political economy context.

Capital deepening vs. improved allocation

Financial development has been associated with higher economic growth (King and Levine (1994), Jayaratne and Strahan (1996), Demirguc-Kunt and Maksimovic (1998), Rajan and Zingales (1998)). By overcoming informational and agency problems, a well functioning financial system can foster growth through two main channels: by increasing the amount of resources available for investment and by better allocating these scarce funds. We can provide some new evidence on the matter by asking whether the shock to the political economy equilibrium is associated with increased investment and/or improved allocation.

Computing investment is relatively straightforward, coming up with a measure of the quality of capital allocation is more involved. We follow Wurgler (2000) and compute the sensitivity of investment growth to value added growth across industries in each country. This is interpreted as the quality of capital allocation because it is high when a country increases investment more in its growing industries and decreases investment more in its declining ones. The change in this variable around trade liberalization is computed in a manner analogous to the case of private credit. Due to lack of data on investment for many countries in our sample we end up with just 12 observations and we are again forced to drop the event fixed effects. The small time windows we use mean that the measures are surely much noisier than those computed by Wurgler. With these warnings in mind, the first column of Table 6 shows that the change in the relative strength of incumbent groups is positively and significantly associated with an improvement in capital allocation quality. Aggregate investment over value added of the industries in the same group of countries used above increases (column two). However, the effect is economically small and not statistically significant. The results provide some indication that it is allocation what gets more affected. This again points to the importance of considering conflict across industrialists in explaining policy and ultimately financial development.

Why would incumbents want to use financial underdevelopment as the mechanism to protect rents?

One important question regarding our political economy mechanism remains. Why would industrial incumbents use financial underdevelopment when there are many other more direct and presumably more efficient ways to protect rents? Direct regulation of entry seems a better alternative. Djankov et al (2002), for instance, present cross-country evidence suggesting that regulation of entry rather than serving the public interest is better understood in a public choice framework.[25] Indeed, the countries that regulate entry the most tend have less open political participation, less controls over the

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[25] Their results, however, are not conclusive in terms of the effect of entry regulation on product market competition or incumbents' rents.

government, and to be more corrupt. They also seem not to achieve the traditional stated goals for regulation.

The effect of good governance and more open participation in our context is ambiguous, though. On one hand, they rule out gross intervention favoring ways that are more subtle, less apparent to the public to achieve the same outcome. While it might no longer be possible to just go and buy monopoly power from politicians, it could still be possible to achieve a similar goal by supporting regulation that restricts entry to the banking system invoking, for instance, the need of preserving safeness and soundness. On the other hand, it is possible that better governance makes the political system more responsive to the demands of different constituencies vis-à-vis the desires of the political class or the median citizen.

In Table 7 we add to the benchmark specification the interaction between the political economy variable and governance measures. First, note that none of these measures displace the political economy variable which remains as significant as before and implies similar economic effects. Nor do they seem to have much of an impact on the coefficient of initial financial development. Both the direct and interactive effects of these measures are always positive, but only the interactions seem to be robustly significant. Except in the case of voice and accountability, the coefficients for the interacted variables are significantly positive.

Financial underdevelopment seems, then, to be a second best, more sophisticated method to be used when the institutional development is such that it only allows subtle interventions. Said a little differently, the results can be interpreted as implying that a minimum level of institutional development is needed to jump start financial development once political conditions are appropriate. When the law has little value and corruption is rampant, the effect of shifts in the strength of different groups on financial development is diminished arguably because other means of protecting rents seem much more efficient. Similarly, when the public has little political power and government officials are not accountable, the cost to them of grossly misbehaving declines. We interpret the political stability effect as measuring the constraints faced by the politician class given the value of reputation. Finally, the balance between policies that manifestly favor particular groups and less obvious ones seems to be reflected in the general perception on the government's effectiveness.

We did not find the more specific measures of the role of the different branches of government to have major impact on the relationship between the political economy variable and subsequent financial development (not reported). Also, the high correlation between the governance measures did not allow us to tell the effect of each one apart.

4. Conclusion

This paper showed that the trade liberalization-induced change in the relative strength of promoters vis-à-vis opponents of financial development is a very good predictor of subsequent changes in financial sector depth. In doing so we provide evidence on the importance of finance for the product market, documenting a mechanism through which financial development has real effects on the economy. The political economy variable derived from the analysis allowed us to –at least partly- solve causality issues and determine the kind of policies that cause financial systems to develop. Finally, we extended previous cross-country evidence on the effect of financial development on capital allocation to the time-series dimension.

From a policy standpoint the results of this paper are important in two ways. First, although deep institutional reasons play a role, to an important extent, countries have the level of financial development they choose. Policy convergence to best-practice standards is not likely to happen automatically unless the political economy conditions for such a change are present. Identifying and co-opting potential opponents might be necessary to ensure the political sustainability of reforms. Second, policies that on average have a liberalizing effect on markets are not by themselves enough to guarantee their extension to the financial system. They can even worsen the situation. In this sense, understanding the interrelation between sectorial reforms, and adjusting the timing accordingly seems of first order importance.

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Table 1. Financial Development and Industry Margins

The table shows the coefficients obtained for the industry dummies that capture the sensitivity of each industry's price-cost margin to financial development in a regression of the price cost margin of each industry in each country on an industry dummy, a country dummy and an industry dummy interacted with each country's level of private credit (this last dummies are the ones reported below). The data for the regression corresponded to averages of the variables for the period 1980-2000. The parameters were obtained by 2SLS, instrumenting the level of private credit for each country's legal origin. The standard errors are robust to heteroskedasticity and clustered by country. The bottom of the table shows the p-value of the Wald's test of equality of coefficients.

Industry	ISIC	Financial Dev. Effect	Standard error	Demeaned Financial Dev. Effect
Food	311	-		-0.036
Beverages	313	-0.007	-0.08	-0.043
Tobacco	314	0.187	-0.249	0.152
Textiles	321	0.01	-0.039	-0.026
Apparel	322	0.045	-0.042	0.009
Leather	323	-0.036	-0.048	-0.071
Footwear	324	-0.038	-0.05	-0.074
Wood	331	-0.003	-0.057	-0.038
Furniture	332	0.052	-0.043	0.016
Paper	341	0.052	-0.048	0.016
Printing	342	0.088	-0.053	0.052
Industrial Chemicals	351	0.075	-0.059	0.039
Other Chemicals	352	0.198	-0.065	0.162
Refineries	353	-0.181	-0.067	-0.216
Petroleum	354	0.039	-0.072	0.004
Rubber	355	0.043	-0.048	0.007
Plastic	356	0.047	-0.051	0.012
Pottery	361	0.038	-0.063	0.003
Glass	362	0.126	-0.048	0.09
Other Mineral	369	0.044	-0.045	0.009
Iron	371	0.068	-0.047	0.032
Other Metals	372	-0.049	-0.048	-0.085
Fabricated Metals	381	0.06	-0.045	0.024
Machinery	382	0.049	-0.047	0.013
Electrical Machinery	383	0.031	-0.048	-0.005
Transportation	384	0.021	-0.055	-0.015
Professional Equipment	385	0.046	-0.064	0.01
Other	390	-0.007	-0.045	-0.043

Test all coefficients equal

	P-value	0.0001		

Table 2. Persistence of Financial Development and Trade Liberalization

Whole Sample (73 countries)

	Mean	St. Dev.	Median	Minimum	Maximum
Private Credit Rank in 1995-99	37.0	21.2	37.0	1	73
Private Credit Rank in 1970-74	37.0	21.2	37.0	1	73
Change in Rank	0.0	0.0	0.0	-28	48

Share of Variance of Rank1995-99					
explained by Rank1970-74	51.0%
Rank Correlation Private Credit 1970-74 / 1995-99	0.677***

Liberalizers (28 countries)

	Mean	St. Dev.	Median	Minimum	Maximum
Private Credit Rank in 1995-99	29.6	15.9	30.0	3	67
Private Credit Rank in 1970-74	25.7	14.5	22.5	4	64
Change in Rank	3.9	19.1	1.0	-28	48

Share of Variance of Rank1995-99					
explained by Rank1970-74	4.6%
Rank Correlation Private Credit 1970-74 / 1995-99	0.136

Non-Liberalizers (45 countries)

	Mean	St. Dev.	Median	Minimum	Maximum
Private Credit Rank in 1995-99	41.6	22.9	48.0	1	73
Private Credit Rank in 1970-74	44.0	21.8	49.0	1	73
Change in Rank	-2.4	13.5	-1.0	-25	32

Share of Variance of Rank1995-99					
explained by Rank1970-74	67.0%
Rank Correlation Private Credit 1970-74 / 1995-99	0.756***

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3. Trade liberalization and the political economy determinants of financial development

The dependent variable is, for each country, the change in private credit to GDP between the period t-5 to t-1 and the period t+5 to t+10, where t denotes the year in which the country liberalized trade. △StrengthPromoters is the difference between the average (value added weighted) change in the price-cost margin of the promoters and opponents groups. Promoters (opponents) are those industries that score higher than median in the measure of the effect of financial development on margins (Panel A) and average firm size (Panel B). The change in the price-cost margin for each group is computed as the difference in the margin between the period t-5 to t-1 and t to t+5. $\triangle PCM^{highly\ dependent} - \triangle PCM^{less\ dependent}$ is difference in the change in margins of the group of industries that score higher and lower than median in Rajan and Zingales (1998)'s index of external finance dependence. The computation is analogous to that of promoters and opponents. Errors (in parentheses below each coefficient) are robust to heteroskedasticity and allow for clustering by year of trade liberalization. Liberalization year fixed effects included but not reported. * significant at 10%; ** significant at 5%; *** significant at 1%.

Panel A: Margins Measure

	(1)	(2)	(3)	(4)	(5)
Initial Private Credit to GDP	-0.086	-0.051	-0.1345	-0.235	-0.098
	(0.148)	(0.167)	(0.182)	(0.207)	(0.146)
△StrengthPromoters	2.398***	2.288***	2.395***	2.411***	2.633***
	(0.631)	(0.664)	(0.646)	(0.701)	(0.714)
GDP growth		0.057			
		(0.332)			
Change in Investment rate			-0.3034		
			(0.855)		
Ln (Initial GDP per capita)				0.067	
				(0.092)	
$\triangle PCM^{highly\ dependent} - \triangle PCM^{less\ dependent}$					-0.493
					(0.679)
Constant					
Observations	41	39	38	40	41
R-squared	0.563	0.572	0.564	0.670	0.568

Panel B: Size Measure

	(1)	(2)	(3)	(4)	(5)
Initial Private Credit to GDP	-0.0484	-0.019	-0.112	-0.123	-0.057
	(0.175)	(0.200)	(0.215)	(0.337)	(0.166)
△StrengthPromoters	1.995***	1.902**	2.006**	1.906***	2.412**
	(0.665)	(0.852)	(0.701)	(0.630)	(0.991)
GDP growth		-0.032			
		(0.359)			
Change in Investment rate			-0.214		
			(0.801)		
Ln (Initial GDP per capita)				0.017	
				(0.068)	
$\triangle PCM^{highly\ dependent} - \triangle PCM^{less\ dependent}$					-0.661
					(0.855)
Constant					
Observations	41	39	38	40	41
R-squared	0.506	0.5194	0.512	0.517	0.514

Robust, event time clustered errors in parentheses. Event time fixed effects included but not reporte
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4. Trade liberalization and the political economy determinants of financial development: Robustness

The dependent variable is, for each country, the change in private credit to GDP between the period t-5 to t-1 and the period t+5 to t+10, where t denotes the year in which the country liberalized trade. △StrengthPromoters is the difference between the average (value added weighted) change in the price-cost margin of the promoters and opponents groups. Promoters (opponents) are those industries that score higher than median in the measure of the effect of financial development on margins. The change in the price-cost margin for each group is computed as the difference in the margin between the period t-5 to t-1 and t to t+5. Panel A. Column (1) computes the impact of trade liberalization on margins as the difference between the period t-3 to t-1 and t to t+3. The change in private credit to GDP is measured between the period t-3 to t-1 and the period t to t+3 in (2), and between the period t-5 to t-1 and the period t+10 to t+15 in (3). (4) excludes the observations corresponding to tobacco and refineries from the measure of △StrengthPromoters. Panel B. *Change in Volume of Trade* is computed as the change in the ratio of imports plus exports to GDP between the period t-5 to t-1 and the period t+5 to t+10. *Ln(Initial Terms of Trade)* is the average Terms of Trade (1995=100) in t-5 to t-1. *Initial GDP pc growth of Trading Partners* is the average from t-5 to t-1 of the growth rate of partner countries weighted by the share in total trade. *Initial US margin promoters – opponents* is the average margin (t-5 to t-1) in the US of promoters minus that of opponents. *Initial IMF Disbursement to GDP* is the average ratio of IMF disbursements to GDP between t-5 and t-1. In (5) only data corresponding to countries liberalizing before 1990 is considered. In (6) only data corresponding to countries liberalizing after 1990 is considered. Errors (in parentheses below each coefficient) are robust to heteroskedasticity and allow for clustering by year of trade liberalization. Liberalization year fixed effects included but not reported. * significant at 10%; ** significant at 5%; *** significant at 1%.

Panel A

	(1)	(2)	(3)	(4)
Initial Private Credit to GDP	-0.233	-0.138	-0.047	-0.048
	(0.155)	(0.127)	(0.081)	(0.165)
△StrengthPromoters	2.012**	0.606	3.01**	2.238**
	(0.714)	(0.476)	(1.083)	(0.985)
Observations	35	38	32	40
R-squared	0.615	0.503	0.517	0.590

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Initial Private Credit to GDP	-0.099	-0.094	-0.109	-0.100	-0.114	-0.055	-0.057
	(0.213)	(0.159)	(0.159)	(0.161)	(0.292)	(0.164)	(0.267)
△StrengthPromoters	2.264***	2.389***	2.295***	2.419***	2.256**	2.695*	2.424***
	(0.672)	(0.678)	(0.617)	(0.658)	(0.771)	(1.312)	(0.758)
Change in Volume of Trade to GDP	-0.144						
	(0.211)						
Ln(Initial Terms of Trade)		-0.036					
		(0.310)					
Initial GDP pc growth of Trading Partners			0.031				
			(0.051)				
Initial US margin promoters - opponents				-0.149			
				(0.285)			
Initial IMF Disbursement to GDP							0.192
							(3.066)
Observations	38	39	41	41	21	20	32
R-squared	0.575	0.562	0.571	0.564	0.574	0.534	0.526

Robust, event time clustered errors in parentheses. Event time fixed effects included but not reported.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. Trade liberalization and the political economy determinants of financial development: The policy link

The dependent variable is, for each country, the change in private credit to GDP between the period t-5 to t-1 and the period t+5 to t+10, where t denotes the year in which the country liberalized trade. The independent variables correspond to the difference in the change of each policy index between the period t-5 to t-1 and the period t to t+5. The indices are taken from Abaiad and Mody (2003). Errors (in parentheses below each coefficient) are robust to heteroskedasticity and allow for clustering by year of trade liberalization. Liberalization year fixed effects not included. * significant at 10%; ** significant at 5%; *** significant at 1%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Initial Private Credit to GDP	-0.275**	-0.275	-0.257*	-0.330***	-0.274	-0.124	-0.255	-0.297*
	(0.102)	(0.151)	(0.131)	(0.055)	(0.195)	(0.187)	(0.159)	(0.125)
△StrengthPromoters	2.345	2.500*	2.485*	1.030	2.622**	2.426	2.368*	1.953***
	(1.297)	(1.110)	(1.261)	(0.984)	(0.801)	(1.434)	(1.211)	(0.416)
Credit Liberalization		0.037						
		(0.057)						
Interest Rate Liberalization			0.016					
			(0.032)					
Entry Barriers Lifting				0.206*				
				(0.088)				
Securities Mkt Regulation					0.124**			
					(0.063)			
Privatization						0.083		
						(0.097)		
Capital Account Liberalization							0.046	
							(0.052)	
Financial Policies Index								1.894**
								(0.774)
Constant	0.205***	0.162**	0.184**	0.019	0.096	0.117	0.134**	0.000
	(0.052)	(0.045)	(0.059)	(0.106)	(0.072)	(0.098)	(0.048)	(0.089)
Observations	15	15	15	15	15	15	15	15
R-squared	0.258	0.286	0.263	0.693	0.541	0.325	0.293	0.712

Table 6. Trade liberalization and the political economy determinants of financial development, Capital deepening or improved allocation?

The dependent variable in (1) is the change in a measure of the quality of capital allocation. Based on Wurgler (2000), this is computed for each country as the change in the sensitivity of growth of investment on growth in value added measured between the period t-5 to t-1 and t+5 to t+10. In (2) the dependent variable corresponds to the change in the ratio of fixed capital formation to value added in the manufacturing sector in each country. Again, the change is measured between the period t-5 to t-1 and t+5 to t+10. △StrengthPromoters is the difference between the average (value added weighted) change in the price-cost margin of the promoters and opponents groups. Promoters (opponents) are those industries that score higher than median in the measure of the effect of financial development on margins. The change in the price-cost margin for each group is computed as the difference in the margin between the period t-5 to t-1 and t to t+5. Errors (in parentheses below each coefficient) are robust to heteroskedasticity and allow for clustering by year of trade liberalization. Liberalization year fixed effects included but not reported. * significant at 10%; ** significant at 5%; *** significant at 1%.

	(1)	(2)
Initial Capital Allocation Quality	-0.761**	
	(0.231)	
Initial Investment Rate		-0.284**
		(0.111)
△StrengthPromoters	7.474**	0.439
	(2.841)	(0.321)
Constant	0.196	0.071
	(0.224)	(0.038)
Observations	12	12
R-squared	0.5469	0.355

Table 7. Trade liberalization and the political economy determinants of financial development, Why use Financial Development?

The dependent variable is, for each country, the change in private credit to GDP between the period t-5 to t-1 and the period t+5 to t+10, where t denotes the year in which the country liberalized trade. △StrengthPromoters is the difference between the average (value added weighted) change in the price-cost margin of the promoters and opponents groups. Promoters (opponents) are those industries that score higher than median in the measure of the effect of financial development on margins. The change in the price-cost margin for each group is computed as the difference in the margin between the period t-5 to t-1 and t to t+5. In each column the independent variables indicated are included. Errors (in parentheses below each coefficient) are robust to heteroskedasticity and allow for clustering by year of trade liberalization. Liberalization year fixed effects included but not reported. * significant at 10%; ** significant at 5%; *** significant at 1%.

	(1)	(2)	(3)	(4)	(5)
Initial Private Credit to GDP	-0.252	-0.034	-0.105	-0.110	-0.115
	(0.174)	(0.290)	(0.181)	(0.092)	(0.210)
△StrengthPromoters	2.289**	20.863**	2.842**	3.229***	2.452***
	(0.827)	(9.376)	(1.010)	(0.433)	(0.716)
Rule of Law x △StrengthPromoters	3.983***				
	(1.245)				
Rule of Law	0.179*				
	(0.088)				
Lack of Corruption x △StrengthPromoters		24.109*			
		(12.170)			
Lack of Corruption		0.534			
		(0.434)			
Voice and Accountability x △StrengthPromoters			2.942		
			(1.791)		
Voice and Accountability			0.139		
			(0.127)		
Political Stability x △StrengthPromoters				6.891***	
				(1.191)	
Political Stability				0.181*	
				(0.090)	
Government Effectiveness x △StrengthPromoters					4.971***
					(1.309)
Government Effectiveness					0.151
					(0.126)
Observations	41	32	41	41	41
R-squared	0.784	0.796	0.712	0.834	0.802

Table A1. Sample Characteristics

| Country | Trade Liberalization Year | Bank Credit to Private Sector / GDP | | | △Strength Promoters |
		Initial	Final	Change	
Argentina	1991	0.22	0.22	0.00	-0.009
Australia	1964	0.19	0.24	0.06	-0.003
Bangladesh	1996	0.17	0.28	0.11	-0.057
Bolivia	1985	0.12	0.37	0.25	0.046
Brazil	1991	0.43	0.30	-0.13	-0.061
Chile	1976	0.08	0.59	0.51	0.047
Cameroon	1993	0.23	0.08	-0.15	0.029
Colombia	1986	0.15	0.15	0.00	-0.001
Costa Rica	1986	0.18	0.12	-0.06	-0.058
Ecuador	1991	0.12	0.25	0.13	-0.047
Egypt	1995	0.24	0.54	0.30	0.152
Ethiopia	1996	0.06	0.23	0.17	-0.051
Ghana	1985	0.02	0.05	0.03	-0.014
Guatemala	1988	0.17	0.15	-0.02	-0.016
Honduras	1991	0.25	0.32	0.07	-0.035
Hungary	1990	0.38	0.25	-0.12	-0.023
Ireland	1966	0.33	0.27	-0.05	0.006
Israel	1985	0.71	0.62	-0.09	-0.019
Jamaica	1989	0.24	0.23	0.00	-0.017
Jordan	1965	0.17	0.23	0.06	-0.042
Japan	1964	0.74	0.86	0.12	0.015
Kenya	1993	0.20	0.25	0.05	-0.028
Korea	1968	0.12	0.33	0.22	0.001
Sri Lanka	1991	0.20	0.29	0.09	-0.002
Morroco	1984	0.17	0.18	0.02	-0.006
Mexico	1986	0.15	0.28	0.13	0.000
Nepal	1991	0.11	0.27	0.15	-0.015
New Zealand	1986	0.20	0.87	0.67	-0.004
Panama	1996	0.55	0.95	0.40	0.011
Peru	1991	0.07	0.25	0.18	-0.014
Philipines	1988	0.22	0.41	0.19	0.088
Poland	1990	0.04	0.23	0.20	0.024
Romania	1992	0.53	0.09	-0.44	0.021
Singapore	1965	0.36	0.52	0.16	-0.007
El Salvador	1989	0.07	0.04	-0.03	0.005
Trinidad and Tobago	1992	0.33	0.32	-0.01	0.081
Turkey	1989	0.18	0.20	0.03	-0.057
Tanzania	1995	0.12	0.05	-0.06	-0.016
Uruguay	1990	0.38	0.38	0.00	0.001
Venezuela	1996	0.14	0.10	-0.04	0.005
South Africa	1991	0.52	0.69	0.17	-0.017
Mean		0.24	0.32	0.08	0.00
Median		0.19	0.25	0.06	-0.01
St. Dev.		0.17	0.22	0.19	0.04
Correlation with:					
Initial Credit		1.00	0.58	-0.23	0.06
Final Credit			1.00	0.66	0.25
Change in Credit				1.00	0.24
△Strength Promoters					1.00

Figure 1. Financial Development and Competition

Panel A: Financial Development and Price-Cost Margins, Average 1980-2000.
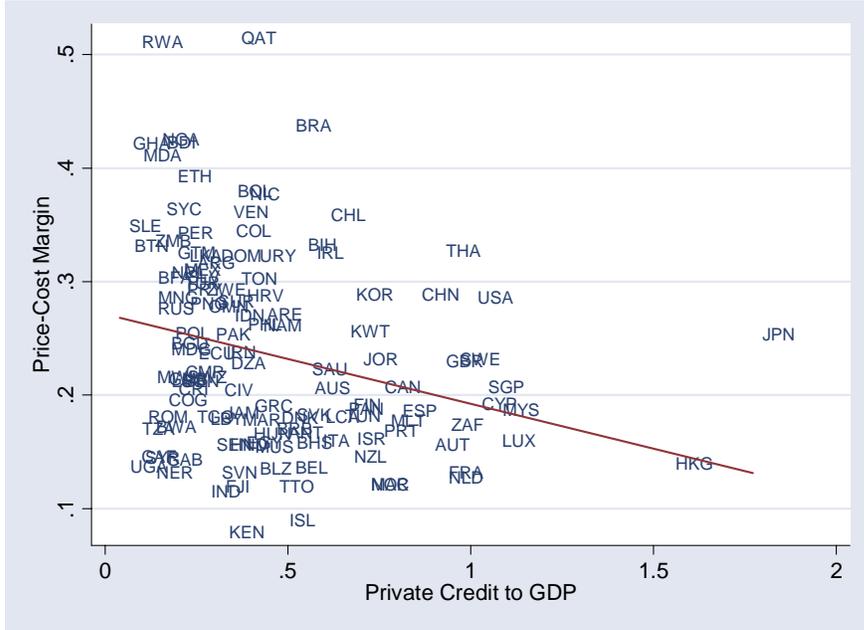
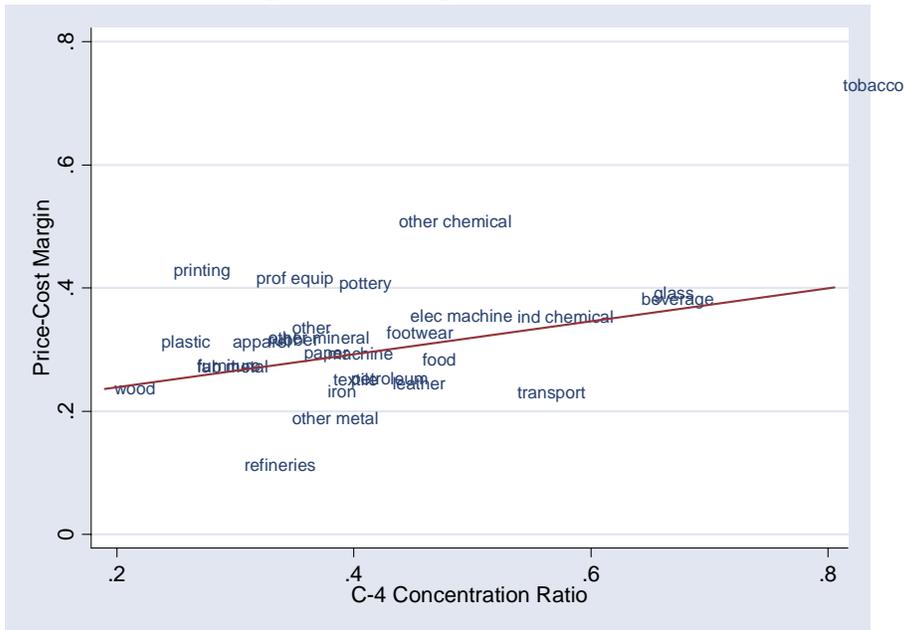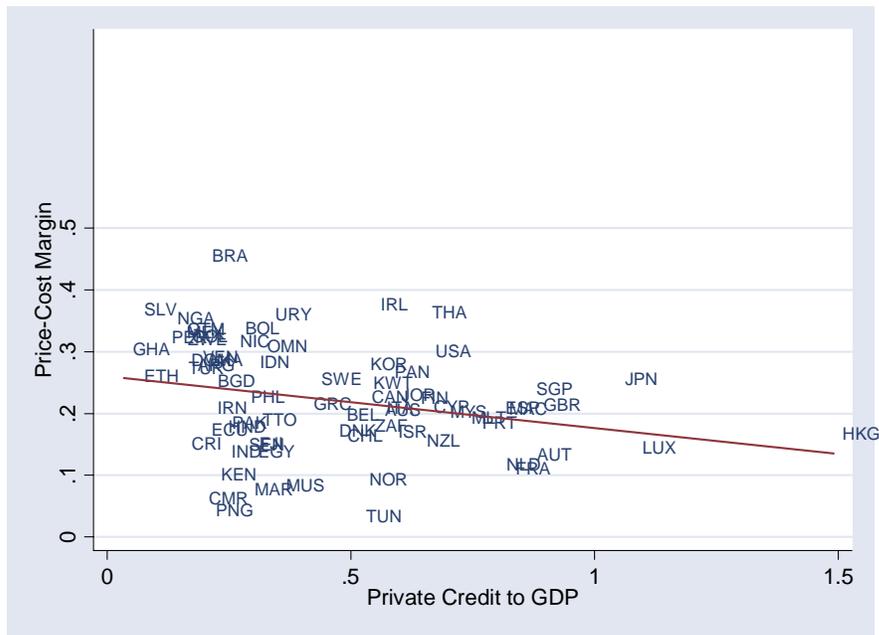Panel B: Price-Cost Margins and Competition in the U.S., 1992.

Figure 2. Financial Development and Margins: Beverages and Machinery
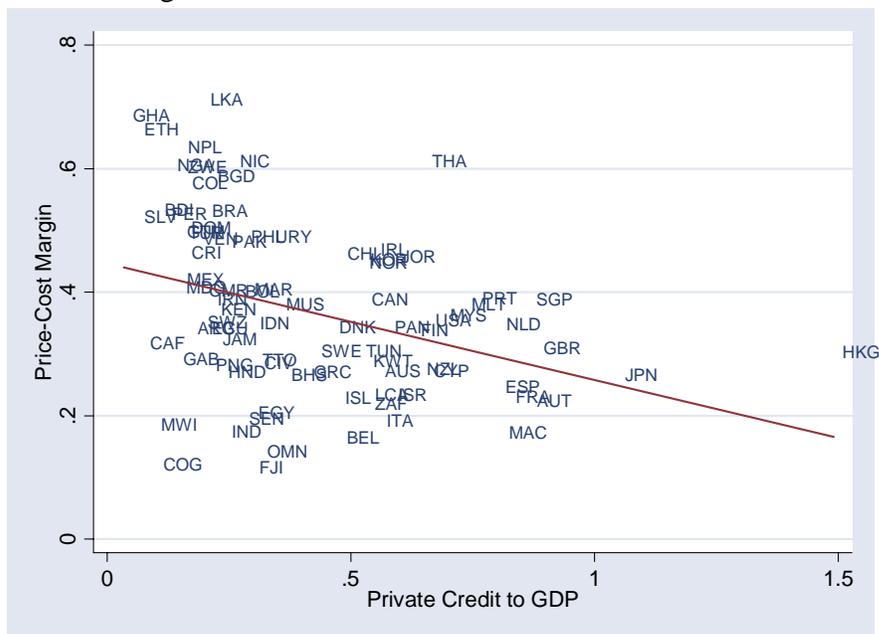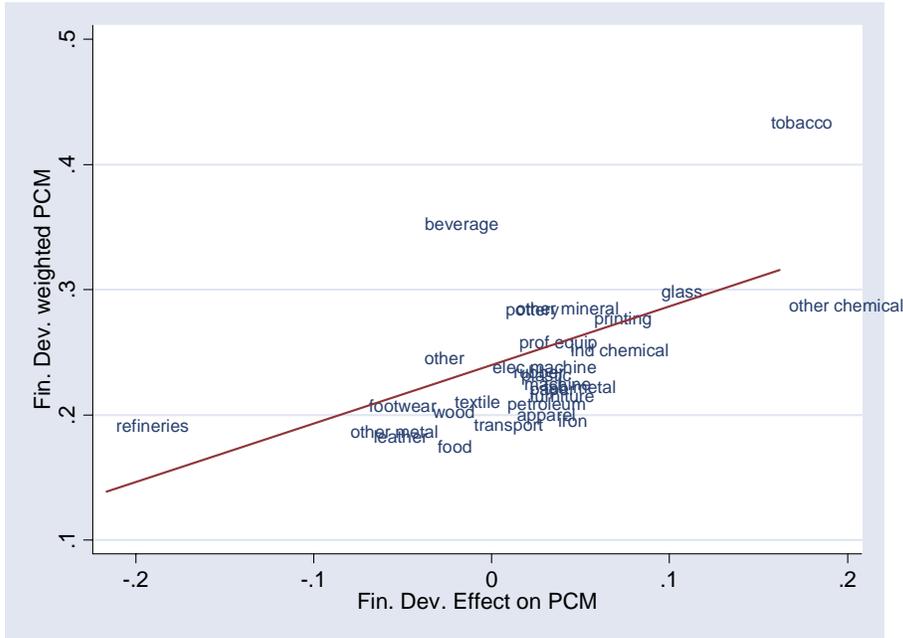
Panel A: Machinery

Panel B: Beverages

Figure 3. Financial Development, Margins, and Average Firm Size across Industries

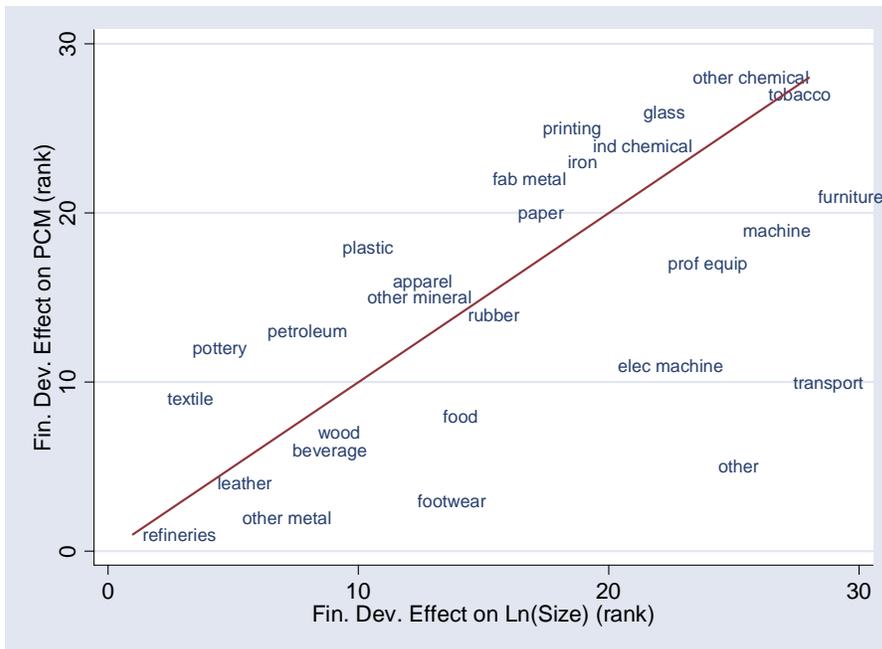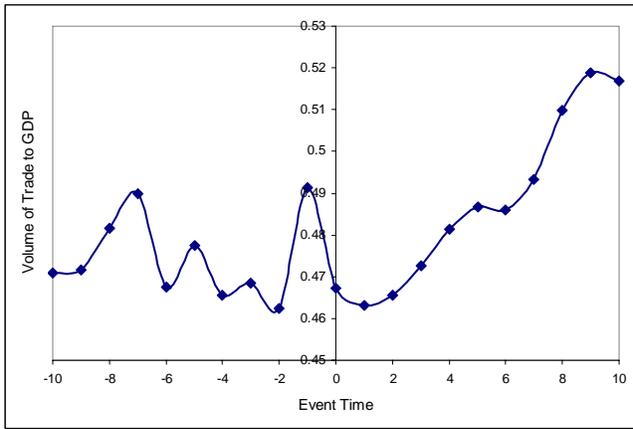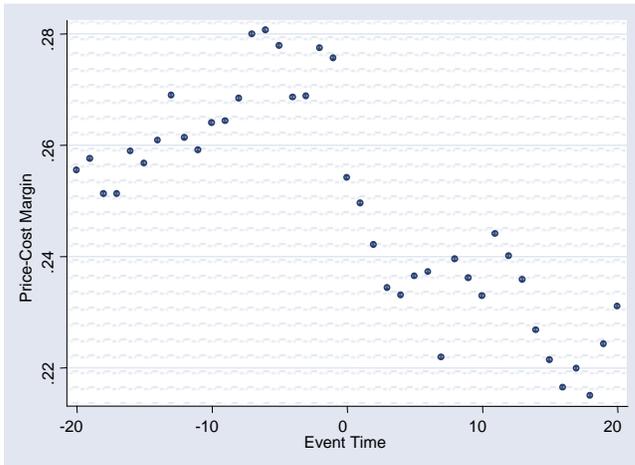Panel A: Financial Development and Margins

Panel B: PCM vs. Margins Measure

Figure 4. Effect of Trade Liberalization

Panel A: Volume of Trade

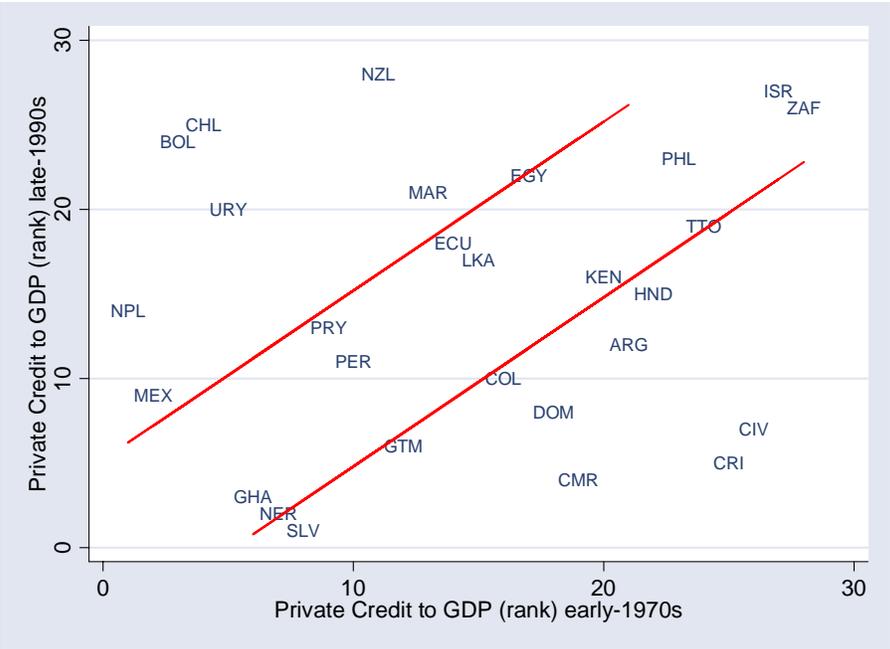Panel B: Aggregate Price-Cost Margin

Panel C: Relative Margins
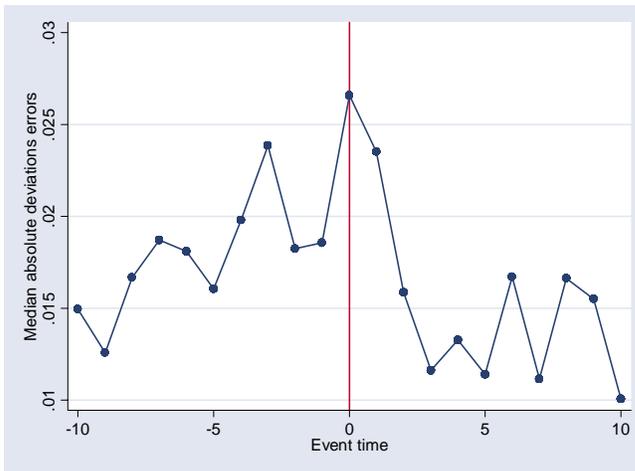
Figure 5. Persistence of Financial Development and Trade Liberalization

Panel A: Trade Liberalizers

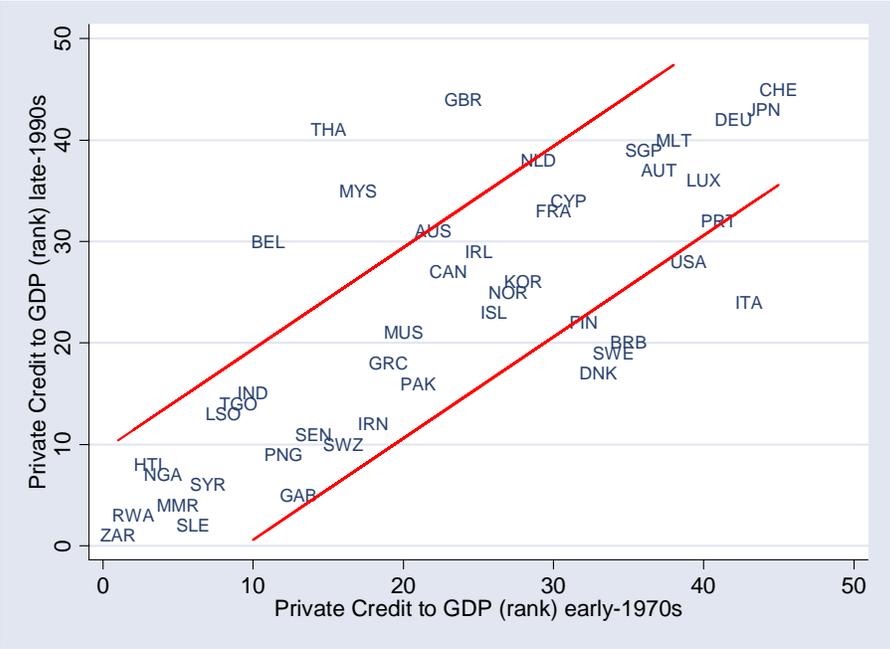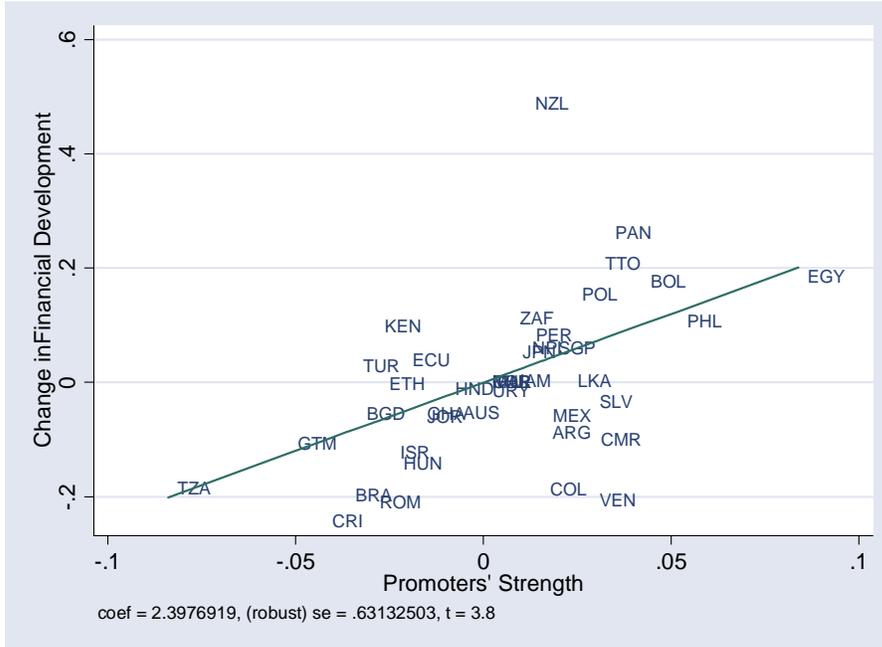
Panel B: Trade Non-Liberalizers

Figure 5. Political Economy Determinants of Financial Development

Panel A: Cross-country

Panel B: Dynamics