# Capital Account Liberalization, Real Wages, and Productivity

**Peter Blair Henry**[*]
**Stanford University**
**Brookings Institution**
**NBER**

**Diego Sasson**[*]
**Stanford University**

**October 2007**

## Abstract

For three years after developing countries open their stock markets to inflows of foreign capital, the average annual growth rate of the real wage in the manufacturing sector increases by a factor of seven. No such increase occurs in a control group of developing countries that do not liberalize. The temporary increase in the growth rate of the real wage permanently drives up the level of average annual compensation for each worker in the sample by 856 US dollars—an increase equal to more than a quarter of their annual pre-liberalization salary. The increase in the growth rate of labor productivity in the aftermath of liberalization exceeds the increase in the growth rate of the real wage so that the increase in workers' incomes actually coincides with a rise in manufacturing sector profitability.

[*]Preliminary version, comments welcome. Correspondence to pbhenry@stanford.edu and dsasson@stanford.edu
We thank Olivier Blanchard, Steve Buser, Paul Romer, Ewart Thomas and seminar participants at the IMF and MIT for helpful comments.

## 1. Introduction

In the late 1980s developing countries all over the world began easing restrictions on capital flows. A decade later many of the same nations experienced a string of financial crises, triggering a debate over the relative merits of capital account liberalization as a policy choice for developing countries. Critics claim that liberalization brings small benefits and large costs (Bhagwati, 1998; Rodrik, 1998; Stiglitz, 1999). Recent work documents evidence to the contrary. Liberalization in developing countries reduces their cost of capital (Bekaert and Harvey, 2000; Henry 2003; Stulz, 2005), temporarily increases investment (Alfaro and Hammel, 2006), and permanently raises the level of GDP per capita (Gourinchas and Jeanne, 2006; Henry, 2007).

In the process of debating the costs and benefits of capital account liberalization, both critics and apologists have neglected the labor market. While it is important to understand how opening up affects prices and quantities of capital, almost two decades after the advent of capital account liberalization in the developing world, there is no systematic evidence on the behavior of wages in the aftermath of the policy change. This paper provides the first attempt to fill that gap.

Figure 1 shows that in a sample of eighteen developing countries that opened their stock markets to inflows of foreign capital between 1986 and 1993, the average annual growth rate of the real wage in manufacturing jumped from 1.3 percent per year in non-liberalization periods to an average of 9.5 percent in the year liberalization occurred and each of the subsequent two years. The temporary 8.2 percentage-point increase in the growth rate of the real wage permanently drives up the level of average annual compensation for each worker in the sample of liberalizing countries by about 856 US dollars—an increase equal to more than a quarter of their annual pre-liberalization salary.

One concern about Figure 1 is that an exogenous productivity shock having nothing to do with opening may drive up real wages in liberalizing and non-liberalizing countries alike. To distinguish the impact of liberalization from that of a common shock, our estimation procedure compares the difference in wage growth before and after liberalization to the same difference for a control group of countries that are not undergoing liberalizations. In every specification, we find an economically and statistically significant increase in real wage growth for countries in the liberalization group and no effect no effect at all for the control group.

While our difference-in-difference approach enables us to test for effects of liberalization on real wages that have previously gone unexamined, difference-in-difference estimation requires caution because the standard errors are susceptible to serial correlation. For instance, of the ninety-two difference-in-difference papers published in top economics journals between 1990 and 2000, only five explicitly deal with serial correlation (Bertrand, Duflo, and Mullainaithan, 2004). Peterson (2006) makes a similar point about panel data studies published in the top three finance journals between 2001 and 2004. The BDM critique of difference in difference estimates applies with special force in the context of the liberalization experiment examined in this paper.

Liberalizing the stock market increases investment, which in turn drives up productivity and wages. Because it takes time for wages to adjust, wage growth for a given country may remain elevated above its steady-state rate for a number of years in the post-liberalization period, thereby inducing serial correlation in the country's wage-growth residuals over time. Similarly, liberalization often occurs at the same time across countries, thereby inducing correlation in the wage-growth residuals across countries at a given point in time. Our empirical analysis uses clustering techniques to adjust the standard errors for the occurrence of both forms of

dependence in the residuals (we also adjust for heteroscedasticity). No matter what specification we use, or how we compute the standard errors, the impact of liberalization on real wage growth remains statistically significant for the treatment group and insignificant for the control group.

An additional concern is that with only eighteen countries in the sample, a few large outliers may drive the central finding. This is not the case. In the aftermath of liberalizations, a temporary increase in real wage growth occurs consistently across countries. In all but three of eighteen countries, the median growth rate of real wages in the post liberalization period exceeds the pre-liberalization median. Finally, the documented effects persist after controlling for movements in the exchange rate and the impact of economic reforms such as inflation stabilization, trade liberalization, and privatization programs.

Standard production theory provides the simplest explanation of the facts about liberalization and real wages documented in this paper. Liberalization reduces the cost of capital and firms respond by increasing their rate of investment. For a given growth rate of the labor force and total factor productivity, a higher investment rate increases the ratio of capital per effective worker, driving up the marginal product of labor, and in turn, the market-clearing wage. Consistent with this interpretation, the growth rate of labor productivity also rises sharply in the aftermath of liberalizations. The average growth rate of labor productivity is 10.3 percentage points higher during the three-year liberalization window than in non-liberalization years. Because the 10.3 percentage point increase in productivity growth outstrips the 8.2 percentage-point increase in wage growth over the corresponding period, profitability in the manufacturing sector actually increases. In other words, the rise in workers' living standards does not reduce firm value.

The rest of the paper proceeds as follows. Section 2 uses theory to generate testable predictions about liberalization and explains how we identify real-life liberalization episodes. Section 3 describes the wage data and construction of the control group. Section 4 presents descriptive findings. Section 5 discusses the formal empirical methodology, results, and alternative interpretations. Section 6 concludes.

## 2. Capital Account Liberalization in Developing Countries

This section generates empirically testable predictions about the impact of capital account liberalization in a developing country on the time-path of the real wage ($w$). To maintain congruency with previous work we employ the well-trodden framework of the neoclassical growth model, but apply it in a way that delivers previously untested theoretical predictions.

The central point about capital account liberalization is that it moves developing countries from a steady state in which their ratios of capital to effective labor are lower (and rates of return to capital higher) than in the developed world, to a steady state in which capital-to-effective labor ratios and rates of return equal those in the developed world. Because capital and labor are complements in production, the marginal product of labor (and hence the real wage) rises as countries open up and the process of capital deepening sets in.

### 2A. Theory

Assume that output is produced using capital, labor, and a Cobb-Douglas production function with labor-augmenting technological progress:

$$Y = F(K, AL) = K^{\alpha}(AL)^{1-\alpha} \qquad (1)$$

Let $k = \frac{K}{AL}$ be the amount of capital per unit of effective labor and $y = \frac{Y}{AL}$ the amount of output per unit of effective labor. Using this notation and the homogeneity of the production function we have:

$$y = f(k) = k^{\alpha} \qquad (2)$$

Also assume that a constant fraction of national income is saved each period and added to the capital stock, capital depreciates at the rate $\delta$, the labor force grows at the rate *n*, and total factor productivity grows at the rate *g*.

When the economy is in steady state $k$ is constant at the level $k_{s.state}$ and the marginal product of capital equals the interest rate (*r*) plus the depreciation rate:

$$f'(k_{s.state}) = r + \delta \qquad (3)$$

Because the impact of liberalization works through the cost of capital, equation (3) has important implications for the dynamics of $k$ and *w* in the aftermath of opening up.

Let *r** denote the exogenously given world interest rate. The standard assumption in the literature is that *r** is less than *r*, because the rest of the world has more capital per unit of effective labor than the developing country. It is also standard to assume that the developing country is small, so that nothing it does affects *r**. Under these assumptions, capital surges in to exploit the difference between *r** and *r* when the developing country liberalizes.

The absence of any frictions in the model means that the country's ratio of capital to effective labor jumps immediately from $k_{s.state}$ to its post-liberalization, steady-state level ($k^{*}_{s.state}$). In the post-liberalization steady state, the marginal product of capital equals the world interest rate plus the rate of depreciation:

$$f'\left(k^{*}_{s.state}\right) = r^{*} + \delta \qquad (4).$$

Instantaneous convergence is an unattractive feature of the model, because it implies that the country installs capital at the speed of light. There are a variety of formal ways to slow down the speed of transition (see Appendix A). For now, the vital point to note is that $\dot{k}$ is greater than 0 during the country's transition to the post-liberalization steady-state.

The temporary growth in *k* has implications for the growth rate of the real wage. Since workers are paid their marginal product of labor, $w = A[f(k) - kf'(k)]$. The growth rate of the real wage is the derivative of the natural log of $w$ with respect to time: $\frac{d}{dt}(\ln(w)) = \frac{\dot{w}}{w} = \frac{\dot{A}}{A} - \frac{kf''(k)\dot{k}}{[f(k) - kf'(k)]}$. For the case of Cobb Douglas technology this expression simplifies to:

$$\frac{\dot{w}}{w} = \frac{\dot{A}}{A} + \alpha\frac{\dot{k}}{k} \tag{5}$$

Prior to liberalization the ratio of capital to effective labor remains at the constant level, $k_{s.state}$ so that $\frac{\dot{k}}{k}$ equals 0 and $w$ grows at the same rate as total factor productivity. Since $\frac{\dot{k}}{k}$ is greater than 0 during the transition to $k^{*}_{s.state}$, the growth rate of the real wage also increases temporarily. Figure 2 illustrates the hypothetical time paths of *r* and the natural log of *k* and *w* under the assumption that the interest rate converges immediately upon liberalization but the ratio of capital to effective labor does not. The hypothetical response of the real wage to liberalization bears close resemblance to the real-life response in Figure 1. The next subsection explains how we identify the liberalization episodes used to construct Figure 1 and that we will ultimately use to systematically test the theory.

**2B. Reality**

Testing the prediction that real wage growth will rise immediately following the removal of restrictions on capital inflows requires information on capital account liberalization dates that is more precise than can generally be obtained. The capital account has many components, so trying to determine exactly when countries liberalize "the" capital account (as in the model), is one of the most difficult parts of trying to assess the economic impact of changes in capital account policy. The difficulty of determining exactly when countries liberalize causes most papers in the literature to ignore the problem (Eichengreen, 2001). Instead of asking whether *opening* the capital account has an impact on a country's growth rate (as theory clearly dictates), most published studies examine whether *openness* and long-run growth are positively correlated across countries (citations). A brief description of the data illustrates why tests of *opening* and *openness* are not equivalent.

To construct measures of openness, previously published work uses the broadest indicator of capital account policy, the IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions* (AREAER). The AREAER lists the rules and regulations governing resident and nonresident capital-account transactions in each country, a table summarizing the presence of restrictions, and a qualitative judgment on whether the country has an open or closed capital account. For the editions of AREAER published between 1967 and 1996, the summary table contains a single line (line E2) entitled, "Restrictions on payments for capital transactions." The presence of a bullet point in line E2 indicates that the country has some form of restrictions on capital account transactions. In effect, line E2 delivers a binary judgment on whether the IMF considers a country's capital account to be open or closed.

The typical study maps the qualitative information from Line E2 into a quantitative measure of openness by tallying the number of years that each country was free from restrictions.

Dividing that tally by the total number of years in the period produces a number called *SHARE*—the fraction of years over a given period that the IMF judged the country as open. For example, if a country was declared open for 15 of the 30 years from 1967 to 1996, then *SHARE* equals 0.5.

Papers that use the variable *SHARE* assess the economic impact of capital account policy by running cross-country regressions of GDP growth on *SHARE*. The problem with such regressions is that they implicitly test whether capital account policy has a *permanent* impact on growth while the theory predicts one that is *temporary*. The distinction between temporary and permanent has consequences. Cross-sectional regressions of growth on SHARE generate false conclusions about the impact of liberalization on growth (Henry, 2007).

The cross-sectional approach to measuring the impact of capital account policy on growth is equally inappropriate for estimating the impact of liberalization on wages. Because theory predicts a short-lived impact of liberalization on wages, it is not enough to know the fraction of years in which a country had an open capital account. We need to know the exact year in which the country opened up. In principle, one could look for the year in which the judgment in line E2 of the AREAER switches from "closed" to "open." The problem is that when the AREAER changes an assessment from closed to open, it provides no information on the specific component of the capital account that was liberalized. Without such information the empirical implications of a change in openness are unclear.

For example, AREAER does not indicate whether the change in judgment about openness results from an easing of restrictions on capital inflows or outflows. The distinction matters. Theory predicts that when a capital-poor country liberalizes capital inflows it will experience a permanent fall in its cost of capital and a temporary increase in the growth rate of wages. In principle, if that same developing country were to liberalize capital outflows nothing

would happen to its cost of capital, investment, or GDP.

In contrast to the previous literature, we address the complexity of identifying liberalization dates by narrowing the scope of the problem. Instead of trying to determine the date on which the entire capital account switches from closed to open, we identify the first point in time that a country liberalizes a specific component of the capital account. One example of liberalizing a specific component of the capital account is a decision by a country's government to permit foreigners to purchase shares of companies listed on the domestic stock market. Liberalizing restrictions on the ownership of domestic shares enables foreign capital to flow into a part of the country's economy from which it was previously prohibited.

Just such a policy change occurred repeatedly in the late 1980s and early 1990s, as a number of developing countries opened their stock markets to foreign investors for the first time. Removing restrictions on the ownership of domestic shares enables foreign capital to flow into a part of the country's economy from which it was previously prohibited. Relative to the broadest conception of the capital account, the easing of restrictions on foreign investment in the stock market may seem like a parochial way to define capital account liberalization. But it is precisely the narrowness of stock market liberalizations that make them useful for testing the theory. As the previous paragraphs explain, changes in broad measures of capital account openness such as the AREAER provide a very noisy measure of liberalization policy. Since measurement error reduces the statistical power of any regression, it is important to focus on policy experiments where the true variation in the data is large relative to noise.

As the closest empirical analogue to the textbook example in Section 2A, stock market liberalizations provide just such experiments (Frankel, 1994). Accordingly, in this paper we use the year in which countries first opened their stock markets to foreign investors as the empirical

counterpart to year "0" in the model of Section 2. According to Standard and Poor's Emerging Markets Database, there are 53 developing countries with stock markets. Of these fifty three, eighteen have stock market liberalization dates that are: (a) consistently used elsewhere in the literature and (b) verifiable from primary sources. Column (1) of Table 1 lists these eighteen countries and the year in which they liberalized.[1] Table 1 also presents summary statistics on the behavior of real wages in each of the eighteen liberalizing countries. The next section explains the source and construction of the wage data.

## 3. Data

The wage data come from the Industrial Statistics Database of the United Nations Industrial Development Organization (UNIDO). UNIDO provides data on total wages and salaries, total employment and output for the manufacturing sector. For a given year, wages and salaries include all payments in cash or in kind paid to employees. Payments include: (a) direct wages and salaries; (b) remuneration for time not worked; (c) bonuses and gratuities; (d) housing allowances and family allowances paid directly by the employer; and (e) payments in kind. Excluded from wages and salaries are employers' contributions on behalf of their employees to social security, pension and insurance schemes, as well as the benefits received by employees under these schemes and severance and termination pay.

Conceptually, total wages and salaries equal W*L*H, where W is the hourly wage rate, L is the stock of labor and H is total hours worked for the year. Since UNIDO provides no data on the number of hours worked or the hourly wage we divide total wages and salaries by total employment (L) to compute the average annual wage (W*H) in the manufacturing sector of each

[1] For further details about the complexities of determining stock market liberalization dates see Section 5 of Henry (2007) and the other references therein.

country. UNIDO reports the value of wages and salaries in US dollars, with the conversion from local currency made at the official nominal exchange rate. We deflate each country's nominal annual wage in US dollars by the US consumer price index (CPI ) to create a dollar-denominated real wage.

For each country in our sample, the annual wage data generally run from 1960 to 2003, with the exact dates differing by countries. After taking the difference of the natural log to compute growth rates, we have a total of 502 country-year observations with which to identify the impact of liberalization on real wage growth. Table 1 shows that the timing of liberalizations is correlated across countries, so these 502 observations are not entirely independent. For instance, liberalizations may coincide with an exogenous global productivity shock that drives up wages in all countries, irrespective of whether or not they liberalize. To address whether this is the case we select a control group of countries in the manner described below.

### 3A. Construction of the Control Group

An ideal control group would consist of developing countries that are identical to the liberalizing countries in every respect except that the control countries did not open their stock markets to foreign investment. In practice, many of the developing countries that have stock markets but never liberalized them are not appropriate control countries. The purpose of the control group is to determine whether the temporary increase in real wage growth in the liberalizing countries was driven by a global economic shock unrelated to opening up. It is therefore critical that the control group not consist of countries in such an abject state of development that real wages would not respond to an external shock, no matter how favorable.

As it turns out, the list of forty-eight countries (see Appendix B) that have stock markets

but never liberalized includes many countries at a low level of economic infrastructure such as Burkina Faso, Chad, Malawi, Sierra Leone, and Togo. For those countries on the list of forty-eight that are at a similar stage of development as the liberalizers, their demarcation as non-liberalizers raises doubts about the reliability of the classification. For instance, Jamaica is classified as having never liberalized, but in 1987 one of the authors of this article purchased stock in Jamaica (as a newly naturalized US citizen).

In the absence of an ideal set of control countries, we use the liberalizing countries as their own control group in the following way. For a given liberalizing country, we define the control group as the subset of the eighteen countries in Table 1 that did not liberalize during the window of time that begins two years before and ends two years after the given country's liberalization date. For example, Venezuela liberalized in 1990, so any country that did not liberalize between 1988 and 1992 appears in its control group. This subset consists of Chile, India, Jordan, Korea, Malaysia, Nigeria, the Philippines, Taiwan, Thailand, and Zimbabwe.

Restricting the control group to countries that did not liberalize between years [-2, -1, 0, 1, 2] makes theoretical sense. The lion's share of the impact of liberalization on wages occurs in years [0, +1, +2] (see Appendix A). Therefore, the question is whether the real wage in liberalizing countries grows faster in years [0, +1, +2] than it does in countries that did not liberalize during that time period. Countries that liberalized in years [-2, -1] are also excluded from the control group because the end of their liberalization period overlaps with the beginning of the given treatment country's liberalization period.

To the extent that liberalization has a substantial impact on the real wage beyond year [+2], the methodology will bias our results against finding significant differences between the treatment and control group. To illustrate the potential bias, consider Venezuela and four

members of its control group: Chile, Korea, Malaysia, and Thailand. Venezuela liberalized in 1990 while Chile, Korea, Malaysia, and Thailand all liberalized in 1987. This means that the third year after liberalization in those four countries is year [0] for Venezuela. If the impact of liberalization on wages in the four control countries persists into the third year after opening up, the difference between wage growth in Venezuela and the control group in year [0] will be artificially compressed.

Extending the control group restriction beyond two years would alleviate the problem of overlap, but for every given liberalizer, it would severely reduce the number of countries in the control group. In the end, we prefer to risk understating the significance of our results to using control groups that are not adequately large.

**4. Descriptive Findings**

Figure 1 dispels the concern that an exogenous global shock drives the liberalizing countries' increase in real wages. The y-axis of Figure 1 measures the natural logarithm of the real wage. The x-axis measures years relative to liberalization. The solid line plots in liberalization time the mean of the natural log of the real wage for all countries that liberalized. The dashed line plots the mean for the control group.[2] Whereas the solid line exhibits a steep positive inflection after year [0], indicating a sharp increase in the growth rate of the real wage, the dashed line remains flat as a pancake. While the flatness of the dashed line in Figure 1 suggests that a common global shock does not explain the inflection in the solid line, with only

[2] We use the control groups to construct the dashed line in Figure 1 as follows. Fix a country and its corresponding liberalization date. For each element of the liberalizing country's control group, calculate the natural logarithm of the real wage for the years in the interval [-2, -1, 0, 1, 2]. This yields a set of control-group-real-wage paths for the fixed liberalization-country. Next, for each year in [-2, -1, 0, 1, 2] calculate the mean over the entire set of all control-group-real-wage paths. Calculating these means creates a single path of the real-wage growth for the control group associated with the given liberalizing country. After repeating this procedure for each of the other seventeen liberalizing countries, we have eighteen control-group-real-wage-growth paths, one for each liberalizing country. The dashed line in Figure 1 is the average of all eighteen of these paths.

eighteen countries in the sample an important question is whether a few outliers drive the increase.

Turning from means to medians, the data in Table 1 demonstrate that this is not the case. In the first year after liberalization (year [1]) five countries experience real wage growth that falls below the median growth rate of their real wage for the entire sample period. Under the null hypothesis that liberalization years are no different than non-liberalization years, the probability of finding no more five countries below their median growth rate is 0.06. Similarly, in year [2], only five countries experience below-median annual real wage growth. Taking years [1] and [2] together, the probability of finding no more than ten episodes of below-median wage growth is 0.03.

Although the numbers in Table 1 suggest a consistent increase in real wage growth across countries, several other questions about the data remain.

First, the necessity of using annual instead of hourly wages raises a potential measurement concern. If the average number of annual hours worked per employee increases following liberalizations, then total annual compensation may rise without any change in the implied hourly wage. In other words, the rise in annual labor income (W*H) documented in Figure 1 could be the result of an increase in hours worked rather than an increase in the hourly wage rate. To interpret the impact of liberalization on total annual compensation as an increase in labor's compensation per unit of time, we need to know that the average number of hours worked does not rise significantly following liberalizations. Section 5C documents that we obtain similar results in a sub sample of countries for which we have data (from a source other than UNIDO) on hourly wages.

Second, UNIDO reports salaries and wages in US dollar terms. In countries with high inflation, the rate of depreciation of the official nominal exchange rate may not keep pace with inflation. Under such a scenario, the real exchange rate appreciates and the dollar value of wages becomes artificially inflated. Similarly, liberalization itself may lead to a real appreciation, because opening the capital account generates a surge in capital inflows that strengthens the value of the local currency vis-à-vis the dollar. If liberalizations coincide with bouts of increased real appreciation, then the temporary rise in the growth rate of the real wage illustrated in Figure 1 may mechanically reflect changes in the bilateral real exchange rate rather than any fundamental impact of capital account liberalization on the labor market. Figure 3 addresses the concern by replicating Figure 1 using wages measured in real local currency terms instead of real dollars. Since Figure 3 is virtually identical to Figure 1, indicating that the choice of currency makes little difference, the rest of the paper focuses on the real wage measured in dollars.

Third, liberalizations often coincide with major economic reforms that could have a significant impact on wages outside of any impact of liberalization. Stabilizing inflation, removing trade restrictions, and privatizing state owned enterprises are all reforms that may affect real wages through their impact on the efficiency of domestic production. Indeed, Table 2 demonstrates that the timing of these reforms makes it plausible that they, not capital account liberalization, are responsible for the increase in real wages apparent in Figure 1. The next section uses the information in Table 2 to control directly for the impact of other reforms and to address a host of other lingering concerns and alternative explanations.

## 5. Empirical Methodology and Results

We evaluate the statistical significance of the temporary increase in wage growth for the

treatment group by estimating the following panel regression:

$$\Delta \ln w_{it} = a_0 + a_1 * LIBERALIZE_{it} + a_2 * CONTROL_{it} + a_3 * TRADE_{it} + a_4 * STABILIZE_{it} + a_5 * PRIVATIZE_{it} + \varepsilon_{it} \quad (6)$$

The left-hand-side variable, $\Delta \ln w_{it}$, is the natural log of the real dollar value of annual compensation for country $i$ in year *t* minus the same variable in year *t-1*. Moving to the right-hand-side of equation (1), the constant $a_0$ measures average annual wage growth across all countries over the entire sample period. In principle, one might argue for the inclusion of country-specific fixed effects on the grounds that wage growth may vary across countries due to differences in total factor productivity growth. To investigate this possibility we estimated regressions of wage growth on a constant and seventeen country dummies. None of the country dummies were significant, so we use a single constant.

The variable $LIBERALIZE_{it}$ is a dummy variable that takes on a value of one in the year that country *i* liberalizes (year [0]) and each of the subsequent two years (years [1] and [2]). This means that the coefficient $a_1$ measures the average annual deviation of the growth rate of the real wage from its long-run mean over the three-year liberalization episode. $CONTROL_{it}$ is a dummy variable that takes on the value one for all members of country *i*'s control group during country *i*'s liberalization episode. The coefficient $a_2$ measures the extent to which an exogenous shock having nothing to do with liberalization drives up wages in the treatment group.

Equation (6) constrains the coefficient on both the liberalization and control dummies to be the same across countries. A different approach would use a seemingly unrelated regression (SUR) to generate a unique set of coefficient estimates for each country. The problem with SUR is that it has low power. SUR also requires a balanced panel, and due to missing observations, creating a balanced panel would result in discarding a number of liberalization events. Given

data limitations, the pooled cross-section time series framework is more appropriate.

The right-hand-side of equation (6) contains three additional dummy variables—*STABILIZE, TRADE,* and *PRIVATIZE*—that help to disentangle the impact of capital account liberalization from concurrent economic reforms. We treat reforms and liberalization symmetrically, constructing dummy variables that take on the value one in the year a reform program begins and each of the two subsequent years.

Turning at last to the error term, $\varepsilon_{it}$, it is important to note that the standard distributional assumptions needed for valid statistical inference will not hold in the presence of: (a) correlation of the residuals across countries within a given time period, or (b) correlation of the residuals within a given country over time. Point (a) matters because liberalizations often occur at the same time for different countries, possibly inducing correlation in the wage-growth residuals across countries at a given point in time. Point (b) matters because it takes time for wages to adjust to their new trajectory; for a given country, wage growth may remain elevated above its steady-state rate for a number of years in the post-liberalization period, thereby inducing serial correlation in the country's wage-growth residuals.

To compute standard errors that are correct, we construct clusters of residuals which allow for correlation within each cluster of observations. First, we cluster by year to produce standard errors that account for the possibility that shocks to wage growth are correlated across countries within a given year. Second, we cluster by country to produce standard errors that account for the possibility that the shocks to wage growth are correlated over time within a given country. We also report estimates that correct for heteroscedasticity.

**5A. Results**

Table 3 (Panel A) shows that the impact of liberalization on real wage growth is economically large and statistically significant. The estimate of the coefficient on the liberalization dummy ranges from 0.06 to 0.088. This means that during liberalization episodes the average annual growth rate of the typical country's real wage exceeds its long-run mean by an average of 6.0 to 8.8 percentage points per year. Every estimate of the liberalization dummy in Panel A of Table 3 is significant at the 1 percent level.

An exogenous shock to wages does not seem to drive the result because the coefficient on the dummy variable for the control group is never significant. The estimate of the coefficient on *CONTROL* ranges from -0.014 to – 0.018 and fails to attain statistical significance in every specification. An F-test confirms that the difference between the estimate of the coefficient on *LIBERALIZE* and *CONTROL* is statistically significant at the one percent level in every specification in Table 3.

Controlling for the other economic reforms that tend to accompany liberalization also does not reduce the impact of capital account opening on the growth rate of the real wage. Column (5) in Panel A of Table 3 shows that after accounting for the effects of inflation stabilization, trade liberalization, and privatization, the coefficient on *LIBERALIZE* is 0.082. Because some of the economic reforms have a significant impact on the growth rate of the real wage, we are confident in the accuracy of the reform dates and the relevance of the corresponding dummy variables as controls. For instance, the coefficient on *STABILIZE* is -0.096 and significant at the one percent level. The negative impact of stabilization programs on real wage growth is consistent with some of the literature on the real effects of inflation stabilization in developing countries.

Finally, the significance of the estimates of the liberalization dummy presented in Panel A of Table 3 is also robust to the statistical concerns we mentioned earlier. Table 4 (Panel A) presents estimates that adjust the standard errors to account for heteroscedasticity. Table 5 (Panel A) presents estimates that adjust for heteroscedasticity and cross-country correlation. Table 6 (Panel A) presents estimates that correct for heteroscedasticity and serial correlation. None of these adjustments alter the results from Panel A of Table 3 in a material way. The liberalization dummy remains statistically significant in every specification of Panel A in Tables 4, 5 and 6. Specifically, in Tables 4 and 5 the level of statistical significance of liberalization on wages occasionally falls to five percent from one percent. In Panel A of Table 6 the liberalize variable is always significant at the five or ten percent level. Because the statistical significance of the estimates of the liberalization dummy are not sensitive to the method of computing standard errors, to economize on space, we turn our attention back to Table 3 (Panel A) and the more central question of economic significance.

There are two ways to examine the economic significance of the results. First, consider the magnitude of the growth rate of the real wage during liberalization episodes relative to the growth rate of the real wage over the entire sample. To do this, use the estimate of the constant and the liberalization dummy from the regression that controls for other economic reforms (Column (5) of Panel A in Table 3). The estimate of the constant is 0.013, indicating that the real wage grows by an average of 1.3 percent per year over the entire sample. The estimate of the coefficient on the liberalization dummy is 0.082. Adding the constant and the coefficient on the liberalization dummy gives the average growth rate of the real wage during liberalization episodes—9.5 percent per year. This means that in the year the liberalization occurs and each of

the subsequent two, the average growth rate of the real wage in the typical country is more than seven times as large as in non-liberalization years.

Of course, the increase in the growth rate of the real wage is temporary, so a second way of assessing economic significance is to compute the impact of liberalization on the permanent level of the real wage. For the countries in the treatment group, the average level of annual compensation in the year before liberalization (year [-1]) is 2951 (correct this number) US dollars. During the three-year liberalization window the real wage grows at 9.5 percent per year, so that by the end of year [2] the level of the real wage is $2951\,e^{.095*3} = 3924$ US dollars. Now assume that that if the country had not liberalized the real wage would have grown by 1.3 percent per year. In that case, the end of year 2 the level of the real wage would be $2951\,e^{.013*3} = 3068$ US dollars. In other words, by the time the impact of liberalization has run its course, the average worker in the manufacturing sector has annual take home pay that is 856 dollars higher than it would have been in the absence of liberalization. This change in the level is greater than a quarter of the level of the average manufacturing worker's pre-liberalization take home pay.

**5A.1 The Impact of Liberalization on Productivity**

The response of wages to capital account liberalization is large. To scrutinize the plausibility of our estimates we cross-checked the results against data on labor productivity. The model in Section 2 demonstrates that liberalization induces capital deepening, and through the increase in capital per worker, drives up productivity, the demand for labor, and the real wage. If this chain of reasoning has any empirical bite then during liberalization episodes labor productivity should rise in concert with wages. If productivity does not rise by the same order of

magnitude as wages, then doubt arise about the mechanisms for which we argue and the credibility of our results.

Figure 3 shows that the time path of the natural log of productivity in the aftermath of liberalization looks very similar to that of the real wage (Figure 1). To test formally the relation between liberalization and the growth rate of labor productivity, we estimate the following regression:

$$\Delta \ln(\frac{Y}{L})_{it} = a_0 + a_1 * LIBERALIZE_{it} + a_2 * CONTROL_{it} + a_3 * TRADE_{it} + a_4 * STABILIZE_{it} + a_5 * PRIVATIZE_{it} + \varepsilon_{it} \quad (7)$$

Equation (7) is identical to (6) except that instead of the change in the natural log of the annual wage, the left-hand-side variable is now the change in the natural log of annual output per worker.

Panel B of Table 3 shows that liberalization has a positive and significant impact on productivity growth. The estimates of the coefficient on the liberalization dummy range from 0.055 to 0.104. Every estimate of the coefficient on the liberalization dummy in Panel B of Table 3 is significant at the one percent level. In contrast, the estimate of the coefficient on the control dummy is never significant. Taken together, these results suggest that liberalization, not an external shock or domestic economic reforms, are responsible the increase in productivity growth.

In particular, Column (5) of Panel B in Table 3 demonstrates that after accounting for the potential impact of other economic reforms, the estimate of the coefficient on the liberalization dummy is 0.104. This means that the average growth rate of productivity is 10.4 percentage points higher during the three-year liberalization window than in non-liberalization years. The 10.4 percentage point increase in productivity growth associated with liberalization is larger than

the 8.2 percent increase in wage growth. Hence the increase in productivity more than compensates for the increase in wage growth and profitability in the manufacturing sector actually rises, a fact consistent with the post-liberalization rise in the rate of return to capital documented in Chari and Henry (2007).

**5B. Discussion**

The growth rate of labor productivity rises by roughly 10 percentage points following liberalization. A few simple calculations reveal that this increase is too large to be explained solely by the increase in the rate of capital accumulation. Equation (5) shows that the growth rate of wages equals the growth rate of total factor productivity plus alpha times the growth rate of capital per worker. This means that the increase in the growth rate of wages and productivity following liberalization is equal to the increase in productivity plus alpha times the increase in capital per worker.

For realistic values of alpha, the increase in productivity is too large to be accounted for by the increase in the growth rate of capital per worker. If alpha equals one-third, the growth rate of capital per worker would have to increase by a more than 24 percentage points to account for the increase in productivity and wages. Our data set does not provide information on the size of the capital stock in the manufacturing sector, but a little reflection suggests that a twenty-four-percentage-point increase in the growth rate of capital per worker is not plausible.

The growth rate of the aggregate capital stock of liberalizing countries increases by a little more than one percentage point following liberalizations (Henry, 2003). The manufacturing sector in these economies accounts for roughly twenty percent of GDP. Assuming zero net growth in capital in agriculture and manufacturing, the largest possible

increase in the growth rate of capital in manufacturing is about five percentage points. This back-of-the-envelope calculation finds empirical support elsewhere in the literature. Using a subset of the countries in this paper, Chari and Henry (2007) calculate that the growth rate of capital in the manufacturing sector increases by about four percentage points per year following liberalizations. Multiplying four by a number between one-third and one-half generates productivity and wage growth of no larger than two percentage points, so it is clear that capital accumulation alone cannot account for the increase in productivity and wages.

Any increase in the growth rate of labor productivity unaccounted for by capital deepening must be the consequence of an increase in the growth rate of total factor productivity (TFP). Because the standard neoclassical model does not provide any channel through which capital account liberalization affects the growth rate of TFP, the question is what drives the increase. Some argue that the simplest explanation lies with the economic reforms that accompany liberalization (Henry, 2003). Economic reforms improve resource allocation, essentially producing a one-time shift in the production function that temporarily raises the growth rate of TFP. Others posit that liberalization brings new managerial and technological know-how that improves the efficiency of the domestic financial system (Mishkin, 2006). Yet another explanation is that capital account liberalization generates unspecified "collateral benefits" that increase productivity (Kose, Prasad, Rogoff, and Wei, 2006).

Sorting through competing explanations for the increase in total factor productivity following liberalizations is an important research challenge that lies beyond the scope of this paper. Whatever the principal driving factor may be, the increase in the growth rate of TFP documented here confirms (using different data) a finding that crops up consistently across studies of liberalization and the real economy.

**5C. Alternative Explanations/Robustness Checks**

We have argued that wages rise following liberalizations because of an increase in labor demand stemming from a capital-deepening induced rise in productivity. Alternatively, the increase in wages may be due as much to a reduction in labor supply as an increase in labor demand. The argument runs as follows. If the impact of liberalization on wages is perceived to be permanent, then it is as if labor market participants received a positive shock to their permanent income. Labor supply decisions may respond accordingly. Specifically, labor supply might decrease in response to the positive shock to permanent income.[3] If labor supply decreases in response to higher expected future income, then it may be the case that the observed rise in wages results from a decrease in labor supply as well as an increase in labor demand.

The data on employment are not consistent with a decrease in labor supply. Using equation (6), we replace the change in the natural log of the real wage with the change in the natural log of the stock of employment. There is no significant change in the growth rate of employment following liberalizations.

We also looked at data on hours worked. If labor supply decreases then the number of hours worked should fall. In our attempt to see whether this is the case we had to rely on data for a smaller set of countries. UNIDO does not provide data on hours worked. The International Labor Organization (ILO) data was also not helpful; the ILO's definition of hours worked is inconsistent across countries and sometimes varies across sectors within a given country. In the end we managed to get consistently constructed data on hours worked from the Groningen Growth and Development Center (GGDC) for the following countries: Argentina, Brazil, Chile, Colombia, Korea, Turkey, Taiwan, and Venezuela. Again, we replicated regression (6), this time

[3] An alternative view is that labor supply is relatively inelastic (see Pencavel 1986 on this point), so that workers do not reduce the number of hours that they want to work in response to the increase in their expected future income.

using the change in the natural log of hours worked as the left-hand-side variable. The results show that there is no significant change in the number of hours worked

The absence of a significant change in the number of hours worked also suggests that the increase in the annual real wage reported in Section 5A is not the result of an increase in hours worked, but indeed an increase in the rate of compensation per unit of time. As a final check we also used the GGDC data to construct a measure of hourly wages for the subset of countries mentioned above. Again, we replicated equation (6), changing the left-hand-side variable to the change in the natural log of the hourly wage. The results are qualitatively identical to those reported in Tables 3 through 6.

Overall, we find no obvious evidence signifying that labor supply changes in response to liberalization. While we cannot definitively exclude the possibility that part of the wage increase results from a decrease in labor supply, the alternative explanation does not seem tenable in face of the evidence. Regardless of the stand you take on the labor supply decision in developing countries, the data are consistent with the prediction that capital account liberalization drives up wages as predicted by standard models of production in which labor and capital are complements—even if we can't say how much of the increase in wages is due to a change in labor supply versus a change in labor demand

## 6. Conclusion

Opening up the stock market to foreign investment induced significant capital deepening in the manufacturing sector of liberalizing countries, driving up the productivity and real wage of manufacturing labor. This does not necessarily mean that aggregate welfare improves, because we do not have data to assess the impact of capital account liberalization on wages in other sectors of the economy. Integration into the world economy during the 1980s and 1990s

increased the ratio of skilled-to-unskilled wages in developing countries (Goldberg and Pavcnik, 2007). We cannot rule out the possibility that easing restrictions on capital inflows contributed to the widening of the gap.

Be that as it may, the results in this paper demonstrate that trade in capital has significant implications for the real economy beyond its impact on asset prices and investment. All else equal, capital account liberalization raises the average standard of living for a significant fraction of the workforce in developing countries without eroding shareholders' profitability. One can think of worse outcomes.

**References**


Alfaro, Laura and Eliza Hammel. 2007. "Capital Flows and Capital Goods," *Journal of International Economics*, 72(1), 128-150.

Bekaert, Geert and Campbell R. Harvey. 2000. "Foreign Speculators and Emerging Equity Markets," *Journal of Finance*, 55(2), 565-613.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Difference in Difference Estimates?" *Quarterly Journal of Economics* 119(1), 249-75.

Bhagwati, Jagdish. 1998. "The Capital Myth," *Foreign Affairs*, 77, 7-12.

Chari, Anusha and Peter Blair Henr. 2007. "Firm-Specific Information and the Efficiency of Investment" *Journal of Financial Economics*, Forthcoming.

Eichengreen, Barry. 2001. "Capital Account Liberalization: What Do Cross-Country Studies Tell Us?" *The World Bank Economic Review*, 16(3), 341-365.

Frankel, Jeffrey A. 1994. "Introduction" in *The Internationalization of Equity Markets*, Jeffrey A. Frankel (ed), Chicago and London: University of Chicago Press, 1-20.

Goldberg, Pinelopi and Nina Pavcnink. 2007. Distributional Effects of Globalization in Developing Countries," *Journal of Economic Literature*, 45(1), 39-82.

Gourinchas, Pierre Olivier and Olivier Jeanne. 2006. "The Elusive Gains from International Financial Integration," *Review of Economic Studies*, 73(3), 715-741.

Henry, Peter Blair. 2003. "Capital Account Liberalization, the Cost of Capital, and Economic Growth," *American Economic Review*, 93(2), 91-96.

Henry, Peter Blair. 2007. "Capital Account Liberalization: Theory, Evidence, and Speculation" *Journal of Economic Literature*, 45(4), 887-935.

Kose, Ayhan, Eswar Prasad, Kenneth Rogoff, and Shang-Jin Wei. 2006. "Financial Globalization: A Reappraisal," NBER Working Paper No. 12484.

Mishkin, Frederic S. 2006. *The Next Great Globalization: How Disadvantaged Nations Can Harness Their Financial Systems to Get Rich*, Princeton University Press, Princeton.

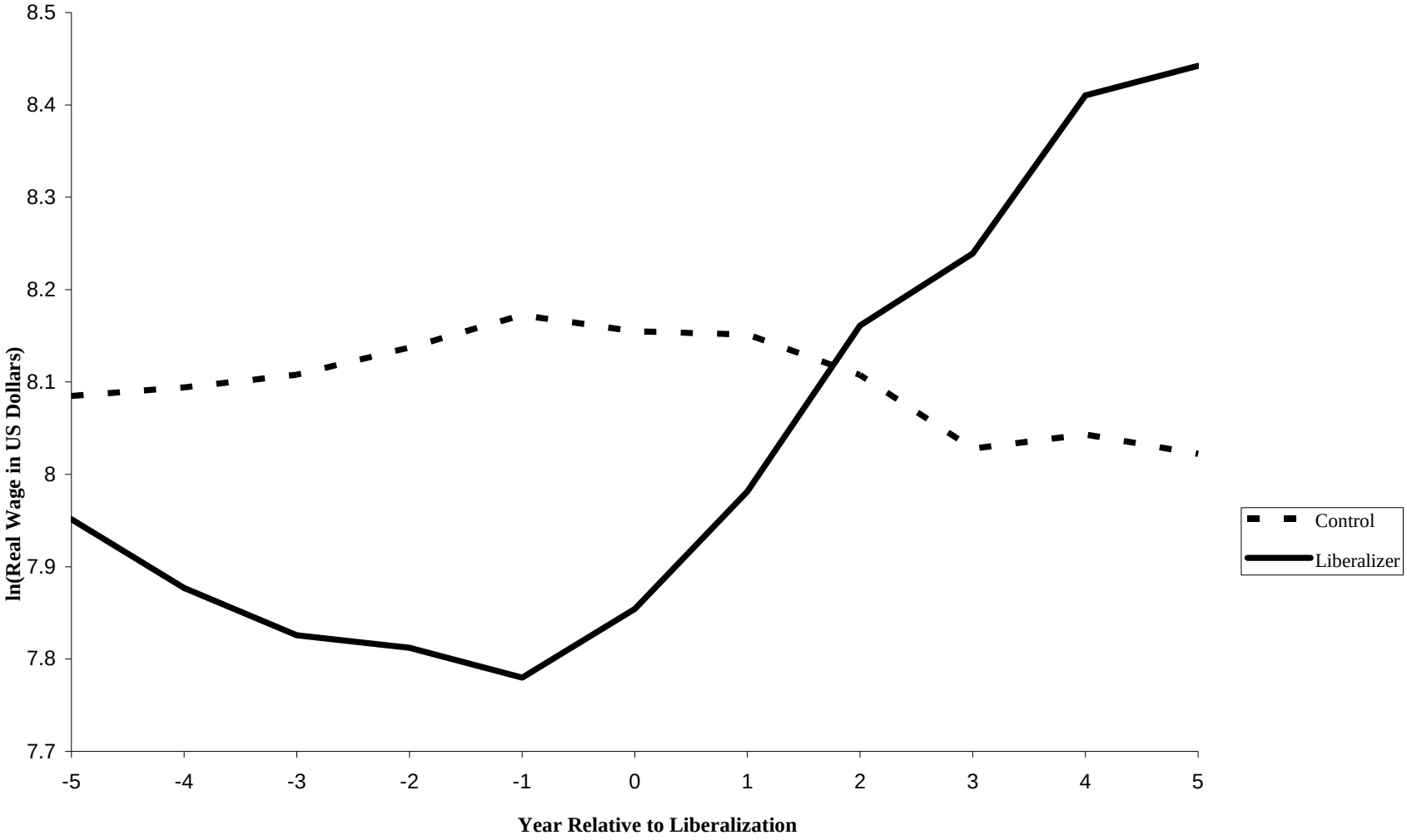
Peterson, Mitchell (2006). "Estimating Standard Errors in Finance Panel Data Sets: ComparingApproaches." Northwestern University Working Paper.

Rodrik, Dani. 1998. "Who Needs Capital Account Convertibility?" *Princeton Essays in International Finance*, 207, 55-65.

Stiglitz, Joseph. 1999. “Reforming the Global Economic Architecture: Lessons From Recent Crises,” *Journal of Finance*, 54(4), 1508-1521.

Stulz, René M. 2005. “The Limits of Financial Globalization,” *Journal of Finance*, 60(4), 1595-1638.

**Figure 1. Real Wage Growth Rises in the Aftermath of Capital Account Liberalizations**



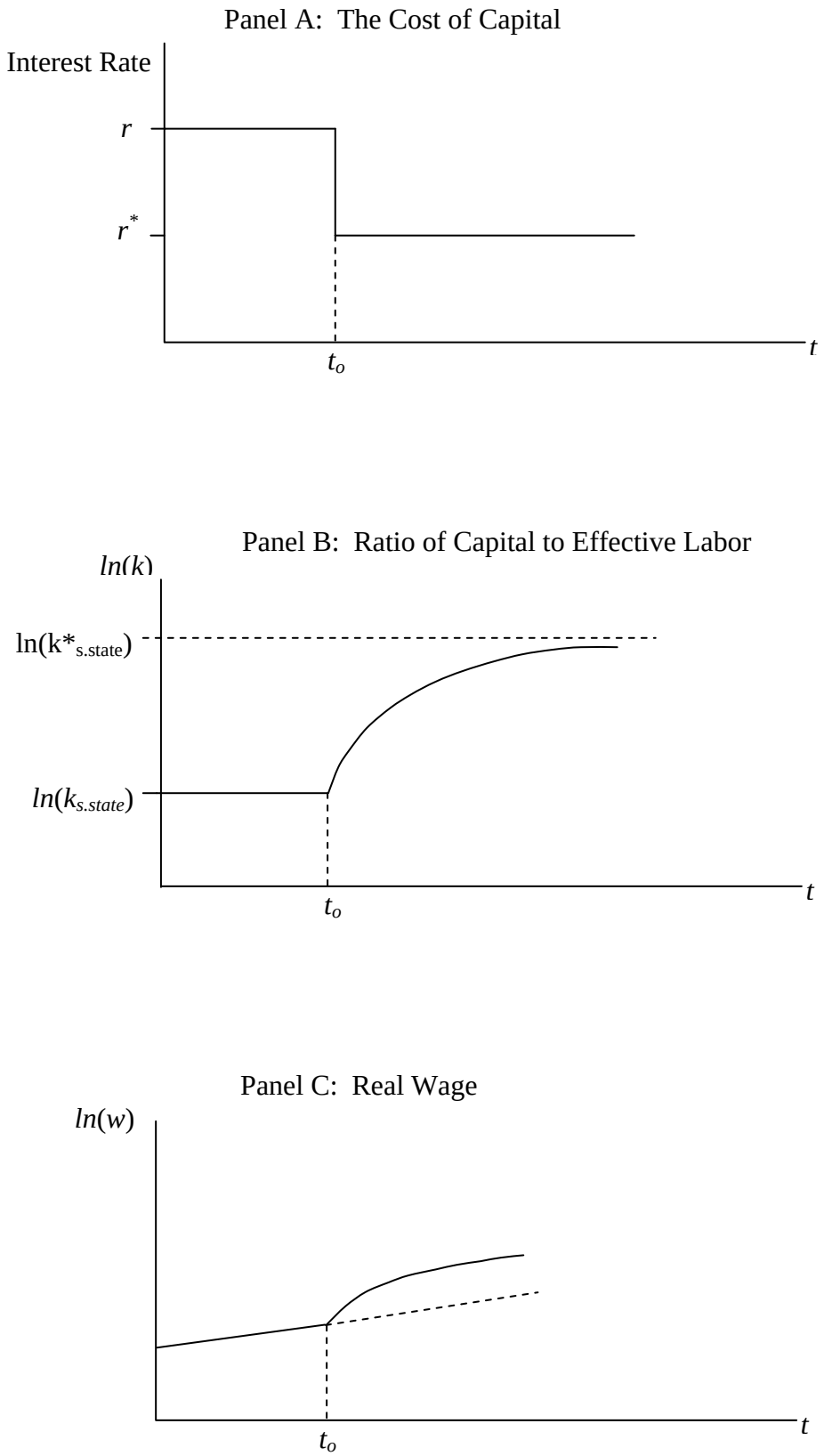



**Figure 2. The Impact of Liberalization on the Cost of Capital, Investment and the Real Wage.**

**Figure 3. Appreciation of the Real Exchange Rate Does Not Drive the Increase in Wage Growth**

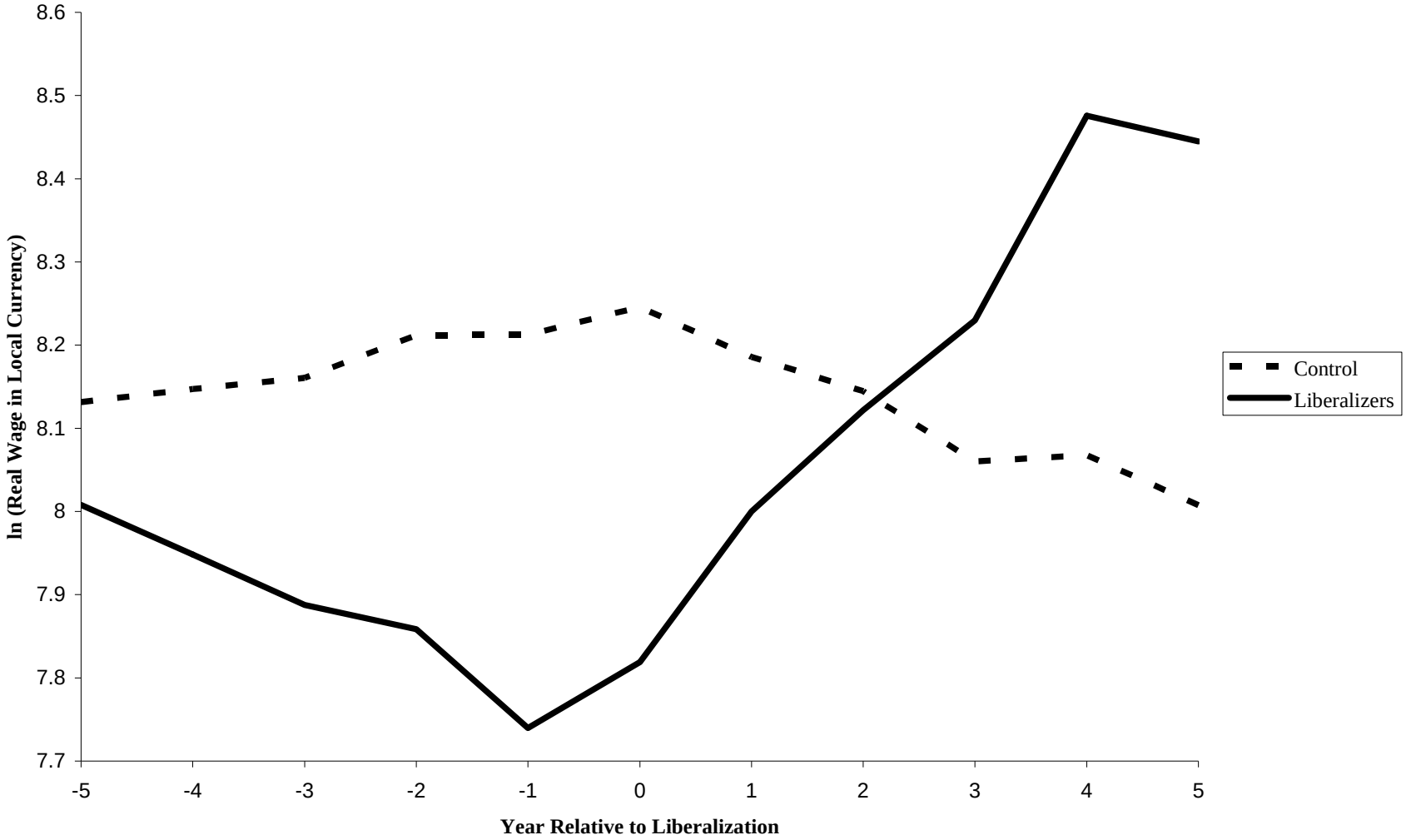


**Figure 4. The Growth Rate of Productivity Rises in the Aftermath of Capital Account Liberalizations**

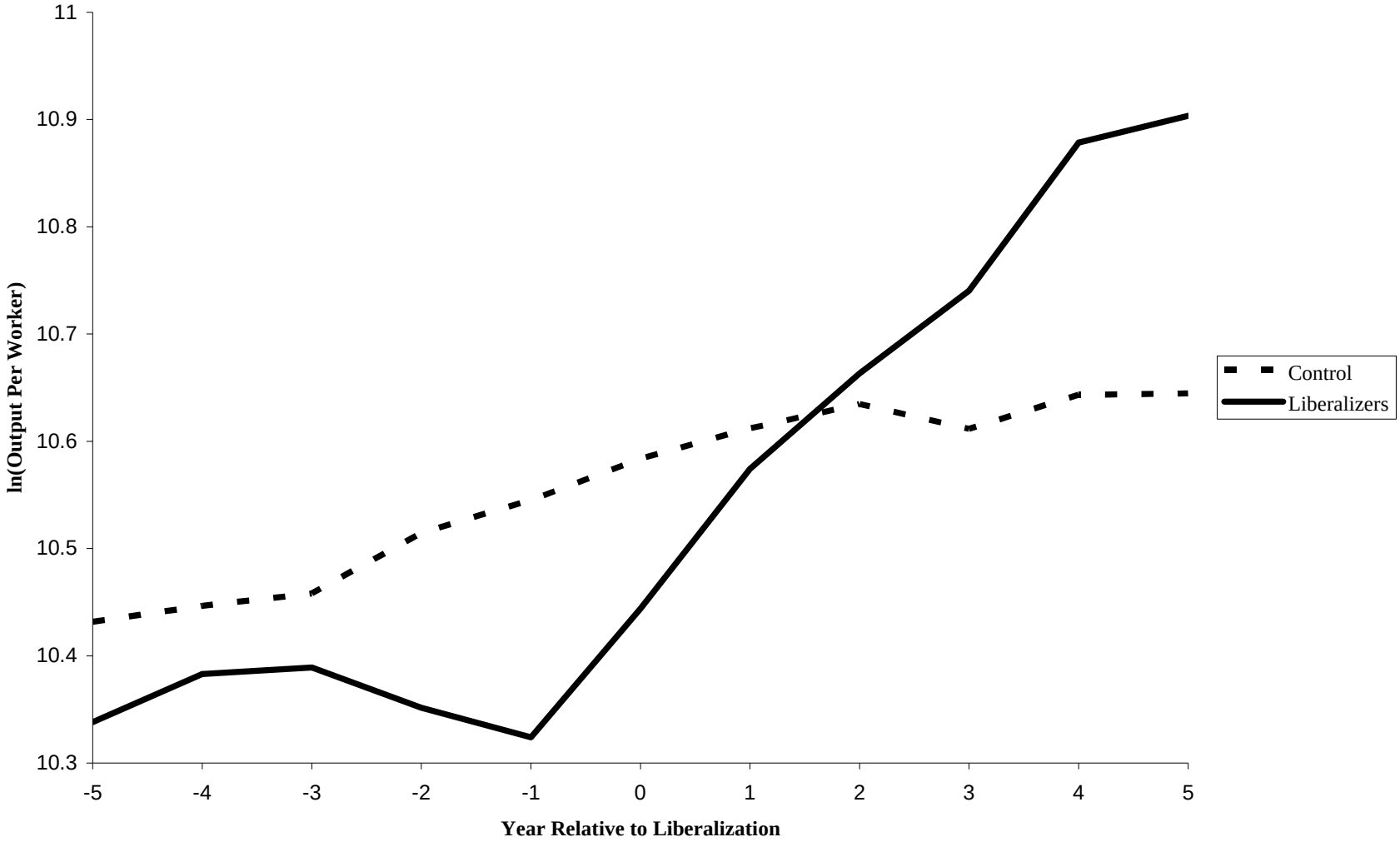


**Table 1. In the Aftermath of Liberalizations, the Growth Rate of the Real Wage Rises Consistently Across Countries.**

| Country | Year of Liberalization | 0 | 1 | 2 | Mean | Median | Standard Deviation | Years of Data Coverage |
|---|---|---|---|---|---|---|---|---|
| | | | | | | | | |
| Argentina | 1989 | -68.5% | 76.2% | N.A. | -2.6% | -2.0% | 32.6% | 1984-2001 |
| Brazil | 1988 | N.A. | N.A. | N.A. | 11.8% | 14.9% | 7.7% | 1990-1995 |
| Chile | 1987 | 1.2% | 3.0% | 4.0% | 0.9% | 4.0% | 18.8% | 1963-2000 |
| Colombia | 1991 | -1.9% | 5.9% | 11.1% | -0.4% | -2.0% | 9.1% | 1963-1999 |
| India | 1986 | 6.5% | 2.1% | -1.4% | -1.4% | 0.0% | 7.7% | 1963-2002 |
| Indonesia | 1989 | 0.8% | -29.9% | 4.1% | 5.9% | 5.3% | 16.6% | 1970-2003 |
| Jordan | 1995 | 6.5% | 2.1% | -1.4% | -0.2% | -0.3% | 14.3% | 1963-2003 |
| Malaysia | 1987 | 0.8% | -29.9% | 4.1% | 5.5% | 8.4% | 14.6% | 1963-2002 |
| Mexico | 1989 | 7.8% | -2.4% | 1.7% | 1.8% | 2.0% | 5.5% | 1968-2002 |
| Nigeria | 1995 | 18.6% | 26.0% | 25.1% | 1.5% | 9.6% | 19.3% | 1984-2000 |
| Pakistan | 1991 | -2.8% | -9.3% | -4.1% | 1.9% | 2.4% | 14.8% | 1963-1996 |
| Philippines | 1986 | 11.7% | 8.7% | 14.5% | -0.1% | 1.3% | 12.7% | 1963-1997 |
| South Korea | 1987 | 15.7% | 7.3% | N.A. | 1.2% | 2.1% | 10.9% | 1963-1996 |
| Taiwan | 1986 | 0.3% | 10.4% | 10.0% | 5.9% | 8.4% | 24.0% | 1967-1994 |
| Thailand | 1987 | -21.6% | N.A. | N.A. | 1.5% | 3.7% | 19.4% | 1963-1997 |
| Turkey | 1989 | N.A. | N.A. | 14.7% | 7.2% | 6.4% | 7.2% | 1973-1997 |
| Venezuela | 1990 | 27.0% | 39.8% | 25.8% | -3.4% | 1.2% | 22.1% | 1963-1998 |
| Zimbabwe | 1993 | 13.1% | 23.4% | 16.3% | -0.7% | -0.1% | 12.7% | 1963-1996 |
| | | | | | | | | |
| Number Negative (P-value) | NA | 7 (0.23) | 5 (0.06) | 5 (0.09) | NA | NA | NA | |

**Table 2. Stock Market Liberalizations Occur Around the Same Time as Other Major Economic Reforms**

| Country | Year of Liberalization | Stabilization Program | Trade Liberalization | Privatization | Brady Plan Debt Relief |
|---|---|---|---|---|---|
| Argentina | November 1989 | November 1989 | April 1991 | February 1988 | April 1992 |
| Brazil | March 1988 | January 1989 | April 1990 | July 1990 | August 1992 |
| Chile | May 1987 | August 1985 | 1976 | 1988 | NA |
| Colombia | December 1991 | NA | 1986 | 1991 | NA |
| India | June 1986 | November 1981 | 1994 | 1991 | NA |
| Indonesia | September 1989 | May 1973 | 1970 | 1991 | NA |
| Jordan | December 1995 | May 1994 | 1965 | January 1995 | June 1993 |
| Malaysia | May 1987 | NA | 1963 | 1988 | NA |
| Mexico | May 1989 | May 1989 | July 1986 | November 1988 | September 1989 |
| Nigeria | August 1995 | January 1991 | NA | July 1988 | March 1991 |
| Pakistan | February 1991 | September 1993 | 2001 | 1990 | NA |
| Philippines | May 1986 | October 1986 | November 1988 | June 1988 | August 1989 |
| South Korea | June 1987 | July 1985 | 1968 | NA | NA |
| Taiwan | May 1986 | NA | 1963 | NA | NA |
| Thailand | September 1987 | June 1985 | Always Open | 1988 | NA |
| Turkey | August 1989 | July 1994 | 1989 | 1988 | NA |
| Venezuela | January 1990 | June 1989 | May 1989** | April 1991 | June 1990 |
| Zimbabwe | June 1993 | September 1992 | NA | 1994 | NA |

**Table 3. Growth in Real Wages and Productivity** (Ordinary Least Squares)

| | Panel A: Real Wages | | | | | | Panel B: Productivity | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | | (1) | (2) | (3) | (4) | (5) |
| Constant | 0.0116 | 0.0107 | 0.0137 | 0.0116 | 0.0127 | | 0.019** | 0.019** | 0.021*** | 0.019*** | 0.021*** |
| | 0.0085 | 0.0085 | 0.0084 | 0.0085 | 0.0084 | | 0.0077 | 0.0078 | 0.0077 | 0.0077 | 0.0077 |
| | | | | | | | | | | | |
| LIBERALIZE | 0.063*** | 0.060 ** | 0.088*** | 0.058** | 0.082*** | | 0.056** | 0.055** | 0.084*** | 0.073*** | 0.104*** |
| | 0.0241 | 0.0242 | 0.0252 | 0.0288 | 0.0299 | | 0.0218 | 0.0220 | 0.0227 | 0.0261 | 0.0269 |
| | | | | | | | | | | | |
| CONTROL | -0.0141 | -0.0179 | -0.0099 | -0.0155 | -0.0143 | | -0.0080 | -0.0090 | -0.0031 | -0.0038 | 0.0003 |
| | 0.0153 | 0.0155 | 0.0152 | 0.0157 | 0.0159 | | 0.0139 | 0.0142 | 0.0138 | 0.0143 | 0.0144 |
| | | | | | | | | | | | |
| STABILIZE | | -0.094*** | | | -0.096*** | | | -0.108*** | | | -0.111*** |
| | | 0.0307 | | | 0.0307 | | | 0.0276 | | | 0.0277 |
| | | | | | | | | | | | |
| TRADE | | | 0.0397 | | 0.0432 | | | | 0.0099 | | 0.0171 |
| | | | 0.0299 | | 0.0298 | | | | 0.0272 | | 0.0268 |
| | | | | | | | | | | | |
| PRIVATIZE | | | | 0.0110 | 0.0038 | | | | | -0.0345 | -0.0404 |
| | | | | 0.0305 | 0.0304 | | | | | 0.0099 | 0.0273 |
| Number of Observations | 502 | 502 | 502 | 502 | 502 | | 495 | 495 | 495 | 495 | 495 |
| | | | | | | | | | | | |
| Adjusted R-Squared | 0.0165 | 0.0200 | 0.0348 | 0.0168 | 0.0293 | | 0.0104 | 0.0147 | 0.0383 | 0.0175 | 0.0392 |

**Table 4. Growth in Real Wages and Productivity** ((robust standard errors))

| | Panel A: Real Wages | | | | | | Panel B: Productivity | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | | (1) | (2) | (3) | (4) | (5) |
| Constant | 0.0116 | 0.0107 | 0.0137 | 0.0116 | 0.0127 | | 0.019*** | 0.019*** | 0.021*** | 0.019*** | 0.021*** |
| | 0.0077 | 0.0078 | 0.0078 | 0.0078 | 0.0078 | | 0.0070 | 0.0071 | 0.0071 | 0.0070 | 0.0072 |
| | | | | | | | | | | | |
| LIBERALIZE | 0.063** | 0.060** | 0.088** | 0.058** | 0.082** | | 0.056** | 0.055** | 0.084*** | 0.073*** | 0.104*** |
| | 0.0296 | 0.0295 | 0.0349 | 0.0280 | 0.0321 | | 0.0266 | 0.0272 | 0.0315 | 0.0285 | 0.0323 |
| | | | | | | | | | | | |
| CONTROL | -0.0141 | -0.0179 | -0.0099 | -0.0155 | -0.0143 | | -0.0080 | -0.0090 | -0.0031 | -0.0038 | 0.0003 |
| | 0.0163 | 0.0161 | 0.0155 | 0.0171 | 0.0162 | | 0.0143 | 0.0145 | 0.0133 | 0.0149 | 0.0138 |
| | | | | | | | | | | | |
| STABILIZE | | -0.094** | | | -0.096** | | | -0.108** | | | -0.111*** |
| | | 0.0410 | | | 0.0410 | | | 0.0418 | | | 0.0419 |
| | | | | | | | | | | | |
| TRADE | | | 0.0397 | | 0.0432 | | | | 0.0099 | | 0.0171 |
| | | | 0.0336 | | 0.0313 | | | | 0.0261 | | 0.0243 |
| | | | | | | | | | | | |
| PRIVATIZE | | | | 0.0110 | 0.0038 | | | | | -0.0345 | -0.0404 |
| | | | | 0.0328 | 0.0326 | | | | | 0.0323 | 0.0319 |
| Number of Observations | 502 | 502 | 502 | 502 | 502 | | 495 | 495 | 495 | 495 | 495 |
| | | | | | | | | | | | |
| Adjusted R-Squared | 0.0165 | 0.0200 | 0.0348 | 0.0168 | 0.0293 | | 0.0104 | 0.0147 | 0.0383 | 0.0175 | 0.0392 |

**Table 5 Growth in Real Wages and Productivity** (standard errors robust and adjusted for cross-country correlation)

| | Panel A: Real Wages | | | | | | Panel B: Productivity | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | | (1) | (2) | (3) | (4) | (5) |
| CONSTANT | 0.0116 | 0.0107 | 0.0137 | 0.0116 | 0.0127 | | 0.0190* | 0.0188* | 0.0214** | 0.0190* | 0.0210 * |
| | 0.0111 | 0.0110 | 0.0110 | 0.0112 | 0.0108 | | 0.0113 | 0.0113 | 0.0111 | 0.0113 | 0.0112 |
| | | | | | | | | | | | |
| LIBERALIZE | 0.063*** | 0.060*** | 0.088*** | 0.058*** | 0.082*** | | 0.056** | 0.055** | 0.084*** | 0.073** | 0.104*** |
| | 0.0188 | 0.0179 | 0.0229 | 0.0207 | 0.0205 | | 0.0236 | 0.0236 | 0.0270 | 0.0295 | 0.0300 |
| | | | | | | | | | | | |
| CONTROL | -0.0141 | -0.0179 | -0.0099 | -0.0155 | -0.0143 | | -0.0080 | -0.0090 | -0.0031 | -0.0038 | 0.0003 |
| | 0.0139 | 0.0140 | 0.0139 | 0.0155 | 0.0153 | | 0.0175 | 0.0175 | 0.0174 | 0.0189 | 0.0184 |
| | | | | | | | | | | | |
| STABILIZE | | -0.094** | | | -0.096** | | | -0.108*** | | | -0.111*** |
| | | 0.0402 | | | 0.0399 | | | 0.0355 | | | 0.0364 |
| | | | | | | | | | | | |
| TRADE | | | 0.0397 | | 0.0432 | | | | 0.0099 | | 0.0171 |
| | | | 0.0276 | | 0.0292 | | | | 0.0248 | | 0.0248 |
| | | | | | | | | | | | |
| PRIVATIZE | | | | 0.0110 | 0.0038 | | | | | -0.0345 | -0.0404 |
| | | | | 0.0401 | 0.0412 | | | | | 0.0326 | 0.0323 |
| Number of Observations | 502 | 502 | 502 | 502 | 502 | | 495 | 495 | 495 | 495 | 495 |
| | | | | | | | | | | | |
| Adjusted R-Squared | 0.0165 | 0.0200 | 0.0348 | 0.0168 | 0.0293 | | 0.0104 | 0.0147 | 0.0383 | 0.0175 | 0.0392 |

**Table 6 Growth in Real Wages and Productivity** (standard errors robust and adjusted for serial correlation)

| | Panel A: Real Wages | | | | | | Panel B: Productivity | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | | (1) | (2) | (3) | (4) | (5) |
| CONSTANT | 0.0116 | 0.0107 | 0.0137 | 0.0116 | 0.0127 | | 0.0190*** | 0.0188*** | 0.0214*** | 0.0190*** | 0.0210*** |
| | 0.0081 | 0.0081 | 0.0078 | 0.0081 | 0.0077 | | 0.0058 | 0.0055 | 0.0060 | 0.0057 | 0.0057 |
| | | | | | | | | | | | |
| LIBERALIZE | 0.063** | 0.060** | 0.088** | 0.058* | 0.082* | | 0.056** | 0.055** | 0.084** | 0.073** | 0.104** |
| | 0.0290 | 0.0266 | 0.0405 | 0.0274 | 0.0457 | | 0.0241 | 0.0242 | 0.0373 | 0.0293 | 0.0419 |
| | | | | | | | | | | | |
| CONTROL | -0.0141 | -0.0179 | -0.0099 | -0.0155 | -0.0143 | | -0.0080 | -0.0090 | -0.0031 | -0.0038 | 0.0003 |
| | 0.0189 | 0.0174 | 0.0190 | 0.0207 | 0.0201 | | 0.0144 | 0.0146 | 0.0127 | 0.0146 | 0.0136 |
| | | | | | | | | | | | |
| STABILIZE | | -0.094* | | | -0.096* | | | -0.108* | | | -0.111* |
| | | 0.0514 | | | 0.0523 | | | 0.0583 | | | 0.0594 |
| | | | | | | | | | | | |
| TRADE | | | 0.0397 | | 0.0432 | | | | 0.0099 | | 0.0171 |
| | | | 0.0376 | | 0.0321 | | | | 0.0253 | | 0.0179 |
| | | | | | | | | | | | |
| PRIVATIZE | | | | 0.0110 | 0.0038 | | | | | -0.0345 | -0.0404 |
| | | | | 0.0326 | 0.0333 | | | | | 0.0300 | 0.0307 |
| Number of Observations | 502 | 502 | 502 | 502 | 502 | | 495 | 495 | 495 | 495 | 495 |
| | | | | | | | | | | | |
| Adjusted R-Squared | 0.0165 | 0.0200 | 0.0348 | 0.0168 | 0.0293 | | 0.0104 | 0.0147 | 0.0383 | 0.0175 | 0.0392 |

**Appendix A**
To be written (for the main idea see Section 4.1 of Henry, 2007**)**

**Appendix B**

**Countries that never liberalized as of 1997:** Algeria, Bangladesh, Barbados, Benin, Burkina Faso, Cameroon, Central African Republic, Chad, Congo, Costa Rica, Cote D'Ivoire, Dominican Republic, Ecuador, El Salvador, Fiji, Gabon, Gambia, Ghana, Guatemala, Guyana, Haiti, Honduras, Iceland, Iran, Jamaica, Kenya, Kuwait, Madagascar, Malawi, Mali, Malta, Mauritius, Nepal, Nicaragua, Niger, Oman, Paraguay, Peru, Rwanda, Saudi Arabia, Senegal, Sierra Leone, Syrian Arab Republic, Togo, Trinidad and Tobago, Tunisia, Uruguay, Zambia.

**Countries that liberalized before 1980** Australia, Austria, Denmark, Finland, Ireland, Norway.

**Countries that liberalized between 1980 and 1997** Argentina, Brazil, Chile, Colombia, Egypt, Greece, India, Indonesia, Israel, Jordan, Malaysia, Mexico, Morocco, New Zealand, Nigeria, Pakistan, Philippines, Portugal, South Africa, Spain, Sri Lanka, Thailand, Turkey, Venezuela, Zimbabwe.

**List of Control Countries**

| Liberalization Year | Control Countries |
|---|---|
| | |
| 1986 | Argentina, Colombia, Indonesia, Jordan, Mexico, Nigeria, Pakistan, Turkey, Venezuela, Zimbabwe. |
| 1987 | Colombia, Jordan, Nigeria, Pakistan, Zimbabwe, Venezuela. |
| 1988 | Colombia, Jordan, Nigeria, Pakistan, Zimbabwe |
| 1989 | India, Jordan, Nigeria, Philippines, Taiwan, Zimbabwe |
| 1990 | Chile, India, Jordan, Korea, Malaysia, Nigeria, Philippines, Taiwan, Thailand, Zimbabwe |
| 1991 | Brazil, Chile, India, Jordan, Korea, Malaysia, Nigeria, Philippines, Taiwan, Thailand |
| 1993 | Argentina, Brazil, Chile, India, Indonesia, Korea, Malaysia, Mexico, Philippines, Taiwan, Thailand, Turkey, Venezuela |
| 1995 | Argentina, Brazil, Chile, Colombia, Indonesia, Korea, Malaysia, Mexico, Pakistan, Philippines, Taiwan, Thailand, Turkey, Venezuela |