Capital Market Integration and Wages

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Abstract

For three years after the typical emerging economy opens its stock market to inflows of foreign capital, the average annual growth rate of the real wage in the manufacturing sector increases by a factor of three. No such increase occurs in a control group of countries. The temporary increase in the growth rate of the real wage drives up the level of average annual compensation for each worker in the sample by 434 US dollars—an increase equal to nearly one-fifth 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. Overall, the results suggest that trade in capital may have a larger impact on wages than trade in goods.

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1. Introduction

The impact of trade on wages occupies a salient space in the collective imagination of the economics profession. When a country opens up to trade with the rest of the world, income shifts away from that country's scarce factor of production and toward the one which is abundant (Stolper and Samuelson, 1941). Inspired by the celebrated Stolper-Samuelson Theorem, economics journals abound in articles examining the extent to which trade induces factor price equalization.

The evidence so far is mixed. The consensus view suggests that trade with developing countries is, at best, a modest force behind the large decline in the relative wages of low-skilled workers in rich countries (Krugman, 1995; Lawrence and Slaughter, 1993; Cline, 1997; Lawrence, 2008).[1] In the case of workers in developing countries, the evidence actually runs contrary to the theory. Whereas Stolper Samuelson predicts that trade with rich countries will increase the relative wages of low-skilled workers in poor countries, trade liberalization during the 1980s and 1990s actually increased wage inequality in the developing world (Goldberg and Pavcnik, 2007).

Moving from trade in goods to trade in factors, an extensive literature also exists on the impact of labor flows on wage inequality. Again, the results are mixed. Some studies find that immigration from developing countries exacerbates wage inequality in the U.S. (Borjas, Freeman and Katz, 1997). Others find little to no effect (Card, 2009, Ottaviano and Peri, 2008).

While many studies examine the impact of cross-border flows of goods and workers on relative wages, the literature pays far less attention to the impact of cross-border financial flows on the absolute level of wages. This is surprising for at least three reasons.

First, trade in capital between nations has implications for real wages that are every bit as

[1] Feenstra and Hanson (2003) provide a dissenting view.

salient as cross-border movements of goods and people. In emerging economies where capital is scarce and labor abundant, opening up to free trade in capital should reduce the rental rate and increase the real wage.[2]

Second, examining the absolute level of wages provides information about the impact of opening up on the distribution of income between capital and labor that is just as important as the information that studies of wage inequality provide about the distribution of labor income between high and low-skilled workers. For instance, many emerging economies experienced unprecedented increases in national income as a result of globalization in the 1980s and 1990s. If all of the income gains from globalization accrued to capital, then the rise in wage inequality documented by Goldberg and Pavcnik (2007) necessarily implies that low-skilled workers experienced income losses. On the other hand, if total labor income grew in line with (or faster than) the economy as a whole, then high skilled workers may have experienced income gains that did not result in losses for low skilled workers.

Third, in the late 1980s emerging economies all over the world began easing restrictions on capital inflows of all kinds, giving economists a series of before-and-after experiments with which to study the impact of factor flows on factor rewards. Two decades later, we still have no systematic evidence about the impact of this sea-change in policy on the average level of wages in these countries. This paper provides the first systematic attempt to fill that gap.

Figure 1 demonstrates that in a sample of twenty five emerging economies that liberalized restrictions on inflows of foreign capital between 1986 and 1996, the average annual growth rate of the real wage in manufacturing jumped from 1.8 percent per year in non-

[2]Aitken, Harrison, and Lipsey (1996), Almeida (2007), and Hale and Long (2008) examine foreign direct investment (FDI) and wages, but do not explore the general connection between financial flows and wages vis-à-vis the cost of capital. The FDI model of Feenstra and Hanson (1997) has a role for the cost of capital, but focuses on the impact of outsourcing on relative wages. Their analysis is also limited to the case of Mexico whereas this paper looks at a cross-section of 25 countries.

liberalization periods to an average of 5.7 percent in the year liberalization occurred and each of the subsequent three years. The 3.9 percentage-point increase in real wage growth drives up the level of average annual compensation for each worker in the sample of liberalizing countries by about 434 US dollars—an increase equal to eighteen percent of their annual pre-liberalization salary.

One concern about Figure 1 is that an exogenous world-wide shock unrelated to capital market opening may drive up real wages in liberalizing and non-liberalizing countries alike. To distinguish the country-specific impact of liberalization policy from that of a common shock, our estimation procedure compares the difference in wage growth before and after liberalization for a group of countries that open up, to the same difference for a group of control countries. Our regressions also include year fixed effects, to account for the possibility of common shocks that affect only the liberalizers, and country fixed effects to allow for differences in underlying unobservable factors that drive wage growth across countries. We also control for the impact of contemporaneous macroeconomic reforms such as inflation stabilization, trade liberalization, privatization and Brady Plan debt relief programs. In every specification, we find an economically and statistically significant increase in real wage growth for countries in the liberalization group relative to the control group.

An open economy interpretation of the Solow growth model provides the cleanest qualitative explanation of the new facts we uncover. Opening up to capital inflows reduces the rental rate in developing countries, and firms respond by increasing their rate of investment. For a given growth rate of the labor force and total factor productivity, a higher rate of investment 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, Figure 2 demonstrates that

the growth rate of labor productivity also rises sharply in the aftermath of liberalizations. After controlling for other factors, the average growth rate of labor productivity is 9.72 percentage points higher during the four-year liberalization window than in non-liberalization years.

From a quantitative perspective, however, it is less clear whether the Solow model captures all relevant features of the data. In the standard growth model, capital account liberalization works strictly through its impact on capital accumulation and has no effect on the growth rate of total factor productivity (Gourinchas and Jeanne, 2006). Without faster growth in total factor productivity, the increase in real wage growth present in the data is too large to be explained exclusively by capital deepening under conventional assumptions about capital shares and the elasticity of substitution. On the other hand, liberalizations increase the quantity of capital goods that emerging economies import from industrial nations (Alfaro and Hammel, 2007). If technology diffuses from developed to emerging economies through the technology embodied in capital goods imports à la Eaton and Kortum (1999, 2001a, b), then liberalizations may indeed drive up the growth rate of total factor productivity.

While our difference-in-difference approach enables us to test previously unexamined real-wage implications of capital market opening, difference-in-difference estimation requires caution because the standard errors are susceptible to serial correlation (Bertrand, Duflo, and Mullainaithan, 2004). Opening up to foreign capital increases investment, which in turn drives up productivity and wages. Because wages take time to adjust, wage growth for a given country may remain elevated above its steady-state rate for a number of years after opening, thereby inducing serial correlation in the country's wage-growth residuals. Similarly, many countries open up at approximately the same time, possibly inducing cross-country correlation in the residuals. Our empirical analysis uses the Petersen (2009) technique to simultaneously adjust the

standard errors for the potential presence of both types of correlation in the residuals. No matter how we compute the standard errors, the impact of capital market opening on wages and productivity remains economically and statistically significant.

The potential endogeneity of the liberalization decision also raises some concerns. If profit-maximizing firms in a financially closed economy face the prospect of rapidly rising labor costs they will want to substitute capital for labor. To the extent that financial opening would reduce their cost of capital, these firms have an incentive to lobby the government to do so. If rising wages cause governments to open up then our estimates will spuriously indicate a strong impact of liberalization on wages, when causation in fact runs the other way round. While theoretically plausible, the endogeneity argument has no empirical support. Figure 1 is not consistent with the view that capital market opening occurs in response to rising labor costs. If anything, wage growth actually falls slightly prior to the opening. Section 4C shows that mean reversion à la Ashenfelter (1978) does not drive our results. The data are also not consistent with the explanation that governments liberalize in anticipation of higher future labor costs. Although wages rise sharply in the aftermath of liberalization, labor productivity rises even faster, so unit labor costs do not increase.

Finally, with only twenty five countries in the sample, one may worry that a few large outliers drive the central finding. This is not the case. Sign tests show that the median growth rate of real wages in the post-liberalization period exceeds the pre-liberalization median too often to be explained by chance.

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 data, construction of the control group, and presents preliminary findings.

Section 4 discusses the empirical methodology, main results, and alternative interpretations. Section 5 examines the consistency of the results with the theory. Section 6 concludes.

2. Capital Market Integration in Emerging Economies

This section uses an open economy version of the Solow model to generate previously untested predictions about the impact of capital flows on the time-path of the real wage (w). The central theoretical point about capital market integration is that it moves emerging economies 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 industrialized world, towards a steady state in which ratios of capital-to-effective labor and rates of return are equal in both regions.

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. This fundamental insight about capital flows and the dynamic path of wages would also hold in an open economy Ramsey model. Since the focus of the paper is on wages, not other variables (e.g., the current account) that depend on endogenous savings decisions, the Solow model provides the most concise exposition.

2A. Theory

Assume that the country produces output using capital, labor, and a constant-returns-to-scale production function with labor-augmenting technological progress:

$$Y = F\left(K, AL\right) \tag{1}$$

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:

$$y = f(k) \tag{2}$$

Also assume that the country saves a constant fraction of national income each period and adds it to the capital stock, capital depreciates at the rate δ, 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:

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

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:

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

Instantaneous convergence implies that interest rates equalize immediately and that the country installs capital at the speed of light. Two remarks about this unattractive feature of the model are

in order.

First, instantaneous capital market convergence is not an artifact of the Solow model, but of the small open economy assumption under which liberalization gives the country access to an infinitely elastic supply of capital at the world interest rate. The same counterfactual phenomenon would also occur in an open economy Ramsey model. Second, although we do not see equalization of interest rates and capital-labor ratios across countries in the real world, a large literature documents that the cost of capital drops and investment booms when developing countries remove barriers to capital inflows.[3]

There are a variety of formal methods for slowing down the speed of transition (e.g., adjustment costs of capital installation), but all of these methods would belabor the exposition without altering the model's fundamental prediction.[4] The vital point is that $\dot{k}$ is greater than 0 during the country's transition to its post-liberalization steady-state. The temporary growth in k has important implications for the time path of real wage growth, which we now derive.

The growth rate of the real wage is the derivative of the natural log of w with respect to time, that is, $\frac{\dot{w}}{w} = \frac{d}{dt}(\ln w(t))$. Since workers are paid their marginal product of labor, $w = A[f(k) - kf'(k)]$. This means that the growth rate of the real wage is given by:

$$\frac{\dot{w}}{w} = \frac{d}{dt}(\ln(w(t)) = \frac{d}{dt}\{A[f(k) - kf'(k)]\} = \frac{\dot{A}}{A} - \frac{kf''(k)\dot{k}}{[f(k) - kf'(k)]}.$$

We may write this expression as

$$\frac{\dot{w}}{w} = \frac{\dot{A}}{A} + \frac{1}{\sigma} * \frac{f'(k)k}{k} * \frac{\dot{k}}{k} \qquad (5)$$

[3] See Henry (2007), Stulz (2005) and the references therein.
[4] See chapter 2 of Barro and Sala-i-Martin (1995) and Section 4.1 of Henry (2007).

where $\sigma = -\frac{f'(k)[f(k) - kf'(k)]}{f(k)f''(k)k}$ is the elasticity of substitution.

The right-hand-side of Equation (5) demonstrates that the growth rate of the real wage depends on the sum of two terms. The first term, the growth rate of total factor productivity ($\frac{\dot{A}}{A}$), is not affected by capital account policy in the canonical version of the neoclassical growth model (Gourinchas and Jeanne, 2006). In Section 5 we discuss the implications of recent work that adopts a more catholic view of the relationship between capital account liberalization and total factor productivity. For now, we proceed as though the impact of liberalization works strictly through the second term, which is the product of the inverse of the elasticity of substitution ($\frac{1}{\sigma}$), capital's share in national income ($\frac{f'(k)k}{f(k)}$), and the growth rate of the ratio of capital per unit of effective labor ($\frac{\dot{k}}{k}$).

Prior to liberalization, the ratio of capital to effective labor is constant at the level $k_{s.state}$, so that $\frac{\dot{k}}{k}$ equals 0, and w simply 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 3 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.

Again, previous work documents that the actual responses of the cost of capital and the quantity of capital to liberalization resemble their hypothetical time paths. Figure 1 demonstrates

that the growth rate of the real wage also behaves in accordance with the theory. In Section 5 we examine whether the size of the real wage increase is consistent with the magnitude of the previously documented increases in the growth rate of capital. The next subsection explains how we identify the real-life liberalization episodes used to construct Figure 1.

2B. Reality

An ideal test of the prediction that real wage growth will rise following the removal of restrictions on capital inflows requires information on capital account liberalization dates that is more precise than one can generally obtain. In theory, opening the capital account is as simple as pulling a lever. In reality, the capital account has many components, so trying to determine exactly when a country liberalizes (as in Section 2A) is not a trivial task. In fact, the difficulty of determining precise liberalization dates 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. A brief description of the data typically employed in such studies 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 available, 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 cross-sectional regressions of growth on SHARE is that they have little statistical power to distinguish the transitional impact of liberalization on growth from the noise in the data and can easily generate spurious conclusions (see Henry, 2007, p, 893). The cross-sectional regression approach is equally ineffectual for estimating the impact of liberalization on the growth rate of 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 its assessment of a country from closed to open, it does not indicate whether the change in judgment 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 country were to liberalize capital outflows nothing would happen to its cost of capital, investment, or GDP.[5]

In contrast to the previous literature which makes no attempt to find periods of opening, this paper identifies liberalization dates using the point in time when countries first permitted foreigners to purchase shares of companies listed on the domestic stock market. Relative to the most general conception of the capital account, the lifting of restrictions on foreign investment in the stock market may seem like a narrow way to define capital account liberalization. But for reasons we now explain briefly, the stock market liberalizations that took place in the late 1980s and early 1990s provide a credible proxy for the broader easing of restrictions on capital inflows.

In many emerging economies, the economic activity of firms listed on the stock-market accounts for a relatively small fraction of GDP. This observation forces harder thinking about the validity of the empirical link between liberalization and real investment. In doing so, an important point to keep in mind is that stock market liberalizations serve as observable *de facto* indicators of harder-to-pinpoint *de jure* policy changes.[6] For instance, the establishment of a country fund is the modal means through which countries first liberalize the stock market (see Table 1). If country-fund establishment dates are valid proxies for the occurrence of broader, undocumented liberalizations, then significant quantities of capital that are not associated with any particular country fund may flow in to the country at the time of liberalization. Three facts suggest that closed-end country fund establishment dates provide non-specious indicators of a larger move towards open capital markets.

[5] For discussions of why this may not occur in practice, see Alfaro, Kalemli-Ozcan, and Volosovych (2008) and Shleifer and Wolfenzon (2002).
[6] See Kose et al. (2006) for a detailed discussion of de facto versus de jure indicators.

First, a steady stream of country funds and issuances of American Depository Receipts typically follow initial stock market liberalizations (Karolyi, 2004; Gozzi, Levine and Schmukler, 2007). For example, Chile liberalized its stock market in May 1987 through the *Toronto Trust Mutual Fund*, a Canadian closed-end fund with a net asset value of 37.7 million US dollars.[7] Six additional country funds with a cumulative net asset value of 991.8 million dollars were established in Chile between 1987 and 1992.[8] Beyond Chile, aggregate net equity inflows to emerging equity markets rise sharply following the median date of country-fund openings (Henry, 2007). In principle, when capital flows into the stock market, the economy as a whole need not experience net capital inflows, because foreign investors could be swapping debt for equity, or the country could experience debt outflows that are larger than the inflow of portfolio equity. In practice, however, the epoch of stock market liberalization coincided with a period of strong net capital inflows to developing countries (Calvo, Leiderman, and Reinhart, 1996).

Second, with the sole exception of Malaysia during the Asian Crisis of 1997-98, none of the stock market liberalization dates from Table 1 were followed by a reversal of freedom of foreign access. Together with the fact about country funds, this second fact confirms that stock market liberalizations signify the beginning of a steady march toward greater freedom of capital inflows.

Third, stock market liberalizations coincide with a significant increase in capital goods imports. In a sample of twenty-five countries that liberalized their stock markets between 1980 and 1997, liberalization leads to a 6-percent increase in capital goods as a fraction of total imports, and the share of total machine imports to GDP rises by 12 percent (Alfaro and Hammel,

[7] See Park and Van Agtmael (1993), Price (1994), and Wilson (1992).
[8] See Park and Van Agtmael (1993), Price (1994), and Wilson (1992).

2007). Because developing countries do not produce a significant portion of the capital goods they use, the observation that imports of capital goods rise in concert with the advent of portfolio equity inflows increases confidence in earlier work on liberalization and aggregate investment.

All of the evidence suggests that stock market liberalizations provide the closest empirical analogue to the textbook example in Section 2A. [9] Accordingly, we use the stock market liberalization dates in Table 1 as the empirical counterpart to year "0" in the model of Section 2A. According to Standard and Poor's Emerging Markets Database, there are 53 emerging economies with stock markets. Of these 53 countries, 25 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 twenty five countries and the year in which they liberalized.[10] Table 2 presents summary statistics on the behavior of real wages in each of the twenty five 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

[9] See Frankel (1994) for similar arguments.
[10] For further details about the complexities of determining liberalization dates see Section 5 of Henry (2007).

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 ($W*H$) of each worker in the manufacturing sector of each country (more on this point in Section 3B.1). UNIDO reports the value of wages and salaries in local currency terms. We deflate each country's nominal annual wage in local currency by the local consumer price index (CPI) to create a local currency-denominated real wage.

In addition to information on wages, employment, and output, we would like to have data on the manufacturing capital stock. Unfortunately, UNIDO only provides data on investment. The standard approach to an absence of capital stock data converts investment flows to capital stocks with the perpetual inventory method by making assumptions about the initial level of capital in some year and using the investment flows to interpolate the subsequent time path of the capital stock.[11]

Interpolation is methodologically sound when the focus is on long-run relationships where assumptions about the initial stock of capital make little difference. In contrast, this paper focuses on short-run dynamics and therefore requires a clear picture of the trajectory of the capital stock during the liberalization window. Simply put, it would be inappropriate to interpolate the growth rate of the capital stock during liberalization episodes when trying to measure the impact of liberalization on capital stock growth. Moreover, the UNIDO data set is missing more than 50 percent of the country-year observations for investment in the aggregate

[11] See, for example, Schoar (2002).

manufacturing sector, and many of these missing observations fall within the liberalization window. In the absence of reliable capital stock data, we will use (in Section 5) estimates of capital stock growth from previously published work to check the consistency of our results with the theoretical channel from capital growth to wages.

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 758 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 758 observations are not 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 it is the case that an exogenous shock unrelated to opening up drives the temporary increase in real wage growth, we select a control group of countries in the following manner. The control group consists of developing countries that are similar to the liberalizing countries in every respect except that the control countries did not open their stock markets to foreign investment. Appendix A provides a list of forty-eight countries that have stock markets but never liberalized. These forty-eight nations comprise the control group against which we compare the real wage growth of the countries in our treatment group.

3B. Descriptive Findings and Data Concerns

Figure 1 exhibits a steep positive inflection after year [0], indicating a sharp increase in the growth rate of the real wage, however, with only twenty five countries in the sample an important question is whether a few outliers drive the increase.

The descriptive statistics in Table 2 suggest that this is not the case. Only four countries have mean growth rates in the aftermath of liberalization that do not exceed their full sample mean (column 2). Turning from means to medians for each country in the sample, we also performed simple Wilcoxon sign rank tests on the data for each country. The sign-rank tests the equality of matched pairs of observations by using the Wilcoxon matched-pairs signed-ranks test (Wilcoxon 1945). The null hypothesis is that both distributions (pre- and post-liberalization) are the same. In the three years following liberalization five countries experience median real wage growth that falls below the median growth rate of their real wage in the pre-liberalization period. Under the null hypothesis that liberalization years are no different than non-liberalization years, using a sign test the probability (p-value) of finding no more than five countries below their median growth rate is 0.0015.

Table 2 (Panel A) shows the following quantities for each country: a) the log wage change from event year [0] to [3] for each country's liberalization episode (column 1), b) the liberalization log wage change for each country expressed relative to the country's mean log wage change from [-3] to [0] (column 2), c) the liberalization log wage change for each country expressed relative to the contemporaneous mean log wage change for the control group countries over the liberalization window [0] to [3] (column 3), and d) the difference-in-difference that subtracts the difference of the real log wage change between the treatment countries and the control group in the pre-liberalization window ([-3],[-1]) from the quantity in column 3 (column

4).

The average of the cumulative log wage change for the treatment group during the liberalization window of years [0] to [3] is 15.8% (column 1), relative to the pre-liberalization window of [-3] to [-1] is 19.1% (column 2), relative to the control group during the liberalization window is 26% (column 3) and the average difference-in-difference cumulative log wage change is 20.2% (column 4).

For each of a) to d), from columns 1-4, we test whether the mean differs significantly from zero and report a heteroscedasticity-consistent estimate of the standard error in Table 2, Panel B. These simple tests support the claim that liberalization leads to large effects on the level of real wages, lending credence to the more sophisticated regression results that follow.

Table 3 also presents coefficient estimates on the liberalization dummy over time or the time profile of the impact of liberalization on the growth rate of real wages. Panel A of Table 3 presents estimates of one coefficient only, but allowing progressively more periods after the reform (from 2 up to 5). For example the coefficient on DUMMY03 estimates the average effect of liberalization on real wage growth in years [0], [1], [2], and [3]. The coefficient estimate of 0.0369 suggests that the average real wage growth in the four years following liberalization is 3.69% or an overall growth of 14.76% (3.69*4). The average estimated effect of liberalization on wage growth differs between specifications. Consistent with theory, the average effect becomes progressively smaller as we add more years subsequent to the liberalization window. We also estimated the coefficient estimates on the liberalization dummy over the [0, 2], [2, 4], [4, 6] event windows. However, the interpretation of the statistical difference of the coefficients across the different windows is difficult given the overlapping nature of the event windows.

Panel A also presents an estimate of the "Ashenfelter" dip. A "pre-liberalization" dummy that takes value of one for the three years before the liberalization shows that the pre-

reform dip in wage growth (2.37%) is smaller and less significant than the post-reform average real wage growth of 3.69% in the four years following liberalization.

Panel B presents the coefficient estimates on the liberalization dummy by individual year ranging from the liberalization year [0] up to five years following the liberalization [5]. Columns 2 and 3 indicate that the impact of liberalization on real wages is positive and significant in both the first [1] and second [2] years following the liberalization. The coefficient estimates on the liberalization dummy are not significant in years [3] to [5]. The estimates in years [1] and [2] are significantly different from the coefficients in the other years.

Although the numbers in Tables 2 and 3 suggest a reasonably consistent increase in real wage growth across countries, two other questions about the data remain.

3B.1 Hours Worked

First, the necessity of using annual data 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 average 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.

In an attempt to address this concern we ran into non-trivial constraints that forced us to rely on data for a subset of the countries in our sample. UNIDO does not provide information on hours worked. Data available from the International Labor Organization (ILO) is also not

helpful because the ILO's definition of hours worked is inconsistent across countries. Within a given country, the ILO's definition of hours worked sometimes varies across sectors and over time as well. In the end, we used data provided by the Groningen Growth and Development Center (GGDC) because GGDC seemed to take most seriously the problems associated with trying to construct a consistent cross-country measure of hours worked. In their own words, the GGDC's estimates of hours worked are based on "…a country-by-country…judgment of which sources made the most appropriate adjustments to achieve the preferred concept of actual hours worked per person employed" (http://www.ggdc.net/databases/ted.htm). The GGDC data include paid overtime and exclude paid hours that are not worked due to sickness, vacation and holidays.

Specifically, the data on hours worked come from GGDC's Total Economy Database, which extends the work of Angus Maddison (1980). Although the Total Economy Database contains annual numbers on GDP, population, employment, hours and productivity for about 125 countries from 1950 to 2008, the series on hours worked per person are available for only 43 countries. Twelve of those 43 countries are also in our dataset: Argentina, Brazil, Chile, Colombia, Greece, Korea, Mexico, Portugal, Spain, Turkey, Taiwan, and Venezuela.

To assess whether the rise in average annual compensation is driven by an increase in hours worked, Figure 4 plots the natural log of hours worked in liberalization time. The graph illustrates that hours worked are invariant to liberalization. Between years [-5] and [-1] the average natural log of hours worked is 7.64. In years [1] through [5] the average is 7.62. In levels these numbers translate to an average of 2094 hours worked per year prior to liberalization and 2066 hours worked per year after. Dividing 2094 and 2066 by the number of weeks in a year (52 not adjusting for vacation time) gives an estimate of roughly 40 hours to the average

work week in these twelve countries before and after opening up. That number seems entirely reasonable and reinforces our confidence in the GGDC data. Looking at medians instead of means does not alter the story. The median natural log of hours worked before liberalization is 7.59. The median after liberalization is also 7.59. In short, the number of hours worked per year does not change with liberalization, so an increase in the number of hours worked does not drive the increase in real wage growth documented in Figure 1.[12]

3B.2 Concurrent Economic Reforms

The second concern is that liberalizations often coincide with major economic reforms that could have a significant impact on wages apart from any effects 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 1 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, which presents our formal empirical methodology and results, uses the information in Table 1 to control directly for the impact of other reforms and to address a host of lingering concerns and alternative explanations.

4. Empirical Methodology and Results

We evaluate the statistical significance of the temporary increase in wage growth by estimating the following difference in difference panel regression specification. Table 4 reports

[12] As a final check we also used the GGDC data to construct a measure of hourly wages for the subset of twelve countries. Regressing the change in the natural log of the hourly wage on the same right-hand-side variables that appear in Equation (6), we find results that are qualitatively identical to those reported in Panel A of Tables 4 through 6.

the results.

Specifically, we estimate:

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

The left-hand-side variable, $\Delta \ln w_{it}^{dif}$, is the change in the natural log of the real local currency value of annual compensation for country i in year t minus the average change in the natural log of the real wage for the group of control countries in year t. Panel A restricts the sample period an event window of three years prior to the liberalization [-3] to three years following liberalization [+3].

Moving to the right-hand-side of equation (6), measures the-growth rate of the country excluded from the country fixed effects if that country had not liberalized and if the coefficient on $LIBERALIZE_{it}$ happened to be zero. 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 three years (years [1] , [2] and [3]). This means that the coefficient a_1 measures the average annual deviation of the growth rate of the real wage in the treatment group from the growth rate of real wages in the control group during the three-year liberalization episode.

The right-hand-side of equation (6) also contains four additional dummy variables—*STABILIZE*, *TRADE*, *PRIVATIZE* and *BRADY*—designed to prevent country-specific shocks in the shape of economic reforms from artificially inflating the coefficient on *LIBERALIZE*. 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 three subsequent years.

When the residuals are correlated across observations, OLS standard errors can be biased and either over or underestimate the true variability of the coefficient estimates. Turning at last to

the error term, ε_{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 (cross-sectional dependence), or (b) correlation of the residuals within a given country over time (time-series dependence). 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. For example, if the time effect is not fixed, then time dummies alone will not remove the dependence completely and even standard errors clustered by country can be biased. To compute accurate standard errors we employ various clustering procedures described below.[13]

4A. Benchmark Estimates: A Difference in Difference Specification

Table 4 (Panel A) presents the results from our estimate of Equation (6). The standard errors are clustered by year to account for potential cross-country correlation in the wage growth residuals. The estimate of the coefficient on *LIBERALIZE* ranges from 0.031 to 0.043. Relative to the control group, the average growth rate of the typical country's real wage exceeds its long-run mean by 3.1 to 4.3 percentage points per year during liberalization episodes. Almost every estimate of the coefficient on the liberalization dummy in Panel A of Table 4 is statistically significant at the 1 percent level.

Controlling for the other economic reforms that tend to accompany liberalization also

[13] The residuals of a given country may be correlated across years for a given country (time-series dependence). Alternatively, the residuals of a given year may be correlated across different countries (cross-sectional dependence).

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 0.031. 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 significant coefficient of 0.046 on the stabilization dummy is consistent with the literature on the real effects of inflation stabilization in developing countries such as Fischer, Sahay, and Vegh (2003). The coefficient on the privatization dummy is also positive and significant with a value of 0.034.

Since researchers do not always know whether the precise form of the dependence in residuals is of time-series or cross-sectional in nature, Peterson (2009) suggests a less parametric approach which is to cluster on two dimensions simultaneously (e.g., country and time) based on the following estimate of the variance-covariance matrix proposed by Cameron, Gelbach, and Miller (2006), and Thompson (2006):

$$V_{Country\&Time} = V_{Country} + V_{Time} - V_{White},$$

which combines the standard errors clustered by country with the standard errors clustered by time. The standard errors clustered by country (the first term) capture the unspecified correlation between observations on the same country in different years (e.g., correlations between ε_{it} and ε_{is}). The standard errors clustered by time (the second term) capture the unspecified correlation between observations on different countries in the same year (e.g., correlations between ε_{it} and ε_{kt}). Since both the country- and time-clustered variance-covariance matrix include the diagonal of the variance-covariance matrix, the White variance-covariance matrix is subtracted off to avoid double counting these terms. This method allows for both a country and a time effect,

although observations on different countries in different years are assumed to be uncorrelated. Peterson (2009) demonstrates through simulation that clustering by two dimensions produces less biased standard errors.

Table 5 (Panel A) presents estimates that use Petersen's (2009) procedure to simultaneously cluster the standard errors by year (to adjust for cross-country correlation) and by country (to adjust for serial correlation).[14] Because it is not possible to include country fixed effects while simultaneously clustering the standard errors by year and country, the magnitude of the estimates in Table 4 are not identical to those in Table 4 (although they are very similar). Focusing, then, on the precision of the estimates, we see that the estimate of every coefficient in Table 5 is significant at the one percent confidence level. This suggests that our general finding is robust to concerns about both serial and cross-country correlation in the error terms.

4A.1 An Alternative Specification: Country and Year Fixed Effects

As an alternative way of examining whether the coefficient on the liberalization variable truly reflects the impact of country-specific liberalization policy versus that of a common global shock, we add a full set of country-fixed and year-fixed effects in our benchmark regression specification.

$$\Delta \ln w_{it} = a_0 + COUNTRY_i + YEAR_t + a_1 * LIBERALIZE_{it} + a_2 * TRADE_{it} + a_3 * STABILIZE_{it} + a_4 * PRIVATIZE_{it} + a_5 * BRADY_{it} + \varepsilon_{it} \quad (7),$$

where the variable $YEAR_t$ is shorthand for the set of year-fixed effects. The standard errors are clustered by year. The left-hand-side variable, $\Delta \ln w_{it}$, is the natural log of the real local

[14] For another discussion of multi-way clustering see Cameron, Gelbach, and Miller (2009).

currency value of annual compensation for country *i* in year *t* minus the same variable in year *t-1* for the treatment group.

The right-hand-side of equation (7) also contains four additional dummy variables—*STABILIZE*, *TRADE*, *PRIVATIZE* and *BRADY*.

Table 6 (Panel A) presents the results from our estimate of Equation (7). The standard errors are clustered by year to account for potential cross-country correlation in the wage growth residuals. The impact of liberalization on real wage growth is economically large. The estimate of the coefficient on *LIBERALIZE* ranges from 0.038 to 0.043. During liberalization episodes the average growth rate of the typical country's real wage exceeds its long-run mean by 3.8 to 4.3 percentage points per year. All but one estimate of the coefficient on the liberalization dummy in Panel A of Table 6 is statistically significant at the 1 percent level. 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 6 shows that after accounting for the effects of inflation stabilization, trade liberalization, and privatization, the coefficient on *LIBERALIZE* is 0.0388.

4B. Alternative Explanations

One interpretation of the evidence says 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 to a reduction in labor supply. The argument runs as follows. If workers perceive the impact of liberalization on wages to be permanent, then they effectively receive a positive shock to their permanent income and may reduce their labor supply accordingly. If this is the case then the observed increase in wage growth may stem from

a decrease in labor supply as well as an increase in labor demand.[15]

The employment data are not consistent with a decrease in labor supply. There is no discernible change in the growth rate of employment following liberalizations. We regressed the change in the natural log of employment on the same right-hand-side variables that appear in Equation (7), and the liberalization dummy was never significant. Also, if labor supply decreases then we would also expect a decline in the number of hours worked. Again, Figure 4 demonstrates that this is not the case. Overall, the evidence does not suggest that workers reduce their labor supply in response to liberalization. While we do not formally estimate the labor supply decision and cannot exclude the possibility that part of the wage increase results from a decrease in labor supply, if this alternative explanation is at work, its overall impact would appear to be second order.

4C. Economic Interpretation

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 in Table 6). The estimate of the constant is 0.018, indicating that the real wage grows by an average of 1.8 percent per year over the entire sample. The estimate of the coefficient on the liberalization dummy is 0.039. Adding the constant and the coefficient on the liberalization dummy gives the average growth rate of the real wage during liberalization episodes—5.7 percent per year. This means that in the year the liberalization occurs and each of the subsequent

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

three, the average growth rate of the real wage is almost three 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 2392 US dollars. During the three-year liberalization window the real wage grows at 5.7 percent per year, so that by the end of year [3] the average level of the real wage is $2392\,e^{.086*3} = 3005$ US dollars. Now assume that that in the absence of liberalization the real wage would have grown at the sample mean of 1.8 percent per year. In that case, the level of the real wage at the end of year [3] would be $2392\,e^{.013*3} = 2571$ 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 434 dollars higher (3005 minus 2571) than it would have been in the absence of liberalization. This change in the level of the wage is equal to a one-fifth of the average manufacturing worker's pre-liberalization take home pay.

It is also important to note that the results do not simply reflect mean reversion following a temporary fall in earnings à la Ashenfelter (1978). While Figure 1 does show a gradual decline in the level of the real wage from years [-5] to [-1], a few hypothetical calculations demonstrate that the results do not simply reflect a bounce-back effect. Five years prior to liberalization, the level of the real wage is 2,836 dollars. Now, suppose that instead of declining for the next four years, real wages grew at their (continuously compounded) long-run rate of 1.8 percent per year for the next decade. Under that scenario, the real wage in year [+5] would have been 3, 395 dollars. The actual level of the real wage in year [+5] is 4,496 dollars, 32 percent higher than the

level that would have prevailed had wages continued to grow at their long-run rate. Simply put, the increase in the real wage is too large to be an artifact of reversion to the mean.

4D. 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.

To formally test the relation between liberalization and the growth rate of labor productivity, we estimate the following difference in difference regression:

$$\Delta \ln(\frac{Y^{dif}}{L})_{it} = a_0 + COUNTRY_i + a_1 * LIBERALIZE_{it} + a_2 * TRADE_{it} + a_3 * STABILIZE_{it} + a_4 * PRIVATIZE_{it} + a_5 * BRADY_{it} + \varepsilon_{it} \quad (8).$$

Equation (8) 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 value-added per worker minus the average change in the natural log of value-added per worker in the control group. Again, to be consistent with the wage estimates we cluster the standard errors by year.

Panel A Table 7 shows that liberalization has a positive and significant impact on productivity growth. The estimates of the coefficient on the liberalization dummy range from 0.076 to 0.097. Every estimate of the coefficient on the liberalization dummy in Panel A of Table 7 is statistically significant. Taken together, these results suggest that liberalization, not an external shock or domestic economic reforms, is responsible for the increase in productivity

growth.

In particular, Column (5) of Panel A in Table 7 demonstrates that after accounting for the potential impact of other economic reforms, the estimate of the coefficient on the liberalization dummy is 0.972. This means that the average growth rate of productivity is 9.72 percentage points higher during the three-year liberalization window than in non-liberalization years. The 9.72 percentage point increase in productivity growth associated with liberalization is larger than the 3.9 percentage point increase in wage growth. Because the increase in productivity outstrips the increase in wage growth, manufacturing-sector profitability actually rises during liberalizations.[16]

Panel B of Table 7 shows that the results for productivity growth and liberalization is robust to all of the statistical concerns raised about the estimation of wage growth and liberalization examined in Sections 4A.

5. Discussion

While the size of the increase in productivity growth more than matches the size of the increase in real wage growth, the important unanswered question is whether the magnitude of either increase is consistent with the model that drives the estimates. To answer the question, begin with the standard assumption that liberalization has no impact on TFP growth and recall Equation (5): $\frac{\dot{w}}{w} = \frac{\dot{A}}{A} + \frac{1}{\sigma} * \frac{f'(k)k}{k} * \frac{\dot{k}}{k}$. With no change in the growth rate of TFP, Equation (5) implies that the change in the growth rate of the real wage equals the product of three numbers: the reciprocal of the elasticity of substitution, capital's share in national income, and the change in the growth rate of capital per effective worker.

[16] Chari and Henry (2008) also find that the return to capital in the manufacturing sector rises during liberalizations.

Specifically, we have:

$$\frac{\dot{w}}{w} = \frac{1}{\sigma} * \frac{f'(k)k}{k} * \frac{\dot{k}}{k} \tag{9}$$

The capital share lies between 1/3 and 1/2. Obtaining an estimate of the change in capital growth requires a little more effort. We know from previous work that aggregate capital stock growth increases by 1.1 percentage points following liberalizations (Henry, 2003). We can use the aggregate number to calculate a rough upper bound for the change in manufacturing sector capital growth. For the countries in our sample, the manufacturing sector accounts for about 1/5 of GDP. Assuming zero net growth in capital for the agriculture and service sectors, the largest possible increase in the growth rate of capital in manufacturing is about 5.5 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 (2008) calculate that the growth rate of capital in the manufacturing sector increases by 4.1 percentage points per year following liberalizations. Increases in capital stock growth between 4.1 and 5.5 percentage points are also consistent with the size of the fall in the cost of capital that occurs following liberalizations.[17]

Suppose the capital share is 1/2 and the change in capital growth is 5.5 percentage points. Then for capital-deepening alone to explain the 3.9-percentage-point increase in wage growth you need an elasticity of substitution less than or equal to 0.7. If the capital share is 1/3 and the change in capital growth is 4.1 percentage points, then the elasticity of substitution must be less than or equal to 0.35. There is little consensus on the size of the elasticity of substitution. Early work estimated small elasticities that were statistically indistinguishable from zero.[18] More recent studies cannot reject the hypothesis that the elasticity of substitution is 1 (Caballero,

[17] See Henry (2007), pp. 897-900.
[18] For a survey of this literature see Chirinko (1993).

1994). Most relevant to the countries in this paper, Coulibaly and Millar (2007) estimate an elasticity of substitution of about 0.8 for South Africa. With a standard error of 0.08, the Coulibaly and Millar estimate could imply an elasticity as small as 0.64. With a change in capital growth of 5.5 percentage points, a capital share of 1/2, and an elasticity of substitution of 0.64, Equation (9) predicts that liberalization would generate a 4.3 percentage-point increase in wage growth.

We do not mean to push any particular value for the capital share. And it is far from clear that we have a consensus estimate of the size of the elasticity of substitution in developed countries, let alone emerging economies. What is clear, however, is that if you want to maintain the orthodox assumption that liberalization has no impact on total factor productivity, then the observed increases in wage growth are consistent with the model only if you are willing to concede that the elasticity of substitution is substantially less than 1 (i.e., the world is not Cobb-Douglas). But if the elasticity of substitution is significantly less than one, then it is hard to understand how capital's income share remains constant (or increases slightly) following the liberalization-induced fall in the cost of capital.

On the other hand, if you maintain that the world is Cobb-Douglas, then our wage results necessarily imply that liberalization has a large impact on TFP growth. Indeed, if one is willing to step outside the confines of the Solow model there are plausible theoretical channels through which liberalization could raise total factor productivity.

For instance, liberalization may enable firms to import more efficient machines (e.g., tractors instead of hoes) that effectively shift the country's production technology closer to the world frontier. DeLong (2004) argues that after liberalizing the capital account "…developing countries…would enjoy the benefits from technology advances and from learning-by-doing

using modern machinery." In other words, to the extent that technological progress diffuses from developed to developing countries, the importation of new machinery provides an important conduit through which the diffusion may occur (Eaton and Kortum, 2001a). Almost all of the world's research and development (R&D) takes place in a small number of industrial countries (Eaton and Kortum, 1999), and the same group of countries accounts for over 70 percent of the world's machine exports in a given year (Eaton and Kortum, 2001b; Alfaro and Hammel, 2007).

Evidence from the literature supports the conjecture that developing countries can import technological progress by liberalizing the capital account. In the immediate aftermath of liberalizations, firms in the manufacturing sector of developing countries accumulate capital at a faster rate than they did before the liberalization (Chari and Henry, 2008). Furthermore, these countries raise their rate of capital accumulation by importing more capital goods. As a result of liberalization, the share of capital goods imports to total imports rises by 9 percent, and the share of total machine imports as a fraction of GDP rises by 13 percent (Alfaro and Hammel, 2007).

The observation that both imports of capital goods and total factor productivity rise in concert with liberalization lends credence to the notion that new capital goods embody technological progress and that developing countries can raise their growth rates of total factor productivity by liberalizing the capital account.[19] The observation is also consistent with work showing that: (a) the cross-country correlation between investment and growth stems primarily from investment in equipment and machinery (DeLong and Summers, 1991, 1993) and (b) cross-country variation in the composition of capital investment explains much of the cross-country variation in total factor productivity (Caselli and Wilson, 2003).

[19]Also, in the spirit of Rajan and Zingales (1998), liberalization may improve domestic firms' access to external finance, which might in turn increase the rate at which firms import capital goods.

On the other hand, some argue that the simplest explanation of capital-account-liberalization-induced TFP growth lies with the economic reforms that accompany liberalization. Economic reforms improve resource allocation, essentially producing a one-time shift in the production function that temporarily raises the growth rate of TFP, without inducing technological process per se. Others posit that liberalization facilitates increased risk sharing, which might encourage investment in riskier, higher growth technologies (Levine, 1997; Levine and Zervos, 1998a). 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. The bottom line of this discussion is that the size of the increases in wage growth and productivity we report are consistent with the model that drives our estimation. Whether the primary source of those increases lies with capital deepening or increased total factor productivity depends on reasonable differences in views about the elasticity of substitution that have yet to be resolved in the literature.

6. Conclusion

In the process of debating the impact of trade on wages, international economists pay relatively little attention to the impact of trade in capital. Debating the costs and benefits of capital account liberalization, macro and financial economists largely ignore the implications of increased capital market integration for wages. Yet labor income typically accounts for about two-thirds of GDP. Almost two decades after the advent of capital account liberalization in the

developing world, our paper provides the first systematic analysis of the impact of liberalization on the level of real wages.

Increased capital market integration in the 1980s and 1990s sharply reduced the cost of capital for manufacturing firms in emerging economies. In response to the fall in their cost of capital, these firms installed new machinery, much of which was imported from abroad, and may have embodied substantial technological progress. The combination of capital deepening and embodied technological progress drove up the productivity of workers in the manufacturing sector. Accordingly, the demand for those workers increased, along with their real wage.[20] Specifically, the advent of increased capital market integration increased the average take-home pay of workers in the manufacturing sector by 18 percent without eroding the profitability of capital.

While the focus of this paper is on the level of real wages, our findings also provide important clues about the rise in wage inequality in developing countries documented by Goldberg and Pavcnik (2007). If the liberalization-induced increase in wages was evenly distributed across skilled and unskilled workers in the manufacturing sector, then no increase in the skill premium would have occurred. However, two observations suggest that the increase in manufacturing sector wages was probably concentrated among highly skilled workers. First, countries' imports of machinery and equipment rise substantially in the aftermath of capital account liberalizations and firms that import machinery and equipment generally employ a larger share of high-skilled workers than firms that do not import such capital (Harrison and Hanson, 1999). Second, work that is characterized as unskilled from a developed country's perspective may be skilled-labor intensive in comparison to typical domestic production activities in a

[20] Cragg and Epelbaum (1996) and Behrman, Birdsall, and Szekely (2000) make a similar argument for Latin America.

developing country (Feenstra and Hanson, 1997, 2003). All things considered, it seems quite plausible that easing restrictions on capital inflows may have contributed to increased wage inequality in developing countries during the 1980s and 1990s.

Be that as it may, the bottom line of this paper is that increased capital market integration raised the average standard of living for a significant fraction of the workforce in developing countries. If labor is mobile across sectors, then over time we would expect the productivity-driven wage gains in manufacturing to translate into higher incomes for workers elsewhere in the economy. The extent to which the labor market in these countries functions well enough to allow workers to respond to wage differentials across sectors is an important issue that lies beyond the scope of this paper.[21] As the quality and breadth of data on labor markets in developing countries continues to improve, future work may produce more definitive conclusions.

[21] See Wacziarg and Seddon (2004) for an analysis of intersectoral mobility of labor in response to trade reforms.

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

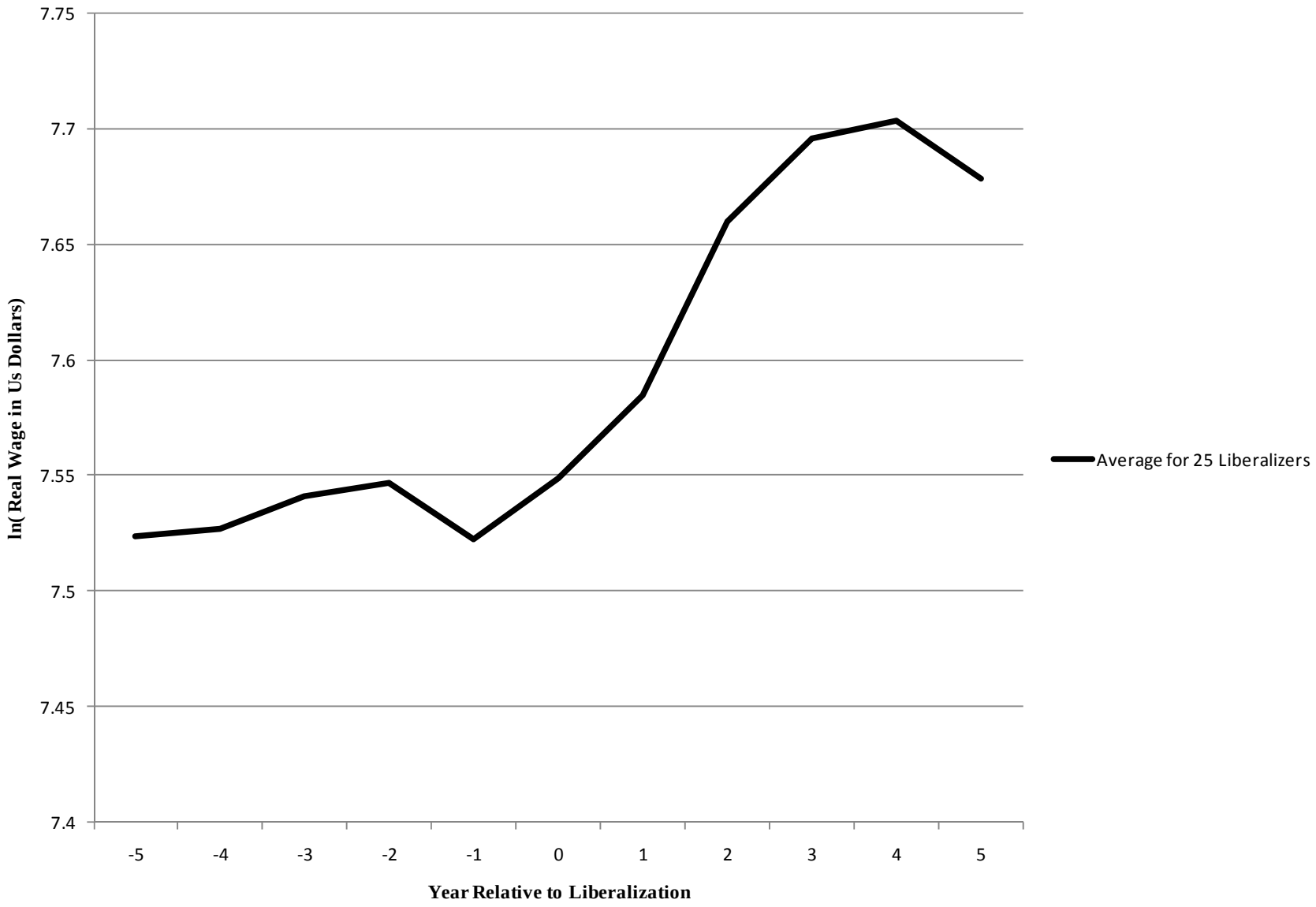

Figure 2. Productivity Growth Rises in the Aftermath of Capital Account Liberalizations.

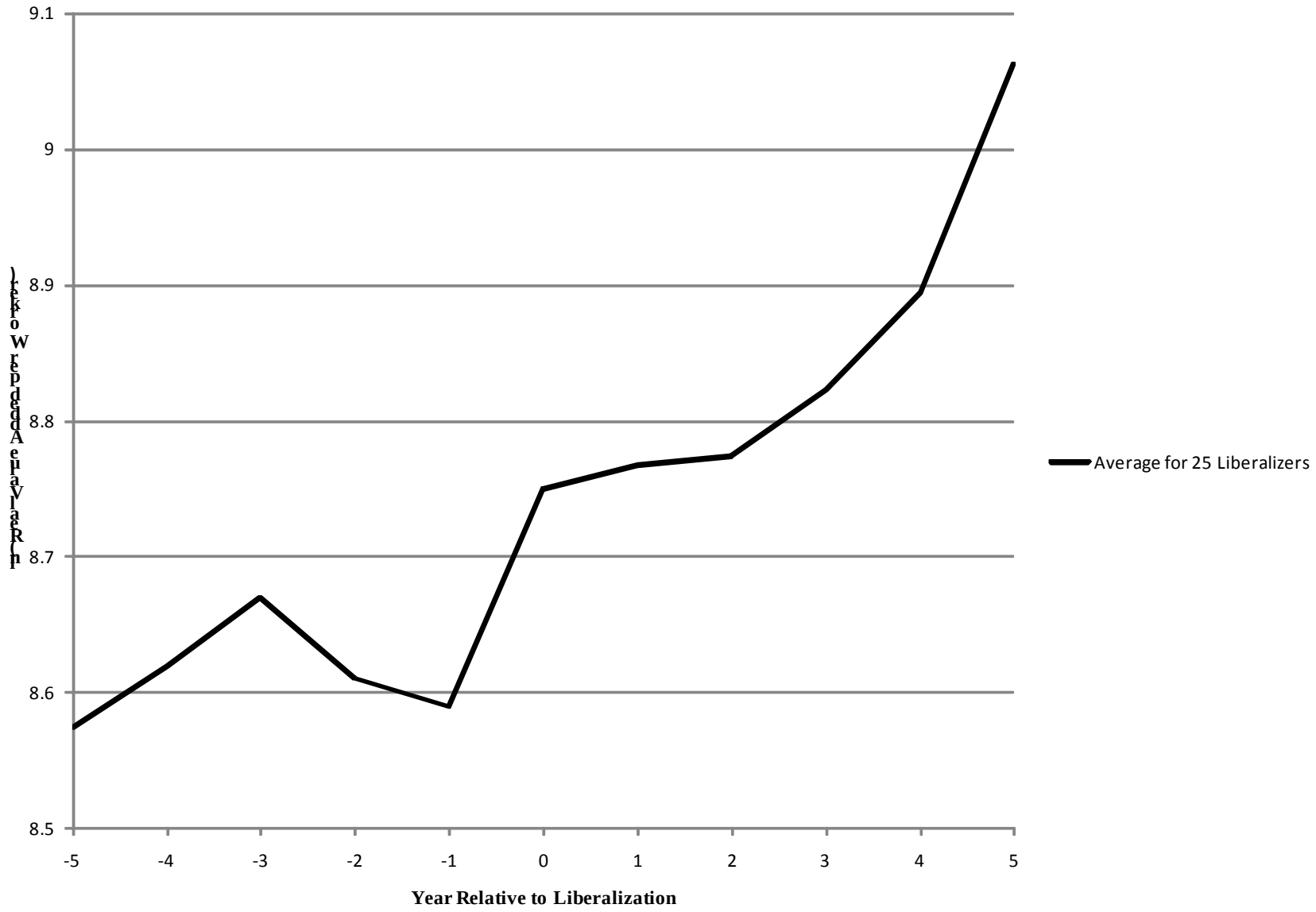

Figure 3. Hypothetical Impact of Liberalization on the Cost of Capital, Investment and the Real Wage.

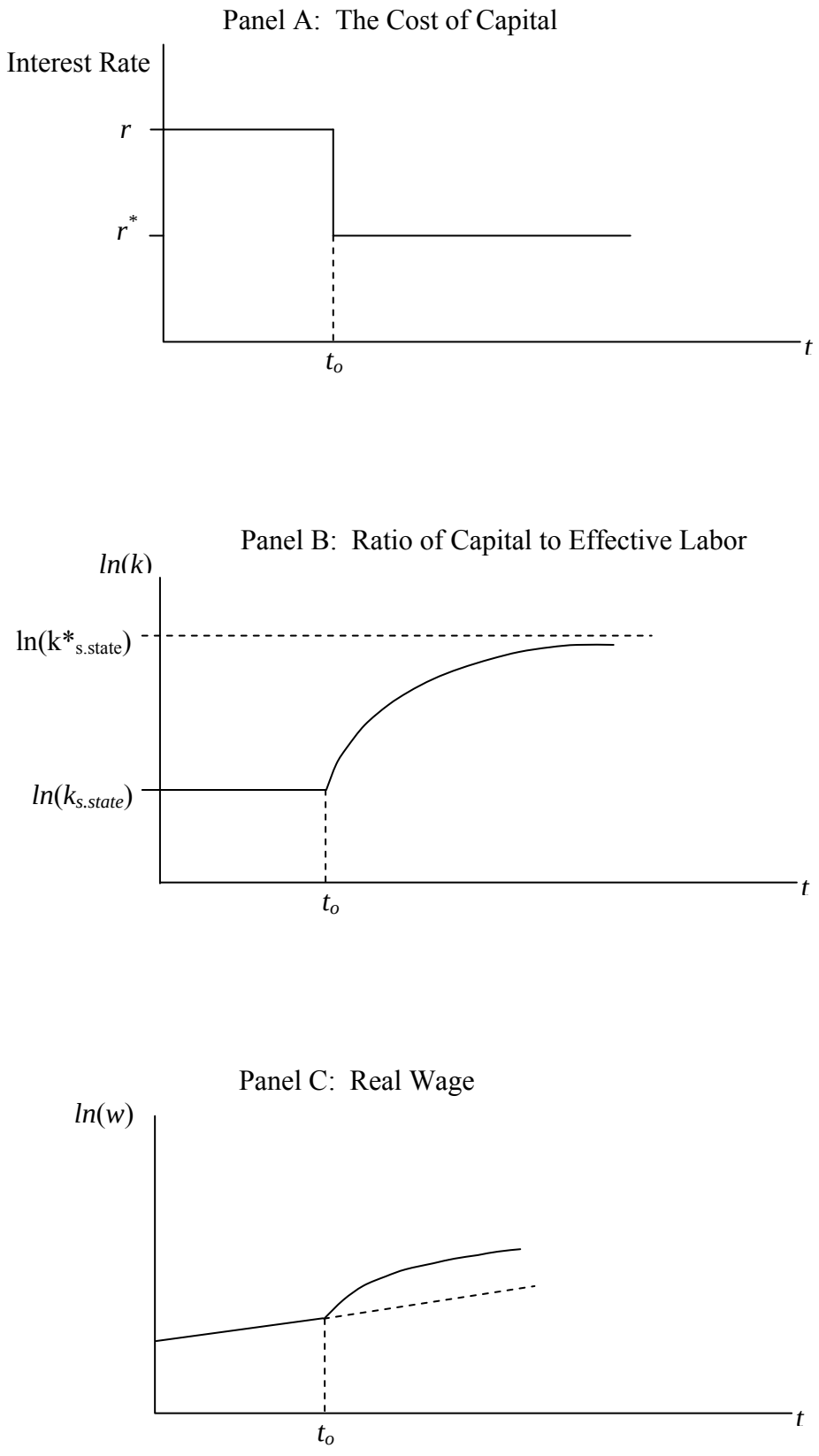

Figure 4. The Average number of Hours Worked Does not Rise with Liberalization

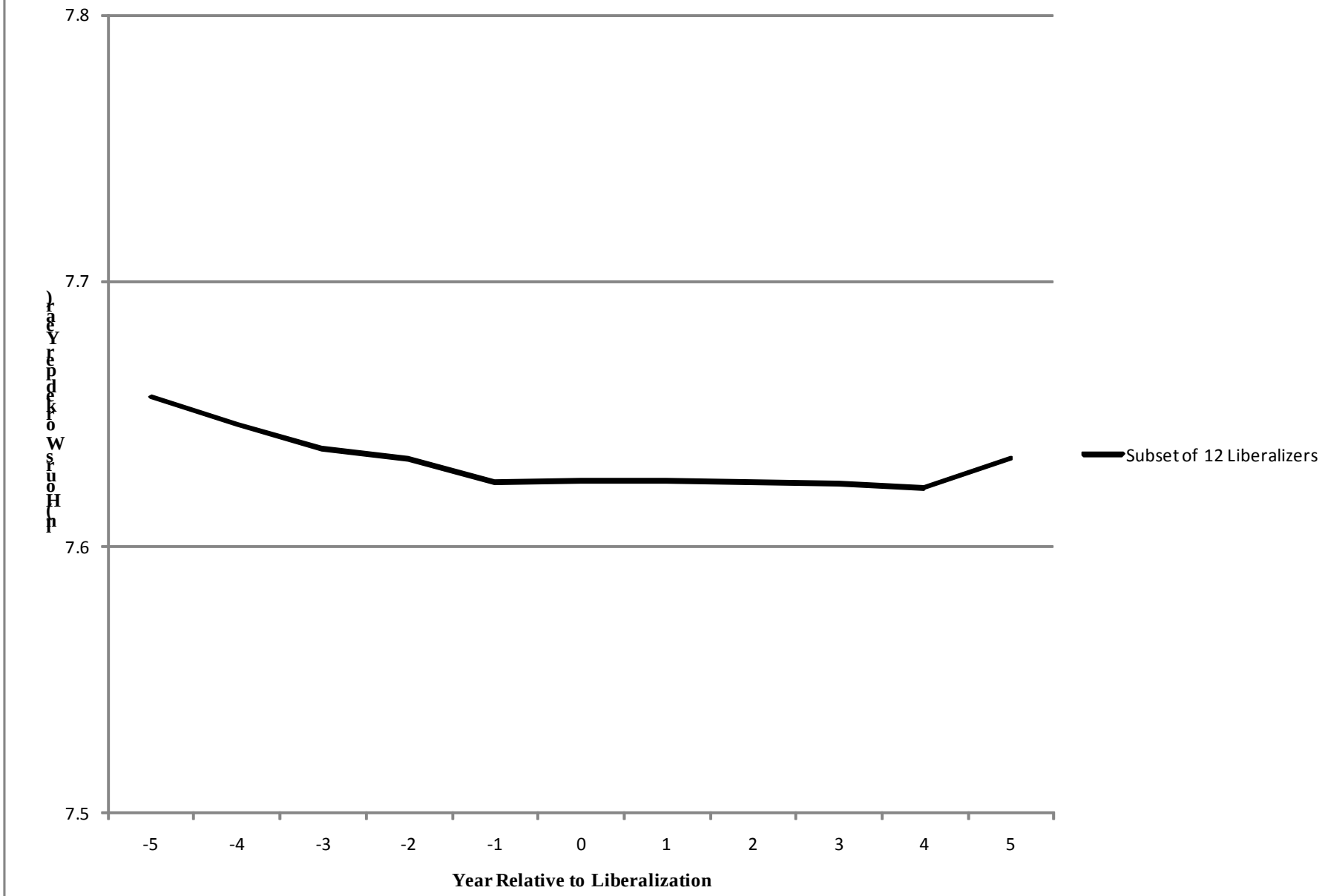

Table 1. Capital Account Liberalizations Occur Around the Same Time as Other Major Economic Reforms.

Country	Capital Liberalization	Stabilization Program	Trade Liberalization	Privatization	Brady Plan Debt Relief
Argentina	Nov-89	Nov-89	Apr-91	Feb-88	Apr-92
Brazil	Mar-88	Jan-89	Apr-90	Jul-90	Aug-92
Chile	May-87	Aug-85	1976	1988	NA
Colombia	Dec-91	NA	1986	1991	NA
Egypt	Feb-91	Apr-91	Apr-91	Apr-91	NA
Greece	Jul-94	Jul-89	Apr-53	Nov-90	NA
India	Jun-86	Nov-81	1994	1991	NA
Indonesia	Sep-89	May-73	1970	1991	NA
Israel	Oct-89	Jul-85	Feb-52	Jan-86	NA
Jordan	Dec-95	May-94	1965	Jan-95	Jun-93
Malaysia	May-87	NA	1963	1988	NA
Mexico	May-89	May-89	Jul-86	Nov-88	Sep-89
Morocco	Dec-92	Jan-84	Sep-83	1993	NA
Nigeria	Aug-95	Jan-91	NA	Jul-88	Mar-91
Pakistan	Feb-91	Sep-93	2001	1990	NA
Philippines	May-86	Oct-86	Nov-88	Jun-88	Aug-89
Portugal	Jan-93	Oct-90	Jan-60	Apr-89	NA
South Africa	Mar-95	Mar-86	Apr-94	Apr-94	NA
South Korea	Jun-87	Jul-85	1968	NA	NA
Spain	Jan-93	Jan-78	Jul-59	1985	NA
Taiwan	May-86	NA	1963	NA	NA
Thailand	Sep-87	Jun-85	Always Open	1988	NA
Turkey	Aug-89	Jul-94	1989	1988	NA
Venezuela	Jan-90	Jun-89	May 1989**	Apr-91	Jun-90
Zimbabwe	Jun-93	Sep-92	NA	1994	NA

Notes: The capital account liberalization dates identified in this table are the dates on which the eighteen countries in Column 1 eased restrictions prohibiting foreign ownership of domestic stocks. The liberalization dates in Column 2 are an amalgamation of those in Henry (2000) and Levine and Zervos (1998b). Columns 3 through 6 list the dates of major economic reforms that occurred around the same time as the capital account liberalizations. The stabilization program dates in Column 4 come from Henry (2002) and various issues of the IMF Annual Reports. Column 5 lists trade liberalization dates from Sachs and Warner (1995). The privatization dates in Column 6 come form the Privatization Data Base maintained by the World Bank. Finally, Column 6 lists the month and year that each country received debt relief under the Brady Plan. The debt relief dates come from Cline (1995), Lexis Nexis, and various issues of the *Economist Intelligence Unit*.
**Venezuela reversed its trade liberalization reforms in 1993.

Table 2. Summary Statistics for the Real Wage Growth During Capital Account Liberalization Episodes

Panel A shows the following summary statistics for each country. Column 1 shows the log real wage change from event year [0] to [3] for each country's liberalization episode. Column 2 presents the liberalization real wage change expressed relative to the country's mean log real wage change from [-3] to [0]. Column 3 is the liberalization log real wage change expressed relative to the contemporaneous mean log real wage change for the control group countries between years [0] and [3]. Column 4 presents the difference-in-difference log real wage change that subtracts the different in the pre-liberalization wage change between the treatment and control groups from the quantity in column 3. For columns 1-4, we test whether the mean differs significantly from zero and report a heteroscedasticity-consistent estimate of the standard error in Panel B.

Panel A

	(1)	(2)	(3)	(4)
Country	Liberalization Real Wage Change	Liberalization Wage Change Relative to Pre-liberalization Wages	Liberalization Wage Change Relative to Control Group	Difference in Difference Wage Change
Argentina	-2.88%	3.10%	18.70%	16.26%
Brazil	25.15%	23.78%	38.17%	19.12%
Chile	20.18%	6.36%	30.98%	1.50%
Colombia	9.75%	9.25%	22.76%	4.37%
Greece	9.33%	13.11%	30.19%	33.61%
India	13.18%	9.79%	23.97%	2.32%
Indonesia	-4.54%	-14.53%	12.25%	-2.16%
Israel	16.38%	14.64%	19.93%	5.51%
Jordan	14.12%	23.63%	13.39%	13.93%
Malaysia	2.57%	-3.71%	27.02%	22.98%
Mexico	23.00%	39.07%	39.80%	44.93%
Morocco	23.68%	19.77%	32.72%	22.49%
Nigeria	-51.99%	25.87%	-51.68%	0.13%
Pakistan	3.01%	-10.01%	5.54%	-22.24%
Philippines	14.15%	6.25%	27.17%	3.22%
Portugal	45.40%	59.56%	60.26%	66.57%
South Africa	6.77%	3.06%	17.56%	-4.33%
South Korea	49.05%	27.45%	69.91%	54.30%
Spain	6.35%	8.30%	11.01%	3.88%
Taiwan	42.79%	17.19%	57.64%	34.13%
Thailand	17.16%	-8.00%	35.18%	12.61%
Turkey	59.46%	65.14%	76.26%	73.59%
Venezuela	-13.13%	35.15%	-2.35%	17.75%
Zimbabwe	51.08%	99.55%	60.29%	87.78%
Mean	15.83%	19.74%	28.19%	21.35%
Median	14.14%	13.87%	27.09%	15.10%

Panel B				
	(1)	(2)	(3)	(4)
Right Hand Side Variable	Liberalization Real Wage Change	Liberalization Wage Change Relative to Pre-liberalization Wages	Liberalization Wage Change Relative to Control Group	Difference in Difference Wage Change
Constant	0.1583***	0.1915***	0.2687***	0.2015***
	(0.048)	(0.051)	(0.054)	(0.054)
Observations	25	25	25	25

Robust standard errors in parentheses.

***, **, * represent 1%, 5% and 10% levels of significance, respectively.

Table 3. The Impact of Liberalization on Real Wage Growth is Temporary and Declines over Time.

Panel A presents coefficient estimates on the liberalization dummy over time or the time profile of the impact of liberalization on the growth rate of real wages. Panel A presents estimates of the coefficient on the liberalization dummy allowing progressively more periods after the reform (from 2 up to 5) and for the pre-liberalization period ([-3],[-1]). The right hand side variable is log of the real wage change. Panel B presents the coefficient estimates on the liberalization dummy by individual year ranging from the liberalization year [0] up to five years following the liberalization [5]. Robust standard errors in parentheses. ***, **, * represent 1%, 5% and 10% levels of significance, respectively.

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)
Dummy ([-3], [-1])	-0.0237**					
	(0.011)					
Dummy ([0], [2])		0.0341***				
		(0.013)				
Dummy ([0], [3])			0.0369***			
			(0.012)			
Dummy ([0], [4])				0.0314***		
				(0.011)		
Dummy ([0], [5])					0.0190*	
					(0.011)	
Dummy ([0], [6])						0.012
						(0.011)
Constant	0.0154***	0.006	0.004	0.004	0.006	0.008
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	437	437	437	437	437	437
R-squared	0.139	0.145	0.15	0.147	0.138	0.135

Panel B						
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy 0	0.0002					
	(0.020)					
Dummy 1		0.0476***				
		(0.015)				
Dummy 2			0.0718***			
			(0.021)			
Dummy 3				0.032		
				(0.020)		
Dummy 4					-0.005	
					(0.023)	
Dummy 5						-0.046
						(0.032)
Constant	0.0114**	0.006	0.008	0.0099*	0.0116**	0.0134***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	437	437	437	437	437	437
R-squared	0.132	0.149	0.152	0.136	0.132	0.139

Table 4. Liberalization Temporarily Increases the Growth Rate of Real Wages
Difference-in-Differece Estimation, Country Fixed Effects, Standard Errors Clustered by Year

The estimation procedure is ordinary least squares. All specifications cluster the standard errors by year to account for the possibility that shocks to wage growth are correlated across countries within a given year. Standard errors appear in parentheses and are clustered by year. The symbols (***), (**), and (*) represent significance levels of 1%, 5%, and 10%, respectively. For the regressions reported in Panel A, the left-hand-side variable is the change in the natural log of the real wage relative to the control group over the window ([-3], [3]) around the liberalization date. *LIBERALIZE* is a dummy variable that takes on the value of one in the year that country *i* liberalizes (year [0]) and each of the subsequent three years ([1] [2] and [3]). *TRADE*, *STABILIZE*, *PRIVATIZE*, and *BRADY* are dummy variables that take on the value one whenever a trade liberalization, inflation stabilization, privatization or Brady plan program takes place during country *i*'s capital account liberalization episode. All specifications contain twenty five country-specific dummy variables. Panel B uses the full sample.

Panel A: Time window ([-3],[+3])

	(1)	(2)	(3)	(4)	(5)	(6)
Liberalization	0.0426***	0.0429***	0.0415***	0.0313**	0.0409***	0.0314***
	(0.011)	(0.011)	(0.01)	(0.011)	(0.009)	(0.011)
Trade		0.0215				0.0051
		(0.019)				(0.021)
Stabilization			0.0460*			0.0228
			(0.024)			(0.023)
Privatization				0.0340*		0.0285
				(0.019)		(0.023)
Brady Plan					0.0551	0.0405
					(0.035)	(0.031)
Constant	0.0249	0.0208	0.0189	0.0183	0.0182	0.0104
	(0.016)	(0.013)	(0.014)	(0.017)	(0.015)	(0.014)
Observations	152	152	152	152	152	152
R-squared	0.3440	0.3480	0.3590	0.3610	0.3610	0.3790

Panel B: Full Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Liberalization	0.0459***	0.0438***	0.0474***	0.0388***	0.0450***	0.0362**
	(0.012)	(0.011)	(0.012)	(0.014)	(0.012)	(0.014)
Trade		0.0173				0.0172
		(0.014)				(0.014)
Stabilization			-0.0196			-0.0303
			(0.018)			(0.02)
Privatization				0.0152		0.0165
				(0.015)		(0.016)
Brady Plan					0.0076	0.0168
					(0.024)	(0.025)
Constant	0.0209***	0.0199***	0.0224***	0.0201***	0.0208***	0.0211***
	-0.0060	-0.0060	-0.0060	-0.0060	-0.0060	(0.006)
Observations	722	722	722	722	722	722
R-squared	0.111	0.113	0.114	0.113	0.111	0.118

Table 5. Liberalization Temporarily Increases the Growth Rate of Real Wages.
Difference-in-Difference Estimation, Standard Errors Clustered by Year and Country

The estimation procedure is ordinary least squares. All specifications simultaneously cluster the standard errors in two dimensions: (1) by year to account for the possibility that shocks to wage and productivity growth are correlated across countries within a given year, and (2) by country to account for the possibility that shocks to wage and productivity growth are correlated over time within a given country. Standard errors appear in parentheses. The symbols (***), (**), and (*) represent significance levels of 1%, 5%, and 10%, respectively. For the regressions reported in Panel A, the left-hand-side variable is the change in the natural log of the real wage relative to the control group over the window ([-3], [3]) around the liberalization date. LIBERALIZE is a dummy variable that takes on the value of one in the year that country i liberalizes (year [0]) and each of the subsequent three years ([1] [2] and [3]). TRADE, STABILIZE, PRIVATIZE, and BRADY are dummy variables that take on the value one whenever a trade liberalization, inflation stabilization, privatization or Brady plan program takes place during country i's capital account liberalization episode. Panel B uses the full sample.

Panel A: Time window ([-3],[+3])

	(1)	(2)	(3)	(4)	(5)	(6)
Liberalization	0.0472***	0.0475***	0.0477***	0.0381***	0.0482***	0.0394**
	(0.012)	(0.012)	(0.013)	(0.014)	(0.014)	(0.015)
Trade		0.0192				0.0211
		(0.021)				(0.021)
Stabilization			-0.0328			-0.0288
			(0.026)			(0.022)
Privatization				0.0292		0.0298
				(0.025)		(0.027)
Brady Plan					-0.0331	-0.0265**
					(0.023)	(0.012)
Constant	0.0225	0.0189	0.0270*	0.0165	0.0266*	0.0196
	(0.017)	(0.016)	(0.014)	(0.019)	(0.015)	(0.016)
Observations	152	152	152	152	152	152
R-squared	0.05	0.055	0.062	0.066	0.062	0.092

Panel B: Full Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Liberalization	0.0492***	0.0477***	0.0513***	0.0466***	0.0534***	0.0474***
	(0.015)	(0.014)	(0.014)	(0.016)	(0.013)	(0.015)
Trade		0.0126				0.0197
		(0.017)				(0.015)
Stabilization			-0.0276			-0.0289
			(0.02)			(0.023)
Privatization				0.0055		0.0100
				(0.015)		(0.016)
Brady Plan					-0.0312	-0.0226
					(0.028)	(0.025)
Constant	0.0205***	0.0198**	0.0227***	0.0203**	0.0211***	0.0215***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	722	722	722	722	722	722
R-squared	0.021	0.022	0.026	0.021	0.024	0.03

Table 6. Liberalization Temporarily Increases the Growth Rate of Real Wages
Time and Country Fixed Effects.

The estimation procedure is ordinary least squares. All specifications contain year-specific and country-specific dummy variables. All specifications cluster the standard errors by year to account for the possibility that shocks to wage growth are correlated across countries within a given year. Standard errors appear in parentheses. The symbols (***), (**), and (*) represent significance levels of 1%, 5%, and 10%, respectively. For the regressions reported, the left-hand-side variable is the change in the natural log of the real wage over the full sample. LIBERALIZE is a dummy variable that takes on the value of one in the year that country i liberalizes (year [0]) and each of the subsequent three years ([1] [2] and [3]). TRADE, STABILIZE, PRIVATIZE, and BRADY are dummy variables that take on the value one whenever a trade liberalization, inflation stabilization, privatization or Brady plan program takes place during country i's capital account liberalization episode.

	(1)	(2)	(3)	(4)	(5)	(6)
Liberalization	0.0433***	0.0427***	0.0428***	0.0419***	0.0430***	0.0388**
	(0.013)	(0.013)	(0.013)	(0.015)	(0.013)	(0.015)
Trade		0.0142				0.0170
		(0.013)				(0.014)
Stabilization			-0.0208			-0.0278
			(0.016)			(0.017)
Privatization				0.0042		0.0055
				(0.018)		(0.019)
Brady Plan					0.0042	0.0155
					(0.022)	(0.023)
Constant	0.0162**	0.0155**	0.0185***	0.0159**	0.0163**	0.0180***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
Observations	758	758	758	758	758	758
R-squared	0.1690	0.1700	0.1710	0.1690	0.1690	0.1730

Table 7. Liberalization Temporarily Increases the Growth Rate of Productivity
Difference-in-Difference Estimation

The estimation procedure is ordinary least squares. In Panel A, all specifications cluster the standard errors by year to account for the possibility that shocks to wage growth are correlated across countries within a given year. In Panel B, standard errors are clustered by year and country. Standard errors appear in parentheses. The symbols (***), (**), and (*) represent significance levels of 1%, 5%, and 10%, respectively. For the regressions reported, the left-hand-side variable is the change in the natural log of the real value added per worker relative to the control group over the window ([-3], [3]) around the liberalization date. *LIBERALIZE* is a dummy variable that takes on the value of one in the year that country *i* liberalizes (year [0]) and each of the subsequent three years ([1] [2] and [3]). *TRADE, STABILIZE, PRIVATIZE,* and *BRADY* are dummy variables that take on the value one whenever a trade liberalization, inflation stabilization, privatization or Brady plan program takes place during country *i*'s capital account liberalization episode. All specifications contain twenty five country-specific dummy variables. Panel B uses the full sample.

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)
Liberalization	0.0763*	0.0768*	0.0799*	0.0943**	0.0785*	0.0972***
	(0.043)	(0.044)	(0.042)	(0.034)	(0.039)	(0.034)
Trade		0.0278				0.0509
		(0.047)				(0.045)
Stabilization			-0.0681			-0.0577
			(0.11)			(0.099)
Privatization				-0.0642		-0.0603
				(0.054)		(0.049)
Brady Plan					-0.0446	-0.0011
					(0.136)	(0.116)
Constant	-0.0473	-0.0518	-0.0376	-0.0325	-0.0427	-0.0332
	(0.073)	(0.075)	(0.082)	(0.079)	(0.086)	(0.09)
Observations	146	146	146	146	146	146
R-squared	0.169	0.17	0.176	0.182	0.171	0.189

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)
Liberalization	0.0837***	0.0843***	0.0874***	0.0957***	0.0883***	0.1026***
	(0.024)	(0.026)	(0.022)	(0.014)	(0.019)	(0.016)
Trade		0.0267				0.0694***
		(0.024)				(0.024)
Stabilization			-0.0911			-0.0622
			-0.0800			(0.067)
Privatization				-0.0451		-0.0430
				-0.0400		(0.041)
Brady Plan					-0.0988	-0.0676
					-0.0950	(0.09)
Constant	-0.0513	-0.0557	-0.0377	-0.0405	-0.0410	-0.0361
	(0.067)	(0.067)	(0.069)	(0.071)	(0.069)	(0.071)
Observations	146	146	146	146	146	146
R-squared	0.032	0.033	0.053	0.04	0.052	0.072

Table 8. Liberalization Does Not Appear to Significantly Impact Employment.
Time and Country Fixed Effects.

The estimation procedure is ordinary least squares. All specifications contain year-specific and country-specific dummy variables. Standard errors appear in parentheses. The symbols (***), (**), and (*) represent significance levels of 1%, 5%, and 10%, respectively. For the regressions reported, the left-hand-side variable is the change in the natural log of employment over the full sample. LIBERALIZE is a dummy variable that takes on the value of one in the year that country i liberalizes (year [0]) and each of the subsequent three years ([1] [2] and [3]). TRADE, STABILIZE, PRIVATIZE, and BRADY are dummy variables that take on the value one whenever a trade liberalization, inflation stabilization, privatization or Brady plan program takes place during country i's capital account liberalization episode.

	(1)	(2)	(3)	(4)	(5)	(6)
Liberalization	-0.0150	-0.0141	-0.0154	-0.0189	-0.0061	-0.0106
	(0.021)	(0.022)	(0.021)	(0.023)	(0.02)	(0.021)
Trade		-0.0159				-0.0101
		(0.083)				(0.089)
Stabilization			-0.0148			0.0106
			(0.019)			(0.028)
Privatization				0.0115		0.0173
				(0.02)		(0.027)
Brady Plan					-0.1201	-0.1260
					(0.079)	(0.087)
Constant	-0.0443**	-0.0421**	-0.0448**	-0.0456**	-0.0211	-0.0203
	(0.019)	(0.021)	(0.019)	(0.019)	(0.027)	(0.03)
Observations	783	783	783	783	783	783
R-squared	0.12	0.12	0.12	0.12	0.122	0.122

Appendix A

Countries that had not 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.