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CAPITAL MARKET INTEGRATION AND WAGES

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ABSTRACT

For three years after the typical developing country 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 seven. No such increase occurs in a control group of developing 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 609 US dollars—an increase equal to 25 percent 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 abundant factor (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 factor 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.

stark as the movement of goods and people across-borders. In developing countries 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 developing countries experienced unprecedented increases in national income as a result of globalization in the 1980s and 1990s. If all of the developing countries' 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 developing countries 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 the developing world. This paper provides the first systematic attempt to fill that gap.

Figure 1 demonstrates that in a sample of eighteen developing countries that liberalized restrictions on 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

²Aitken, 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 18 countries.

to an average of 8.6 percent in the year liberalization occurred and each of the subsequent two years. The 7.3 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 609 US dollars—an increase equal to twenty-five 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 trend wage growth across countries. We also control for the impact of contemporaneous macroeconomic reforms such as inflation stabilization, trade liberalization, and privatization programs. In every specification, we find an economically and statistically significant increase in real wage growth for countries in the liberalization group but no effect for 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 10.1 percentage

points higher during the three-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 developing countries import from industrial nations (Alfaro and Hammel, 2007). If technology diffuses from developed to developing countries 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 Mullainathan, 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 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 real wage growth (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 eighteen 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 Developing Countries

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 is that capital market integration 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, towards a steady state in which ratios of capital-to-effective labor 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. 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:

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

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:

$$f'(k_{s.state}^*) = r^* + \delta \tag{4}$$

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

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

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

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).

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

Testing 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 can generally be obtained. In theory, opening the capital account is as simple as pulling a lever. In reality, a country's capital account has many components, so trying to determine exactly when it liberalizes the capital account (as in Section 2A) is perhaps the most difficult task in trying to assess the economic impact of changes in capital account policy.

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 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* can generate spurious 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 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 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.⁵

In contrast to the previous literature, this paper addresses 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, it identifies 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

⁵ For discussions of why this may not occur in practice, see Alfaro, Kalemli-Ozcan, and Volosovych (2008) and Shleifer and Wolfenzon (2002).

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.⁶

As the closest empirical analogue to the textbook example in Section 2A, stock market liberalizations provide just such experiments (Frankel, 1994). Accordingly, 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 2A. According to Standard and Poor’s Emerging Markets Database, there are 53 developing countries with stock markets. Of these 53 countries, 18 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.⁷ 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

⁶ For a detailed explanation of why stock market liberalizations are an accurate proxy for a broader easing of restrictions on capital inflows, see Section 6.2 of Henry (2007).

⁷ 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 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.

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.⁸

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 for us to interpolate the growth rate of the capital stock during liberalization episodes when we are 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

⁸ 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 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 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 a manner we now describe.

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 whose stock markets remain closed to foreign investment are not appropriate for the task at hand. The purpose of the control group is to determine whether a global economic shock unrelated to opening up drives the temporary increase in real wage growth in the liberalizing countries. 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 A) 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. Also, 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 paper purchased stock there (as a 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.⁹

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 occurs in years $[0, 1, 2]$.¹⁰ 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

⁹ The entire set of control countries for each liberalization year appears in Appendix A.

¹⁰ See Section 4.1 of Henry 2007.

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 (year [3]) 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 then the estimated 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.

3B. Descriptive Findings and Data Concerns

Figure 1 dispels the concern that an exogenous global shock drives the real wage increase in liberalizing countries. The y-axis 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 undergoing liberalization. The dashed line plots the mean for the control group.¹¹ 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. While the flatness of the dashed line in Figure 1 suggests that a common global shock does

¹¹ 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, 2]. This yields a set of control-group-real-wage paths for the fixed liberalization-country. Next, calculate the mean over the entire set of all control-group-real-wage paths to create a single path of the real-wage growth for the control group associated with the given liberalizing country. Repeating this procedure for each of the other seventeen liberalizing countries gives 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.

not explain the inflection in the solid line, with only eighteen countries in the sample an important question is whether a few outliers drive the increase.

The descriptive statistics in Table 1 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. Turning from means to medians, we also performed simple sign tests on the data for each country. 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 than 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 reasonably consistent increase in real wage growth across countries, three other questions about the data remain.

3B.1 Hours Worked

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 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 (<http://www.ggdc.net/databases/ted.htm>), 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." 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 only available for 43 countries. Eight of those 43 countries are also in our dataset: Argentina, Brazil, Chile, Colombia, Korea, 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 7.63. In levels these numbers translate to an average of 2080 hours worked per year prior to liberalization and 2059 hours worked per year after. Dividing 2080 and 2059 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 eight 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 Exchange Rate Issues

The second additional concern about the data is that 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 market integration on

¹² As a final check we also used the GGDC data to construct a measure of hourly wages for the subset of eight 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 3 through 5.

the labor market. Figure 5 addresses the concern about real appreciation by replicating Figure 1 using wages measured in real local currency terms instead of dollars. Since Figure 5 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.

3B.3 Concurrent Economic Reforms

The third 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 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, which presents our formal empirical methodology and results, uses the information in Table 2 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 panel regression:

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

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 (6), the constant, a_0 , measures average annual wage growth over the entire sample period after controlling for the country-fixed effects denoted by the variable $COUNTRY_i$. 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 during the three-year liberalization episode.

The variable $CONTROL_{it}$ 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 liberalizing countries. An alternative way of ensuring that the coefficient on the liberalization variable reflects the impact of country-specific liberalization policy and not that of a common shock would be to drop the variable $CONTROL_{it}$ and add year-fixed effects. We report the output from such a regression in Subsection 4A.1.

The right-hand-side of equation (6) also contains three additional dummy variables— $STABILIZE$, $TRADE$, and $PRIVATIZE$ —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 two subsequent years.

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.

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, 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 accurate standard errors we employ various clustering procedures described below.

4A. Benchmark Estimates

Table 3 (Panel A) presents the results from our main estimate of Equation (6). 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 0.051 to 0.086. During liberalization episodes the average growth rate of the typical country's real wage exceeds its long-run mean by 5.1 to 8.6 percentage points per year. Every estimate of the coefficient on the liberalization dummy in Panel A of Table 3 is statistically significant at the 1 percent level.

An exogenous global shock to wages does not seem to drive the result. The estimate of the coefficient on *CONTROL* ranges from negative 0.011 to negative 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 0.073. 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 -0.088 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).

Table 4 (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).¹³ 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 3 (although they are very similar). Focusing, then, on the precision of the estimates, we see that the estimate of every coefficient in Table 4 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 A Different Specification: Country and Year Fixed Effects, no Control Dummy

As an alternative way of examining whether the coefficient on the liberalization variable

¹³ For another discussion of multi-way clustering see Cameron, Gelbach, and Miller (2009).

truly reflects the impact of country-specific liberalization policy versus that of a common global shock, we drop the variable $CONTROL_{it}$ in Equation (6) and add a full set of year-fixed effects in its stead. Specifically, we estimate:

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

where the variable $YEAR_t$ is shorthand for the set of year-fixed effects. The standard errors are clustered by year to be consistent with our benchmark specification in Table 3.

Table 5 (Panel A) demonstrates that the results from the year and country fixed effects regression specification are very similar to the results that rely on the control group dummy variable. The coefficient on the liberalization dummy is statistically significant in every specification, and the point estimates range from 0.086 to 0.111. Although the point estimates of the liberalization coefficient in the year and country fixed effects regression are larger than those in the specification that uses the control group dummy variable, we prefer to use the smaller, more conservative estimates from our benchmark specification (Panel A of Table 3).

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.¹⁴

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 (6), 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) 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 0.073. Adding the constant and the coefficient on the liberalization dummy gives the average growth rate of the real wage during liberalization episodes—8.6 percent per year. This means that in the year the liberalization occurs and each of

¹⁴ 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.

the subsequent two, the average growth rate of the real wage is almost 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 2392 US dollars. During the three-year liberalization window the real wage grows at 8.6 percent per year, so that by the end of year [2] the average level of the real wage is $2392 e^{0.086*3} = 3096$ US dollars. Now assume that that in the absence of liberalization the real wage would have grown at the sample mean of 1.3 percent per year. In that case, the level of the real wage at the end of year [2] would be $2392 e^{0.013*3} = 2487$ 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 609 dollars higher (3096 minus 2487) than it would have been in the absence of liberalization. This change in the level of the wage is equal to a quarter 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 1.3 percent per year for the next decade. Under that scenario, the real wage in year [+5] would have been 3, 230 dollars. The actual level of the real wage in year [+5] is 4,496 dollars, 39.2 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 regression:

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

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. Again, to be consistent with the wage estimates we cluster the standard errors by year.

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 0.056 to 0.101. Every estimate of the coefficient on the liberalization dummy in Panel B of Table 3 is statistically significant. 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, is responsible for 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 0.101. This means that the average growth rate of productivity is 10.1 percentage points higher during the three-year liberalization window than in non-liberalization years. The 10.1 percentage point increase in productivity growth associated with liberalization is larger than the 7.3 percentage point increase in wage growth. Because the increase in productivity outstrips the increase in wage growth, manufacturing-sector profitability actually rises during liberalizations.¹⁵

Tables 4 and 5 (Panel B) show that the results for productivity growth and liberalization are robust to all of the statistical concerns raised about the estimation of wage growth and liberalization examined in Sections 4A and 4A.1.

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): $\frac{\dot{w}}{w} = \frac{\dot{A}}{A} + \frac{1}{\sigma} \cdot \frac{f'(k)k}{f(k)} \cdot \frac{\dot{k}}{k}$. 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.

¹⁵ Chari and Henry (2008) also find that the return to capital in the manufacturing sector rises during liberalizations.

Specifically, we have:

$$\Delta \frac{\dot{w}}{w} = \frac{1}{\sigma} \cdot \frac{f'(k)k}{f(k)} \cdot \Delta \frac{\dot{k}}{k} \quad (9)$$

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.¹⁶

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 7.3-percentage-point increase in wage growth you need an elasticity of substitution less than or equal to 3/8. 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 1/5. There is little consensus on the size of the elasticity of substitution. Early work estimated small elasticities that were statistically indistinguishable from zero.¹⁷ More recent studies cannot reject the hypothesis that the elasticity of substitution is 1 (Caballero,

¹⁶ See Henry (2007), pp. 897-900.

¹⁷ 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.¹⁸ 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).

¹⁸Also, 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.¹⁹ Specifically, the advent of increased capital market integration increased the average take-home pay of workers in the manufacturing sector by 25 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

¹⁹ 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.²⁰ As the quality and breadth of data on labor markets in developing countries continues to improve, future work may produce more definitive conclusions.

²⁰ 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.

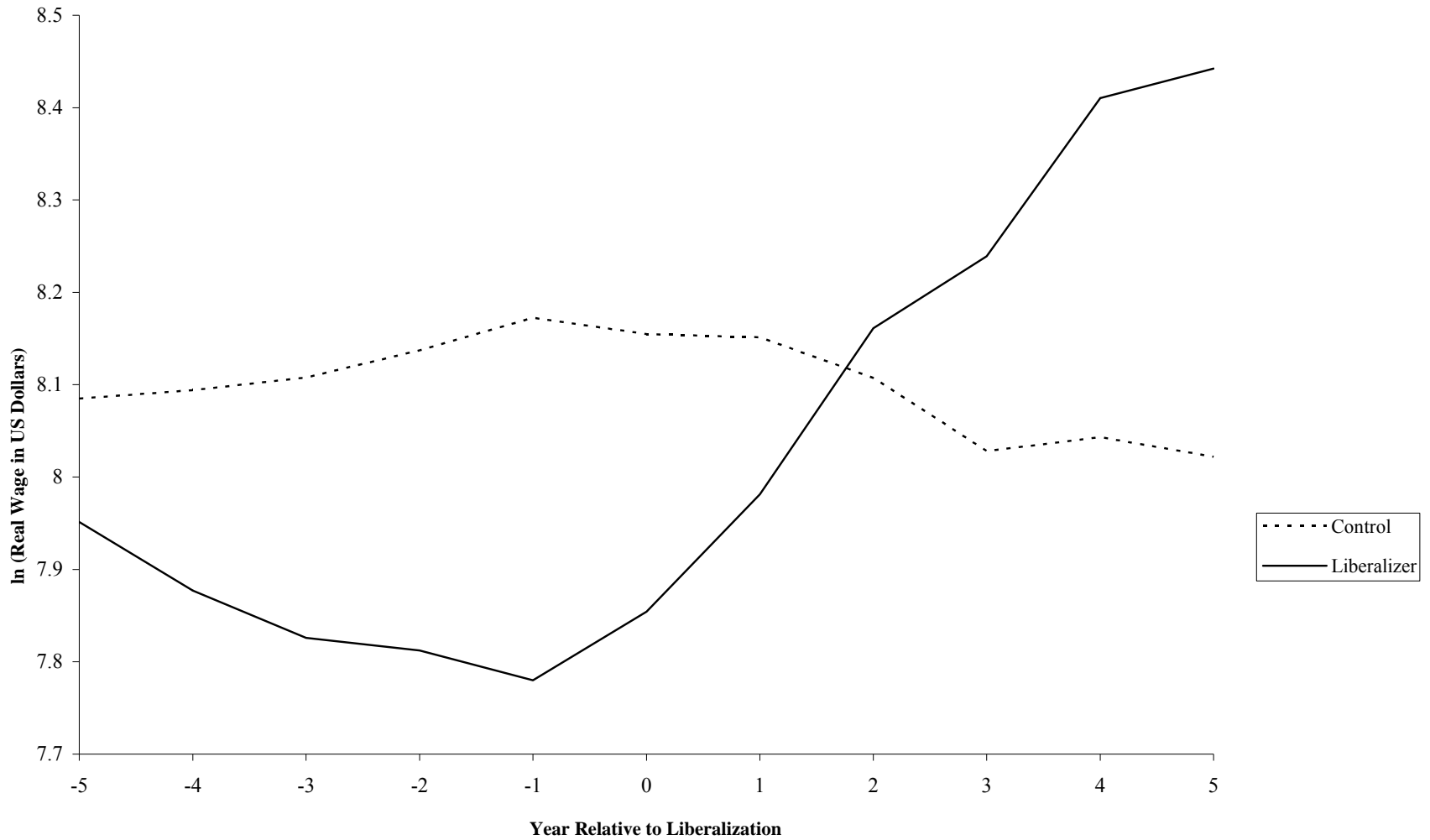


Figure 2. Productivity Growth Rises in the Aftermath of Capital Account Liberalizations.

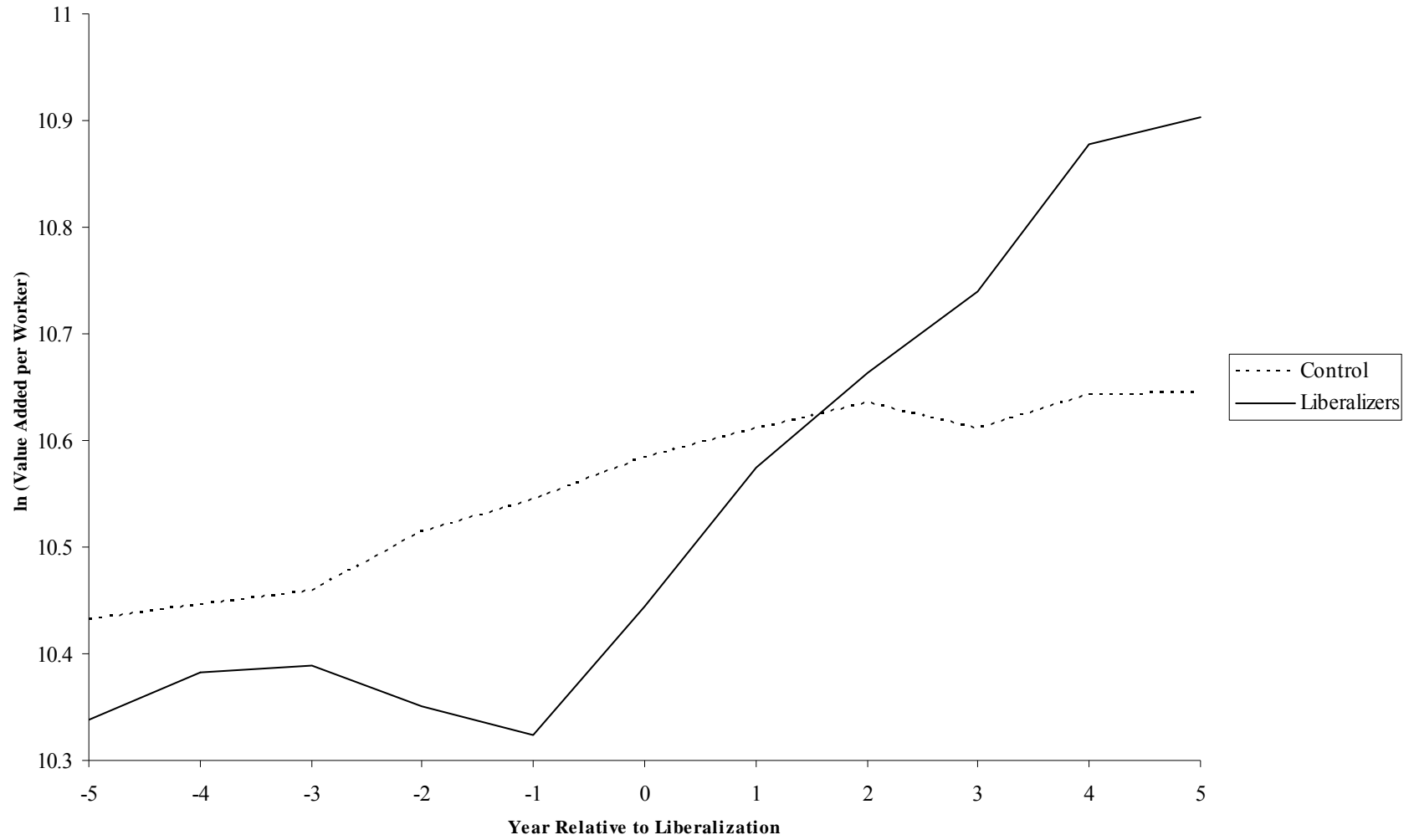


Figure 3. Hypothetical Impact of Liberalization on the Cost of Capital, Investment and the Real Wage.

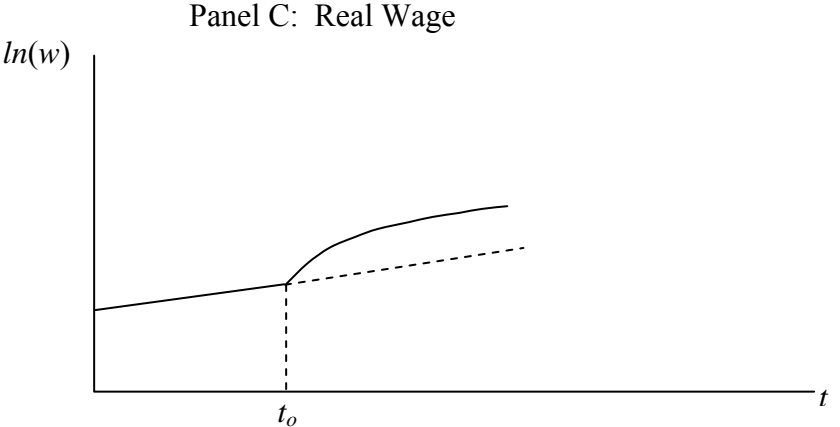
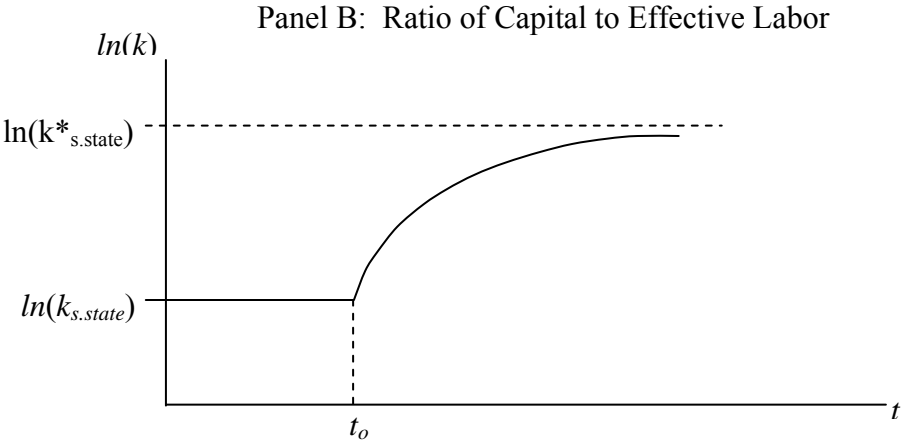
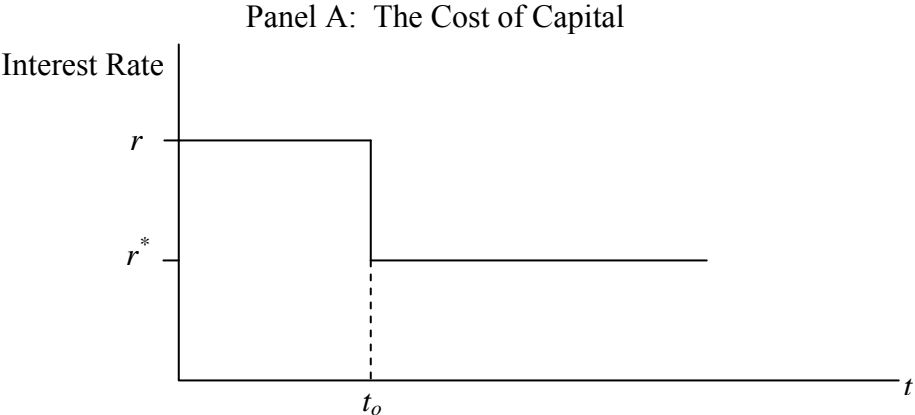


Figure 4. The Average Number of Hours Worked Does Not Rise With Liberalization

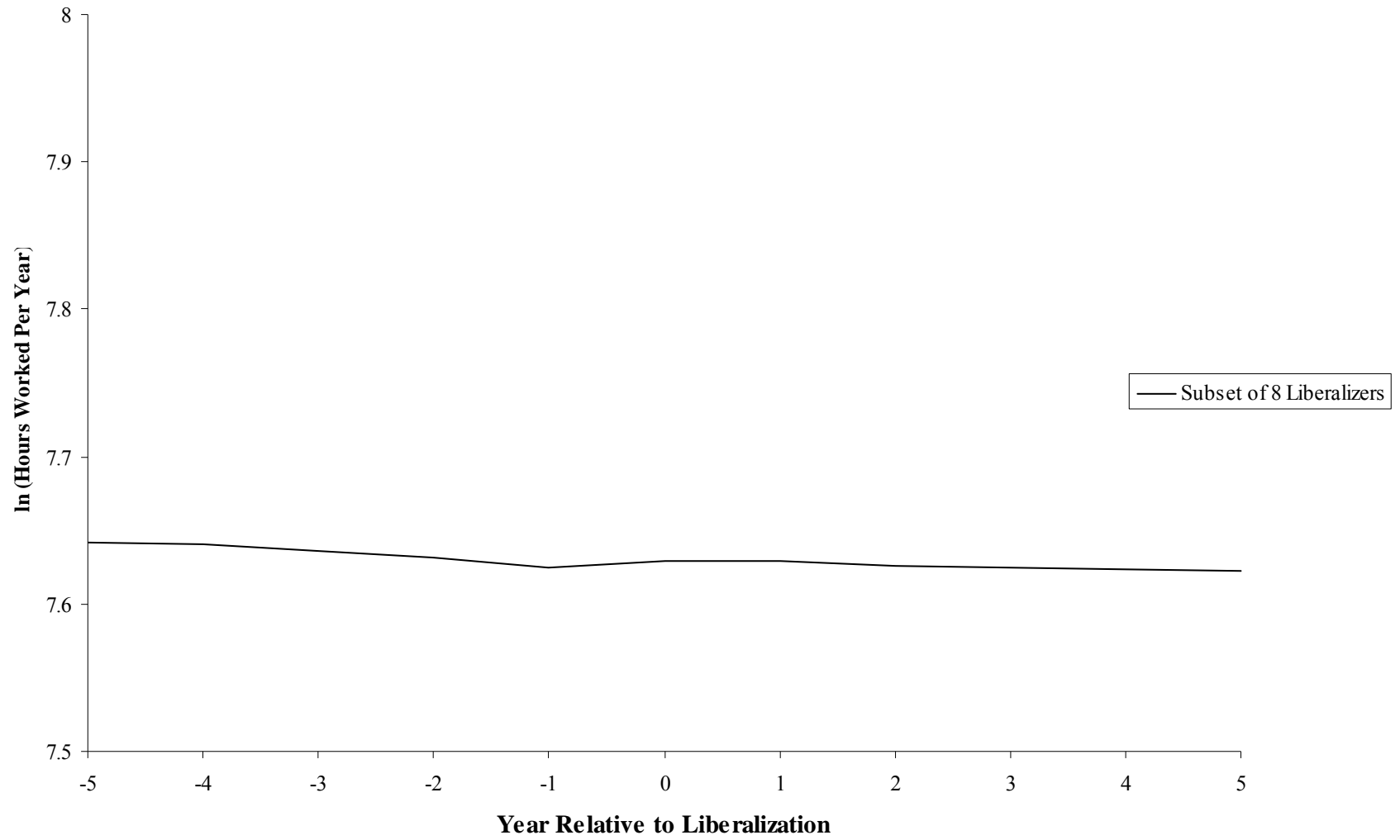


Figure 5. Appreciation of the Real Exchange Rate Does Not Drive the Increase in Wage Growth.

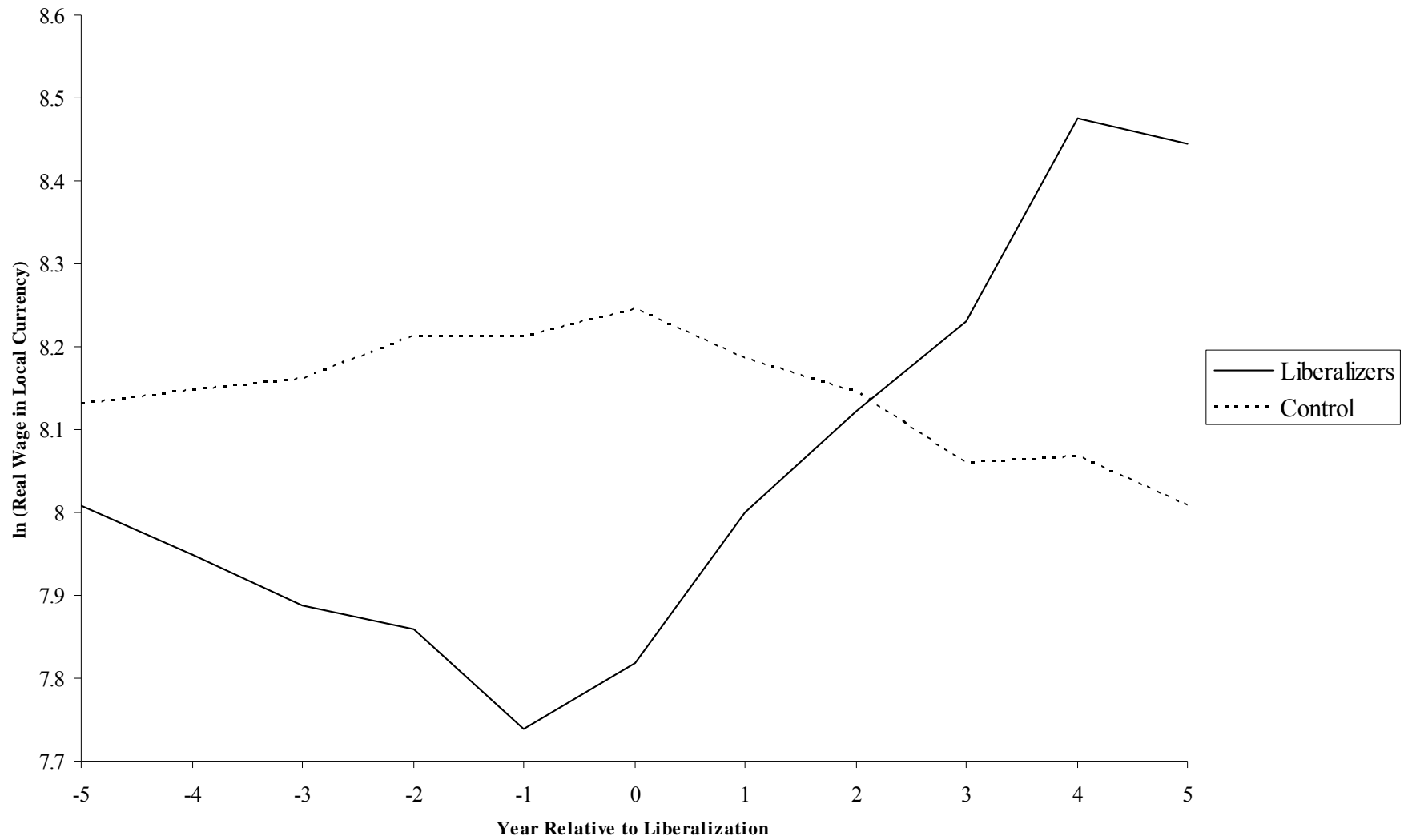


Table 1. In the Aftermath of Liberalizations, the Growth Rate of the Real Wage Rises Consistently Across Countries.

Country	Year of Liberalization	Liberalization Aftermath		Full Sample			Years of Data Coverage
		Mean	Median	Mean	Median	Standard Deviation	
Argentina	1989	3.9%	3.9%	-2.6%	-2.0%	32.6%	1984-2001
Brazil	1988	n.a.	n.a.	11.8%	14.9%	7.7%	1992-1995
Chile	1987	2.8%	3.0%	0.9%	4.0%	18.8%	1963-2000
Colombia	1991	5.0%	5.9%	-0.4%	-2.0%	9.1%	1963-1999
India	1986	2.4%	2.1%	-1.4%	0.0%	7.7%	1963-2002
Indonesia	1989	-8.3%	0.8%	5.9%	5.3%	16.6%	1970-2003
Jordan	1995	2.4%	1.7%	-0.2%	-0.3%	14.3%	1963-2003
Malaysia	1987	-5.4%	-4.1%	1.8%	2.0%	5.5%	1968-2002
Mexico	1989	11.7%	11.7%	1.5%	9.6%	19.3%	1984-2000
Nigeria	1995	11.5%	11.5%	1.9%	2.4%	14.8%	1963-1996
Pakistan	1991	-2.2%	-2.2%	1.2%	2.1%	10.9%	1963-1991
Philippines	1986	6.9%	10.0%	-0.1%	1.3%	12.7%	1963-1997
South Korea	1987	2.3%	5.6%	5.5%	8.4%	14.6%	1963-2002
Taiwan	1986	17.6%	16.3%	7.2%	6.4%	7.2%	1973-1997
Thailand	1987	14.7%	14.7%	5.9%	8.4%	24.0%	1967-1994
Turkey	1989	30.9%	27.0%	1.5%	3.7%	19.4%	1963-1997
Venezuela	1990	0.3%	0.0%	-3.4%	1.2%	22.1%	1967-1998
Zimbabwe	1993	5.9%	4.4%	-0.7%	-0.1%	12.7%	1963-1996

“Liberalization Year” is the year in which the country identified in column 1 opened its stock market to foreign investment as defined in Section 2A. The columns labeled “Liberalization Aftermath” give the mean and median growth rate of the real wage over the liberalization year and the two subsequent years. The column labeled “Full Sample” gives the mean and median over the entire sample for each country. The column labeled “Data Coverage” gives the years for which we have data for each country.

Table 2. 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	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

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 from 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 3. Liberalization Temporarily Increases the Growth Rate of Real Wages and Productivity.
Country Fixed Effects, Standard Errors Clustered by Year**

	Panel A: Real Wages					Panel B: Productivity				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
CONSTANT	0.0119 (0.0116)	0.01084 (0.0114)	0.0138 (0.0114)	0.0119 (0.0116)	0.0127 (0.0113)	0.0195* (0.0116)	0.0192 (0.0117)	0.0218* (0.0115)	0.0195 (0.0116)	0.0215* (0.0116)
LIBERALIZE	0.0636*** (0.0202)	0.0589*** (0.0189)	0.0864*** (0.0239)	0.051*** (0.0221)	0.073*** (0.0223)	0.0556** (0.0253)	0.0543** (0.0251)	0.0832*** (0.0285)	0.0708** (0.0308)	0.101*** (0.0314)
CONTROL	-0.0153 (0.0144)	-0.01979 (0.0146)	-0.0112 (0.0145)	-0.0182 (0.0163)	-0.0177 (0.0163)	-0.0099 (0.0195)	-0.0111 (0.0196)	-0.0049 (0.0193)	-0.0063 (0.0206)	-0.0021 (0.0200)
TRADE		0.0479* (0.0266)			0.0484* (0.0278)		0.0131 (0.0225)			0.0176 (0.0228)
STABILIZE			-0.0882** (0.0420)		-0.0883** (0.0419)			-0.107*** (0.0362)		-0.109*** (0.0369)
PRIVATIZE				0.0245 (0.0374)	0.0165 (0.0384)				-0.0295 (0.0315)	-0.0364 (0.0317)
Number of Observations	502	502	502	502	502	502	502	502	502	502
Adjusted R-Squared	0.0165	0.0199	0.0348	0.0165	0.0385	0.0144	0.0146	0.0441	0.0174	0.0489

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. 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. For the regressions reported in Panel B, the left-hand-side variable is the change in the natural log of value-added per worker. *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 two years ([1] and [2]). *CONTROL* 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. *TRADE*, *STABILIZE*, and *PRIVATIZE* are dummy variables that take on the value one whenever a trade liberalization, inflation stabilization, or privatization program takes place during country *i*'s capital account liberalization episode. All specifications contain seventeen country-specific dummy variables.

Table 4. Liberalization Temporarily Increases the Growth Rate of Real Wages and Productivity.
Standard Errors Clustered by Year and Country

	Panel A: Real Wages					Panel B: Productivity				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
CONSTANT	0.012 (0.011)	0.011 (0.011)	0.014 (0.010)	0.012 (0.011)	0.013 (0.011)	0.019* (0.011)	0.019* (0.010)	0.021** (0.010)	0.019* (0.011)	0.021** (0.010)
LIBERALIZE	0.063*** (0.012)	0.060*** (0.013)	0.088*** (0.031)	0.058*** (0.023)	0.082*** (0.038)	0.055*** (0.021)	0.055*** (0.020)	0.084** (0.034)	0.073** (0.030)	0.104*** (0.040)
CONTROL	-0.014 (0.017)	-0.018 (0.016)	-0.010 (0.018)	-0.015 (0.019)	-0.014 (0.019)	-0.008 (0.017)	-0.009 (0.018)	-0.003 (0.017)	-0.004 (0.019)	0.000 (0.018)
TRADE		0.040 (0.032)			0.043 (0.030)		0.010 (0.024)			0.017 (0.018)
STABILIZE			-0.094* (0.051)		-0.096* (0.051)			-0.108** (0.054)		-0.111** (0.056)
PRIVATIZE				0.011 (0.04)	0.040 (0.042)				-0.035 (0.030)	-0.040 (0.030)
Number of Observations	502	502	502	502	502	495	495	495	495	495
Adjusted R-Squared	0.017	0.020	0.035	0.017	0.039	0.014	0.015	0.044	0.018	0.049

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. For the regressions reported in Panel B, the left-hand-side variable is the change in the natural log of value-added per worker. *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 two years ([1] and [2]). *CONTROL* 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. *TRADE*, *STABILIZE*, and *PRIVATIZE* are dummy variables that take on the value one whenever a trade liberalization, inflation stabilization, or privatization program takes place during country *i*'s capital account liberalization episode.

Table 5. Liberalization Temporarily Increases the Growth Rate of Real Wages and Productivity
Time and Country Fixed Effects, Standard Errors Clustered by Year

	Panel A: Real Wages					Panel B: Productivity				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
CONSTANT	0.015 (0.026)	0.015 (0.026)	0.015 (0.026)	0.015 (0.026)	0.016 (0.026)	0.033 (0.057)	0.033 (0.057)	0.032 (0.058)	0.033 (0.057)	0.032 (0.058)
LIBERALIZE	0.097*** (0.019)	0.097** (0.019)	0.118*** (0.022)	0.086*** (0.020)	0.111*** (0.020)	0.074*** (0.026)	0.074*** (0.026)	0.101*** (0.029)	0.082*** (0.027)	0.113*** (0.028)
TRADE		0.044 (0.027)			0.047 (0.030)		0.011 (0.024)			0.019 (0.024)
STABILIZE			-0.086** (0.043)		-0.087*** (0.043)			-0.111*** (0.036)		-0.112*** (0.037)
PRIVATIZE				0.028 (0.048)	0.020 (0.050)				-0.022 (0.036)	-0.029 (0.036)
Number of Observations	502	502	502	502	502	495	495	495	495	495
Adjusted R-Squared	0.11	0.114	0.128	0.111	0.131	0.167	0.167	0.198	0.169	0.210

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 in Panel A, the left-hand-side variable is the change in the natural log of the real wage. For the regressions reported in Panel B, the left-hand-side variable is the change in the natural log of value-added per worker. *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 two years ([1] and [2]). *TRADE*, *STABILIZE*, and *PRIVATIZE* are dummy variables that take on the value one whenever a trade liberalization, inflation stabilization, or privatization program takes place during country *i*'s capital account liberalization episode.

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.

Countries in the Control Group for a Given Liberalization Year

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