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Unbundling the Effects of Reforms

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Abstract: This paper investigates the effects of financial and trade reforms on manufacturing output performance in a large sample of developed and developing countries. To identify the channels through which reforms affect economic performance and to address endogeneity concerns, we estimate differential effects of reforms across various industries. We find that financial and trade reforms impact manufacturing output in a way that is consistent with standard theories of finance and trade. Specifically, reforms of the financial sector improve the efficiency of intermediation by reallocating capital towards sectors that need it most, and contribute in improving countries' resilience to external shocks. Trade reforms foster output growth in export sectors that rely more intensively on imported intermediated goods. We also find that trade and financial sector reforms are more effective in countries with a better protection of property right. Our results are consistent across different empirical approaches.

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I. INTRODUCTION

Over the past decades, countries around the world have liberalized their financial sectors and reduced their barriers to international trade. Figure 1 illustrates the trend of reforms in these areas. It suggests that structural changes have taken place in most economies. What effects have these reforms had? Have they fostered economic growth?

The link between financial development and economic growth has been well documented (see Levine, 1997 and Levine, 2005, for surveys of this literature). The finance and growth literature has shown that well functioning financial systems, enabled by good contracting right institutions (La Porta et al., 1998,1999) and protection of property rights (Acemoglu and Johnson, 2007), matter both for macroeconomic and microeconomic performance. Similarly, the relationship between openness to trade and growth is also well established (see Berg and Krueger, 2003, and Edwards, 1993, for a discussion of the earlier literature).[2]

This paper exploits a new dataset of financial sector and trade reforms covering a sample of 91 countries over the period 1973-2005 to study the channels through which reforms affect economic growth. We also analyze the extent to which the effectiveness of financial and trade reforms depend on the institutional environment (in particular the protection of property rights). In spite of the substantial body of research on the link between growth and financial development or trade openness, relatively few studies have analyzed the relationship between economic performance and *reforms*, which is of more direct interest to policy makers. Existing studies focused on the impact of stock market liberalizations on growth (Bekaert, Campbell and Lundblad ,2007), investment (Henry, 2000), and on the relationship between economic and trade liberalization episodes and economic growth (Sachs and Warner, 1995, and Wacziarg and Welch, 2003).[3] [4]

A challenge faced by this literature is to ensure that the estimated econometric relationship is not biased by endogeneity problems resulting from omitted variables or reverse causality. The identification problem is all the more severe as recent studies have confirmed that the social, economic, legal and political organizations of a society (its broad "institutions") are primary determinants of economic performance (Acemoglu, Johnson and Robinson, 2001, 2002), as was first argued by North (1981). The key question is how to identify the effect of a specific policy or institutional change on economic performance, when many other institutional features of an economy are also potentially correlated with that policy or

[2] Rodriguez and Rodrik (2000) present a skeptical reading of the trade and growth literature.

[3] See also Dollar and Kraay (2004) for the link between average trade tariffs and economic growth.

[4] Giavazzi and Tabellini (2005) show that the effects of economic reforms are not independent of the political environment.

institution (Acemoglu, 2005). For instance, the degrees of liberalization of the financial sector or of trade are strongly correlated with each other, as well as with indicators of the institutional environment (Table 3). This points at the difficulty to identify the effects of these reforms. A general strategy has been to rely on time invariant factors to identify the effect of a specific policy or institution on economic performance, including the legal tradition inherited from colonizers as a determinant of contracting rights (La Porta et al., 1998, 1999), the geographic conditions faced by colonizers as a determinant of property rights (Acemoglu and Johnson, 2007), and geography as a determinant of trade openness (Frankel and Romer, 1999). Unfortunately, time invariant geographical and historical factors are unlikely to be good instruments for time varying economic reforms. In this paper, we suggest an instrumental variable strategy for the reform indices based on the hypothesis that reforms diffuse across countries.

Another strategy to identify the effect of a policy or institution on economic performance is to exploit the fact that specific policies or institutional arrangements have differential effects across sectors of an economy. This strategy was pioneered by Rajan and Zingales (1998) to identify the causal effect of financial development on economic growth, as well as the channels through which economic growth is affected.[5] We start from Rajan and Zingales' (1998) argument that financial development affects sectors differentially: sectors that, for technological reasons, depend more on external finance to grow, will benefit more from reforms that improve the efficiency of financial intermediation. The estimate of the differential effect of a financial sector reform will be unbiased as long as the likelihood of financial sector reforms does not increase when sectors more financially intensive experience higher growth opportunities. Given that property rights have been found to differentially affect sectors according to a different dimension (Claessens and Laeven, 2003, and Aghion, Alesina and Trebbi, 2007), such an approach should in principle allow to "unbundle" the effect of financial sector reforms from the impact of the broader institutional environment.

A similar logic can be applied to trade reforms to address endogeneity and identify the channels through which trade reform affect output performance. Trade theories have shown how trade enhances growth through the creation and import of new varieties (Romer, 1987) in which technical knowledge is embodied (Grossman and Helpman, 1990). These theories imply that a reduction in import tariffs should disproportionately benefit industries that, for technical reasons, depend more in intermediate traded inputs. The findings of recent micro-econometric and country studies confirm the importance of a trade channel going through imports of intermediate goods (Pavcnik, 2002, Edwards and Lawrence, 2006, Amiti and Konings, 2005, and Broda, Greenfield and Weinstein, 2006). Our hypothesis is that trade

[5] This approach has also been used to identify the effects of banking crises (Kroszner, Laeven and Klingebiel, 2007), the role of trade credit (Fisman and Love, 2003), and of entry regulations (Klapper, Laeven and Rajan, 2006).

reforms – such as reductions in import tariffs – should disproportionately foster growth in sectors in which a country has a comparative advantage and that, for technological reasons, depend on traded intermediate goods in the production process. For each 3 digit manufacturing sector, we construct a measure of intensity of the use of traded inputs, defined as the share of imported inputs in the total valued of intermediate inputs.

Using industry-level data for a large cross-section of countries, we first show that financial sector reforms have a positive differential effect on sectors that depend more on external finance to grow, and that trade reforms have a positive differential effect on sectors more intensive in traded intermediate inputs. These results are consistent with existing theories, and should correctly identify the differential effects of reforms under certain assumptions. A remaining concern, however, is that financial or trade reforms may be more likely to take place in countries that have already a certain production and trade structure in place. Indeed, trade reforms may affect the development of financial systems in a way reflecting the initial production structure (Do and Levchenko, 2008) and distribution of rents among existing industrialists (Braun and Raddatz, 2008). Conversely, trade patterns may also reflect the initial level of financial development (Antras and Caballero, 2007, Beck, 2002) or the institutional quality (Levchenko, 2007).

Reverse causality is less likely to be a concern when estimating the effects of reforms during episodes of external shocks.We focus on negative terms of trade shocks to assess the extent to which financial and trade reforms make an economy more resilient to shocks. We exploit the fact that negative terms of trade shocks are more likely to affect industries that rely more on imported inputs. We find that in countries with more repressed financial systems, terms of trade shocks have a relatively stronger effect on the output growth of sectors that depend more on intermediate imported inputs. This result is consistent with existing theories emphasizing the role of the financial frictions in propagating economic fluctuations (Bernanke and Gertler, 1989, Holmstrom and Tirole, 1997, Aghion et al., 2005) and the role of financial systems in the development process (Banerjee and Newman, 1991, Greenwood and Jovanovic, 1990).

The third approach to identify the impact of reforms is to estimate the effect of *changes* in the reform indices on *changes* in output growth of manufacturing industries. Given the relative stability of the property rights and contracting rights environment over time, this further reduces the concern that our results may be capturing the effects of the broader institutional environment rather than of financial and trade reforms. Last but not least, we develop an instrumental variable strategy based on the assumption that reforms diffuse across countries, by a process of imitation. We thus instrument *changes* in the reform indices by *changes* in the state of liberalization of neighboring countries. As an additional instrument, we also consider the occurrence of IMF programs. As explained in section III, the identifying assumptions are likely to be valid as long as reforms in neighboring countries or IMF programs do not occur when, for the country considered, industries with a higher dependence

on external finance (in the case of financial reforms) or with a higher use of imported inputs (in the case of trade reforms) experience *higher* growth opportunities.

The second broad set of results pertains to the importance of property rights. We show that the differential effects of reforms depend on the property right environment: financial reforms and trade reforms are more effective in countries in which private agents are better protected from expropriation from the State or powerful elites. This result holds across the three approaches summarized above. A potential explanation for these results is that, in countries with weak institutional environments, the protection of rents of powerful elites is a multi-dimensional process. The loss of one instrument of rent protection triggers an intensification of rent seeking activities along another dimension. This mechanism applied to structural reforms is similar to the argument of the "seesaw" effect proposed by Acemoglu, Johnson, Robinson and Thaicharoen (2002): if elites are prevented from using one particular instrument of expropriation, a likely outcome is that they will pursue their objective using other instruments.

The paper is organized as follows. Section II presents the methodology and the data. Section III presents the results of the empirical analysis. Section IV concludes.

II. METHODOLOGY AND DATA

A. Dependent variable and indices of reforms

Manufacturing sectoral data are from the 2006 UNIDO Industrial Statistics Database. We use the version that reports industrial production, in nominal and real terms, according to the 3-digit ISIC Revision 2 classification over the period 1963-2004, and that covers 28 manufacturing sectors. We look at the distribution of the growth rate of real output, to identify large and potentially implausible changes. We define outliers as observations in which the growth rate fell in the top 95 percentile or in the bottom 5 percentile of the distribution, and drop these observations. Next, to avoid breaks in the time series, we replace these missing observations by a simple linear interpolation of adjacent observations when they are available. Finally, we keep only countries for which at least 10 sectors are covered with at least 10 consecutive years of data.

The index of domestic financial reforms is from Abiad, Detragiache and Tressel (2008). It is a graded index varying between 0 and 3 that covers 91 countries over the period 1973-2005. A higher value of the index corresponds to a more reformed financial sector. The index is a simple average of the six following graded indices covering the following dimensions:

(i) Controls on credit (its allocation and aggregate expansion)
(ii) Controls on interest rates (deposit and lending rates);

(iii) Entry barriers and scope of activities in banking;
(iv) State ownership of banks (share of bank assets controlled by state-owned banks);
(v) Prudential regulations and supervision of the banking sector (independence and effectiveness);
(vi) Policies to encourage the development of securities markets.

We consider three indicators of trade reforms. The first indicator, which will be our main indicator of trade reforms, is an index based on a simple average of import tariffs, and is available for the same sample of countries and years as the financial reform dataset. The index varies between 0 and 1, with a higher value corresponding to lower import tariffs.[6] The second indicator of trade reforms is the liberalization date constructed by Wacziarg and Welch (2003).[7] The third indicator is the date of entry of a country into the WTO/GATT.[8]

Figure 1 shows the evolution of the financial reform index and the trade index over time.[9] It shows a broad liberalization trend over the past decades, for both trade and the financial sector.

(Figure 1)

B. Sectoral characteristics

To test the differential effect of financial reforms across sectors, we use the measure of dependence on external finance introduced by Rajan and Zingales (1998). They argue that the average proportion of external finance in capital expenditures of each industry in the U.S. is a reasonable proxy for each industry's need for external financing that would prevail if financial markets were perfect. Hence, it captures a technological dependence on external financing. It is computed on U.S. firms over the 1980s and is averaged over industries. We use the version of this external dependence ratio computed by Kroszner, Laeven and

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[6] The trade index is defined as: $TR\_Index_{it} = 1 - \frac{Average\_tariff_{it}}{Max\_tariff}$, where Max_tariff is the maximum average import tariff observed in the sample (60 percent).

[7] It is an extension of the liberalization variable initially developed by Sachs and Warner (1995), in which a country is classified as non-liberalized if it displayed at least of the following characteristics: (i) average tariff rates of at least 40 percent, (ii) non-tariff barriers covering at least 40 percent of trade, (iii) a black market exchanger rate depreciated by at least 20 percent relative to the official exchange rate, (iv) a state monopoly on major exports, (v) a socialist economic system.

[8] WTO membership may be an imperfect measure of trade policy, as argued by Rose (2002).

[9] The financial reform index has been normalized between 0 and 1.

Klingebiel (2007) which is computed over a longer period (1980-1999) than the original measure of Rajan and Zingales.[10]

Trade reforms will also have differential effects across sectors in each country. The argument is as follows. According to standard Ricardian theory, the impact of trade liberalization will not be homogenous across sectors. In particular, import competing sectors should contract following a liberalization of trade. In contrast, trade liberalization should be accompanied by an expansion of sectors in which a country has a comparative advantage. We will differentiate these two types of sectors in each country by looking at average net exports of each sector in each country over two decades (1980s and 1990s) and define as import competing sectors those in which net exports were negative and export sectors those in which net exports were positive.

Next, we define a sectoral measure of dependence on intermediate goods in the production process. Our argument is that different sectors have, for technological reasons, a different need for intermediate traded inputs. Such technological differences should allow us to identify the effect of a trade reform on output. Indeed, new trade theories suggest that a key benefit of trade reforms occurs via a reduction in the cost of intermediate inputs. As a result, a reduction in import tariffs – an implicit tax on exports -should disproportionately boost the output of sectors that, for technological reasons, rely more on a large variety of intermediate *traded* goods in production. This variable also captures how sensitive to terms of trade shocks an industry is.

Identifying the intermediate input channel of international trade may be complicated by the fact that we do not have direct measures of tariffs and other import restrictions on intermediate goods. However, as discussed in section III E, instrumental variable regressions may help address this measurement problem.

To proxy for such technological reliance on traded goods in the production process, we construct, for each country i and sector j, the variable $Inter\_good_{ij}$, defined as:

$$Inter\_good_{ij} = \frac{imported\_input_{ij}}{total\_input_{ij}}$$

where $imported\_input_{ij}$ is the total value of imported inputs in local currency for country i and sector j, and $total\_input_{ij}$ is the total value of all intermediate inputs of sector j and country i. To construct this measure, we use country specific input-output tables obtained from the GTAP 6 Data Base. Sectors in the GTAP 6 database are available according to ISIC Rev 3 classification, and therefore must be matched to the ISIC Rev 2 classification of

[10] We check that our main results hold when the original measure is instead used.

the UNIDO. Input-output tables are available typically for one year, for most countries of our sample. For the remaining countries for which input-output tables are not available, we use an average of the measure of neighboring countries. Finally, for each sector, this measure is averaged across countries by two groups of countries (advanced countries and other countries). Averaging over a large sample of countries allows to reduce the dependence of this measure on country specificities, and to better capture technological factors. But we also want to account for the fact that the structure and technologies of production may differ between industrialized countries and developing countries.

Finally, we consider a measure of sectoral sensitivity to the protection of property right. The measure is a Herfindhal index capturing the concentration of intermediate inputs. Following Blanchard and Kremer (1997) and Levchenko (2007), we hypothesize that sectors in which there is a greater complexity of the production process are relatively more likely to thrive in countries in which there is a good protection of property rights. The argument is that, when contracts are incomplete and with weak commitment mechanisms between suppliers and buyers, the production process is more likely to break down when it necessitates a diverse set of inputs. Conversely, when production is dominated by a few intermediate inputs, production of final goods really necessitates a good relationship only with a few suppliers, reducing the scope for expropriation in the production process. The Herfindhal index is constructed from the input-output tables of the GTAP database and is averaged across countries as done for the previous measure.[11]

C. Country level characteristics

The existing theoretical and empirical literature suggests a number of control variables.

In line with the finance and growth literature, we use the ratio of private credit to GDP as a proxy for financial development. If we find that the effect of financial reforms becomes insignificant when controlling for financial development, this would be consistent with the hypothesis that financial reforms affect the real sector by increasing the size and depth of the financial system. In contrast, if the effect of financial reforms remains significant, this would be consistent with the hypothesis that financial reforms affect output performance by allocating capital more efficiently across sectors and/or firms. Data on financial development are from the World Bank Financial Structure Database (2007). In some regressions, we will

[11] An alternative measure considered in robustness tests is to differentiate sectors according to the degree of intangibility of firms' assets, as in Claessens and Laeven (2003). The argument is that the risks of expropriation by the State or by powerful groups are more important for firms that have a larger share of intangible assets (for example, patents, copyrights, and trademarks are difficult to protect in weak property rights environments). The measure of intangibility is from Kroszner, Laeven and Klingebiel (2007).

also control for the *de facto* openness to trade, as measured by the ratio of exports plus imports to output of each sector.

The quality of contracting rights affects the development of financial systems and firms' access to external finance, as shown by La Porta et al. (1997,1998). The measure of protection of creditor rights – measuring laws on the books - is from Djankov, Mc Liesh and Shleifer (2007). To our knowledge, it is the only available measure of contracting rights that is available over a long period (1980-2003). As a measure of contract *enforcement*, we use the number of days to enforce contracts from the World Bank Business Environment Database which provides a set of measures of administrative and regulatory obstacles to business activity in a large cross section of countries. The drawback is that this variable is available for recent years only. However, to the extent that the quality of contracting rights does not vary significantly over time, this should not affect our identification strategy - we therefore assume that it is stable over time within countries. Finally, we also control for differences in legal traditions, which has been shown to be an important determinant of the contracting environment and of the depth of financial systems across countries (see Levine, 2005, for a survey). Indeed, La Porta et al. (1998) found that countries with a common law legal origin have deeper financial markets than other countries, while a civil law legal origin tends to be associated with weaker protection of investors and shallower financial markets.

Finally, the recent literature has shown that the quality of property rights has first order effects on the development of financial systems and on economic performance in general (Acemoglu and Johnson (2007)), while contracting rights matter only for the form of finance. We consider two measures of property rights. The first one is the index of constraint on the executive from Polity IV. It captures the rules of checks and balance that restrict the power of the executive, hence reflects how individuals are protected from expropriation risk from the State. The second measure is the index of civil liberties from Freedom House which captures a broader set of factors related to the protection of private property rights. Both measures are available over a long period and for a large sample of countries around the world.

D. Empirical Strategy

First, we estimate several versions of the following panel regression:

$$y_{ijt} = \alpha \cdot ref_{it-1} + \beta \cdot X_{ijt-1} + f_i + g_j + d_t + \varepsilon_{ijt} \qquad \textit{(specification 1)}$$

where y_{ijt} is the growth rate of real manufacturing output, ref_{ijt-1} is an index of reforms lagged by one period, X_{ijt-1} is a vector of control variables, f_i is a country fixed effect, g_j an industry fixed effect, d_t a time fixed effect and ε_{ijt} is the error term. Observations are clustered by country-year to correct standard errors for the downward bias that results from the fact that reforms are country level variables while the dependent variable is defined at the

sectoral level. Country and industry fixed effects control for country and industry time invariant factors that affect the performance of manufacturing sector. Year fixed effects control for global trends in manufacturing performance or in the reform process.

An obvious problem is that reforms are likely to be endogenous – because of omitted variables and reverse causality – and the estimated parameter α is likely to be biased. To get around endogeneity bias, the literature has exploited the fact that a given country level variable could have a differential effect across manufacturing sectors, and that the differential effect itself is less likely to be biased. Estimating a differential effect allows to include a full set of country-year fixed effects, which control for all possible sources of country level omitted variables. It also permits to reduce reverse causality concerns. For example, if reforms are implemented when policy makers anticipate an improvement in overall economic performance, but do not favor specific manufacturing sectors more than others, the estimate of the *differential* effect of the reform should be unbiased (see identifying assumption below). The fact that each sectors accounts for only a small share of total manufacturing output on average (see Table 2), suggests that such an assumption may indeed be reasonable.

Accordingly, to estimate differential effects of reforms across sectors, the econometric specification becomes:

$$y_{ijt} = \delta \cdot Char_j \cdot ref_{it-1} + \beta \cdot X_{ijt-1} + f_{it} + g_j + \varepsilon_{ijt} \quad \textit{(specification 2)}$$

where the dependent and independent variables are defined as before, $Char_j$ is a sectoral characteristic (more on this below), f_{it} is a country-year fixed effect, and g_j, d_t and ε_{ijt} are defined as before. The identifying assumption is :

$$E\left(Char_j \cdot ref_{it-1}, \varepsilon_{ijt} \middle| X_{ijt-1}, f_{ij}, g_j\right) = 0$$

We also estimate a variant of the difference-in-difference specification in which we allow for heterogeneity of the coefficient δ across country and over time: $\delta = \delta_{it}$.

Another empirical strategy to identify the effects of reforms on output performance is to conduct an event study around the date of a reasonably exogenous shock and estimate differential effects of reforms when a country is hit by a shock. We estimate the parameters of the following regression:

$$\Delta y_{ij} = \eta + \delta \cdot Char_j \cdot ref_{i-1} + \beta \cdot X_{ij-1} + f_i + g_j + \varepsilon_{ij} \quad \textit{(specification 3)}$$

Where Δy_{ij} is the log change in average real output growth of the 3 years following the shock relative to the 3 years preceding the shock, ref_{i-1} is the level of the reform index in years preceding the shock (3 years before the shock in our baseline regressions), X_{ij-1} is a vector of control variables prior to the shock, f_i is a country fixed effect, g_j an industry fixed effect and ε_{ij} is an error term.

To identify the effect of a reform, we assume that (i) the magnitude of the shock is not systematically correlated (positively or negatively) to the level of reforms ref_{i-1}, (we will check that this is indeed the case); and (ii) the shock is not anticipated by policy makers in the years preceding the shock, and therefore reforms are not "timed" to increase the resilience to future shocks for sectors that are more susceptible to those shocks. As in specification (2), we also allow for some heterogeneity of the coefficient δ across country and over time. Observations are clustered by country-year to account for possible correlations of error terms across sectors within countries during each episode.

A final approach to address endogeneity problems is to rely on an instrumental variables strategy. Instrumental variables help address potential concerns of reverse causality - if for example policy makers are more likely to liberalize the financial sector when manufacturing sectors that depend more on external finance are expected to experience higher growth opportunities. The instrumental variable strategy is laid down in Section III.E. For reasons that we will explain later, we estimate specification 2 in *changes* rather than in levels, and as a five years overlapping panel:

$$\Delta y_{ij,t,t-5} = \eta + \delta \cdot Char_j \cdot \Delta ref_{ij,t-5,t-10} + \beta \Delta X_{ij,t-5,t-10} + f_{it} + \varepsilon_{ijt} \quad \textit{(specification 4)}$$

Where $\Delta y_{ij,t-5,t-10}$ is the first difference of average output growth over a five years periods, relative to the previous five years, $\Delta ref_{ij,t-5,t-10}$ is the first difference of the level of reforms at the beginning of each period, $\Delta X_{ij,t-5,t-10}$ is the first difference of the initial value of the vector of controls, f_{it} is a country-year fixed effect, and ε_{ijt} is an error term. Using 5 years periods in the first difference equation permits to have sufficient variation of the reform indices over time.[12] Note that industry fixed effects have been removed by first differencing the equation. Finally, to correct standard errors for the auto-correlation of error terms

[12] Results are broadly similar with 3 years periods. We also checked that the main results hold when we use a non-overlapping panel.

introduced by the overlapping panel structure, we cluster observations by country. Clustering by country allows arbitrary correlations within countries.[13]

Before turning to the empirical analysis, one remark on the *frequency* of observations is in order. In our main specifications, we use annual data instead of averaging over long periods for the following reasons.[14] First, a lot of information is lost when averaging over long periods. In our case, this includes the fact that the reform indices vary significantly over time (see Figure 1). For example, the degree of liberalization of the financial sector of France was very different in 1985/86 than it was a few years earlier. By averaging over long periods, one ignores the fact that countries' economic structures vary over time, and may therefore miss the effect of a particular reform. Second, when averaging, the length and start date of the periods is arbitrary, so there is no guarantee that business cycles are cut in the right way, as their length varies over time and across countries. Third, by averaging over long periods, one is restricted to mainly using cross-sectional variability, which makes more difficult to identify sources of heterogeneity across countries. Fourth, by averaging, one ignores the dynamics of the effect of a reform. This includes not only the possibility that a reform may have different short-run and long-run effects, but also the fact that a reform may follow a smooth process, rather than being a one-off, "big bang" event. Comparing the average growth performance over arbitrary defined periods may not allow to identify smooth changes in economic performance caused by gradual reforms.

III. THE RESULTS

A. A First Look at the Data

Table 1 shows the list of countries included in the regression analysis: there are 62 countries among which 21 industrialized countries. The period of observations in the regression sample is 1974-2003.[15] Descriptive statistics are presented in Table 2. The mean per capita income is about 9000 constant US $. The mean value of the domestic financial reform index is 1.5, with a significant variation around the mean (standard deviation of 0.88). The mean trade index is of 0.72 – which corresponds to an average import tariff of about 17 percent - with significant variation around the mean (standard deviation of 0.24). The lower bound corresponds to Bangladesh, which, until 1992, had a 60 percent average import tariffs (index

[13] The overlapping panel approach introduces a moving average component in the error term. Clustering by countries correct standard errors for such correlation among residuals, as suggested by Bertrand, Duflo and Mullainathan (2005).

[14] We checked that our main results hold with non-overlapping panels over periods of 3 or 5 years (results not reported). See Attanasio, Picci and Scorcu (2000) for a discussion on the use of annual data in panel regressions.

[15] Descriptive statistics are based on regression (1) of Table 5.

normalized to zero), while the upper bound corresponds to Hong-Kong which does not impose any tariffs on its imports. According to Wacziarg and Welch's index, 72 percent of country-year observations were considered as liberalized. The institutional variables show a fair amount of variations across countries. In the sample, constraints on the executive vary from 0 (the minimum of the index), to 7 (the maximum value of the index), with an average of 5.5.[16] There is also significant variation *across* countries of creditor rights and of the time it takes to enforce contracts. Finally, one third of the countries have a Common Law legal origin, and the frequency of IMF programs is 30 percent in the regression sample. Finally, the average manufacturing output growth was 3 percent in our sample, and each sector accounts for about 2 percent of total manufacturing output on average.

Table 3 reports correlations among country-level variables. We find that countries that have more reformed their domestic financial reform have higher per capita GDP (correlation of 0.53), deeper banking systems (correlation of 0.46), and more open trade regimes (correlation of 0.62 with the tariff index). The correlations of the financial reform index with indicators of property rights (index of constraint on the executive and index of civil liberties) and of contracting rights (index of creditor rights, and number of days to enforce contracts) are also significant, but are quite lower (they all fall below 0.4). Surprisingly, the index of financial reforms appears to be uncorrelated with the legal origin of the country, suggesting that, over the past decades, reforms have taken place irrespective of the legal tradition of a country. The correlations of the trade index with other variables follow similar patterns. The strong correlation between the financial reform index and the import tariff index may reflect a common overall trend of liberalization, for which we control by including country-year fixed effects in the regressions.

Sectoral measures are reported in Table 4. The two measures of dependence on external finance show significant differences, which may reflect differences in the periods considered. For example, for Machinery, Electrical Equipment, the Rajan and Zingales' dependence on external finance is 0.77, while it is only 0.24 for the Kroszner, Laeven and Klingebiel's measure. However, the two measures are strongly correlated (correlation of 0.74), suggesting that sectoral rankings remain stable over time.[17] Next, the intensity of use of imported inputs is on average about 0.3, with a standard deviation of 0.1; it is (slightly) higher for industrialized countries, which could reflect greater intra-industry trade in those countries. There are exceptions to this pattern. For example, the intensity of imported inputs is higher in developing countries for Machinery, incl. Electrical Equipment and for Fabricated Metal

[16] Violent transition between political regimes were recoded as 0, instead of -77 as in the Polity IV database. Over the sample period, the lower bound of the property right index was reached by El Salvador, Ghana,Greece, the Philippines, Portugal, South Africa, Spain and Zimbabwe.

[17] This also suggests that inferences we may draw on the *relative* performance of two sectors within a country will be robust to the measure of external dependence used in the analysis.

Products. Industries that tend to rely less on imported inputs are: Food Products, Glass and Products, and Misc. Petroleum and Coal Products. Industries that use imported inputs more intensively are: Petroleum Refineries, Transport Equipments and Machinery, incl. Electrical Equipment. Finally, the Herfindhal index shows a greater concentration of inputs in developing countries (0.19) than in industrialized countries (0.15), as may be expected from theories of development based on an increased variety of inputs in the production process. The highest concentration of inputs is in industries such as Petroleum Refineries (above 0.4), Misc. petroleum and coal products, and Non-ferrous Metals (about 0.2); it is lowest in Beverages, Fabricated Metal Products, and Tobacco (around 0.1).

B. The average and differential effects of reforms

Table 5 presents panel regressions of real manufacturing output growth on the financial reform index and the various measures of trade reform according to *specification 1* (average effect of reforms). The regressions include country fixed effects to control for unobserved country factors, and time dummies to control for any global factors, such as common trends in the reform process across countries. The index of financial reforms enters the regressions positively and is significant at the 1 percent level in most specifications, both in the full sample or when advanced countries are excluded. The estimated effect is large: an increase in the financial reform index by one standard deviation is associated with an increase in manufacturing output growth of 2.4 percentage points, on average. Controlling for GDP per capita, property rights, or contracting rights does not alter the significance or the size of the partial correlation estimated (these regressions are not reported).

Turning to trade reforms, two of the three measures of trade liberalization enter positively and significantly in the baseline regression, but the trade index enters the regression with a negative sign and is significant at 10 percent in the full sample. However, when we simultaneously control for domestic financial reforms, the association between trade liberalization measures and manufacturing output growth becomes insignificant, suggesting that the estimated effect may have been picking up the effect of financial reforms.[18]

While providing some indications of the likely average effects of trade and financial reforms on manufacturing output growth, these results are subject to endogeneity bias: reforms may be more likely to occur when output performance is expected to improve (reverse causality), while key time varying factors affecting output growth may have be omitted from the regressions.

[18] As argued by Rose (2002), WTO/GATT membership may be a poor indicator of trade policies. He finds that WTO members seem to have better economic freedom.

In Table 6, we take a first step at tackling the endogeneity problem by estimating the *differential* effects of financial and trade reforms on manufacturing output growth, as in *specification 2*. The inclusion of country-year fixed effects allows controlling for all potential omitted variables at the country level. We find that financial reforms have a positive differential effect on manufacturing output growth in industries that require more external finance to grow (Panel A). This result sheds light on the channel through which financial reforms have an impact on the real economy: financial reforms foster output growth by making the financial sector more efficient, e.g. by reallocating capital to the industries that need it most. The non significance of the coefficient on the interaction between dependence on external finance and the ratio of private credit to GDP is consistent with the interpretation that what matters is not the volume of capital intermediated by the financial sector, but how this capital is intermediated and reallocated by the financial sector. However, the significance of the positive differential effect drops when advanced countries are excluded from the regression, suggesting a potentially weaker link between financial reforms and the performance of the real sector in developing countries than in advanced countries. These results are robust to the inclusion of a number of other interaction terms. For example, our results do not reflect differences in overall development (column 8), in contracting rights (columns 10, 12 and 13) or in property rights (column 11).[19]

Next, we estimate the differential effect of trade reforms across industries (Panel B). The measure of external dependence on finance and the measure of intensity of imported inputs are not correlated (correlation of about 0.1), suggesting that our approach may allow us to identify in the same empirical framework the effects of financial and trade reforms, and the channels through which they affect the performance of manufacturing industries. When interacting each of the trade liberalization indices with the measure of imported input intensity, we find a positive and significant association between trade reforms and output growth in sectors in which a country has a comparative advantage (we use a dummy variable for sectors that are net exporters) and that rely more intensively on traded intermediate inputs. We obtain a similar result across all three measures of trade liberalization. Moreover, the estimated coefficients remain significant when the differential effect of financial reforms is also controlled for.

From this analysis we conclude that financial and trade reforms seem to have the output effects predicted by standard theories. Financial reforms have beneficial effects by allowing a more efficient intermediation and use of capital, not simply by allowing more capital to be intermediated. Trade reforms favor sectors in which a country has a comparative advantage,

[19] We also find that in countries that have weaker enforcement of contracting rights, sectors that depend more on external finance experience a relatively slower growth rate, which is consistent with the law and finance literature (Levine, 2005).

in particular by lowering the cost of imported intermediate inputs. An interesting question is whether these positive effects of reforms hold across all types of countries.

C. Reforms and Property Rights

In this section we present a first set of results suggesting that the estimated relationship between financial and trade reforms on the one hand and output performance on the other hand is affected by the property right environment.

For this purpose, we explore heterogeneity of coefficient δ. Heterogeneity of parameters along a dimension z is approached in two different ways. First, we explore a linear relationship:

$$\delta_{it} = \delta_0 + \delta_1 \cdot z_{it}$$

Country heterogeneity is explored in panels A and B of Table 7.[20] In the first set of regressions, we include an interaction of the financial reform index with the index of civil liberties (similar results are obtained with the measure of constraints on the executive). The coefficient on the interaction term is positive and significant at the 1 percent level. Hence the differential effect of financial reforms in stronger in countries with better protection of property rights, suggesting that, in these countries, financial reforms are more effective at promoting an efficient allocation of capital. We also find that sectors that use a more diverse set of inputs (lower Herfindhal index) grow relatively faster in countries with better property rights, a result reminiscent of Claessens and Laeven (2003).

The interaction term remains positive and significant when we control for other factors which might also affect industries differentially, according to their dependence on external finance. These includes: the depth of the banking system (column 3), GDP per capita (column 4), differences in legal traditions (columns 5 and 6), the quality of contract enforcement (column 7), and the protection of creditor rights (column 8)

We next explore whether the results hold when we consider instead a spline functional form (Panel B of Table 7 is reported in the appendix):

$$\delta_{it} = \delta_0 \cdot \left(1 - 1\left[z_{it} - \bar{z}\right]\right) + \delta_1 \cdot 1\left[z_{it} - \bar{z}\right]$$

where $\bar{z}$ is the sample median value of variable z and $1[x]$ is the indicator function equal to one if and only if $x \geq 0$, and zero otherwise.

[20] Panel B of Table 7 is reported in the appendix.

The results with this new specification point at a similar complementarity between reforms and property rights. We find that financial reforms have a positive and significant differential effect on output performance only when property rights are sufficiently well protected. Again, the estimated coefficient holds to a number of robustness tests (reported in appendix). In particular, an alternative heterogeneity of coefficient δ along measures of the contractual environment (legal origin, protection of creditor right or enforcement of contracts) do not suggest any such complementarity with financial sector reforms. Finally, in one more set of regressions, we explore whether the impact of trade reforms also depends on the institutional environment. The results suggest that property rights matter for trade reforms as well. We find that trade reforms foster output growth in export sectors using imported inputs intensively only in countries that have a sufficiently good protection against expropriation risks.

To summarize, we find that the positive differential relationship between financial and trade reforms on the one hand and output performance on the other hand is higher in countries with a good protection of property rights.

D. Reforms and resilience to terms of trade shocks

As discussed in the introduction, reforms may also contribute in making an economy more resilient to negative shocks. For example, financial reforms may enhance the resilience of an economy to exogenous shocks by providing access to external sources of finance to firms facing shortages of cash when hit by temporary negative shocks. In absence of access to external finance, costly bankruptcies of cash-constrained firms may have lasting effects on sectoral output. Even in absence of costly bankruptcies, firms facing temporary liquidity shocks may choose inefficient projects when they have limited access to external sources of finance (Aghion et al., 2005).

Conducting an event study during episodes of negative terms of trade shocks also provides an alternative to the difference-in-difference approach of the previous sections to address endogeneity issues. We define an episode of negative shock as a year during which the terms of trade of country deteriorates by more than 10 percent relative to the previous year. Our hypothesis is that, for any given country, the shock will be larger in industries that rely more intensively on imported inputs. This provides a very simple way to test the resilience theory outlined above. The regressions include country fixed effects. These fixed effects allow to control for the possibility that countries that are more subject to external shocks may be more likely to undertake economic reforms, and more generally for any omitted variables at the country level.

Our identification strategy relies on the assumptions that (i) the magnitude of the shock is not systematically correlated (positively or negatively) to the level of reforms, and (ii) shocks are not anticipated by policy makers. On the first assumption, the correlation between the

intensity of the terms of trade shock and the index of banking reform is low (0.17), making it unlikely that such an effect could be driving our results. Moreover, controlling for an interaction between the measure of imported inputs intensity and the percent change in the terms of trade does not alter our result, as discussed below. On the second assumption, it would seem far fetched that policy makers could time financial reforms in the years preceding a specific terms of trade shock. Moreover, as explained above, we already include country fixed effects to control for the fact that some countries may be more prone to large negative terms of trade shocks. However, to be sure, we check that changes in the financial reform index were not more likely in the years preceding one of these episodes than in the full sample. In the three years preceding a shock, the average change in the index is 0.16, which is comparable (and even smaller) than the complete sample average change of 0.19 over 3 years periods.

Table 8 reports the event study regressions. The dependent variable is the change in the average output growth in the three years following the shock, relative to the three previous years. The variable of interest is the interaction term between the measure of imported inputs intensity and the reform indices. The baseline regression includes the lagged output share as a control variable. The interaction term between the financial reform index and the measure of imported input intensity is positively and significantly associated with the change in output growth, but there is no significant association with the change in output growth when the trade index is considered instead. Our interpretation of this result is that, in countries that have more (respectively less) reformed their financial sector, sectors that use imported inputs more intensively experience a relatively smaller (respectively larger) fall of output growth around episodes of negative shocks.

A number of robustness tests are reported in Table 8. Richer countries may be, in general better equipped to deal with negative shocks, as even sectors at the 3 digit may be for instance more diversified and have access to a set of substitutable domestic suppliers. We check that the coefficient on the interaction term is not driven by differences in the overall level of development by controlling for an interaction with GDP per capita. The result remains significant when we alternatively introduce interaction terms with the Common Law legal origin, and the depth of the banking system. One concern is that the result may depend on the threshold chosen for the definition of the episode. In regressions that are not reported we check that this is not the case by varying the threshold (we report the extreme case of terms of trade shocks corresponding to a percent reduction of more than 25 percent in column 6). We also control for the intensity of the shock (column 5). Finally, columns 7 to 10 show that the complementarity of financial reforms with the property rights environment also seems to matter for a country resilience to terms of trade shocks.

E. First Difference and IV Regressions

In this paper, we have exploited the predictions of existing theories according to which reforms should have differential effects across sectors. This approach has allowed us to address potential endogeneity problems. We also have conducted an event study, based on a reasonably exogenous shock, to identify a differential effect of reforms across sectors. So, one could argue that we have addressed many potential sources of endogeneity bias.

However, it could possible that countries that have a structure of production and a comparative advantage tilted towards sectors that are more financially intensive choose to liberalize their financial systems more than other countries. Do and Levchenko (2007) argue that financial development is an equilibrium outcome of the economy's productive structure and trade patterns, and that financial systems are more developed in countries with large financially intensive sectors. A similar argument could be made for trade reforms. For instance, countries that have a comparative advantage in sectors that, for technological reasons, depend more on a large variety of traded intermediate goods, may be more likely to liberalize their trade regime. For this purpose, we now explore *specification 4*, in which the relationship between manufacturing output performance and reforms is now estimated in first differences, as a an overlapping panel over five year periods.[21] In other words, we estimate the association between the *change* in the initial level of liberalization and the *change* in average manufacturing output growth of the five following years.

As discussed in the introduction, another reason to estimate regressions in first difference is to better disentangle the effect of specific economic reforms from that of the broader institutional environment. Table 9 describes the time variation of the financial reform index, trade liberalization index, and indicators of property rights and contracting rights. Over the past decades, there has been an active process of reforms in finance and trade, as shown by the respective frequency of changes in each of these indices.[22] In contrast, changes in indicators of property rights (index of constraint on the executive or index of civil liberties) are less frequent, and changes in the index of creditor rights happen to be a rare event. This suggests that changes in output growth within countries may not be driven exclusively by the institutional environment.

Table 10 reports first difference OLS regressions. For the reasons explained before, observations are now clustered at the country level to correct standard errors from the moving average component of the error term in the overlapping panel approach. The

[21] Regressions on a non-overlapping five year panel were also explored, with no major differences in the main results.

[22] Looking at changes in average tariff rates may understate the true occurrence of changes in trade tariff policies which may affect only a subset of imported goods.

coefficient on the interaction term between the external dependence measure and the five year change in the financial reform index is positive and significant in all specifications. The order of magnitude is comparable to the one estimated in the regressions in level. The coefficient on the interaction term between the trade index and the measure of imported input intensity becomes now insignificant. Next, we explore again whether protection of property rights affects the differential effect of reforms found in regressions in level. When splitting observations into two groups, we find that the positive differential effect of financial reform occur mainly in countries with a good protection of property rights. We again check that this heterogeneity of the effect of financial reforms across countries does not reflect differences in the contracting environment, by also allowing heterogeneity of coefficient δ according to the legal origin or the quality of contract enforcement.

Last but not least, we address endogeneity concerns with instrumental variables estimations. The difficulty is to find instruments that are correlated with the reform indices but that have no direct or no other indirect effect on manufacturing output performance, conditional on the set of control variables. Specifically, the problem is that any potential instrument for the level of each reform index is likely to be also correlated with other, maybe broader, institutional characteristics of the country considered. Indeed, the reform indices and the indicators of institutional quality are quite correlated with each others in levels (see Table 3). Fortunately, the correlation among changes of these variables is very small, even at long horizons.[23] This suggests that finding valid instrumental variables may be feasible with the first difference specification.[24]

Another benefit of instrumental variables estimations is to reduce attenuation bias that may result from measurement problems. For example, if the reform index is imperfectly measuring the true policy change, the estimated effect may be biased toward zero. This problem may be relevant especially for trade reforms, given the channels through which trade reforms are likely to affect manufacturing output performance. As discussed earlier, our indices of trade reforms does not directly measure reductions in tariff and non-tariff barriers to trade in intermediate goods well. To the extent that the instruments are also correlated with

[23] Correlations among the financial reform index, the trade index and the two property rights measures are all below 0.04 at a five year horizon, and below 0.11 at a 20 year horizon.

[24] For example, the average index level in neighboring countries – a potential instrument as explained in the text - is strongly correlated with the index of the country considered; but it is also strongly correlated with other institutional features of the economy, including property rights and various measures of contracting rights. More generally, as argued by Acemoglu (2005), in absence of time variation, unbundling the effects of institutional characteristics or policies in specific areas is likely to be challenging as any potential instrument is also likely to be correlated with other institutional features of the country considered. We address this issue by applying IV methods to first difference regressions, instead of regressions in level, to exploit the time dimension of the reform indices.

tariff barriers on intermediate goods, the effect of trade reforms may be better estimated with instrumental variables.

We use the following two instruments. The general hypothesis for the choice of instruments is that reforms are often triggered by external events. The first instrument is the 5 year change in the average reform index of neighboring countries in the same region, in the previous five years. The assumption is that successfully implemented reforms diffuse across borders, as a result of learning or imitation process. The second instrument for the change in the reform index is the presence of not of an IMF program in the five years considered. IMF programs were typically accompanied by structural conditionality in the areas of trade and finance. While the presence of an IMF program or not may not be entirely exogenous to macroeconomic conditions, we argue that an IMF program are likely to be unrelated to the performance of a specific manufacturing sector, *conditional on country-year fixed effects* and on a set of observable characteristics. Indeed, IMF programs are typically triggered by balance of payment difficulties; it is therefore unlikely that countries decide to enter an IMF program because financially intensive industries or industries that use imported inputs intensively experience *higher* growth opportunities (see identifying assumptions below).

The first stage of the 2SLS is the following:

$$Char_j \cdot \Delta ref_{ij,t-5,t-10} = \varphi + \phi \cdot Char_j \cdot \Delta \underline{ref}_{ij,t-5,t-10} + \gamma \cdot Char_j \cdot IMF_{it-5,t-10} + \lambda \Delta X_{ij,t-5,t-10} + f_{it} + \omega_{ijt}$$

Where $\Delta \underline{ref}_{ij,t-5,t-10}$ is the average change in the reform index of other countries in the same region, and $IMF_{it,t-5,t-10}$ is the average number of years spent under an IMF program between dates $t-5$ and $t-10$. F tests of joint significance of the instruments in the first stage allow to test that the instruments are not weak.

The identifying assumptions are the following:

$$\begin{cases} E\left(Char_j \cdot \Delta \underline{ref}_{i,t-5,t-10}, \varepsilon_{ijt} \middle| \Delta X_{ijt,t-5,t-10}, f_{it}\right) = 0 \\ \\ E\left(Char_j \cdot IMFprog_{i,t-5,t-10}, \varepsilon_{ijt} \middle| \Delta X_{ij,t-5,t-10}, f_{it}\right) = 0 \end{cases}$$

Therefore, as explained above, there are likely to be satisfied as long as reforms in neighboring countries or the occurrence of IMF programs do not occur when, for the country considered, industries with a higher dependence on external finance (in the case of financial reforms) or with a higher use of imported inputs (in the case of trade reforms) experience increase in growth opportunities.

The instrumental variables regressions are in Table 11. The coefficient on the interaction term between the measure of external dependence and the change in the financial reform

index is strongly significant (at the 1 percent level) and positive, with the same order of magnitude as before. As for the interaction between the measure of dependence on imported inputs and the trade reform index, the coefficient is insignificant is most regressions, as in the OLS regressions. When splitting countries by the quality of property rights, the IV estimations confirm earlier results that reforms are effective in countries with a good institutional environment. In particular, the estimated effect of trade reforms is strongly significant (at the 1 percent level), suggesting some attenuation bias in the OLS regressions – which may be the result of measurement problems in the trade index. As discussed earlier, our index of average tariff may be an imperfect measure of tariffs on intermediate goods. The J tests of over-identifying restrictions confirm the validity of the instruments. Finally, the first stage confirms that our estimations are unlikely to be affected by a weak instrument bias (first stage regressions are reported in the appendix). Indeed, the F tests of joint significance of the excluded instruments are all well above the critical tests for weak instruments developed in the econometric literature. [25]

IV. CONCLUSION

What has been the impact of the past decades of financial and trade reforms? Through which channels have financial sector and trade reforms affected economic performance? In spite of the large literature on the role of finance and trade in accelerating economic growth, relatively few studies have estimated the impact of pure financial sector *reforms* and trade *reforms* on economic performance. The difficulty in identifying the effect of policies lies in the fact that, in the cross-section, countries adopting good economic policies in the areas of trade and finance usually also have a good institutional environment in a broader sense. How can one disentangle the effect of financial sector and trade policies from the effect of the property right or the contracting right environment?

In this paper, we addressed this issue by estimating differential effects of financial sector and trade reforms across manufacturing industries. We find evidence consistent with existing theories. Financial sector reforms affect economic performance by improving the allocation of capital across countries. Reforms of the financial sector also contribute in making countries more resilient to terms of trade shocks. Trade liberalizations benefit sectors that rely on intermediate imported inputs. We follow several approaches that contribute in reducing endogeneity concerns, including conducting an event study around episodes of

[25] The literature has suggested two criteria to evaluate the likelihood of a weak instrument bias, and has computed related thresholds for the F test of joint significance. The first criterion is that the bias of the IV regression is less than 10 percent of the bias of the OLS regression, with a critical value of the F test around 10. The second criterion is that the true significance level is 10 percent when the nominal level is 5 percent, with a critical value of the F test around 20 (see Stock and Yogo, 2005).

terms of trade shocks, and estimating instrumental variable regressions. So, the answer to the question is: the impact of reforms has been positive overall.

However, there is one major qualification to these results. We find that the positive differential effects of financial sector and trade reforms are much weaker – or even insignificant – in countries with a weak protection of property rights. This is consistent with the recent literature arguing that the protection of property rights is a major determinant of countries' economic performance in the long-run (Acemoglu and Johnson, 2007). This conclusion also suggests that economic reforms should not be contemplated in isolation from the broader institutional context of a country.

Figure 1 Liberalization in Finance and Trade (1973-2005)

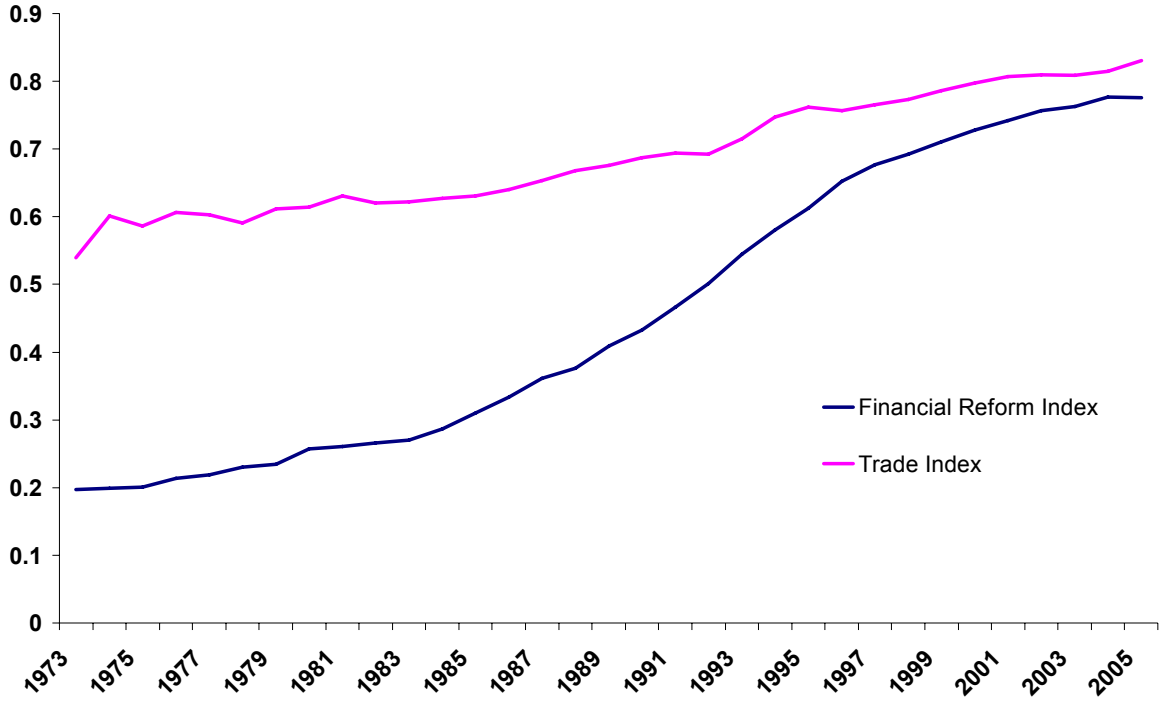

Table 1 Country Sample

	Country		Country
1	Algeria	32	Jordan
2	Argentina	33	Kenya
3	Australia	34	Korea
4	Austria	35	Latvia
5	Bangladesh	36	Lithuania
6	Belgium	37	Malaysia
7	Bolivia	38	Mexico
8	Bulgaria	39	Morocco
9	Canada	40	Netherlands
10	Chile	41	New Zealand
11	China,P.R.:Hong Kong	42	Norway
12	Colombia	43	Paraguay
13	Costa Rica	44	Peru
14	Côte d'Ivoire	45	Philippines
15	Denmark	46	Poland
16	Ecuador	47	Portugal
17	Egypt	48	Romania
18	El Salvador	49	Russia
19	Estonia	50	Senegal
20	Finland	51	Singapore
21	France	52	South Africa
22	Germany	53	Spain
23	Ghana	54	Sri Lanka
24	Greece	55	Sweden
25	Hungary	56	Tanzania
26	India	57	Tunisia
27	Indonesia	58	Turkey
28	Ireland	59	United Kingdom
29	Israel	60	United States
30	Italy	61	Uruguay
31	Japan	62	Zimbabwe

Table 2 Descriptive Statistics

	Mean	Standard deviation	Source
Sectoral variables			
Growth of real output	0.031	0.131	UNIDO
Share of manufacturing output	0.020	0.029	UNIDO
(Imports+Exports)/Output	1.3871	10.347	UNIDO
Country level variables			
GDP per capita (constant USD 2000)	9104.02	9047.00	WDI
Domestic financial reform index	1.50	0.88	Abiad, Detragiache and Tressel (2008)
Credit to the private sector / GDP	0.45	0.33	World Bank financial structure database 2007
Average tariff index	0.72	0.24	IMF reform database
Liberalization index (0/1)	0.72	0.45	Wacziarg and Welch (2003)
WTO membership (0/1)	0.87	0.33	Subramanian and Wei (2003)
Constraints on the executive	5.48	2.09	Polity IV
Civil liberties	4.13	1.64	Freedom House
Creditor rights	1.97	1.17	Djankov, McLiesh and Shleifer (2007)
Days to enforce contracts	494.54	191.57	World Bank Doing Business database
Common law legal origin (0/1)	0.33	0.47	Djankov, McLiesh and Shleifer (2007)
Terms of trade growth	0.01	0.12	2007 World Economic Outlook
IMF program (0/1)	0.30	0.46	IMF

Table 3 Bivariate Correlations

	Private credit / GDP	GDP per capita	Index average tariff	Financial reform index	Index civil liberties	Constraints on the executive	Index creditor rights	Common law legal origin	Days to enforce contracts	Wacziarg & Welch liberalization	WTO membership	IMF program
Private credit / GDP	1											
GDP per capita	0.63	1										
	0											
Index average tariff	0.42	0.58	1									
	0	*0*										
Financial reform index	0.46	0.54	0.62	1								
	0	*0*	*0*									
Civil liberties	0.34	0.69	0.48	0.36	1							
	0	*0*	*0*	*0*								
Constraints on the executive	0.27	0.50	0.40	0.39	0.76	1						
	0	*0*	*0*	*0*	*0*							
Creditor rights	0.22	0.16	0.15	0.12	0.01	0.07	1					
	0	*0*	*0*	*0*	*0.8545*	*0.0156*						
Common law	0.04	0.05	-0.04	0.07	-0.09	0.00	0.49	1				
	0.1	*0.0*	*0.1*	*0.0*	*0.0*	*0.9*	*0*					
Days to enforce contracts	-0.22	-0.42	-0.31	-0.29	-0.16	-0.13	-0.22	-0.20	1			
	0	*0*	*0*	*0*	*0*	*0*	*0*	*0*				
Wacziarg & Welch liberalization	0.41	0.47	0.62	0.59	0.42	0.37	0.00	-0.14	-0.21	1		
	0	*0*	*0*	*0*	*0*	*0*	*0.9936*	*0*	*0*			
WTO membership	0.23	0.28	0.14	0.28	0.16	0.27	0.19	0.22	-0.14	0.33	1	
	0	*0*	*0*	*0*	*0*	*0*	*0*	*0*	*0*	*0*		
IMF program	-0.41	-0.50	-0.31	-0.19	-0.35	-0.28	-0.20	-0.17	0.19	-0.20	-0.16	1
	0	*0*	*0*	*0*	*0*	*0*	*0*	*0*	*0*	*0*	*0*	

Note: p-values are reported in italics

Table 4 Sectoral Measures

	External Dependence		Imported Inputs Intensity		Herfindhal Index		Intangibility
	RZ	KLK	Industrialized	Developing	Industrialized	Developing	KLK
1 Beverages	0.08	0.03	0.24	0.22	0.10	0.12	0.14
2 Fabricated metal products	0.24	-0.25	0.29	0.36	0.09	0.12	0.09
3 Food products	0.14	-0.15	0.19	0.19	0.17	0.24	0.07
4 Footwear, except rubber or plastic	-0.08	-0.74	0.41	0.33	0.12	0.19	0.04
5 Furniture, except metal	0.24	-0.38	0.34	0.23	0.16	0.17	0.09
6 Glass and products	0.53	0.03	0.20	0.21	0.12	0.18	0.06
7 Industrial chemicals	.	.	0.38	0.36	0.15	0.15	.
8 Iron and steel	0.09	0.05	0.32	0.29	0.14	0.22	0.02
9 Leather products	-0.14	-0.95	0.41	0.33	0.12	0.19	0.09
10 Machinery, electric	0.77	0.24	0.45	0.52	0.14	0.19	0.03
11 Machinery, except electrical	0.45	-0.04	0.41	0.45	0.10	0.14	0.13
12 Misc. petroleum and coal products	0.33	0.13	0.16	0.21	0.23	0.30	0
13 Non-ferrous metals	0.01	-0.12	0.35	0.31	0.17	0.26	0.05
14 Other chemicals	0.22	-0.3	0.38	0.36	0.15	0.15	0.2
15 Other manufactured products	0.47	0.28	0.34	0.31	0.11	0.10	0.15
16 Other non-metallic mineral products	0.06	-0.29	0.20	0.21	0.12	0.18	0.03
17 Paper and products	0.18	-0.35	0.28	0.32	0.14	0.16	0.11
18 Petroleum refineries	0.04	-0.02	0.65	0.51	0.43	0.48	0
19 Plastic products	1.14	-0.02	0.38	0.36	0.15	0.15	0.18
20 Pottery, china, earthenware	-0.15	-0.41	0.20	0.21	0.12	0.18	0
21 Printing and publishing	0.2	-0.42	0.28	0.32	0.14	0.16	0.43
22 Professional & scientific equipment	0.96	0.72	.	.	.	.	0.15
23 Rubber products	0.23	-0.02	0.38	0.36	0.15	0.15	0.06
24 Textiles	0.4	0.01	0.43	0.36	0.15	0.19	0.01
25 Tobacco	-0.45	-1.14	0.24	0.22	0.10	0.12	0.34
26 Transport equipment	0.31	-0.08	0.42	0.44	0.12	0.17	0.11
27 Wearing apparel, except footwear	0.03	-0.21	0.44	0.32	0.16	0.28	0.07
28 Wood products, except furniture	0.28	0.05	0.34	0.23	0.16	0.17	0.01
Average	0.24	-0.16	0.34	0.31	0.15	0.19	0.10
Correlations	0.74		0.84		0.89		

Notes: RZ refers to the external dependence measure of Rajan and Zingales (1998), and KLK to the external dependence and intangibility measures of Kroszner, Laeven and Klingebiel (2007).

Table 5 The Average Effect of Financial and Trade Reforms on Manufacturing Output

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Sample	full	developing	full	developing	full	developing	full	developing	full	full	full
Domestic financial reforms (t-1)	0.028	0.044							0.023	0.028	0.029
	[4.77]***	[5.13]***							[4.20]***	[4.82]***	[5.03]***
Index average tariff (t-1)			-0.018	-0.002					-0.008		
			[1.96]*	[0.16]					[0.64]		
WTO membership (t-1)					0.011	0.024				0.001	
					[2.01]**	[3.88]***				[0.16]	
Wacziarg and Welch liberalization (t-1)							0.012	0.032			0.001
							[2.17]**	[4.16]***			[0.13]
Log GDP per capita (t-1)	-0.074	-0.067	-0.060	-0.059	-0.057	-0.052	-0.051	-0.040	-0.070	-0.074	-0.078
	[5.59]***	[4.44]***	[5.70]***	[5.19]***	[6.82]***	[5.76]***	[5.22]***	[3.64]***	[4.79]***	[5.49]***	[5.21]***
Log output share (t-1)	0.002	-0.002	0.001	-0.001	0.000	-0.002	0.000	-0.002	0.003	0.002	0.002
	[1.31]	[0.93]	[0.65]	[0.71]	[0.00]	[1.37]	[0.05]	[1.52]	[2.05]**	[1.31]	[1.27]
country fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	36251	21193	45847	27386	52617	32666	51633	31682	34533	36251	35492
R-squared	0.09	0.08	0.1	0.08	0.1	0.09	0.1	0.09	0.08	0.09	0.09

Robust t statistics in brackets, observations are clustered by country-year
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6 The Differential Effects of Financial and Trade Reforms

PANEL A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Sample	full	developing	full	developing	full	developing	full	developing	full	full	full	full	full
Domestic financial reforms (t-1)	0.029	0.044			0.022	0.041							
	[4.94]***	[5.17]***			[3.98]***	[4.66]***							
Dependence * domestic financial reforms (t-1)	0.009	0.006	0.009	0.006	0.011	0.009	0.010	0.009	0.005	0.009	0.011	0.009	0.009
	[3.83]***	[1.37]	[3.86]***	[1.44]	[3.56]***	[1.67]*	[3.49]***	[1.70]*	[1.74]*	[4.04]***	[4.33]***	[4.04]***	[3.45]***
Private credit / GDP (t-1)					-0.015	-0.021							
					[1.47]	[0.89]							
Log GDP per capita (t-1)	-0.076	-0.068			-0.060	-0.064							
	[5.70]***	[4.54]***			[3.60]***	[3.54]***							
Dependence * private credit / GDP (t-1)					-0.002	-0.010	-0.003	-0.012					
					[0.24]	[0.42]	[0.29]	[0.56]					
Dependence * GDP per capita (t-1)									6.50E-07				
									[2.14]**				
Dependence * log days to enforce contracts										-0.012			
										[2.42]**			
Herfindhal * constraint on the executive (t-1)											0.008		
											[1.61]		
Dependence * common law legal origin												-0.012	
												[2.42]**	
Dependence * creditor rights (t-1)													-0.004
													[2.41]**
Log output share (t-1)	0.00091	-0.00217	0.00137	-0.00156	0.00142	-0.00231	0.0015	-0.00208	0.00091	0.00151	0.00095	0.00151	0.00376
	[0.69]	[1.16]	[1.13]	[0.92]	[0.96]	[1.04]	[1.08]	[1.01]	[0.70]	[1.25]	[0.71]	[1.25]	[3.12]***
country fixed effects	yes	yes	no	no	yes	yes	no	no	no	no	no	no	no
industry fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
country-year fixed effects	no	no	yes	yes	no	no	yes	yes	yes	yes	yes	yes	yes
Observations	34878	20357	35619	21098	30969	17043	30969	17043	34878	35619	32944	35619	29431
R-squared	0.09	0.08	0.02	0.01	0.09	0.08	0.02	0.01	0.02	0.02	0.01	0.02	0.02

Robust t statistics in brackets, observations are clustered by country-year
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6 (continued)

PANEL B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
	full	developing	full	full	full	developing	full	full	full	dev
Wacziarg and Welch liberalization (t-1)	0.004	0.024								
	[0.64]	[3.02]***								
Intensity imported inputs (exporters) *	0.037	0.029	0.026	0.021						
Wacziarg and Welch liberalization (t-1)	[7.40]***	[2.55]**	[5.37]***	[4.34]***						
Index average tariff (t-1)					-0.022	-0.005				
					[2.37]**	[0.40]				
Intensity imported inputs (exporters) *					0.044	0.035	0.033	0.023		
Index average tariff (t-1)					[7.82]***	[2.99]***	[5.88]***	[4.18]***		
WTO membership (t-1)									0.001	0
									[0.10]	[1
Intensity imported inputs (exporters) *									0.039	0
WTO membership (t-1)									[7.81]***	[3.
Dependence * domestic financial reforms (t-1)			0.014	0.013			0.013	0.013		
			[5.37]***	[4.24]***			[5.19]***	[4.19]***		
Log (Exports+Imports)/Output (t-1)				0.003				0.003		
				[1.94]*				[1.86]*		
Log GDP per capita (t-1)	-0.059	-0.051			-0.061	-0.059			-0.062	-0
	[4.93]***	[3.92]***			[5.76]***	[5.20]***			[5.95]***	[5.
Log output share (t-1)	-0.001	-0.002	0.0004	0.004	-0.001	-0.001	0.001	0.004	-0.001	-0
	[0.73]	[1.22]	[0.30]	[2.33]***	[0.51]	[0.91]	[0.48]	[2.33]**	[0.70]	[
country	yes	yes	no	no	yes	yes	no	no	yes	
industry	yes	yes	yes	yes	yes	yes	yes	yes	yes	
country-year	no	no	yes	yes	no	no	yes	yes	no	
Observations	44351	26372	31985	24540	44455	26621	32770	24421	45155	2
R-squared	0.1	0.08	0.02	0.02	0.1	0.08	0.02	0.02	0.1	

Robust t statistics in brackets, observations are clustered by country-year
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7 The Differential Effects of Reforms and the Role of Property Rights
PANEL A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample	full	developing	full	full	full	full	full	full	full
Dependence * domestic financial reforms (t-1)	-0.004	-0.008	-0.003	-0.006	-0.003	-0.003	0.001	-0.004	-0.006
	[7.08]***	[4.85]***	[6.68]***	[5.91]***	[7.05]***	[6.15]***	[6.26]***	[6.13]***	[5.65]***
Herfindhal * civil liberties (t-1)	-0.019	-0.004	-0.015	-0.020	-0.019	-0.019	-0.017	-0.012	-0.010
	[3.58]***	[0.36]	[2.65]***	[3.60]***	[3.58]***	[3.58]***	[3.05]***	[2.41]**	[1.67]*
Dependence * domestic financial reforms	0.004	0.007	0.004	0.005	0.004	0.004	0.004	0.004	0.004
* civil liberties (t-1)	[3.92]***	[4.31]***	[3.65]***	[4.76]***	[3.87]***	[3.88]***	[3.64]***	[3.61]***	[3.22]***
Dependence * private credit / GDP (t-1)			-0.00802						
			[0.81]						
Dependence * GDP per capita (t-1)				-3.08E-07					
				[1.28]					
Dependence * common law legal origin					-0.01247				
					[2.27]**				
Dependence * domestic financial reforms (t-1)						-0.00326			
* common law legal origin						[1.10]			
Dependence * domestic financial reforms (t-1)							-0.00001		
* Log days to enforce contracts							[2.04]**		
Dependence * creditor rights (t-1)								-0.00187	
								[1.27]	
Log (Exports+Imports) / output (t-1)									0.00342
									[2.50]**
Log output share (t-1)	0.0004	-0.001	0.0004	0.0004	0.001	0.0005	-0.001	0.003	0.004
	[0.30]	[0.83]	[0.23]	[0.25]	[0.38]	[0.34]	[0.36]	[2.29]**	[2.86]***
country-year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
industry fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	33870	20480	29791	33148	33870	33870	29895	27991	24249
R-squared	0.02	0.01	0.02	0.02	0.02	0.02	0.02	0.02	0.02

Robust t statistics in brackets, observations are clustered by country-year
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8 Reforms and Resilience to TOT Shocks

	(1)	(2)	(3)	(4)	(5)	(6) Growth TOT < - 25 %	(7)	(8)	(9)	(10)
Intensity imported inputs * domestic financial reforms (t-3)	0.181		0.229	0.179	0.183	0.234				
	[3.23]***		[4.37]***	[3.16]***	[3.10]***	[2.10]*				
Intensity imported inputs * average tariff index (t-3)		0.647								
		[0.68]								
Intensity imported inputs * domestic financial reforms (t-3)							0.183	0.183	0.186	0.182
Above median civil liberties							[2.98]***	[3.01]***	[2.86]***	[2.86]***
Intensity imported inputs * domestic financial reforms (t-3)							0.163	0.148	0.162	-0.062
Below median civil liberties							[1.84]*	[1.38]	[1.83]*	[0.41]
Intensity imported inputs * private credit / GDP (t-3)			-0.068							
			[1.55]							
Intensity imported inputs * common law legal origin				0.077				0.089		
				[0.33]				[0.35]		
Intensity imported inputs * growth rate of TOT (t)					-0.185				-0.191	
					[0.33]				[0.34]	
Intensity imported inputs * Log days to enforce contracts										-0.001
										[1.51]
Log output share (t-3)	-0.006	-0.013	-0.006	-0.006	-0.006	0.020	-0.006	-0.006	-0.006	-0.006
	[0.66]	[0.94]	[0.64]	[0.67]	[0.65]	[1.11]	[0.65]	[0.66]	[0.64]	[0.61]
country fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
industry fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	995	385	978	995	995	170	995	995	995	799
R-squared	0.13	0.15	0.14	0.13	0.13	0.16	0.13	0.13	0.13	0.16

Robust t statistics in brackets, observations are clustered by country-year
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9 Frequency of Economic Reforms and of Changes in Institutional Environment (1980-2003)

	# changes	# obs	Frequency of change
Financial reform	426	1346	32%
Trade reform	335 [1/]	1289	26%
Constraints on the executive	67	1281	5%
Civil liberties	186	1319	14%
Creditor rights	14	1151	1%

1/ Absolute percent change in average tariff greater or equal to2 percentage points

Table 10 Regressions in First Difference (5 years Overlapping Panel)

PANEL A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	full	developing	full	developing	full	developing	full	full
Dependence * Δ domestic financial reforms $_{t-5,t-10}$	0.013	0.013	0.014	0.015			0.012	0.015
	[2.72]***	[1.82]*	[3.11]***	[2.30]**			[2.21]**	[2.81]***
Intensity imported inputs * Δ tariff index (export industries) $_{t-5,t-10}$			0.007	0.008	0.007	0.008		
			[1.33]	[0.56]	[1.55]	[0.69]		
Dependence ∗ Δ domestic financial reforms $_{t-5,t-10}$							0.002	
* Common Law legal origin							[0.18]	
Dependence * Δ private credit / GDP $_{t-5,t-10}$								-0.048
								[2.84]***
Δ log output share $_{t-5,t-10}$	-0.029	-0.033	-0.028	-0.032	-0.031	-0.034	-0.029	-0.029
	[6.47]***	[5.67]***	[6.13]***	[5.35]***	[7.11]***	[6.18]***	[6.47]***	[5.55]***
country-year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Observations	24557	14026	22364	12523	33957	19805	24557	20317
R-squared	0.03	0.03	0.02	0.03	0.03	0.03	0.03	0.02

Robust t statistics in brackets, observations are clustered by country
* significant at 10%; ** significant at 5%; *** significant at 1%

PANEL B

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	full	full	full	full	full	full
Dependence * Δ domestic financial reforms $_{t-5,t-10}$	0.014	0.013	0.018			0.012
Above median constraint on the executive	[2.39]**	[1.77]*	[3.51]***			[2.51]**
Dependence * Δ domestic financial reforms $_{t-5,t-10}$	0.010	0.010	0.014			0.018
Below median constraint on the executive	[1.24]	[1.29]	[1.52]			[1.88]*
Intensity imported inputs * Δ tariff index (export industries) $_{t-5,t-10}$				0.007	0.008	0.008
Above median constraint on the executive				[1.73]*	[1.91]*	[1.91]*
Intensity imported inputs * Δ tariff index (export industries) $_{t-5,t-10}$				0.007	0.003	0.003
Below median constraint on the executive				[0.50]	[0.18]	[0.18]
Dependence ∗ Δ domestic financial reforms $_{t-5,t-10}$		0.001				
* Common Law legal origin		[0.12]				
Dependence * Δ domestic financial reforms $_{t-5,t-10}$			-0.008			
Below median days to enforce contracts			[0.85]			
Dependence * Δ domestic financial reforms $_{t-5,t-10}$					0.014	
					[3.09]***	
Δ log output share $_{t-5,t-10}$	-0.029	-0.029	-0.029	-0.031	-0.028	-0.028
	[6.45]***	[6.45]***	[6.46]***	[7.11]***	[6.12]***	[6.11]***
country-year fixed effects	yes	yes	yes	yes	yes	yes
Observations	24557	24557	24557	33957	22364	22364
R-squared	0.03	0.03	0.03	0.03	0.02	0.02

Robust t statistics in brackets, observations are clustered by country
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 11 Instrumental Variables Regressions (2SLS)

Second Stage (2SLS)	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Dependence * Δ domestic financial reforms $_{t-5,t-10}$	0.012		0.015			
	[2.65]***		[3.26]***			
Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$		0.006	0.006			
		[1.02]	[1.03]			
Dependence * Δ domestic financial reforms $_{t-5,t-10}$				0.015	0.015	
Above median constraint on the executive				[2.74]***	[3.10]***	
Dependence * Δ domestic financial reforms $_{t-5,t-10}$				0.006	0.015	
Below median constraint on the executive				[0.60]	[1.36]	
Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$					0.012	0.011
Above median constraint on the executive					[2.63]***	[2.41]**
Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$					-0.027	-0.026
Below median constraint on the executive					[1.07]	[1.08]
Δ log output share $_{t-5,t-10}$	-0.029	-0.031	-0.028	-0.029	-0.028	-0.031
	[6.51]***	[7.15]***	[6.16]***	[6.50]***	[6.16]***	[7.16]***
country-year fixed effects	yes	yes	yes	yes	yes	yes
Observations	24557	33957	22364	24557	22364	33957
R-squared	0.02	0.02	0.027	0.02	0.03	0.02
Hansen J Test	2.67	0.03	3.57	2.69	5.94	0.48
p value	0.26	0.86	0.17	0.26	0.2	0.78

Robust z statistics in brackets, observations are clustered by country
* significant at 10%; ** significant at 5%; *** significant at 1%

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Appendix
Table 7 (continued)
PANEL B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	full	developing	full	full	full	full	full	full	developing	full	developing
Dependence * domestic financial reform (t-1)	0.01293	0.0157	0.01375	0.01336	0.01329	0.0140161	0.01081			0.01465	0.01849
Above median constraint on the executive	[5.41]***	[3.76]***	[4.47]***	[4.71]***	[5.48]***	[4.98]***	[4.38]***			[5.87]***	[4.20]***
Dependence * domestic financial reform (t-1)	-0.00049	-0.00711	-0.00117	-0.00074	-0.00007	0.0005079	-0.00208			0.00146	-0.00448
Below median constraint on the executive	[0.11]	[1.10]	[0.26]	[0.16]	[0.02]	[0.13]	[0.44]			[0.33]	[0.68]
Herfindhal * constraint on the executive (t-1)	0.00615	-0.00984	0.00579	0.01544	0.00627	0.0061694	0.00407	0.01731	0.01063	0.00847	-0.00366
	[1.18]	[1.30]	[1.09]	[3.13]***	[1.20]	[1.18]	[0.72]	[4.03]***	[1.72]*	[1.56]	[0.46]
Trade Index (exporters - high dependence on imported inputs)								0.0414	0.03709	0.03329	0.02223
Above median constraint on the executive								[7.85]***	[3.39]***	[5.81]***	[1.87]*
Trade Index (exporters - high dependence on imported inputs)								0.04041	0.02434	0.03428	0.01357
Below median constraint on the executive								[3.55]***	[1.22]	[2.72]***	[0.59]
Dependence * GDP per capita (t-1)			-1.79E-07								
			[0.60]								
Dependence * creditor rights (t-1)				-0.00446							
				[2.83]***							
Dependence * Common Law legal origin					-0.01256						
					[2.26]**						
Dependence * domestic financial reform (t-1)						0.0008579					
Common Law legal origin						[0.64]					
Dependence * log days to enforce contracts							-0.0086				
							[1.67]*				
Log output share (t-1)	0.00081	-0.00141	0.0007	0.00338	0.00091	-0.0028436	-0.00013	-0.00062	-0.00166	0.00064	-0.00073
	[0.60]	[0.78]	[0.50]	[2.56]**	[0.68]	[-0.94]	[0.09]	[0.53]	[1.11]	[0.45]	[0.39]
Country-year	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Industry	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	32944	19554	32222	27267	32944	32944	28969	43488	26172	31430	18165
R-squared	0.02	0.01	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02	0.01

Robust t statistics in brackets, observations are clustered by country-year
* significant at 10%; ** significant at 5%; *** significant at 1%

Appendix
Table 11 First Stage of Instrumental Variable Regressions (2SLS)

Regression number (2nd stage)	**(1)**	**(2)**	**(3)**	**(3)**	**(4)**	**(4)**	**(5)**	**(5)**	**(5)**	**(5)**	**(6)**	**(6)**
Endogenous variable (1st stage)	A	B	A	B	AH	AL	AH	AL	BH	BL	BH	BL
Dependence * Δ domestic financial reforms $_{t-5,t-10}$	1.053		1.030	-0.004								
	[14.22]***		[13.91]***	[-1.39]								
Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$		0.996	0.018	0.995								
		[56.14]***	[1.85]*	[55.86]***								
Dependence * IMFprog $_{t-5,t-10}$	0.117		0.054	-0.002								
	[1.59]		[0.97]	[-0.6]								
Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$		0.016	-0.011	0.011								
		[0.76]	[-0.59]	[0.42]								
Dependence * Δ domestic financial reforms $_{t-5,t-10}$					1.056	-0.0002	1.045	0.0002	-0.005	0.00001		
Above median constraint on the executive					[12.5]***	[-0.71]	[12.37]***	[0.56]	[-1.49]	[0.14]		
Dependence * Δ domestic financial reforms $_{t-5,t-10}$					0.000	0.992	0.000	1.019	0.000	-0.001		
Below median constraint on the executive					[-0.66]	[7.85]***	[-0.7]	[7.62]***	[-0.15]	[-0.19]		
Dependence * IMFprog $_{t-5,t-10}$					-0.0157	0.0001	-0.0232	0.0001	-0.0023	1.03E-06		
Above median constraint on the executive					[-0.26]	[0.91]	[-0.38]	[0.94]	[-0.67]	[0.16]		
Dependence * IMFprog $_{t-5,t-10}$					0.0002	0.17	0.0002	0.13	0.0002	-0.0011		
Below median constraint on the executive					[0.99]	[2.96]***	[0.6]	[2.16]**	[1.41]	[-0.27]		
Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$							0.016	-0.0003	1.009	2.65E-06	1.010	-0.00003
Above median constraint on the executive							[1.51]	[-0.79]	[70.77]***	[0.03]	[68.3]***	[-0.39]
Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$							-0.0005	0.031	-0.0004	0.906	-0.0003	0.908
Below median constraint on the executive							[-0.75]	[1.73]*	-1.21	[13.28]***	[-1.27]	[13.73]***
Intensity imported inputs *IMFprog $_{t-5,t-10}$							-0.011	-0.001	0.045	2.04E-06	0.04105	-0.00010
Above median constraint on the executive							[-0.6]	[-1.09]	[1.22]	[0.01]	[1.23]	[-0.44]
Intensity imported inputs *IMFprog $_{t-5,t-10}$							-0.0008	-0.037	-0.001	0.007	-0.0007	0.0181
Below median constraint on the executive							[-0.78]	[-1.06]	-1.21	[0.22]	[-1.48]	[0.71]
Partial R-squared	0.62	0.85	0.62	0.85	0.62	0.65	0.6062	0.66	0.85	0.85	0.8553	0.8383
F statistics (excluded Ivs)	152.84	1567.69	94.86	783.62	64.91	28.08	39.49	21.76	660.74	50.25	1175.46	67.37

Endogenous variables:	Labels:	Endogenous variables:	Labels:
Dependence * Δ domestic financial reforms $_{t-5,t-10}$	A	Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$	B
Dependence * Δ domestic financial reforms $_{t-5,t-10}$ Above median constraint on the executive	AH	Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$ Above median constraint on the executive	BH
Dependence * Δ domestic financial reforms $_{t-5,t-10}$ Below median constraint on the executive	AL	Intensity imported inputs * Δ trade index (export industries) $_{t-5,t-10}$ Below median constraint on the executive	BL