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Banking on Politics

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Abstract

This paper presents new data from 150 countries showing that former cabinet members, central bank governors, and financial regulators are many orders of magnitude more likely than other citizens to become board members of banks. Countries where the politician-banker phenomenon is more prevalent have higher corruption and more powerful yet less accountable governments, but not better functioning financial systems. Regulation becomes more pro-banker where this happens more often. Furthermore, a higher fraction of the rents that are created accrue to bankers, former politicians are not more likely to be directors when their side is in power, and banks are more profitable without being more leveraged. Rather than supporting a public interest view, the evidence is consistent with a capture-type private interest story where, in exchange for a non-executive position at a bank in the future, politicians provide for beneficial regulation.

This paper—a product of the Growth and the Macroeconomics Team, Development Research Group—is part of a larger effort in the department to understand the determinants of financial development. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at craddatz@worldbank.org.

BANKING ON POLITICS*

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1. Introduction

In this paper we gather new data that shows that, in a large number of countries, former cabinet members, central bank governors, and financial regulators are many orders of magnitude more likely to become board members at banks than the regular citizen in the average country. Politicians and bankers are so deeply intertwined that one can reject that they were randomly selected from a common pool, even if one assumes that the relevant population corresponds to just a very small fraction (1 to 5%) of the people with tertiary education in a given country.

A benign, public interest view would see this as a way in which ability, knowledge or experience are fruitfully shared between private and public sectors.[1] In this setting the phenomenon would be positively related to the expertise of the former politician, and would most likely produce better, more efficient banks. There is no particular reason why it should be related to neither the power of the authority to affect regulation nor its accountability or the general disposition of people to engage in corruption. The cross-country evidence, however, is not particularly supportive of this view since our measure of prevalence correlates in the opposite way with the degree of development of the banking system, and, far from being randomly distributed across countries, it happens more often in countries with higher corruption and more powerful yet less accountable governments. On the reasons for doing it under the public interest view, the evidence is at least not decisive; politicians with skills similar to those needed to run a bank (i.e. those in charge of finance and economic functions and central bankers) are indeed more likely to sit on the board of banks than other cabinet members. However, those with probably the highest expertise but lesser influence on rent-shifting regulation (supervisors of banks, insurance companies, and securities) have the same chance as the average politician of ending up sitting on the board.

An alternative, less benign view is one whereby people relate to each other with the goal of creating and/or shifting rents via regulation, and at the expense of society. The mechanism and the players are probably the most likely in a game like this: the financial sector incumbents are the ones that stand to lose and win the most from alternative regulation schemes, while the politicians are the ones more directly in charge of designing and implementing those regulations. A hallmark of the private-interest view of financial regulation is the presence of transfers between the incumbents and the regulator. Campaign contributions, gifts, or bribes are very direct but very hard to document. As a vehicle for transfers, a seat on the bank's board of directors may not be the most direct but it can be as effective: it is typically well paid and, although there is

[1] Pigou (1938).

evidence that board composition matters for firm performance, board members may also simply passively collect payments. Also, it cannot be easily differentiated from a legitimate hiring and any lack of technical competence is not directly damaging to the firm, as it would be the case if the politician were appointed to an executive position.

There are at least two ways in which bankers can interact with politicians in a private-interest setting: the tollbooth and the capture mode (a distinction borrowed from Djankov et al (2003)). The tollbooth view would have politicians benefiting from the bankers and using the bank to extract rents or for other political purposes.[2] The more traditional capture view sees the bankers capturing the politician in order to get more favorable treatment.[3] Both stand in sharp contrast to the public interest theory and fit better the evidence above because they imply a higher prevalence where the politician has more power, where the authority is less accountable, and where there lower costs of engaging on it. Both are independent of the degree of financial expertise of the politician, and both should have a cost in terms of worse-functioning banking systems. But the two flavors differ critically in the way the benefits of the relationship are split between the two groups: while the board seat is an imposition on the bank and an enforcement device for the politician, in the tollbooth story it is a reward for accommodating regulators. We are able to test the alternative views based on three differences in their implications. First, the tollbooth view would predict a higher prevalence when the politician's sector is in government because the politician would be acting as an agent in charge of extracting rents or favors from the regulated firm. Second, banks would be more profitable under the capture than the tollbooth view; leverage should be higher in the latter because it allows to shield shareholders from expropriation. Third, while rents created can be measured and should accrue primarily to bankers in the capture story, under the tollbooth view rents are dissipated via higher costs.

Overall the evidence favors the capture view: former politicians are not more likely to be directors when their side is in power, banks are more profitable without being more leveraged, rents can be measured and a higher fraction of these accrue to bankers, and regulation becomes more pro-banker where the occurrence is higher.

Our paper is most closely related to the literature emphasizing political factors as a necessary complement to initial factors in understanding the determinants of financial development (see Haber and Perotti (2008) for a thorough recent review). As such, it follows insights from recent work by Rajan and Zingales (2003), Pagano and Volpin (2005), and Perotti and von Thadden (2006), and systematic evidence for a large number of countries from Barth,

[2] De Soto (1990).
[3] Stigler (1971).

Caprio, and Levine (2006). We take that politics and finance are far from independent as a given, and set out to document a particular mechanism –that of former politicians sitting in banks' boards of directors- through which this may be working in practice.[4]

A recent strand of the literature has documented that good things do happen to firms that are politically connected.[5] In a paper very close to ours, Faccio (2006) shows that the overlap between controlling shareholders and (current) government officials is quite widespread across countries, and seems to have a positive impact in the connected firms' value via regulation. We take this lead –especially in the way the phenomenon is measured- and complement the line of research by focusing on banks and by providing further evidence on what happens to the other groups likely to be involved in this game: politicians appear to be also benefiting, while the public seems to suffer from poorly developed financial system. We also allow these connections to be based not only on current links but also on interactions at different moments in time.

The rest of the paper is structured as follows. Section 2 describes the data and the matching procedure used to measure the prevalence of the banker-politician phenomenon. Section 3 presents our results on what kind of politicians and banks are more likely to be related this way, and where this happens more often. Section 4 concludes.

2. Measuring the Prevalence of Banker-Politicians

The basic sources of data for politician names were the *Economist Inteligence Unit Country Reports*, which we revised twice a year for each country between 1996 and 2005. From this we obtained a total of 72,769 names of cabinet members and central bank governors. These names were complemented by a smaller set of 593 names of financial sector supervisors obtained from the 2000, 2002, 2003, and 2004 issues of *How Countries Supervise their Banks, Insurers and Securities Markets*. These two sets together provide extensive coverage for cabinet members and financial sector supervisors in about 150 countries.

The names of banks' board members come from *Bankscope*, which has data on both listed and unlisted banks in about 150 countries at one moment in time (generally December 2005). A total of 109,645 board member names were collected. Although this is the most comprehensive source of data on bank information around the world, only a fraction of banks in

[4] The mechanism has been vividly documented in recent historical country case studies such as Maurer and Gomberg (2005), Maurer (2002), and Razo (2009).
[5] See, for instance, Faccio (2006), Fisman (2001), Faccio, Masulis, and McConnell (2005), and Khwaja and Mian (2005).

Bankscope reports data on board composition, as shown in Table A1 in the appendix. We will address this sampling issue below, but for the meantime it is important to notice that this incomplete coverage will result in underestimating the number of bankers-politicians.

The process of finding coincidences between politicians and bankers' names involves four steps. First, we standardize the strings containing the names by converting them to lowercase only, and removing punctuations and titles (Sir, PhD, etc.). In the second step, we remove duplicate entries by determining in each of the datasets those observations that correspond to different spellings of the same name (for instance, with and without the middle initial). In a third stage, we pool the different datasets containing politician names and again determine those observations across the datasets that correspond to the same individual. Once the names have been cleaned in this way, we compare the names included in the politicians and bankers datasets to obtain the matching observations.

To determine whether there was a matching name in each step we use a *record-linkage algorithm* that forms all the possible pairs of names within each country, and then ranks the pairs based on three standard measures of string similarity used in the record linkage literature: Bigram, Levenshtein, and Longest Common Subsequence.[6] When comparing two strings containing names, each of these criteria results in a value between 0 and 1 that measures the likeliness of the two names. We kept all pairs with a minimum value of 0.8 in at least one of the three criteria and then visually checked all pairs to determine whether they corresponded to an actual match.

Following the process above, after the second step we end up with 10,829 different politicians and 62,981 different bankers in 146 countries. The third step produces a total of 211 matching names across these two lists of people in this set of countries (see columns (3) to (5) in Table 1).

Based on the raw numbers documented above, one might conclude that the mechanism cannot be of first order importance simply because it is highly infrequent. After all, having found only 211 politicians that are also bankers out of more than 70,000 names, and finding none in roughly half the countries considered looks unimpressive. We argue this not so simply because of the sheer size of the pool where both politicians and bankers are extracted.

The expected number of matches one would get by randomly choosing people for the politician and banker posts is much lower in many countries than the actual number of matches we found. We derive the probability of obtaining a given number of coincidences under the

[6] The record linkage software we use is *Merge Toolbox (MTB),* a Java based tool created by the members of the *Safelink* project (see Schnell et al (2004)).

assumption that the officers needed to fill the politician and banker posts are drawn randomly and with replacement (at the sample level) from a common pool in the appendix. We use this probability to compute the expected number of matches one would find assuming the common pool is the entire population of each country. The results presented in Table 1 are striking: the number of actual matches for the average country is 3,984 times higher than one would expect (Column (7)). If one considers just the countries for which we found at least one match, the ratio of actual to expected matches grows to 7,321 times. The figure varies greatly across countries: from a minimum of 34 times in Luxembourg to a maximum of 150,000 in Myanmar. What is clear, though, is that in virtually every country the number of matches found is many orders of magnitude larger than the one expected.

Since for many countries it is highly unlikely that every person has the same probability of being chosen for a politician or a banker post, the figures above are probably exaggerated. Still, the magnitudes are so different that even if one assumes that the relevant pool is only 1% of the population with tertiary education, the average country ends up with 2.3 times more matches than expected (among those countries with at least one match, see Column (8)).

The ratios described above suggest that the prevalence of banker-politicians is higher than what one could expect if they happened for pure chance, but do not provide a formal metric for how much the observed number of matches depart from the random benchmark. Such a benchmark is provided by the probability of finding the observed number of matches under the null hypothesis that they come from random draws. These probabilities are typically very small and a test of the random draw hypothesis based on them would typically lead to reject the null. Even when considering one percent of the population with tertiary education as the relevant pool where bankers and politicians are drawn, and using a five percent critical value to reject the null, the hypothesis of random matching is rejected in 40 percent of the countries. A related, but probably more intuitive metric of the prevalence of banker-politicians is the size of the pool (as fraction of the tertiary population) where the sample of bankers and politicians would have to be drawn so that the random draw hypothesis could not be rejected at a five percent level. This figure is reported in Column (6) of Table 1. The median value of this fraction across countries is five percent, and the 25 and 75^{th} percentiles are located at one and 25 percent, respectively. Thus, in most countries the size of the pool where bankers and politicians and bankers are drawn would have to be a very small fraction of the population with tertiary education for not rejecting the random draw hypothesis at a five percent level. At the same time the results indicate that there is large variation across countries in the value of this fraction.

In most of the analysis that follows we drop from our sample the countries with zero matches and concentrate on the variation across the almost eighty countries where at least one match was found. About 45 percent of the countries were dropped for this reason. Under reasonable assumptions, the probability of finding zero matches is high even under strong versions of the alternative hypothesis of non-random matching. For instance, assuming that the common sampling population equals the population with tertiary education, and that the probability of finding a match is ten times higher than under random matching, the average probability of finding no matches between politicians and bankers is still 65 percent (the median probability is 78 percent). Thus, finding no matches provides almost no information on whether the matching process is likely to be random. Evaluated at zero, a test of the randomness hypothesis has very little power against a wide range of alternatives. In contrast, finding even one match provides considerable information on the likely randomness of the findings, since a match is typically a low probability event under the null of random matching.

In addition to the theoretical reason above, we were not confident that observing zero matching meant the same across countries. There are some countries where one could reasonably trust the quality of the data, but these were very few.[7] The vast majority of the countries we dropped are not even typically included in systematic cross-country analyses simply because they lack reliable data (see Table 3). Nevertheless, we also check the robustness of the main results to the inclusion of the zeroes.

The different measures of prevalence reported above are significantly correlated and can all be used to quantify the importance of the phenomenon. The correlation between the (log) ratio of actual to expected matches based on the whole population and that based on one percent of the population with tertiary education is 0.7 (excluding the zeroes), and the correlation of the former with the (log) fraction of the population with tertiary education required for not rejecting the random draw null is -0.68, both statistically significant at 1 percent level. In what follows we use the measure based on the whole population as benchmark, and refer to it as the measure of *PREVALENCE* unless explicitly citing a different measure.

When put it into context, the figures reported above are even more impressive once one considers that this is just one very particular mechanism through which politicians and bankers can be related. Family and social networks and other business relationships are also possible and, in some contexts, are as effective as a formal business relationship. Said differently, there is no need at all to occupy a seat in the board of a bank to have a relationship with it. Also, we are just

[7] We would subjectively classify the following in this group: Costa Rica, Czech Republic, Greece, Guatemala, Ireland, Malaysia, Panama, Sweden, New Zealand, Ecuador, and Uruguay.

considering the top posts in both politics and banking. Most likely what we are finding here is the tip of the iceberg.

3. Results

In this section we relate the information on what banks have politicians in their board, and what politicians become bankers. We also explore how the country-measures of prevalence relate to various bank, politicians, and country characteristics. The aim is to shed light on the causes and consequences of the banker-politician phenomenon and determine whether it is more likely to be related to the public or private interest hypothesis. Our empirical approach is based on reduced-form evidence: we document a series of correlations and contrast them with the ones predicted by different explanations. When taken together, the different pieces of evidence turn out to be quite supportive to the regulatory capture story.

3.1. What Politicians Become Bankers?

To address this question we define each politician as the unit of observation, and explore the variables that correlate with politicians being also bankers. Our basic data, described above, are complemented with political data from Polity IV.

Politicians that become bankers are different from those that don't across two dimensions that can help disentangling the different hypotheses. First, politicians in charge of finance and economic functions and central bankers are much more likely to sit on the board of banks (Table 2, Panel A). On the other hand, ministers in charge of politics and public services such as health and education are significantly less prone to be also bankers. These results can be interpreted in different ways. The fact that banker-politicians seem to be on cabinets that require similar skills to those needed to run a bank supports in principle the public interest view. On the other hand, under the private interest hypothesis the larger the influence of the politician on the regulation of the sector, the better for both the banker and the politician. This would make it much more attractive to offer a board seat to someone related to these regulatory matters.

Interestingly, we find that the supervisors of banks, insurance companies, and securities, are no more likely to be also bankers than the average politician. In the light of the discussion above, this would mean that supervisors are either not influential enough or not knowledgeable enough on these matters. Although one can image it either way depending on the country, we feel more inclined to think that these typically lower-rank, less political and more technical officers simply lack the power to radically change the size and distribution of rents in the industry. We

therefore interpret both pieces of evidence as generally supportive (albeit weakly) of the private interest hypothesis.

Panel B shows that the chances of being a banker are not higher when the same political sector is in power as when the politician was in office. Under the tollbooth view, politicians can more effectively extract rents from the bank while they are in power. This implies that we should more often observe that the politician bankers are of the same political sector as the ones currently in power. Neither the regulatory capture nor the public interest views require such correlation. Under the regulatory capture view, politicians may be paid ex-post for favorable regulatory treatment, even if their party has left power. Of course, the public interest hypothesis does not require any correlation between the presence of a former politician in a bank's board and the party in government.

3.2. What Banks Are Politically Connected?

In this section we define the bank as the unit of observation and correlate bank characteristics with whether the bank is politically connected or not, that is, whether it has a board member whose name was matched to a name in our sample of politicians. In particular, we look at measures of size, profitability, leverage, riskiness and liquidity that were constructed directly from Bankscope data using bank's statements at the end of 2004. As explained above, most of our board composition information comes from the years immediately before 2004 (67 percent of all information comes from the years 2003 and 2004). By focusing on data from the end of 2004 we ensure that the financial information compared comes from a year in which a bank either is or has already been connected and, at the same time, avoid pooling nominal data from different years. This year also has the advantage of not comprising any banking crisis. However, this restricts a little our sample, since some connected banks either do not exist or do not report financial data in 2004. While across all years there are 4,601 banks with information on board composition across of which 194 banks were connected, we have financial data in 2004 for 3,863 banks (3,271 privately-owned and 753 foreign) . Of these, 168 banks in 75 countries have at least one politician in their board.[8]

We estimate the parameters of the following parsimonious specification in the sample of banks for which we have information on board composition and financial data at the end of 2004

$$Y_{i,c} = \alpha + \beta CONNECTED_{i,c} + \gamma X_{i,c} + \theta_c + \varepsilon_{i,c} \quad (1)$$

[8] The complete list with the number of banks with data and the number of connected banks by country is available upon request.

where $Y_{i,c}$ corresponds to the financial characteristics of bank *i* in country *c*, which include measures of size, profitability, riskiness, liquidity, and leverage; $CONNECTED_{i,c}$ is a measure of the connectedness of the bank. We consider two such measures, the first is a dummy variable that takes the value one if at least one of the bank's directors has been a politician or bank supervisor, and zero otherwise; the second is the fraction of a bank's board members that has previously been a politician. $X_{i,c}$ are other bank characteristics included as control variables. Since we are not interested in modeling the determinants of bank's financial characteristics, we typically include just a measure of bank's size as captured by its total assets (except when the left hand side variable itself is a measure of size). Finally, θ_c is a country fixed effect included to make sure that the identification comes from the within country differences between connected and unconnected banks, and $\varepsilon_{i,c}$ is a residual term.

For our benchmark results we estimate the parameters of equation (1) by weighted-OLS, where the weights given to each observation are inversely proportional to the number of banks in a country for which we have information. This ensures that the results are not disproportionably driven by countries with a larger number of banks (see Demirguc-Kunt and Huizinga, 2000). Since bank level data are notoriously noisy, we measure all variables in logarithms to reduce the influence of outliers (variables corresponding to ratios that can plausible take negative values are expressed as the logarithm of one plus the variable). We estimate the parameters of the benchmark model separately for all the banks and banks that have no public ownership.

Table 3 reports the OLS estimates of β for the dummy measure of connectedness. The parameters obtained for the other controls are omitted. Different columns of the table report the coefficients associated with of size, profitability, leverage, and riskiness. Panel A reports the results obtained in the sample of all banks reporting board composition data, while panel B restricts the sample to include only banks with no public ownership.

The coefficients show that connected banks tend to be the largest in the country (Column (1)). Assets of connected banks are about 34 percent larger. Similar results are obtained for other measures of size, such as loans and country ranking (not reported). Connected banks also tend to be more profitable and typically have a return on average assets that is between 0.6 and 0.8 percent higher than the average bank (Column (2)). Regarding balance-sheet structure, leverage is significantly lower among connected banks (Column (3)). The ratio of equity to total assets is 2 percent higher in connected banks than in the average bank, and in the sample of fully privately owned bank this difference increases to 3 percent. Connected banks also seem to take on less risk

since they tend to have a lower proportion of write-offs and impaired loans relative to gross loans and reserves (Column (4)).

Results exploiting differences in the intensity of connectedness (not reported) are qualitatively similar, but in most cases the statistical significance of the estimated coefficients is reduced. This indicates that whether a bank is connected or not is typically more strongly correlated with some of its characteristics than the intensity of the connection (as captured by the fraction of directors that were politicians).

Overall, the results indicate that connected banks are larger, more profitable, less leveraged, and less risky than unconnected ones, regardless of whether the government has some participation in their ownership.

The results indicate the presence of rents associated with the relation between banks and politicians. When taken as success measures, however, the size and profitability findings are consistent with both the private and the public-interest story. Absent an appropriate instrument or source of identification we do not know whether being connected makes a bank larger and more profitable or whether banks with these characteristics attract former politicians. However, some aspects of the evidence fit better with the capture story. First, the leverage finding is more suggestive of a capture than a tollbooth mechanism so far debt is not used to protect shareholders from expropriation. Similarly, far from increasing risk to increase the value of the residual claim in a tollbooth context, connected banks are actually less risky.[9] Also, the fact that connected banks are taking lower risks makes their higher profitability even more striking. Second, the results are weaker for the relation between the intensity of connection and banks characteristics. If better banks attracted former politicians, either as a way of extracting tollbooth or for career concerns, they should cluster in these banks and the fraction of directors that are former politicians would be a better proxy of a bank's desirable characteristics than the simple dummy variable.

Putting aside the issue of causality, a more immediate concern with our results is that the regressions are estimated on the sample of banks for which information on board composition is available, so that we can be sure whether the bank is or not connected as of December 2005. Of course, we cannot assume that banks for which this information is unavailable are unconnected. This problem raises concerns about sample selection and its likely effect on our estimates of the differences between connected and unconnected banks. We addressed these concerns by

[9] They also tend to hold more liquid assets, although not robustly significantly so.

modeling the selection process assuming that the probability of Bankscope reporting information on a bank's board composition depends on whether the bank is or has been listed and on the bank's size as measured by its assets (except for the size equations) and follow a standard Heckman (1979) two step estimation process. The results are qualitatively and quantitatively similar to those reported above.

3.3. Where Is the Phenomenon More Prevalent?

A. Quality of Institutions

In this section we aggregate the data by computing the actual to expected number of matches in each country under the assumption that the pool where both the politicians and bank board members are extracted from is the entire country population. We then relate this variable to several measures of a country's institutional quality by estimating the parameters of the following specification:

$$PREVALENCE_c = \alpha + \beta INSTITUTIONS_c + \gamma' X_c + \varepsilon_c \quad (3)$$

where $PREVALENCE_c$ is the log of the actual to expected number of matches of bankers-politicians found in country *c*, previously reported in the ninth column of Table 1, $INSTITUTIONS_c$ is a measure of a country's institutional quality (described below), and X_c is a set of control variables that includes a country's population and GDP level. We define our benchmark measure of prevalence using the log to account for potential non-linearities and smooth the impact of outliers, which is especially important considering the extremely high ratios obtained for some of the measures. However, at the same time, this transformation automatically removes the countries with zero matches from the estimation. We consider this to be an advantage of the measure, because from an empirical point of view, it is unclear how these observations should be treated. There are two reasons for excluding the cases with zero matches. First, many of the countries with zero matches have a very small number of politicians and bankers in the sample. Thus, the expected number of matches is effectively zero even when considering a very small pool of individuals (one percent of population with tertiary education). Our ratio of actual to expected matches is a poor measure of prevalence in those cases. Second, the cases with zero matches encompass two extremely different types of countries: (i) those that actually do not exhibit any intertwining of bankers and politicians, and (ii) those where none of the connected bank reported information on board composition to Bankscope. In other words, we cannot separate true from false zeroes. Although this potential mixing of true ratios and those resulting from misreporting is present at all levels, the econometric consequences are potentially

more severe for extreme values of the distribution of prevalence (i.e zero). Nevertheless, we will also check our results to the inclusion of the zeroes and show that they remain largely unaffected.

By correlating the prevalence of the phenomenon with cross-country measures of institutional quality, we find that it is significantly more prevalent in countries where institutions that limit the powers of the government vis-à-vis the citizens, and where institutions that prevent corruption are less developed (even after controlling for GDP per capita and population size, see Table 4, columns one and two). The power of financial regulators is also positively correlated with the prevalence of banker-politicians (Column (3)). The relation is not driven by a few outliers but seems a robust feature of the data (see both panels of Figure 1). Furthermore, the result stands pretty much unchanged when one considers just the private banks when measuring *PREVALENCE* (columns (4) to (6)).

The economic effect is large: the difference in the measure of corruption between the UK and Russia is commensurate to their difference in terms of the prevalence of the banker-politician arrangement. Although one cannot rule out that this strong cross-country correlation is not causal, at least it is clear that the phenomenon does not appear to be neutral but rather to be associated to other bad outcomes traditionally related with institutional weakness.

As mentioned above, our baseline measure of prevalence disregards the zeroes by construction. The correlations reported above are, therefore, conditional on exhibiting a non-empty intersection between the sets of politicians and bankers, and are akin to those obtained in a truncated regression. Nevertheless, results obtained including countries with a ratio of actual to expected matches equal to zero are qualitatively similar to those reported above (unreported).

B. Financial Regulation

Here we search for a link between banking sector regulation and the prevalence of the banker-politician phenomenon. The private interest story, after all, depends critically on both parties having something to win from their exchange. Regulation that favors incumbents in the banking system is the obvious service that politicians have to exchange for a seat on a bank's board.

Caprio et al (2007) provide data on the way countries regulate their financial systems on five dimensions: restrictions to bank activities, entry regulation, supervisory powers, private monitoring and self-regulation, and capital requirements. Each of these broad ways of regulating is assigned a score based on answers to surveys conducted to regulators in each country in 2001 and 2007.

We built three different measures of the pro-banker character of regulation across countries to address two problems: data are not available on all measures for all countries, and some of the categories can more easily be thought as pro or anti banking system incumbents, while for others the distinction is less clear. We deem the stringency of entry requirements and the extent of restrictions on activities as being more clearly pro and anti-banker respectively. Weather giving responsibility for the supervision and monitoring functions to the public or private sector is more debatable. However, since private monitoring includes monitoring by other firms that may be associated in other ways to banks and even banks themselves, we believe private monitoring and stronger public supervision are pro and anti-banker respectively. As for the size and characteristics of capital required, the effect can go either way. On one hand, capital is more readily available to incumbents than to potential entrants. But on the other hand, more capital means less profitability. We think, however, that since there are more direct ways of restricting entry to the industry, the effect of higher capital requirements is on balance anti-banker.

Our first measure is simply the value of the entry requirement index minus the value of the restriction on activities variable, so a higher value means that the country has a more pro-banker regulation. This first measure is not only the less debatable but it also turns out to yield the largest number of observations for the test (45). For the second measure, we add private monitoring and subtract supervisory powers to the first measure, losing 12 observations. The third measure subtracts the capital requirements index, and makes us lose 3 more data points.

The specification of our test is as follows:

$$Y_c = \alpha + \beta PREVALENCE_c + \gamma X_c + \varepsilon_c \qquad (3)$$

where Y_c is the change of the pro-banker regulation index between 2003 and 2007 in country c, and $PREVALENCE_c$ is the log of actual to expected number of bankers and politicians matches in country c as defined above. In addition to the log per capita GDP and log population, the set of controls X_c include the initial value (2001) of the pro-banker regulation index to capture mean reversion effects.

Table 5 presents the results, with different columns reporting our findings for different measures of pro-incumbent regulation. The coefficient of PREVALENCE is significantly positive meaning that it is indeed the case that countries where the banker-politician phenomenon is more prevalent banking regulation becomes more biased toward benefiting bankers. Figure 2 shows that the relation is indeed a robust feature of the data. The economic magnitude of the effect is quite large: moving from the 10^{th} to the 90^{th} percentile of prevalence is associated with climbing 8

positions in the 45-country pro-banker index, an increase roughly commensurate to the difference between the index in Russia and France.

The results are robust to changing the definition of the pro-banker regulation index. The coefficients change little and remain significant when they are estimated using the Ordered Probit model to account for the fact that the dependent variable is ordinal (unreported). The last column shows that, albeit a bit weaker because of the reduction in sample size, the qualitative results are very similar when prevalence is measured over private banks only. The effect is not entirely due to the higher incidence of state-owned banks in some countries. While the statistical significance is sometimes lost, the qualitative results and economic magnitude are also robust to changes in the sample and the specification.

The lack of universal information on board composition across banks and the potential sample selection issues associated with it previously discussed (section 3.c) can also affect the cross-sectional regressions reported above. However, the manner in which sample selection at the micro level can potentially bias results based on aggregate data is quite different and more complex. If banks that are connected are less likely to report their board composition the measure of prevalence built using reported data might contain non-classical measurement error (measurement error that is correlated with the measure itself). The potential bias arising from this type of error depends on the sign of the correlation between the true value and the measurement error, and the strength of this correlation relative to the variance of the measure itself and to the variance of the error. If the measurement error is positively correlated with the true variable, the non-classical measurement error still results in attenuation bias. If the correlation is negative, the magnitude and sign of the bias will depend on the strength of this correlation relative to the variance of the true variable and to the variance of the measurement error. In our case, the sign and strength of the correlation depends on whether connected banks are more or less likely to report director data in countries with higher prevalence. From an economic standpoint this is unclear, and depends on whether the relation between the probability of reporting and the degree of connectedness is based on an absolute threshold or a relative one. The fact that we do observe connected banks reporting data in countries with high degrees of prevalence suggests that a relative criterion is more likely and that the correlation is probably not so strong.

There are few econometric tools currently available to deal with the issue of non-classical measurement errors, most of which require having a second measure or an instrument, none of which are available in our case. For this reason, we instead follow Hu (2006), who proposes an approach that, under some assumptions, yields lower and upper bounds for the value of the true parameter in a regression with non-classical measurement error. The lower and upper bounds for

the true parameter estimated in this way are reported at the bottom of each of the columns of Table 5. In all cases both bounds have the same sign of the estimated coefficient and, although the bounds are not tight, the baseline parameter lies close to the lower bound for its magnitude, indicating that is most likely underestimating the true degree of correlation between our measure of prevalence and the extent of pro-incumbent regulation.

In this way, in countries where there is more connection between politicians and bankers, bankers are (to a greater extent) allowed to conduct more activities, to be monitored by private firms or themselves instead of being more heavily regulated by the government, are required to have less capital, and are granted higher barriers of entry. This piece of evidence, then, supports the private interest-capture view in the sense that, at least, it seems to be the case that bank owners would be getting a sweet deal in exchange for just a seat on the board.

C. Rents and Rent Allocation

Private interest stories differ markedly from public interest ones on the issue of rents. Higher rents where the banker-politician arrangement is more prevalent would be *prima-face* evidence that at least some private interest component is at work. Who, bankers or politicians, then gets the rent determines the flavor of private interest theory: tollbooth or capture.

We define total rents as net interest revenue to assets, and the share of rents accruing to bank owners as the return on assets (net income to assets) to total rents. The complement of that share is how we measure what goes to politicians under the assumption that what they gain is somehow accounted in the books as costs to the bank (over employment, higher salaries, taxes, etc). We measure these using *Bankscope* data for each bank, and then aggregate by taking the mean value for each country and year, and then average out over the years for each country. In what follows we regress the different measures on *PREVALENCE* and controls as in (3). Results are reported in Table 6.

We first check whether a higher prevalence of the phenomenon is associated with higher rents in the banking system. The idea being that, no matter who gets the rents it is going to be interested in making them grow. The evidence here is inconclusive. *PREVALENCE* and total rents are indeed positively and significantly correlated, both when all the banks are considered and when prevalence is measured only on private banks (columns two and three). Furthermore, the effect is large: increasing *PREVALENCE* from the 10^{th} to the 90^{th} percentile is associated with a 40% increase in total rents (63% for private banks only). However, the effect and its significance drop when controlling for per capita GDP and total population.

What seems quite robust is, on the other hand, the fact that even in the presence of our usual controls the percentage of total rents accruing to bankers is significantly and positively associated to the prevalence of the banker-politician phenomenon. The economic effect is quite large: going from the 10^{th} to the 90^{th} percentile of *PREVALENCE* would approximately double the share of rents accruing to bankers. Figure 3 shows this graphically.

As it was the case above, taking into account the possibility of non-classical measurement error resulting from sample selection at the micro level as in Hu (2006) yields bounds for the parameter of interest that, across specifications, always include and maintain the sign of the OLS coefficient, which again typically lies in the lower part of the range spanned by these bounds. Sample selection at the micro level does not seem to be importantly driving the qualitative results.

This piece of evidence, then, supports the capture story version of the private interest story. Even if it is not clear that these arrangements are able to produce larger rents, the way rents are divided between profit accruing to bankers and costs more likely to benefit politicians is far from independent of the presence of politicians on the board of banks. In places where the banker-politician phenomenon is more prevalent, whatever rents are created are much more likely to accrue to bank owners than to politician constituencies.

D. Financial Development

The evidence above suggests that the prevalence of the banker-politician phenomenon does have sizable effects on the way the banking sector operates and is regulated. Insofar as these differences have no impact on the efficiency with which the system works, the issue would not be of any public interest, and it would just be a matter of different preferences across countries. The case is different if the prevalence of banker-politicians hinders the ability of the system to efficiently allocate funds. Here we test whether the phenomenon is related to the degree of development of the banking system. The specification is the same as in (3), with Y_c being now each country's ratio of bank credit to the private sector to GDP. The controls include, as before per capita GDP and population.

The results are presented in Table 7. The coefficient of *PREVALENCE* is negative and significant. The phenomenon is indeed associated with lower degree of banking sector development. The effect is large in economic terms: moving from the 10^{th} to the 90^{th} percentile of prevalence is associated with a ratio of private credit to GDP 46 percentage points higher, an increase roughly commensurate to the difference between Brazil and Belgium. Figure 4 illustrates this relation and shows that a few outliers do not drive it.

The politician-banker phenomenon has explanatory power beyond the traditional measures used to explain financial development across countries such as the degree of protection granted to creditors (Column (2)), the quality of accounting practices (Column (3)), and investment opportunities measured with the decade's effective GDP growth rate (Column (4)). Both creditor rights and accounting quality enter positively as expected (although not significantly). When a creditor right is instrumented with the origin of the legal systems it recovers its significance. The coefficient of investment opportunities is unexpectedly negative. However, when all three variables are considered jointly each of them enters with the expected sign, insignificantly though (Column (5)). In all cases, however, *PREVALENCE* remains significantly negative and of similar magnitude suggesting that the result is quite robust to changes in the specification and the omitted variable bias.

Finally, Column (6) shows that when prevalence is defined over private banks only, the results are barely changed. This again suggests that is it is not just that countries with lower banking system development are more likely to have a higher fraction of the system in government's control.

As in all previous cases, the baseline parameter lies within the bounds reported at the bottom of the table to control for the possibility of having non-classical measurement error in our indicator of prevalence, which themselves are consistent with *PREVALENCE* having a negative impact on financial development. Qualitatively, sampling issues do not drive the results.

4. Conclusion: Public or Private Interest?

Political economy considerations are frequently mentioned as causes of cross-country differences in financial system development. Intuitively, these explanations are compelling; financial development is typically associated with dynamism in the allocation of credit across firms and with regulations that favor competition across providers of financial services, all of which result in the dissipation of rents that previously accrued to lucky or well connected industrial and financial incumbents.

This paper presented evidence of the political economy motives for financial underdevelopment by documenting the prevalence of the relation between politicians and bankers and correlating it with several of its potential determinants and likely consequences. We attempt to sort out private (tollbooth and regulatory capture) and public interest explanations for the existence of these relations by asking which alternative fits better the overall set of stylized facts that we document. In our view, the regulatory capture, private interest story better connects the

different pieces of evidence provided, especially the documented relation between the prevalence of banker-politicians, pro-incumbent regulations, and the fraction of rents accruing to bankers, but also other bits of evidence presented in the paper. Although some of the findings could in principle be explained by more than one view, only the regulatory capture one is able to provide a rationale consistent with almost all of them.

If the direction of causality actually goes in the way we conjecture in this paper, the connections between politicians and bankers result in regulatory barriers to entry and have real consequences for the development of the financial system. Imposing restrictions on this type of coalescence could therefore limit the ability of incumbent financiers to tilt regulations in their favor and restrict financial development. However, it is important not to take direct, partial equilibrium policy conclusions from this exercise. As long as there are rents from the restriction of entry into the financial sector, incumbent financiers will have incentives to impose or maintain them. Hiring politicians to act as board directors is only one manifestation of the struggle of these incumbents to lobby for their interests. Absent this avenue, these incentives could manifest themselves in a different way, such as outright bribes, that could be even more detrimental to the overall stability of the institutional framework.

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Appendix

A.1 Distribution of the number of matches under random draws.

Consider a population where there are N_P politicians and N_B bankers. The intersection of the two groups consists of N_{PB} banker-politicians. From the population of bankers and politicians we take two samples consecutively and with replacement at the sample level,[10] the first consisting of $n_B \leq N_B$ bankers and the second of $n_P \leq N_P$ politicians and match them. Let X be a random variable that counts the number of matches. This random variable will be distributed according to:

$$P(X=k) = \frac{\binom{N_{PB}}{k}\sum_{i=0}^{N_{PB}-k}\binom{N_{PB}-k}{i}\binom{N_B-N_{PB}}{n_B-k-i}\binom{N_P-k-i}{n_P-k}}{\binom{N_P}{n_P}\binom{N_B}{n_B}}$$

The denominator corresponds to the number of ways in which two samples of sizes n_P and n_B can be chosen from populations of sizes N_P and N_B respectively. The numerator has various components. The first term corresponds to the number of ways in which the k common elements can be chosen among the N_{PB} members of the intersection. The summation that follows counts the number of ways in which the remaining $n_P - k$ and $n_B - k$ terms can be chosen. The first term counts the manners in which i of those elements can be picked among the rest of the intersection. If i are chosen in this way, they can only be in one of the samples. For instance, assume that among the remaining $n_B - k$ components of n_B one also belongs to. N_{PB}. This one term can be chosen in $\binom{N_{PB}-k}{1}$ manners and the remaining $n_B - k$ that are only bankers can be chosen in $\binom{N_B-N_{PB}}{n_B-k-1}$ ways. Given that one of the terms in $n_B - k$ belongs to the intersection, it cannot be selected in the remaining $n_P - k$ draws from N_P, so we can choose those terms in $\binom{N_P-k-1}{n_P-k}$ only.

[10] This means that all individuals from the first sample are replaced in the population before taking the second sample, so that an individual from the intersection of the two samples can be drawn twice.

We use this distribution to estimate the expected number of matches in a country considering the actual size of the samples of bankers and politicians available from the data, which pin down n_P and n_B, and assuming that both are drawn from a common pool corresponding to a country's total population. In the notation above, the assumption of a common pool corresponds to assuming that $N_P = N_B = N_{PB}$.

Table 1. Prevalence of Politician-Banker Phenomenon

Country	Population	Population with tertiary education	Number of Politicians	Number of Bankers	Matches	Fraction of tertiary population for Proba matches >= 0.05	Actual / Expected Number of Matches	Actual / Expected Number of Matches (tertiary)	Prevalence (ln[actual / expected])	Prevalence (ln[actual / expected]) (tertiary)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Albania	3139	114	111	14	0	--	0	0.0	--	--
Algeria	30200	1107	88	76	0	--	0	0.0	--	--
Angola	12300	295	57	25	2	0.010	16667	4.1	9.72	1.42
Argentina	35500	4710	83	358	1	0.100	1190	1.6	7.08	0.46
Armenia	3151	92	80	31	1	0.500	1266	0.4	7.14	-0.99
Aruba	99	--	35	23	0	--	0	--	--	--
Australia	19100	4123	56	408	1	0.100	833	1.8	6.73	0.59
Austria	8015	907	50	940	3	0.050	512	0.6	6.24	-0.55
Azerbaijan	8002	237	51	9	0	--	0	0.0	--	--
Bangladesh	130000	2273	77	594	12	0.002	34286	6.0	10.44	1.78
Belarus	10000	426	81	115	5	0.009	5376	2.3	8.59	0.83
Belgium	10300	1470	50	619	5	0.009	1667	2.4	7.42	0.86
Benin	6156	53	69	51	0	--	0	0.0	--	--
Bolivia	8233	598	139	90	0	--	0	0.0	--	--
Botswana	1640	29	39	78	0	--	0	0.0	--	--
Brazil	169000	9258	110	506	4	0.003	12121	6.7	9.40	1.90
Brunei	331	--	24	14	0	--	0	--	--	--
Bulgaria	8097	402	24	55	0	--	0	0.0	--	--
Burkina Faso	11200	239	65	59	1	0.300	2941	0.6	7.99	-0.47
Burundi	6745	17	101	73	6	0.100	5505	0.1	8.61	-1.95
Cambodia	12500	318	50	32	0	--	0	0.0	--	--
Cameroon	14900	133	78	27	1	0.300	7143	0.6	8.87	-0.46
Canada	30700	12435	92	536	2	0.010	1242	5.0	7.12	1.62
Cayman Islands	42	--	28	8	0	--	0	--	--	--
Chile	15100	1542	69	202	3	0.009	3261	3.3	8.09	1.20
China	1250000	22586	47	495	2	0.002	100000	19.4	11.51	2.97
Colombia	41900	2353	103	153	2	0.010	5263	3.0	8.57	1.09
Costa Rica	3758	417	80	3	0	--	0	0.0	--	--
Cote d'Ivoire	15600	385	110	56	0	--	0	0.0	--	--
Croatia	4499	299	98	177	5	0.020	1295	0.9	7.17	-0.15
Cuba	11200	--	46	23	0	--	0	--	--	--
Cyprus	754	87	55	131	1	1.000	105	0.1	4.65	-2.11
Czech Republic	10300	1049	85	352	0	--	0	0.0	--	--
Denmark	5325	827	60	685	2	0.100	259	0.4	5.56	-0.91
Djibouti	653	17	41	6	0	--	0	0.0	--	--
Dominican Republic	8281	681	82	69	1	0.150	1471	1.2	7.29	0.19
Ecuador	12300	1262	170	3	0	--	0	0.0	--	--
Egypt, Arab Rep.	63400	4294	55	245	2	0.008	9524	6.4	9.16	1.85
El Salvador	6162	335	56	64	1	0.200	1724	0.9	7.45	-0.07
Estonia	1380	94	77	49	0	--	0	0.0	--	--
Ethiopia	63500	1295	40	57	0	--	0	0.0	--	--
Finland	5167	863	43	222	1	0.200	541	0.9	6.29	-0.10
France	58800	8130	76	3484	1	0.600	222	0.3	5.40	-1.18
Gabon	1242	60	51	42	4	0.020	2326	1.1	7.75	0.12
Gambia, The	1287	2	49	18	0	--	0	0.0	--	--
Georgia	5246	141	87	25	2	0.030	4878	1.3	8.49	0.26
Germany	82200	10776	60	9723	5	0.020	704	0.9	6.56	-0.08
Ghana	19400	104	100	166	0	--	0	0.0	--	--
Greece	10900	1092	74	211	0	--	0	0.0	--	--
Guatemala	11300	288	89	21	0	--	0	0.0	--	--
Guyana	757	17	38	43	0	--	0	0.0	--	--
Haiti	7885	32	97	35	0	--	0	0.0	--	--
Honduras	6379	198	88	31	0	--	0	0.0	--	--
Hong Kong, China	6607	811	49	745	5	0.010	904	1.1	6.81	0.11
Hungary	10100	861	84	173	3	0.010	2083	1.8	7.64	0.58
Iceland	279	30	25	90	2	0.150	248	0.3	5.52	-1.31
India	1010000	27622	85	1323	3	0.004	27273	7.4	10.21	2.00
Indonesia	205000	6396	105	466	1	0.100	4167	1.3	8.33	0.27
Iran, Islamic Rep.	63300	2346	56	71	0	--	0	0.0	--	--
Ireland	3799	528	51	447	0	--	0	0.0	--	--
Israel	6194	1076	100	173	1	0.300	358	0.6	5.88	-0.47
Italy	57600	6541	90	4968	5	0.030	644	0.7	6.47	-0.31
Jamaica	2570	53	27	152	0	--	0	0.0	--	--
Japan	127000	23225	122	2725	1	0.250	382	0.7	5.94	-0.36
Jordan	4815	620	143	88	2	0.050	766	1.0	6.64	-0.01
Kazakhstan	15300	612	73	22	0	--	0	0.0	--	--
Kenya	29700	167	93	276	0	--	0	0.0	--	--
Korea, Rep.	46700	8724	143	458	5	0.003	3571	6.7	8.18	1.90
Kuwait	2146	219	67	150	3	0.050	641	0.7	6.46	-0.42
Kyrgyz Republic	4865	117	75	2	0	--	0	0.0	--	--
Latvia	2394	144	84	176	3	0.100	486	0.3	6.19	-1.23
Lebanon	4291	170	74	223	2	0.250	519	0.2	6.25	-1.57
Lesotho	1734	8	51	18	0	--	0	0.0	--	--
Libya	5197	--	75	30	0	--	0	--	--	--

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Continuation

Country	Population	Population with tertiary education	Number of Politicians	Number of Bankers	Matches	Fraction of tertiary population for Proba matches < 0.05	Actual / Expected Number of Matches	Actual / Expected Number of Matches (tertiary)	Prevalence (ln[actual / expected])	Prevalence (ln[actual / expected]) (tertiary)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Liechtenstein	33	--	4	146	0	--	0	--	--	--
Lithuania	3525	227	93	36	1	0.250	1053	0.7	6.96	-0.39
Luxembourg	432	128	29	861	2	0.500	35	0.1	3.54	-2.27
Macao, China	429	52	24	104	0	--	0	0.0	--	--
Macedonia, FYR	2018	100	113	68	1	1.000	262	0.1	5.57	-2.04
Madagascar	15300	314	94	32	2	0.020	10000	2.1	9.21	0.74
Malawi	10200	27	76	65	0	--	0	0.0	--	--
Malaysia	23000	933	41	609	0	--	0	0.0	--	--
Mali	10700	23	82	43	0	--	0	0.0	--	--
Malta	389	18	40	41	2	0.200	475	0.2	6.16	-1.50
Mauritania	2605	15	103	41	1	1.000	617	0.0	6.43	-3.32
Mauritius	1179	20	46	88	0	--	0	0.0	--	--
Mexico	97400	6412	63	347	6	0.001	27273	17.6	10.21	2.87
Moldova	4282	100	96	56	2	0.100	1587	0.4	7.37	-0.99
Mongolia	2390	58	80	28	0	--	0	0.0	--	--
Morocco	28500	1013	73	131	4	0.006	11765	4.2	9.37	1.44
Mozambique	17500	15	59	46	0	--	0	0.0	--	--
Myanmar	47100	--	58	16	3	--	150000	--	11.92	--
Namibia	1855	89	50	70	0	--	0	0.0	--	--
Nepal	22800	307	109	104	1	0.700	2000	0.3	7.60	-1.31
Netherlands	15900	2569	50	601	3	0.010	1587	2.6	7.37	0.94
New Zealand	3858	1124	70	66	0	--	0	0.0	--	--
Niger	10600	22	106	13	0	--	0	0.0	--	--
Nigeria	125000	2571	128	401	2	0.050	4878	1.0	8.49	0.00
Norway	4472	788	80	341	1	0.650	164	0.3	5.10	-1.24
Oman	2389	205	53	60	1	0.250	752	0.6	6.62	-0.44
Pakistan	137000	1983	65	340	1	0.200	6250	0.9	8.74	-0.11
Panama	2830	342	64	59	0	--	0	0.0	--	--
Papua New Guinea	5069	36	91	23	0	--	0	0.0	--	--
Paraguay	5220	251	98	64	0	--	0	0.0	--	--
Peru	25700	3333	131	172	3	0.007	3409	4.4	8.13	1.49
Philippines	75700	9521	101	255	4	0.001	11765	14.8	9.37	2.69
Poland	38500	2989	100	404	1	0.250	952	0.7	6.86	-0.30
Portugal	10200	961	95	365	1	0.650	294	0.3	5.68	-1.28
Romania	22300	1085	136	149	3	0.020	3297	1.6	8.10	0.48
Russian Federation	146000	8015	108	805	8	0.002	13333	7.4	9.50	2.00
Rwanda	7386	18	71	42	2	0.400	5000	0.1	8.52	-2.08
Samoa	171	7	49	4	0	--	0	0.0	--	--
Saudi Arabia	20600	1886	52	211	0	--	0	0.0	--	--
Senegal	9392	107	79	99	0	--	0	0.0	--	--
Serbia and Montenegro	9986	--	112	135	2	--	1325	--	7.19	--
Sierra Leone	4979	22	132	27	2	0.400	2778	0.1	7.93	-2.07
Singapore	3976	356	33	337	1	0.550	357	0.3	5.88	-1.14
Slovak Republic	5383	426	75	192	0	--	0	0.0	--	--
Slovenia	1990	218	77	140	0	--	0	0.0	--	--
South Africa	43000	2223	52	591	3	0.010	4225	2.2	8.35	0.77
Spain	40300	4924	76	1836	1	0.500	289	0.4	5.67	-1.04
Sri Lanka	18300	339	61	85	0	--	0	0.0	--	--
Sudan	31200	305	72	144	2	0.050	6061	0.6	8.71	-0.53
Suriname	423	19	59	14	0	--	0	0.0	--	--
Swaziland	1022	16	50	25	0	--	0	0.0	--	--
Sweden	8886	1474	48	436	0	--	0	0.0	--	--
Switzerland	7188	870	21	2917	1	1.000	117	0.1	4.77	-1.95
Syrian Arab Republic	16000	1094	93	6	0	--	0	0.0	--	--
Tanzania	33200	622	40	104	0	--	0	0.0	--	--
Thailand	60500	4694	104	344	2	0.010	3390	2.6	8.13	0.96
Togo	4434	45	74	58	0	--	0	0.0	--	--
Trinidad and Tobago	1288	29	58	87	0	--	0	0.0	--	--
Tunisia	9496	350	72	131	4	0.010	4040	1.5	8.30	0.40
Turkey	66800	3501	163	546	3	0.020	2256	1.2	7.72	0.17
Uganda	23000	82	60	94	1	1.000	4000	0.1	8.29	-1.92
Ukraine	49700	1905	103	53	0	--	0	0.0	--	--
United Arab Emirates	3105	180	40	229	7	0.010	2373	1.4	7.77	0.32
United Kingdom	58800	8133	63	2814	3	0.020	993	1.4	6.90	0.32
United States	280000	100081	86	9145	7	0.002	2491	8.9	7.82	2.19
Uruguay	3312	271	69	213	0	--	0	0.0	--	--
Uzbekistan	24500	592	77	38	2	0.010	16667	4.1	9.72	1.40
Venezuela, RB	24100	2393	105	157	0	--	0	0.0	--	--
Vietnam	77900	2120	52	107	0	--	0	0.0	--	--
Yemen, Rep.	17400	355	73	24	1	0.050	10000	2.0	9.21	0.71
Zambia	9751	74	78	79	2	0.200	3175	0.2	8.06	-1.42
Zimbabwe	12500	246	55	159	0	--	0	0.0	--	--
Mean	39360	2621	73.6	428.4	1.4	0.196	3984.0	1.3	7.4	-0.129
Mean (non zero matches)			44	389	3		7321	2		

Columns (1) and (2) report each country's overall population and the population above 25 years of age with tertiary education (in thousands). Data on tertiary education comes from the UN. Columns (3) to (5) show for each country the number of politicians obtained from EIU (various issues) and How Countries Supervise (various issues), the number of directors reported in Bankscope (2005), and the number of matches between bankers and politicians found in the data. Column (6) reports the size of the pool (as a fraction of the population with tertiary education) from which the number of politicians reported in Column (3) and bankers reported in Column (4) would have to be drawn to obtain the number of matches reported in Column (5) with a probability of five percent, assuming that both samples are randomly drawn. Columns (7) and (8) report the ratio of the actual number of matches (Column (5)) to the expected number of matches that would be randomly obtained drawing the politicians and bankers randomly from the whole population (Column (7)) and one percent of the population with tertiary education (Column (8)). Columns (9) and (10) report the measures of prevalence used in the paper that correspond to the log of the ratios reported in columns (7) and (8), respectively.

Table 2. What Politicians Become Bankers?

	coef		se
A. Cabinet			
Finance and Economics	0.021	***	(0.003)
Labor-related	0.002		(0.005)
Politics	-0.005	**	(0.002)
Public Services	-0.006	***	(0.002)
Regulated Sectors	-0.002		(0.003)
Banking Supervisor	-0.010		(0.017)
Insurance Supervisor	0.005		(0.017)
Securities Supervisor	-0.008		(0.014)
Central Banker	0.045	***	(0.011)
Other	-0.005	*	(0.003)
B. Platform			
Right	0.011	*	(0.007)
Center	-0.015		(0.012)
Left	0.025		(0.026)
Same R,C,L as when Politician	-0.016		(0.016)

The table presents the results of testing whether the characteristic in each of the rows matters for whether a politician is also a banker or not. The test is based on regressions of an indicator variable that takes a value of 1 if the politician is also a banker (and zero otherwise) against an indicator of whether the characteristic is met or not. Country fixed effects are included to focus on the within country variation. Politicians where grouped into the first five categories in Panel A based on keywords contained in the name of the position. The bottom categories are based on the source of the data. Data from Polity IV were used to assign the characteristics in Panel B to each politician. The first column presents the coefficient, the second its significance, and the third the robust standard error. * significant at 10%, ** 5%, *** 1%.

Table 3. Connected Banks Are Different

	Total Assets	Return On Avg Assets (ROAA)	Equity / Tot Assets	NCO / Average Gross Loans
	(1)	(2)	(3)	(4)
A. All Banks				
Connected	0.3358**	0.0062**	0.0225**	-0.0054**
	(0.1349)	(0.0025)	(0.0105)	(0.0023)
Obs	3312	3285	3311	1176
R2	0.635	0.150	0.329	0.294
B. Private Banks				
Connected	0.3131*	0.0079**	0.0284***	-0.0050*
	(0.1600)	(0.0031)	(0.0108)	(0.0026)
Obs	2845	2819	2845	1016
R2	0.611	0.145	0.324	0.239

The dependent variable in each regression is reported at the top of each column. All dependent variables are in logs, those corresponding to rations that can take negative values are measured as the log of one plus the corresponding ratio. *Connected* is a dummy variable that takes the value 0ne if a bank has at least one former politician among its board members and zero otherwise. All regressions included a country fixed effect, and all regressions, except the one reported in Colum (1) also control for the (log) total assets. Robust standard errors in parentheses. * significant at 10%, ** 5%, *** 1%.

Table 4. Prevalence and Institutions

	All Banks			Private Banks Only		
	(1)	(2)	(3)	(4)	(5)	(6)
Control of Corruption	-0.566***	--	--	-0.406**	--	--
	(0.155)			(0.185)		
Voice & Accountability	--	-0.637***	--	--	-0.516***	--
		(0.139)			(0.148)	
Regulator Staff	--	--	0.0212**	--	--	0.0282***
			(0.00845)			(0.00862)
Ln(GDP pc)	0.454***	0.470***	0.394***	0.397***	0.439***	0.316***
	(0.0685)	(0.0662)	(0.0950)	(0.0727)	(0.0689)	(0.0713)
Ln (Population)	-0.232	-0.276**	-0.961***	-0.342**	-0.346***	-0.966***
	(0.141)	(0.112)	(0.234)	(0.162)	(0.108)	(0.262)
Constant	2.261	2.322	9.279***	3.808**	3.091**	10.26***
	(1.689)	(1.597)	(3.233)	(1.600)	(1.544)	(3.098)
Number of Obs.	78	78	39	63	63	33
R-square	0.675	0.694	0.627	0.619	0.646	0.627

The dependent variable in each regression is the measure of *Prevalence* which corresponds to the log of the ratio of actual to expected matches between politicians and bank directors found in each country. *Control of Corruptio*n is an inverse measure of the extent to which a politicians engage in corrupt activities in a country. The measure was obtained from Kaufman and Kraay (2004) and is the average of the biannual values reported for the period 1996-2002. *Voice and Accountability* measures the extent to which a country's citizens are able to participate in selecting their government and was also obtained from Kaufman and Kraay (2004) and computed similarly. *Regulator Staff* measures the number of staff working for the financial regulator per million people in each country and was obtained from Howell & Roe (2009). Ln(GDP pc) and Ln(Population) are each country's 1995-2005 average log GDP per capita and population, both from the World Bank (2008) World Development Indicators. Robust standard errors in parentheses. * significant at 10%, ** 5%, *** 1%.

Table 5. Prevalence and Financial Regulation

	(1) Index 1	(2) Index 2	(2) Index 3	(4) Index 1
Prevalence	0.841 **	1.267 *	1.799 *	--
	(0.391)	(0.629)	(0.937)	
Initial Pro-Banker Regulation	-0.384 **	-0.425 **	-0.364 *	-0.421 *
	(0.149)	(0.198)	(0.177)	(0.193)
Ln(GDP pc)	0.946	1.772 **	2.556 *	1.958
	0.601	(0.851)	(1.392)	(1.280)
Ln (Population)	-0.302	-0.218	-0.220	0.052
	(0.190)	(0.368)	(0.556)	(0.612)
Prevalence Private Banks Only	--	--	--	1.514
				(0.897)
Constant	-11.18	-23.29 **	-25.08	-20.26
	(7.690)	(9.887)	(15.424)	(14.419)
Number of Obs.	45	32	29	26
R-square	0.27	0.29	0.27	0.30
Bounds for Prevalence Coefficient				
Lower	0.633	0.954	1.354	1.131
Upper	5.486	6.234	8.752	7.567

The dependent variables are the percentage change in three different indexes of the extent of how pro-incumbent is financial regulation in various countries during the period 2001-07. These are based on a number of subindices constructed by Caprio et al (2007). Index 1 adds the value of the entry requirement subindex and subtracts the value of the restriction on activities subindex. Index 2 adds private monitoring and subtracts supervisory powers to the first measure. Index 3 further subtracts the capital requirement stringency subindex. *Prevalence,* is the log of the ratio of actual to expected matches between politicians and bank directors found in each country. Initial Pro-Banker Regulation is the value of each of the indexes in 2001. Ln(GDP pc) and Ln(Population) are each country's 1995-2005 average log GDP per capita and population, both from the World Bank (2008) World Development Indicators. The last two rows of each column report the lower and upper bounds estimated for the coefficient of *Prevalence* under the possibility of non-classical measurement error following Hu (2006). Robust standard errors in parentheses. * significant at 10%, ** 5%, *** 1%.

Table 6. Prevalence and Rents

Dependent Variable:	Total Rents			Rents to Bankers		
	(1)	(2)	(3)	(4)	(5)	(6)
Prevalence	0.151	0.439 **	--	0.146 *	0.157 *	--
	(0.288)	(0.205)		(0.082)	(0.086)	
Prevalence Private Banks Only	--	--	0.684 ***	--	--	0.225 *
			(0.235)			(0.117)
Ln (Population)	-0.380 *	--	--	0.007	-0.021	-0.015
	(0.207)			(0.100)	(0.091)	(0.110)
Total Rents	--	--	--	--	-0.074 *	-0.094
					(0.044)	(0.058)
Constant	20.14 ***	1.381	-0.183	-2.575	-1.087	-1.61
	(4.105)	(1.444)	(1.526)	(1.786)	(1.111)	(1.388)
Number of Obs.	77	78	63	77	77	62
R-square	0.273	0.056	0.114	0.04	0.077	0.105
Bounds for Prevalence Coefficient						
Lower	0.113	0.330	0.511	0.110	0.118	0.168
Upper	28.260	1.395	1.512	4.722	4.271	3.667

The dependent variables are based on bank financial ratios from the Bankscope dataset that are aggregated by taking the mean value for each country and year, and then averaging out over the years for each country. *Total Rents* corresponds to net interest revenue over assets, while the measure for the share of rents accruing to bank owners -*Rents to Bankers*- is the return on assets (net income to assets) to total rents. *Prevalence*, is the log of the ratio of actual to expected matches between politicians and bank directors found in each country. *Prevalence Private Banks Only* is the same variable constructed excluding all the government-owned banks in the sample. *Ln(Population)* is each country's 1995-2005 average population, from the World Bank (2008) World Development Indicators. The last two rows of each column report the lower and upper bounds estimated for the coefficient of *Prevalence* under the possibility of non-classical measurement error following Hu (2006). Robust standard errors in parentheses. * significant at 10%, ** 5%, *** 1%.

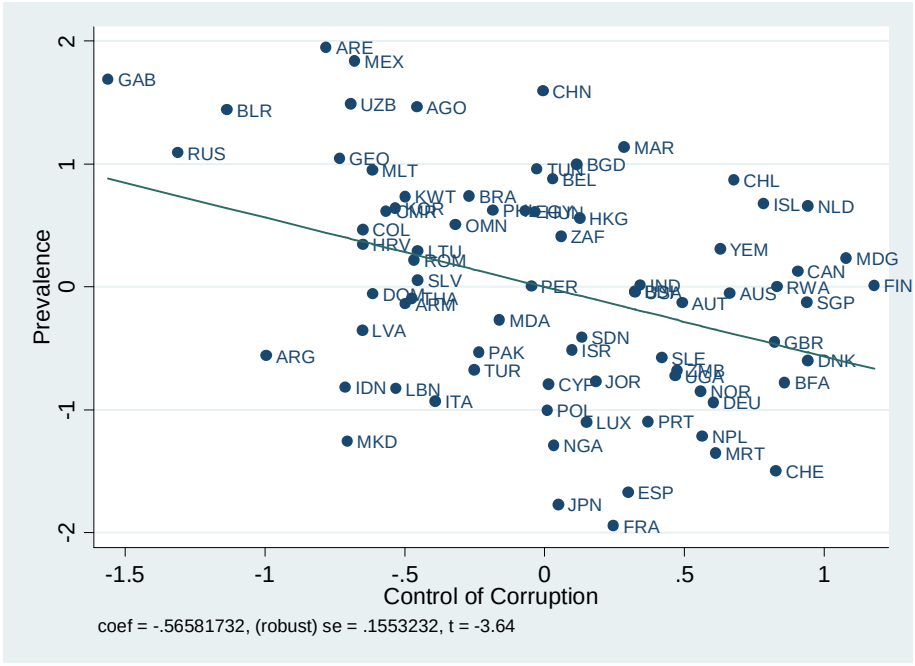
Table 7. Prevalence and Financial Development

	(1)	(2)	(3)	(4)	(5)	(6)
Prevalence	-0.122 ***	-0.115 ***	-0.126 ***	-0.114 ***	-0.111 **	
	(0.037)	(0.039)	(0.047)	(0.038)	(0.052)	
Ln(GDP pc)	0.210 ***	0.205 ***	0.223 ***	0.214 ***	0.216 **	0.213 ***
	(0.034)	(0.033)	(0.047)	(0.035)	(0.047)	(0.035)
Ln (Population)	0.080 ***	0.078 ***	0.086 ***	0.075 ***	0.083 ***	0.078 ***
	(0.027)	(0.028)	(0.032)	(0.028)	(0.032)	(0.028)
Creditor Rights		0.038			0.048	
		(0.033)			(0.036)	
Accounting Practices			0.019		0.028	
			(0.090)		(0.090)	
Investment Opportunities				-0.026 *	0.028	
				(0.015)	(0.090)	
Prevalence Private Banks Only						-0.133 ***
						(0.043)
Constant	-1.736 ***	-1.782 ***	-1.97 ***	-1.692 ***	-1.980 **	-1.641 ***
	(0.495)	(0.517)	(0.676)	(0.484)	(0.753)	(0.539)
Number of Obs.	70	70	59	70	59	58
R-square	0.621	0.63	0.59	0.635	0.592	0.654
Bounds for Prevalence Coefficient						
Lower	-0.092	-0.087	-0.095	-0.087	-0.083	-0.099
Upper	-0.592	-0.616	-0.696	-0.672	-0.798	-0.562

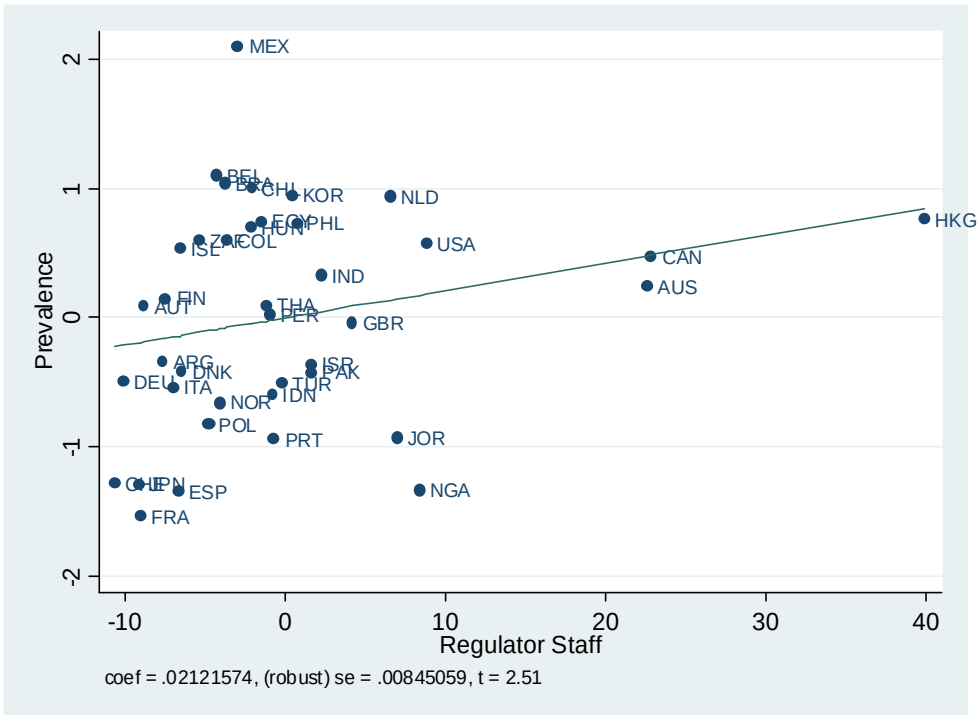
The dependent variable is each country's *Private Credit to GDP* averaged for the 1995-2005 period, from Beck et al (2000). *Prevalence*, is the log of the ratio of actual to expected matches between politicians and bank directors found in each country. *Prevalence Private Banks Only* is the same variable constructed excluding all the government-owned banks in the sample. *Ln(GDP pc)* and *Ln(Population)* are each country's 1995-2005 average log GDP per capita and population. *Investment Opportunities* is the growth rate of real GDP from 1995 to 2005. These last three variables are taken from the World Bank (2008) World Development Indicators. *Creditor Rights* is an index that measures legal protection granted to creditors in each country, it ranges from 0 (weak creditor rights) to 4 (strong creditor rights), and is from Djankov, McLiesh and Shleifer (2006). Acc*ounting Practices* measures whether a number of international accounting standard are used in each country, from La Porta et al (1998). The last two rows of each column report the lower and upper bounds estimated for the coefficient of *Prevalence* under the possibility of non-classical measurement error following Hu (2006). Robust standard errors in parentheses. * significant at 10%, ** 5%, *** 1%.

Figure 1. Country Characteristics

A.Control of Corruption and Prevalence of Banker-Politicians

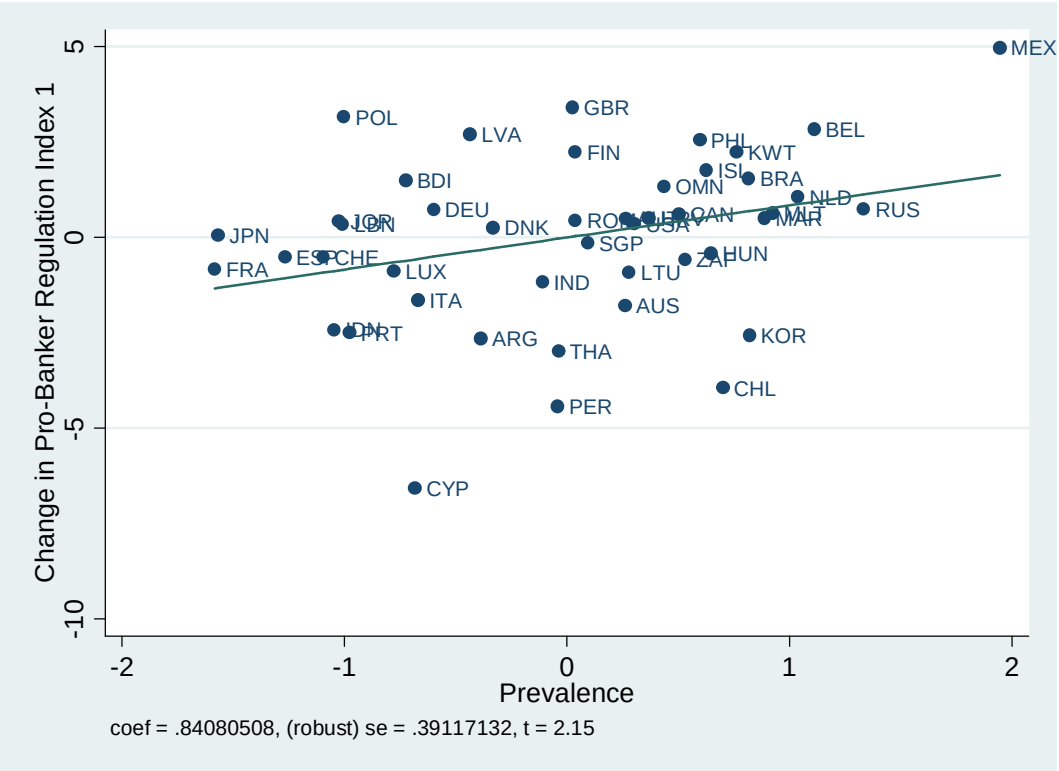


B. Strength of the Regulator and Prevalence of Banker-Politicians



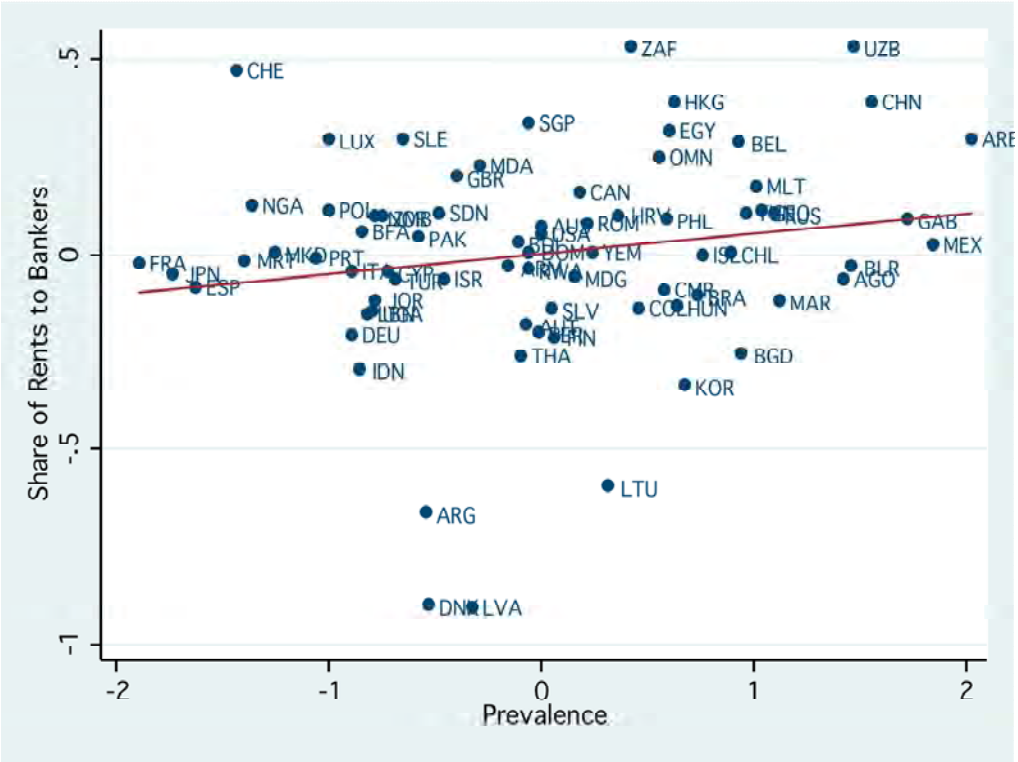
Panel A of the figure displays the relation between *Prevalence*, the log of the ratio of actual to expected matches between politicians and bank directors found in each country, (on the y-axis) and *Control of Corruption* (an inverse measure of the extent to which a politicians engage in corrupt activities in a country, obtained from Kaufman and Kraay (2004)), after controlling for a country's level of GDP and population. Panel B displays the relation between the same measure of *Prevalence* and *Regulator Staff,* which measures the number of staff working for the financial regulator per million people in each country, obtained from Howell & Roe (2009).

Figure 2. Financial Regulation



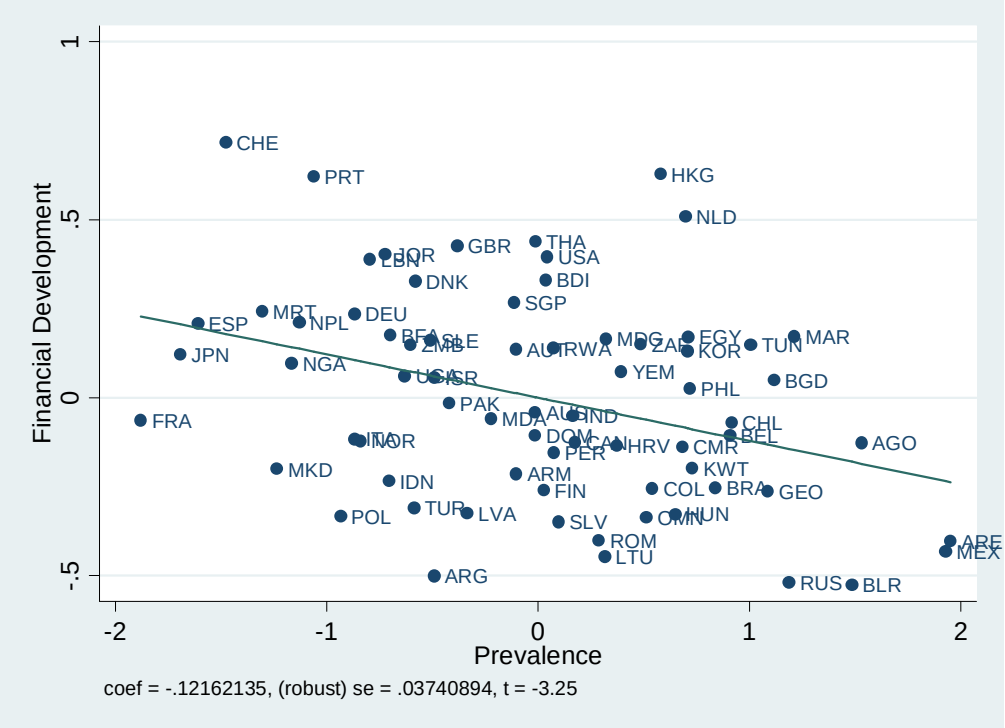
The figure displays the relation between *Prevalence*, the log of the ratio of actual to expected matches between politicians and bank directors found in each country, (on the x-axis) and the percentage change in an index of the extent of how pro-incumbent is financial regulation in various countries during the period 2001-07, after controlling for each country's level of GDP and population.. The index is based on a number of subindices constructed by Caprio et al (2007) and adds the value of the entry requirement subindex and subtracts the value of the restriction on activities subindex

Figure 3. Rents



The figure displays the relation between *Prevalence*, the log of the ratio of actual to expected matches between politicians and bank directors found in each country, (on the x-axis) and the *Share of Rents to Bankers*, which is the average of the return on assets (net income to assets) to total rents across all banks with information in Bankscope in each country, after controlling for each country's level of GDP and population.

Figure 4. Financial Development



The figure displays the relation between *Prevalence*, the log of the ratio of actual to expected matches between politicians and bank directors found in each country, (on the x-axis) and *Financial Development,* which is each country's Private Credit to GDP averaged for the 1995-2005 period, from Beck et al (2000), after controlling for each country's level of GDP and population.

Table A1. Summary Statistics Connected Banks

	All Banks		100% Private Banks	
	# Banks with Board Info.	# Connected Banks	# Banks with Board Info.	# Connected Banks
	(1)	(2)	(3)	(4)
Albania	2	0	1	0
Algeria	1	0	--	--
Andorra	2	0	2	0
Angola	1	1	--	--
Antigua And Barbuda	1	0	1	0
Argentina	75	1	57	0
Armenia	5	1	4	1
Aruba	1	0	1	0
Australia	36	1	25	0
Austria	60	3	57	3
Azerbaijan	3	0	2	0
Bahamas	3	0	3	0
Bahrain	10	0	9	0
Bangladesh	29	10	22	8
Barbados	1	0	1	0
Belarus	8	5	6	3
Belgium	39	3	37	3
Benin	3	0	3	0
Bermuda	3	0	3	0
Bolivia	4	0	4	0
Botswana	6	0	3	0
Brazil	35	3	24	2
Bulgaria	9	0	7	0
Burkina Faso	4	1	2	1
Burundi	2	2	1	1
Cambodia	2	0	1	0
Cameroon	1	0	1	0
Canada	22	2	17	2
Cape Verde	1	0	1	0
Cayman Islands	1	0	1	0
Chile	10	2	9	2
China·People'S Rep.	26	2	--	--
Colombia	10	2	6	0
Costa Rica	1	0	--	--
Croatia	17	2	14	0
Cuba	2	0	1	0
Cyprus	10	1	9	1
Czech Republic	25	0	23	0
Denmark	61	2	58	2
Dominican Republic	6	1	5	0
Ecuador	1	0	1	0
Egypt	24	2	11	1
El Salvador	4	1	3	0
Estonia	5	0	4	0
Ethiopia	5	0	4	0
Finland	13	0	9	0
France	177	2	167	2
Gabon	3	3	--	--
Gambia	2	0	2	0
Georgia Rep. Of	1	1	1	1
Germany	485	5	463	2
Ghana	17	0	14	0
Greece	15	0	13	0
Grenada	1	0	1	0
Guatemala	2	0	1	0
Guyana	3	0	2	0
Haiti	1	0	1	0
Honduras	2	0	2	0
Hong Kong	52	6	49	6
Hungary	12	3	9	3
Iceland	12	2	6	1
India	65	3	39	2
Indonesia	34	1	26	1
Iran	9	0	2	0
Ireland	41	0	36	0
Israel	10	1	6	1
Italy	237	3	213	2
Ivory Coast	3	0	2	0
Jamaica	8	0	5	0
Japan	146	1	138	1
Jordan	9	2	9	2
Kazakhstan	8	0	4	0
Kenya	21	0	17	0
Korea Rep. Of	27	4	17	2
Kuwait	11	3	7	3
Kyrgyzstan	1	0	1	0
Latvia	19	2	15	1
Lebanon	12	2	12	2
Lesotho	2	0	2	0

Continues Next Page

Continuation

	All Banks		100% Private Banks	
	# Banks with Board Info.	# Connected Banks	# Banks with Board Info.	# Connected Banks
	(1)	(2)	(3)	(4)
Libyan Arab Jamahiriya	2	0	--	--
Liechtenstein	9	0	8	0
Lithuania	7	1	7	1
Luxembourg	63	2	60	1
Macau	7	0	7	0
Macedonia (Fyrom)	7	1	5	1
Madagascar	3	2	1	1
Malawi	4	0	3	0
Malaysia	49	0	42	0
Maldives	1	0	--	--
Mali	2	0	1	0
Malta	4	2	2	0
Mauritania	1	0	1	0
Mauritius	7	0	2	0
Mexico	19	1	15	0
Moldova Rep. Of	5	1	3	1
Monaco	7	0	7	0
Mongolia	2	0	2	0
Morocco	7	3	4	3
Mozambique	4	0	3	0
Namibia	5	0	3	0
Nepal	10	1	9	1
Netherlands	44	3	34	2
Netherlands Antilles	4	0	3	0
New Zealand	7	0	6	0
Niger	2	0	2	0
Nigeria	16	1	14	0
Norway	35	1	25	0
Oman	3	0	1	0
Pakistan	26	1	15	0
Panama	5	0	3	0
Papua New Guinea	1	0	1	0
Paraguay	5	0	5	0
Peru	8	3	7	2
Philippines	8	1	7	1
Poland	31	1	24	1
Portugal	22	1	20	1
Qatar	7	3	6	2
Romania	15	1	10	0
Russian Federation	68	5	50	3
Rwanda	2	1	1	1
Saint Lucia	1	0	--	--
San Marino	2	0	2	0
Saudi Arabia	12	0	12	0
Senegal	7	0	3	0
Serbia And Montenegro	8	0	7	0
Seychelles	2	0	1	0
Sierra Leone	2	1	1	1
Singapore	17	0	15	0
Slovakia	17	0	10	0
Slovenia	13	0	10	0
South Africa	37	2	32	2
Spain	79	1	72	1
Sri Lanka	7	0	4	0
St. Kitts And Nevis	1	0	1	0
Sudan	4	2	3	2
Suriname	1	0	--	--
Swaziland	3	0	3	0
Sweden	32	0	21	0
Switzerland	184	1	159	1
Syria	1	0	--	--
Taiwan	31	4	19	1
Tanzania	8	0	5	0
Thailand	12	2	6	1
Togo	3	0	2	0
Trinidad And Tobago	4	0	4	0
Tunisia	11	3	6	0
Turkey	29	3	23	3
Uganda	12	1	12	1
Ukraine	7	0	7	0
United Arab Emirates	19	6	8	4
United Kingdom	228	4	210	4
Uruguay	11	0	9	0
Usa	446	6	421	4
Uzbekistan	1	1	1	1
Venezuela	5	0	5	0
Vietnam	10	0	6	0
Yemen	6	1	3	1
Zambia	8	2	6	2
Zimbabwe	10	0	8	0
Total	3863	168	3271	113

Columns (1) and (2) report the number of banks with directors information in Bankscope (2005) in each country, and the number of banks that had at least one politician in their board. Columns (3) and (4) present the same information for the subset of banks that are 100 percent privately owned.