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Economic Reform and Wage Differentials in Latin America

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Abstract¹

This paper develops and applies a new approach to the estimation of the impact of economy-wide reforms on wage differentials, using a new high-quality data set on wage differentials by schooling level for 18 Latin American countries for the period 1980-1998. The results indicate that reform overall has had a short-run disequalizing effect of expanding wage differentials, although this effect tends to fade over time. This disequalizing effect is due to the strong impact of domestic financial market reform, capital account liberalization and tax reform. On the other hand, privatization contributed to narrowing wage differentials, and trade openness had no effect on wage differentials. Technological progress, rather than trade flows, appears to be a channel through which reforms are affecting inequality. The paper also explores the effects of reforms on wage levels; tentative results suggest that reforms have had a positive effect on real average wages, but a negative effect on the wages of less-schooled workers.

Keywords: reform, inequality, wages, trade, distribution

JEL Classification: D31, J31

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Introduction

The two-decade old trend of an increase in the wage differential between less-schooled and more-schooled workers in the U.S. is conventionally attributed to some combination of skill-biased technological change and the effect of trade. In particular, it is trade with low-wage developing countries that has been blamed, with more imports from low-wage developing countries reducing the demand for and wages of the less-schooled, as the threat of more imports and of employers investing overseas undermines wage demands of the less-schooled in the U.S.²

The same trend of growing wage differentials between less-schooled and more-schooled workers is evident in emerging markets as well. As we document below, the trend is notable in the last 10 to 15 years in Latin America. Traditionally, the high income inequality and wage differentials in this region had been attributed to supply-side factors such as the scarcity of well-educated labor.³ But during the late 1980s and the 1990s, the discussion has shifted to emphasizing the major changes taking place on the demand side, due mainly to the economic restructuring and opening to international markets undertaken by most countries. Many analysts and policymakers had assumed that these reforms would better tap the comparative advantage of the region vis-à-vis the northern markets, generate new jobs for relatively less-schooled workers, and reduce wage differentials between less-schooled and more-schooled workers. From this perspective, the increasing wage differentials in the region are indeed an unwelcome surprise.⁴

This paper assesses the effects of various economic reforms on wage differentials in Latin America during the past two decades. We investigate whether the effects of six different policy reforms have immediate and/or lasting effects on relative wages. If reforms have affected wages, then

² Trade as opposed to technological change is estimated to account for between about 20 and 40 percent of the increase in the skilled-unskilled wage differential (e.g., Helpman and Krugman, 1989 and Wood, 1997. Cline (1997) summarizes the literature for the United States, and also cites increased immigration as important. Aghion, Caroli and García-Peñalosa (2000) review literature suggesting additional mechanisms through which trade affects the differential, e.g., trade liberalization reduces the price of intermediate goods that are substitutes for unskilled labor.

³ Birdsall, Ross and Sabot (1995) compare the effects of schooling access on wage and income inequality in East Asia and Latin America. They emphasize the effect in Latin America of limited public spending on basic schooling in reducing university access and generating high returns to higher education for the limited number of successful graduates. Behrman, Duryea and Székely (1999) compare schooling developments in Latin America and some of the fastest growing economies in East Asia and document the increasing divergence in recent decades.

⁴ French-Davis (2000) concludes that the potential benefits of trade realization were lost in many countries of the region because exchange rates remained overvalued (often due to their use as anti-inflation anchors). See also Escaith and Morley (2000), who conclude that the implicit strategy of export-led growth only succeeded in the late 1990s in a few countries of the region, including Mexico, that were able to tap the high-growth U.S. market. In South America, most countries have actually lost market share in the industrialized countries to other developing countries in the 1990s.

income inequality has increased, or decreased less than it might have, because it is primarily the distribution of labor income that governs the overall distribution of income in the region.⁵ The question is important because of long-standing concerns about high inequality in the region, but uncertainty regarding whether stabilization and structural reforms have contributed to that inequality.⁶

Our basic contribution is to develop and apply to a rich new data set a new approach to estimating the differential impact of reforms on labor market returns to workers at different schooling levels, while controlling for all fixed and time-varying country characteristics -- the effects of which otherwise probably would be confounded with the effects of the reforms. The estimates are based on a new high quality data set on Latin America that we developed for this purpose. This data set includes comparable information on urban wages and education for 18 Latin American countries over the period 1980-1998, which we compute directly from 79 household surveys, merged with annual indices of six basic aspects of economy-wide policy reforms. The combination of household-level survey data for many countries and years, with country and year-specific information on policy reform efforts constitutes a significant advance in itself.⁷ Lack of such data in the past meant that previous studies of the effect of reforms on wage differentials have had to focus on specific industries or small regions within a country, implying limited variation in aggregate reforms on which to base their analysis. For example, studies focusing only on specific industries have missed an important part of

⁵ Székely and Hilgert (1999a) show that changes in the distribution of labor income have been the main reason why overall income inequality has failed to decline in Latin America during the 1990s. Changes in the returns to education, which we analyze below, are of course not the only factor affecting the overall distribution of labor income. Changes in the educational composition of the labor force can offset or reinforce growing wage differentials between more and less-schooled workers, as can increases in the variance of the returns within education groups. See Birdsall, Ross and Sabot (1995) for a comparison of East Asia and Latin America taking into account composition as well as changing returns, and Morley (forthcoming) for analysis of these different effects for some countries of Latin America.

⁶ For the effects of reforms on growth in Latin America, see IDB (1997) and Lora (1997). Morley *et.al.* (1999) report that despite reforms, average per capita income growth, which was 2.9 percent in the region for the years 1991-94, fell to 0.8 percent between 1995 and 1999. Morley (2000a) reports a small disequalizing effect of reforms on income inequality in Latin America; and, controlling for reforms, unexplained changes in the effect of growth on income inequality over time, i.e. that the effects of growth have become less progressive between the 1970s and the 1990s in the region.

⁷ To our knowledge, this panel data set is the most comprehensive and up-to-date on wage differentials for Latin America. The other available data sets with information on inequality or industry-specific differentials are not suited for our analysis. For instance, the well-known compilation of income distribution indicators by Deininger and Squire (1996) mixes information on wages with other income sources, which make it difficult to interpret the effects of reform. Furthermore, the coverage of non-labor incomes is very heterogeneous, making it impossible to know how much of the differences in inequality across countries is genuine and how much is “noise” introduced by the lack of consistency. Other options such as the data base on selected industries by UNIDO (2000) refer to a small sample of manufacturing industries that only could

the picture. One of the major effects of reforms is to trigger resource reallocations throughout the economy that affect the size and wages of some sectors directly, but that can also have important indirect effects on other sectors. For instance, due to reforms, wage differentials in some manufacturing sub-sectors may decline, but due to the same reforms, the differences among manufacturing sub-sectors, or the wage differentials in other sectors of the economy may be expanding. Analysts looking at a subset of industries observe only partial effects, yet the magnitude and direction in which wage differentials change overall may be much different than such partial effects.

Because reforms were designed to make economies more competitive and to increase economic growth, the question also arises of whether reforms have affected wage levels regardless of their distributive impact. We use our data set to explore this issue, but as discussed below, because the data has greater limitations for this question, these results require stronger assumptions and thus are more qualified. Additionally, it is of interest to know if the effects of reform in Latin America are a harbinger of increasing wage inequalities in other developing regions engaging in market liberalization processes, or if they are the outcome of interactions with country-specific factors that are not relevant for other countries. We also address this issue, but because there are limits to how well we can characterize different environments, these results also must be qualified.

The paper is divided into five sections. Section 1 discusses a framework for understanding the potential effects of various economy-wide reforms on wage differentials between less-schooled and more-schooled workers. Section 2 presents the data and provides up-to-date evidence on the evolution of wage differentials and on the pace of reform in Latin America. Section 3 discusses estimation issues. Section 4 presents our empirical results. Section 5 concludes.

1. Framework for Analysis

Our primary interest in this paper is in the question of how economy-wide reforms have affected the wages of less-schooled relative to more-schooled workers. We also have a secondary interest in the impact of such reforms on real wages for all schooling levels, and on the effect of reforms in

different environments. We consider six types of economy-wide reforms:

be used to capture partial effects of reforms, which, as discussed in the text, may be much different than the overall effects.

- (1) privatization of former state enterprises,
- (2) trade liberalization,
- (3) capital account liberalization,
- (4) domestic financial market liberalization,
- (5) tax reforms,
- (6) labor market reforms.

Each of these reforms may have effects on the demands for and the supplies of both less-schooled and more-schooled workers, and thus on the wages for both types of workers. Their impact on relative wages depends on the relative magnitudes of the underlying labor demand and labor supply shifts in the two inter-related markets for less- and more-schooled workers.

Given the available data described in Section 2, it is not possible to identify the exact mechanisms through which these reforms may affect the relative wages of less- versus more-schooled workers.⁸ Moreover, for several of the reforms there are counteracting possibilities, so that the signs of the effects cannot be predicted unambiguously from theory. However, it is still useful to consider some of the possibilities within standard frameworks of the underlying behaviors for entities on both sides of these labor markets. Most of the literature on increasing inequalities has focused on labor demands, to which we turn first.⁹ We then turn to labor supplies, which have been more emphasized in the traditional labor and human resource literatures (e.g., Pencavel, 1986). We also comment on the possible differences between initial and longer-term effects of the various reforms, and suggest a general hypothesis regarding the lagged effects of reforms.

⁸ To identify such mechanisms would require information with which to estimate, on disaggregated levels, the structural production and other relevant relations underlying the determination of labor demand relations through profit maximization on one hand and the preference and other relevant relations underlying the determination of labor supply relations through constrained individual or household welfare maximization on the other.

⁹ See, for example, Aghion, Caroli and García-Peñalosa (1999), Bartel and Lichtenberg (1997), Beyer, Rojas and Vergara (1999), Borjas and Ramey (1995), Currie and Harrison (1997), Hanson and Harrison (1999), Harrison and Gordon (1999), Katz and Murphy (1992), Murphy and Katz (1992), Murphy and Welch (1992), Revenga (1992, 1997), Spilimbergo, Londoño and Székely (1999), and Wood (1997). Birdsall, Ross and Sabot (1995) contrast the effect on increasing labor demand, including that for skilled workers, associated with manufactured export production in East Asia in comparison with Latin America, even given the more rapid increase in the supply of skilled labor in East Asia.

Labor Demands

Labor demands of profit-maximizing firms can be derived from profit maximization given the nature of product markets, factor markets, firms' fixed factors and production technologies, and the policy regimes in which they operate. Firms may have market power in product or in factor markets, and the extent of this market power may be affected by reforms.¹⁰ In the short run firms can adjust their product and factor demands, given their fixed factors and technology. The extent of adjustment is likely to depend importantly on the nature of the underlying technology—in particular, to what extent different factors of production are substitutes or complements. For example, more-schooled workers and capital are often assumed to be complements (or at least more complements than less-schooled workers and capital, e.g., Galor and Moav 2000a), and less-schooled workers and intermediate inputs are often assumed to be substitutes (or at least more substitutes than more-schooled workers and intermediate inputs, e.g., Aghion, Caroli and García-Peñalosa, 2000). If so, reductions in the price of capital increase demand relatively for more-schooled workers, and reductions in prices of intermediate inputs reduces demand relatively for less-schooled workers. With more time firms have more scope to adjust what are fixed factors and technologies in the short run. But the question of to what extent different factors of production that can be adjusted over the longer time period are substitutes or complements remains central.

The aggregate impact of reforms on labor demands, of course, depends on the net effects of the reforms across many production entities in many sectors. The composition of production—and thus of labor demands—is likely to change because reforms are likely to induce expansion of some firms and some sectors and contraction of others among those that may be characterized as behaving approximately as if they are profit maximizers. Moreover, economy-wide reforms of the sort being considered may have important effects on the extent to which production and labor demands come from firms that behave as if they are approximately maximizing profits versus those that are not. This is most directly the case for reforms that involve privatization of previous public enterprises. But it also may be the case for other types of reform to the extent that they change the incentives for profit-maximizing sectors versus others.

¹⁰ See, for example, Helpman and Krugman (1989) and Levinson (1993). This may have impact on labor markets; for one illustration, several recent studies have found evidence consistent with greater market discipline reducing gender gaps in wages (e.g., Behrman and King, 1999 and Black and Brainerd, 1999).

The complexity of the changes that might be induced by reforms, including such compositional changes, as well as timing effects, means that it is not possible to predict their effects on the relative wages of less- and more-schooled workers. The issue is fundamentally an empirical one. Nevertheless, it is useful to sketch out some of the more likely effects suggested by the literature.

(1) *Privatization of former state enterprises*: If state enterprises have too many workers of particular schooling levels given their level of production, privatization would seem to reduce the demand for that type of labor controlling for the production level. If privatization increases production sufficiently by making the former state enterprises more efficient and more aggressive in expanding their market shares, the result might be increased demands for the types of labor that are particularly employed by privatized enterprises. On balance, the overall result is likely to be reduced wage premia for schooling if, for example, state enterprises have relatively large numbers of managers (with more schooling) per production worker (with less schooling) than privatized firms in the same sector.

(2) *International trade liberalization*: Trade liberalization may have important effects on product markets, intermediate factor markets and capital goods markets, all of which may feed back on labor markets. On one hand, liberalization will reduce wage differentials if:

- Product market changes shift production in line with the classical comparative advantage theory towards a country's comparative advantage, which would seem to benefit less-schooled workers relative to more-schooled workers in most developing countries within the assumptions of the classical framework;
- Pre-liberalization policy in effect subsidized capital (for example via overvalued exchange rates), suppressing wages of the less-schooled because capital and skilled labor are complements in production.

But a number of possible counter effects could widen wage differentials if:

- The pre-liberalization framework protected unskilled workers, e.g. in agriculture or

textiles (possibly the case in Mexico—see Harrison and Hanson, 1999);

- Intermediate inputs of a given quality become cheaper and low-schooled workers are (at least relatively) substitutes for intermediate inputs;
- Capital goods of a given quality are likely to become cheaper and more-schooled workers are relatively complements with physical capital;
- New, more-schooled-worker intensive technologies become available and increase the demand for skills (as in Galor and Moav, 2000b);
- The gains from learning about new markets and new technologies increase due to more rapid changes in markets and technologies, for which schooling may have high returns, as emphasized by Welch (1970), Schultz (1975) and Rosenzweig (1995);
- The developing country or region of interest is a middle-income area with a comparative advantage that no longer is in low-wage, less-schooled labor, as suggested by Spilimbergo, Londoño and Székely (1999) for Latin America due to the expansion of China and other low-wage Asian economies into global markets.

(3) *Capital account liberalization*: Capital account liberalization is expected to increase the availability of capital for domestic investment, in which case some of the last aspects of trade liberalization just noted would seem to increase the wages of more-schooled workers. Of all the broad reforms we consider, moreover, capital account liberalization is most tightly tied to the credibility of reform. The reform sequencing literature suggests that capital market liberalization should be last in the sequence of reforms. If the relevant economic entities believe that this is the case, then capital account liberalization may be a strong positive indicator of the credibility of overall reform. If so, this could reinforce incentives for capital investments and technological changes of the types that would seem in the shorter run to increase the returns to more-schooled workers.

(4) *Domestic financial market liberalization*: Effective domestic capital market liberalization

is likely to facilitate financing of both current production and of longer-run investments in capital and technology. Improved financing of current production presumably lowers the effective cost of using all types of labor and of intermediate inputs. If intermediate inputs are greater substitutes for low-schooled than for more-schooled workers, *ceteris paribus* this leads to increases in wages of more- relative to less-schooled workers. Improved financing of longer-run physical capital investments and of new (at least imported) technologies also, as noted above, is likely to lead to increases in wages of more- relative to less-schooled workers. On the other hand, in a number of cases lessening domestic financial capital market distortions has meant lessening subsidies that larger firms previously received for capital investments through rationed credit that favored such investment. If such a phenomenon dominates and if capital and more-schooled workers are relatively complements, the result may be an increase in the wages for less-schooled relative to more-schooled workers.

(5) *Tax reforms*: Tax reforms ideally broaden the tax base and reduce price distortions in the economy. Effort in Latin America has focussed on implementing value-added taxes, reducing reliance on trade taxes, and reducing marginal income tax rates. These changes complement trade liberalization by increasing incentives for investment, with possible effects on wage differentials similar to the effects of trade liberalization. Similarly, reductions in corporate marginal taxes will promote investment, while reductions in personal marginal tax rates will reduce the progressivity of the income tax, and thus widen disposable income differentials. While the last of these has no obvious short run direct effect on pre-tax wage differentials, it may induce increased supplies of more-schooled workers, particularly in the long run, and thus reduce pre-tax wage differentials.

(6) *Labor reforms*: Labor market reforms are different than the other reforms discussed to this point because they directly may have impact not only on labor demands, but also on labor supplies (that are discussed next). Changes in labor laws and regulations that reduce labor market rigidities and distortions should raise the demand for labor relative to capital, with effects on returns to different levels of schooling depending on whether any pre-existing bias against labor was in fact favoring more- schooled over less- schooled workers (or *vice versa*). The effect of labor reforms that improve the framework for collective bargaining, for

example by improving the governance of unions, is probably favorable to less-schooled workers among those covered by unions because unions tend to lead to more compressed wage distributions among those covered. However, because of the high percentage of less-schooled workers in the informal and other non-unionized sectors in the region, more effective unions are more likely to help relatively more-schooled workers in the overall economy-wide distribution of workers. Reforms that reduce the costs of changing the number of workers employed by a firm (e.g., reducing hiring or separation costs) are likely to favor relatively workers at the schooling levels for whom shifts in demands would be relatively great in the absence of costs of changing a firm's workforce. If more-schooled workers are harder to replace or require more firm-specific investments, it would seem that less-schooled workers would be more likely to benefit from lower costs of changing the size of the workforce. Such considerations once again, however, are relevant only to workers covered by whatever regulations and procedures make it costly to change the size of the workforce prior to the reforms. And once again, these are likely to be primarily workers in the formal sector, with the result that, even if workers with less schooling among those covered benefit, these may not be the lower-schooled workers in the overall economy.

Labor Supplies

There also may be labor supply shifts induced directly or indirectly by reforms that in turn could have differential effects on wages of less- versus more- schooled workers. For example, the opportunity cost of schooling tends to increase (fall) if a boom (bust) results from reform. This effect in itself means that a boom is likely to draw young people with less schooling into the labor market, probably increasing the relative return to those who already have more schooling. Similarly, labor reforms that make the market more flexible might increase the supply of women and young workers to the labor force, raising the wage differential. On the other hand, over time, if the reforms have the impact of increasing the rate of return to schooling, this might attract more-schooled adults, particularly women, into the labor force, reducing the wage differential by increasing the supply of more-schooled workers. In any event, because labor reform in the region is recent compared to other reforms, we expect the effects of the other reforms to dominate even on the labor supply side.

Short vs. Long-Term Effects of Reforms

The effects of economy-wide reforms on demands for and supplies of different types of labor may intensify or fade over time. It also may take some time for reforms to affect economic behavior, implying a lag between the time reforms are initiated or intensified and any evidence of effects on labor demands and supplies and thus on wage differentials. The time patterns of these responses depend upon factors such as the extent of substitution or complementarity in production and in consumption and how they change over time, the rate of depreciation or obsolescence of various physical and human capital stocks, the rate at which new markets develop, and the credibility of the reforms. The time patterns in relative wages depend on the interactions between the induced changes in labor demands and supplies. It is possible that, because of such interactions, the initial impact of reforms induces adjustments (indeed in many cases that is the point) that eventually reduce the initial impacts on relative wages. Some conjecture, for example, that if the initial distribution of such assets as education is unequal, then reforms that make markets more competitive will be disequalizing initially as returns rise to existing assets.¹¹ However it is likely that with time, the supply of more-schooled workers will rise and that of less-schooled workers will fall, eventually reducing the wage gap due to schooling.

2. Data and Patterns in Wage Differentials and Reform over Time

To explore empirically the relationships between reforms and the relative returns to schooling we need: (a) data for characterizing wages by schooling levels over time, and (b) statistics that summarize the depth and pace of reform regimes in each country over time. These data requirements are considerable, and there is only limited research in this area. This section describes our data set and provides some background about the evolution of the critical data. We start by characterizing our data on wages, which is, to our knowledge, the most comprehensive and up-to-date comparable information of this kind for the Latin American region. We then turn to reform indices and other economy-wide changes of interest.

¹¹ See, for example, Birdsall and Londoño (1997) and Birdsall, (1999).

2.1 Data on Wages and the Returns to Schooling and Wage Differentials

Definition of the Sample

We construct a panel of data on wages by education level, computed directly from the 79 household surveys that are available to us. The data include information for various years between 1980 and 1998 for 18 countries, covering about 95% of the total population of the region (surveys are listed by country and year in Appendix Table A1.) We construct this data set because previous alternatives are much more limited in coverage or refer to wider income concepts that are plagued by comparability problems (e.g., see note 7).

We restrict our data set to employed urban males aged 30 to 55, which controls for three individual characteristics: age, gender and geographic location. This group represents around one fifth of the total population employed, 30% of the population employed in urban areas, and 31.7% of all males employed (first three columns in Appendix Table A2).¹² The gender and age restriction minimizes gender- and age-related sample selection problems so that the changes in wage differentials observed are due primarily to changes in labor demands induced by the reforms, not due to changes in labor force participation decisions that affect labor supply. The labor force participation rate of this group (across all years for which data are available) is around 95% on average, while unemployment rates are only around 3.8% (fourth and fifth columns in Appendix Table A2). High participation and low unemployment in this group guarantee that by restricting the analysis to wage differentials and wage levels, we are *not* missing other potentially important effects of the reforms, such as changes in employment levels. The restriction to urban areas is because data quality on labor incomes is higher for urban than in rural areas, and more importantly, because rural activities (such as agricultural self-employment) involve the use of own labor and capital simultaneously, which make it difficult to obtain a pure measure of income from labor net of payments to physical capital. However, by considering urban areas as a whole, we are able to examine the effect of reforms over most sectors of production in the economy because GDP from agriculture—the prime activity in rural areas—accounts for only about 15 percent of total GDP in Latin America.¹³

The income concept that we use is real hourly wages from all jobs. Household surveys in

¹² The sample includes relatively larger shares of these groups in Argentina, Chile, Uruguay and Venezuela and relatively smaller shares in Costa Rica, Ecuador, Guatemala, Honduras, Paraguay and Nicaragua.

¹³ See IDB (1999).

Latin America report after-tax incomes, so we are only able to perform the analysis for net incomes. All labor-income earners that belong to the sample are included (except those that report missing or zero incomes), regardless of whether individuals are self-employed, or employed in the formal or informal sector. Entrepreneurs reporting labor income are also included. As shown by Székely and Hilgert (1999b), this is the only definition of income that is comparable across the household surveys that we use, so restricting income to this definition minimizes the potential bias introduced by the use of household surveys that are not comparable in their definition of overall income. Our sample accounts for one third, almost 42%, and almost 50% of all wages in the economy, urban wages, and male wages, respectively (sixth through eighth columns in Appendix Table A.2).

Characterization of Wage Differentials

We characterize information on wage differentials in two ways. The first is the standard Mincer-type semi-log wage regression, where the dependent variable is the log of hourly wages, and the independent variables are dummies for completed years of schooling, potential work experience (age minus six minus years of schooling) and potential work experience squared.¹⁴ The estimated coefficients for the dummy variables are normally interpreted as the returns to schooling.¹⁵

Figure 1 summarizes the country-year information for the marginal return to each level of schooling for the years between 1990 and 1998. Because our panel of country-year observations is unbalanced, rather than presenting yearly averages across all countries, which are quite “noisy,” we interpolate the coefficients for the missing years and present smoothed profiles normalized to the value of the coefficient for 1990 for ease of comparison.¹⁶ According to Figure 1, the return to an extra year of schooling in Latin America has increased by about 7 per cent during the 1990s. The

¹⁴ Not all the countries in our sample organize their schooling system in the same way. Adjustments are made where necessary so that the dummy variables are defined in a comparable way across countries. For our purposes, primary education is defined as the first cycle comprising 5 to 6 years, depending on the country. Secondary refers to the second cycle of 5 to 6 years, while in higher education we include any post-secondary schooling.

¹⁵ As explained by Willis (1986), this interpretation is only correct under certain conditions. One of the problems with the standard interpretation is that schooling and ability (as well as other factors often not observed or not measured, such as motivation, parents’ connections, and so on) are highly correlated, and it is difficult to disentangle the effect of each of these elements (see Cawley *et al.*, 1996, Behrman and Rosenzweig, 1999 and Blundell *et al.*, 2000).

¹⁶ Specifically, to smooth out the profiles we first estimate the log-wage regression for each household survey and then put together a panel for each of the three coefficients that represent the returns to each level of schooling. We then take each panel of estimates as the dependent variable in turn and run a country fixed effects regression in which the independent variables are dummies for each year. The figure only plots the patterns after 1989 because household surveys for previous

disaggregation by level reveals that the increase is totally driven by the large rise in the marginal return to higher (post-secondary) schooling. The returns to primary and secondary schooling declined during the decade.¹⁷

Our second approach to characterizing wage differentials is to compare the difference in (log) hourly wages across the three schooling categories. To control for different experience levels we divide the sample of 30-55 year old urban males from each household survey into five five-year age groups, and compare only across the same age groups. Appendix Table A3 presents some summary statistics by country for the difference between the log wage of individuals with higher education relative to those with secondary and primary complete, respectively, averaged over age groups and years. Figure 2 presents patterns for these ratios for the Latin American region, smoothed in the same way as in Figure 1, and normalized to their 1990s values. The figure reveals that the wage gap between individuals with higher schooling and those with primary or secondary complete has widened considerably during the 1990s in Latin America, though with some closure for the higher-to-primary gap after 1994. The ratio between those with secondary and primary schooling increased in the early 1990s to a peak 26% above the 1990 ratio in 1994, but declined after 1994 so the marginal return to secondary schooling relative to that to primary schooling in 1998 was only about 13% higher than in 1990.

2.2 Characterization of Reforms

To characterize the pace and depth of different types of reforms, we use reform indices developed by Lora (1997) and modified and extended by Morley *et al.* (1999). These indices summarize information on trade reform, financial liberalization, tax reform, liberalization of external capital transactions, and privatization for the period 1970-1995. A labor reform index, also developed by Lora (1997), is available for the shorter period from 1985 to 1995.

Unlike proxies commonly used in the literature, these reform indices have the advantage that they are based on direct indicators of governmental policies, so that they reflect policy “effort.” Two examples of common proxies used in the literature are exports plus imports over GDP, used as an indicator of trade liberalization, and M2 over GDP, used as an indicator of financial market reform.

years are more scattered.

¹⁷ Attanasio and Székely (1999) present a detailed account of the evolution of returns to education by country.

The problem with these variables is that they reflect not only or necessarily policies, but reactions to policies by individuals and entities in both the private sector and the public sectors. They are thus contaminated by responses to the reforms and do not just represent the reforms per se.

The Lora trade reform index is the average of the average level of tariffs and the average dispersion of tariffs. The index of domestic financial reform is the average of an index that controls for borrowing rates at banks, an index of lending rates at banks, and an index of the reserves to deposit ratio. The index for international financial liberalization averages four components: sectoral controls of foreign investment, limits on profits and interest repatriation, controls on external credits by national borrowers and capital outflows. The tax reform index averages four components: the maximum marginal tax rate on corporate incomes, the maximum marginal tax rate on personal incomes, the value added tax rate, and the efficiency of the value-added tax; the higher the tax reform index, the lower the average of the marginal tax rates. The privatization index is calculated as one minus the ratio of value-added in state owned enterprises to non-agricultural GDP. Finally, the labor market reform index considers firing costs after 1 and 10 years of work, mandatory costs for overtime work, restrictions on temporary contracts, and the value of contributions to social security. All the indices are normalized between 0 and 1, where in each case 0 refers to the minimum value of the index across all Latin American countries in the relevant time period (including those that do not appear in our data on wage differentials), and 1 is the maximum registered in the whole sample. Thus, the indices are comparable across countries in the region, which is critical for making comparisons among countries, including in our econometric estimates.

Figures 3a and 3b present the evolution of each reform index, plus the average for the first five indices (the index for labor market reforms is not included in this average because it is not available before 1985) over the 1970-1995 period. These figures have three interesting features. First, the value of the average reform index nearly doubled between 1970 and 1995, illustrating the rapid pace of reform. Second, beginning in 1985, the pace of overall reform accelerated. Third, there are substantial differences across indices. The financial market reform index increased by about 200 per cent, the trade and tax reform indices doubled, and the capital account liberalization index increased by 50 per cent. In sharp contrast, the privatization and labor market indices varied much less than the others and for most of the 1990-1995 period were below previous peak levels (though the former was increasing to about the previous peak level in this period). Lora (1997) and Morley

et al. (1999) present detailed descriptions of the evolution of each reform by country and of the synchronicity of reforms.

2.3 Other Changes at the Country-Wide Level Correlated with Reforms

A major problem in identifying the effect of reforms on wage differentials is that reforms may be correlated with other country characteristics that also may affect wages. If such variables are included in estimates of the effects of reforms, their inclusion reduces the limited degrees of freedom and increases possible multicollinearity problems. But if they are excluded and are correlated with the reform indices, their exclusion may cause unobserved variable bias in the estimated coefficients for the effects of reform.

We assess the problem in Table A5 in the Appendix, which presents correlation coefficients for the reform indices and a set of macro variables including: a) the coefficient of variation of the GDP growth rate during the past five years—which is a measure of volatility, b) inflation (bounded to exclude the effect of outliers), c) an index of the real exchange rate, d) trade flows as a share of GDP, e) external capital flows as a share of GDP, and f) high-tech exports as a share of GDP, all of which are likely to have direct effects on relative wages.¹⁸ As expected, with very few exceptions the reform indices are inversely correlated with volatility, inflation and the level of the real exchange rate, and they are positively correlated with the share of high-tech exports in GDP. Surprisingly, capital flows as a share of GDP are negatively correlated with the average reform index. The relation appears to be driven by the negative relation between capital flows and the privatization and tax reform indices. Also surprisingly, the variable with the smallest correlation with the average reform index is the relative importance of trade flows; in particular, the correlation between trade flows and trade reforms is low. These low correlations might be an indication that reforms take some time to affect economic outcomes. Though some of these correlations are low, about two fifths are greater in absolute value than 0.20, so there is a risk of significant omitted variable bias in the absence of controls for these and other possibly important country-wide variables. In Section 3 we explain how we ensure such controls.

¹⁸ All macro variables are taken (or calculated) from the World Bank (1999).

3. Estimation Issues

For our estimates of the impact of reforms on the returns to different schooling levels we use information on real hourly wage rates, schooling level completed, and age for urban males aged 30 to 55, as described above, and we link this information with country-specific and year-specific indicators of the five (and for a smaller number of years, six) types of reforms. To describe the estimation approach we extend the basic semi-log wage relation to include possible effects of reforms that may differ by schooling levels, along with possible effects on wages independent of schooling:

$$(1) \quad \ln W = (\mathbf{a}_p + \mathbf{b}_p R)P + (\mathbf{a}_s + \mathbf{b}_s R)S + (\mathbf{a}_h + \mathbf{b}_h R)H + (\mathbf{a}_R + \mathbf{b}_R R) + \mathbf{d} + \mathbf{g} + \mathbf{e}$$

where P, S, and H are dichotomous variables that refer to the highest completed schooling being primary (P), secondary (S) and higher (H) schooling; R is a vector of reform indicators; I is a vector of individual variables (e.g., age); C is a vector of country variables (e.g., capital per worker, state of technology);¹⁹ and ϵ is a stochastic shock. All of the variables could have subscripts for time and country and the individual variables also could have subscripts for individuals, but these are suppressed to lessen clutter. In this specification the impact of primary schooling on ln wages is $(\alpha_p + \beta_p R)$, the impact of secondary schooling on ln wages is $(\alpha_s + \beta_s R)$, and the impact of higher education on ln wages is $(\alpha_h + \beta_h R)$. Thus, policy reforms are allowed to have effects that differ by the schooling level of workers in addition to effects that are common for all schooling levels (i.e., given by the coefficient vector β_R), all controlling for individual and country characteristics. Our primary interest is in obtaining estimates of the coefficients of the differential effects of reforms by schooling levels—that is, of the relative magnitudes of the coefficient vectors β_p , β_s and β_h . We also have a secondary interest in obtaining estimates of the impact of reforms on wages that are common to all schooling levels, that is the coefficient vector β_R . Estimates of the impact of other individual characteristics (the coefficient vector δ) and of other country characteristics (the coefficient vector γ) are not central for this study.

There are a number of problems in obtaining good estimates of the coefficient vectors of interest (β_p , β_s and β_h) from direct estimates of relation (1). Four of these are:

¹⁹ The variables that enter in linearly in the semi-log relation (1) interact in the determination of wage levels.

(1) There are a large number of parameters. With five reform indices, three individual characteristics, and five country characteristics, for example, there would be 32 coefficient estimates plus the estimate of the variance of the stochastic term. Even with the 79 country-time household surveys in the data set that we use, that does not leave many degrees of freedom for the estimation of country-wide effects, such as of those of reforms. While this does not in itself cause biases, it is likely to lead to limited precision for the coefficients of the reform and other economy-wide variables.

(2) The (possibly large number of) economy-wide variables are likely to be fairly highly correlated, leading to further imprecision and possible problems in sorting out the effects of particular variables.

(3) Not all of the possibly relevant country-level variables are observed in our (or any other) data. As noted in Section 2.3 above, if the unobserved variables are correlated with the interaction between the reform indices and schooling, the result is unobserved variable bias in the estimated effects of reform on the returns to different schooling levels. For example, if the extent of reform is correlated with the nature of the work ethic and the latter is not controlled in the estimates because it is not observed in the data, then the estimated impact of the reform will be biased because it will include not only the effect of the reform but also the correlated effect of the work ethic. One possible partial resolution for this problem is to control for country fixed effects with country dummy variables in the estimation of relation (1). But this strategy has at least two limitations: (i) it adds a number of parameters (16 in our case) in a context in which the degrees of freedom for estimating the countrywide effects are already limited; and (ii) it controls only for unobserved fixed country characteristics, not for unobserved time-varying country characteristics (such as a change from ineffective to effective leadership or vice versa).

(4) The country-wide factors that affect $\ln W$ independently of schooling in relation (1) arguably include not only current variables but also the whole history of such variables since the time that the individual was making marginal schooling/labor force entry decisions because they affect the nature of human resource investments (through experience and

training in addition to schooling) and the nature of options of the individual in the labor market.²⁰ This raises the question for observed countrywide characteristics of how to include lags over differential time periods for different birth cohorts. And even if that issue is ignored or dealt with (e.g., by arguing that the conditions at the time of entry are particularly important and ignoring the differential histories for the differing time periods since the time of the initial entry decision), the other three problems with estimating relation (1) discussed above are exacerbated with the addition of more coefficients to be estimated, more variables that are likely to be fairly highly correlated, and more variables that are unobserved.

We have therefore devised a new estimation strategy that permits us to reduce or eliminate all four of these problems and to obtain estimates of the relative impact of reform on schooling returns in relation (1). We sum relation (1) by averaging it over quinquina of birth cohorts and by school levels. We aggregate by birth cohorts in order to control for the differential amounts of time between the marginal schooling/labor force entry decisions and the time of the survey for different birth cohorts. Then we difference relation (1) between pairs of schooling levels for each age group to obtain:

$$(2a) \quad \ln WS - \ln WP = (\mathbf{a}_s - \mathbf{a}_p) + (\mathbf{b}_s - \mathbf{b}_p)R + (\mathbf{e}_s - \mathbf{e}_p)$$

$$(2b) \quad \ln WH - \ln WS = (\mathbf{a}_h - \mathbf{a}_s) + (\mathbf{b}_h - \mathbf{b}_s)R + (\mathbf{e}_h - \mathbf{e}_s)$$

$$(2c) \quad \ln WH - \ln WP = (\mathbf{a}_h - \mathbf{a}_p) + (\mathbf{b}_h - \mathbf{b}_p)R + (\mathbf{e}_h - \mathbf{e}_p)$$

where $\ln W_i$ (for $i = P, S, H$) is the average for a birth cohort over a quinquinum of $\ln W$ for the schooling level i and ε_j (for $j = p, s, h$) is the stochastic disturbance term for a birth cohort of a quinquinum for schooling level i . Only two of these relations are independent, as can be seen by subtracting (2b) from (2c) to obtain (2a).

Estimation of relation (2) yields direct estimates of the parameters of principal interest, whether the impact of reforms differs by the schooling level (i.e., $(\beta_p - \beta_s)$, $(\beta_h - \beta_s)$, $(\beta_h - \beta_p)$), and

²⁰ We present evidence on the impact of macro conditions on marginal schooling decisions and thus the extent of intergenerational schooling mobility in Behrman, Birdsall and Székely (1999). Earlier studies document the impact of factors such as relative cohort size and school quality (e.g., Behrman and Birdsall 1983, 1985, 1988; and Behrman, Birdsall and Kaplan, 1996).

direct statistical tests of the statistical significance of these differences. These estimates have a number of advantages over efforts to estimate relation (1) directly as can be seen by reconsidering each of the four problems discussed above with direct estimates of relation (1): (i) For estimating each relation in (2) there are only six parameters (one for each reform index plus one for the difference independent of the reform indices) rather than at least five times as many for estimates of relation (1). (ii) There likewise are many fewer variables for estimating relations (2) than relation (1) so the problems of collinearity are reduced. (iii) This specification controls for all unobserved country characteristics whether fixed over time or time-varying so there are not problems with omitted variable bias. (iv) This approach controls for the whole history of countrywide effects since the time of marginal schooling/labor force entry decisions for each birth cohort because relation (2) is estimated within a (five-year) birth cohort. Moreover, with this formulation the well-known common problem of fixed effects exacerbating right-side variable measurement error biases towards zero is not present because in relations (2) the coefficients of the reform variables of interest do not enter in difference form. Thus, estimation of relations (2) offers a number of advantages over estimation of relation (1) with regard to the question of primary interest for this paper, i.e, are there differential effects by schooling levels of the impact of different reforms on workers' wages?

Empirical Specification and Estimation Strategy

For estimating relation (2) we need to specify the relevant timing of reforms. As already mentioned, this is a crucial issue because reforms lead to economic restructuring through resource reallocations that can have differential effects over time. For instance, the main short-term effect of a policy such as trade liberalization that introduces competition into the system may be a period of job destruction due to the disappearance or shrinkage of firms. However, in the medium term, when new firms appear and old ones are able to adjust to the new circumstances, there might be a period of job creation. The effect on wage differentials depends on whether less-schooled or more-schooled workers are more (less) prone to lose their jobs initially or more (less) able to take advantage of the opportunities that are generated later. Because analytical frameworks do not provide specific guidance regarding what the timing of the effects of reform is, for practical purposes we experiment

with a range of alternatives by lagging the independent variables from 1 to 8 years.²¹ By doing this we are able to explore some aspects of the dynamic effects of reforms.

To explore the robustness of our estimates, in what follows we estimate relation (2) in three different ways. First, we estimate our base regressions using random effects with clusters by country and year. These results use the information on the time-variation within countries, and even though the specification controls for country effects, the coefficient estimates also are identified from the between country variation, which uses the information on reforms more efficiently. Second, we estimate all regressions using fixed effects and perform Hausman tests to compare the results with the random effects estimates. In cases in which the differences in coefficient estimates are not systematic, we only report the random effects results, but also give the Hausman test statistic. The fixed effects estimates address the possible concern that the main interest is in the relation between reforms and wage differentials over time within each country. Third, we estimate the specification in differences, which can be interpreted as the “static” effects of reform, and which can be used as a benchmark.²² For all three types of estimates we report results with robust standard errors.²³

4. Empirical Results

Because for the individual reforms there are counteracting possibilities, as discussed in Section 1, the signs of the effects on wage differentials cannot be predicted unambiguously from theory. Because the effects of the individual reforms are not clear *a priori*, it is obvious that their combined effects are not clear either. The direction and magnitude of the effects of reforms and how those effects vary with time are fundamentally empirical questions, which we explore in this section.

4.1 *The Effect of Reforms on Wage Differentials in Latin America*

Table 1 shows the results of estimates of (2b) (higher minus secondary) and (2c) (higher minus primary) using the average reform index as the dependent variable. Because the individual reform

²¹ Lags of more than eight years result in loss of information on wage differentials, so we do not experiment with longer lags. Another possibility would be to include all lags in the same regression, but this would imply excessive loss of degrees of freedom given the size of our sample.

²² In these estimates we use a three-year lag for the reform indices to maximize the number of observations.

²³ All the regressions we present below were also run by including a time trend as independent variable. This assures that the coefficients are not significant only because wage differentials and reforms are trended in a similar way. Including a year trend does not modify any of our results (coefficients and standard errors change only marginally), so we do not include them here for brevity and restrict the discussion to the more simple specification.

indices are normalized across all country-year observations, the average of the five indices covering the 1970-1995 period represents the overall reform effort of each country relative to others. We present the results in terms of the differentials between higher and the other two levels of schooling because such results most transparently are related to other evidence that much of the “excessive” inequality of income in Latin America is due to the heavy concentration of income in the top decile—apparently due primarily to differences in labor, and not non-labor, income.²⁴ Our results should help clarify the extent to which concentration of labor income has been exacerbated by the economic reforms of the last two decades. Note that because the reform indices span the period 1970-1995, when 1 or 2-year lags are used the 1997 and 1998 surveys are dropped from the sample. We performed the same set of regressions in the table holding the (smaller) sample of household surveys constant for the 3 to 8 year lags, but none of our conclusions changes. Thus, we present the results using the largest numbers of observations possible in each case to increase the precision of the coefficient estimates. The same consideration applies for all the regressions presented below.

The results in Table 1 are striking. First, the coefficient estimates for the average reform index are consistently positive and statistically significant for both dependent variables. This conclusion applies for the regression estimated in differences (first column), for each of the random effects regressions that lag the reform variables from 1 to 8 years (second through last columns), and for the fixed effects estimates that lag the reform variables from 1 to 8 years. Because the results pass the Hausman test comfortably (reported in the table) we only present the random effects estimates for brevity, but it should be borne in mind that the fixed effects results lead to exactly the same conclusions. Second, the magnitude of the effect of overall reform tends to decline as the lags of the reform variable increase. Apparently responses are induced, possibly both on the demand or production side and in the supply of different skills to the labor market, that tend to offset the initial change in wages. Third, the explanatory power of the regression is not very high. Thus, although reforms have a statistically significant effect on wage differentials, they are only a limited part of the reason why such differentials have changed.

²⁴ See IDB (1999) and Székely and Hilgert (1999). As noted in Section 3, the estimate of relation (2a) for the difference between ln wages at the secondary level minus that at the primary level does not contain any information beyond that in relations (2b) and (2c).

Table 2 shows parallel estimates, with the five different reform indices entered separately.²⁵ Different reforms have differential estimated effects, but they are always jointly significant. The overall effect of reform on increasing wage differentials appears to result from the effects of capital market opening, financial sector liberalization, and tax reform on wage gaps—which more than offset the opposite effects of privatization. The initial disequalizing effect of reforms is stronger for the higher-primary wage differential, but it also fades away faster. These conclusions are robust to estimation in differences (first column), estimation with random effects with different lags (second through last columns), and estimation with fixed effects. The random effects estimates are consistent with much higher proportions of the variances—over half—in ln wage differentials than if the average reform index is used (Table 1).

According to our results, trade liberalization *per se* has not widened wage gaps. On the contrary, trade reform reduces wage gaps with some lag (although not significantly), a notable finding given the concern that it is the opening of economies that has exacerbated those gaps. This may be because of the strong countervailing forces that this policy induces. Capital account opening raises wage gaps but its effect is considerably reduced with greater lags. The coefficient estimate for a seven-year lag is only a little over half of those for one or two years lags. The initial effect of liberalization may be to increase the demand for more skilled workers in existing sectors. The longer run effect may be to alter the sectoral mix, possibly in response to the high cost of scarce skilled labor. This change with time in the effect of capital market opening is probably contributing to the decline in the magnitude of the estimated impact of the overall index. Financial sector liberalization also has a consistently positive effect in increasing wage gaps, which declines in magnitude over time for the higher-primary differential. A lower cost of borrowing or improved access to financing apparently favors skilled labor, possibly because skilled labor is complementary to capital. Tax reform raises wage gaps, and its effect becomes stronger over time (though with a peak with a four-year lag for the higher-secondary differential). The reasons may be: (i) reducing the maximum marginal tax rates for personal incomes increases the net wage of more-schooled workers; (ii) reducing marginal tax rates on profits may stimulate capital investment, which is complementary to skills; and (iii) value added taxes may be added to goods that use unskilled labor relatively less intensively, which reduces

²⁵ The correlation coefficients across indexes is not particularly high in most cases, so potential multicollinearity is less of a concern (see Appendix Table A4).

the demand for less-schooled workers. Finally, privatization has a significant and increasingly negative effect on wage differentials, which is consistent with firms restructuring by reducing the demand for possibly overpaid more-schooled workers.

The magnitude of the estimated effects of reforms on the wage differentials is not so small as to be irrelevant. Taking the regression estimated in differences as reference, an increase in the average reform index by 0.4, which is the change observed between 1970 and 1995, would increase the log wage differential between higher and secondary school workers by around 22 percentage points (30 percent of its mean). The same increase would raise the log wage differential between higher and primary school workers by 10 percentage points (around 15 per cent of its mean). The individual effects of some reforms are also fairly substantial. The financial market and tax reform indices alone, for example, increase by 0.6 and 0.4 points, respectively, during 1970-1995, which would expand the log wage differential between those with higher and those with primary schooling by around 16 and 11 percentage points. At the same time, privatization has been partially offsetting that disequalizing effect, reducing the same differential by about 14 percentage points.

Labor reform (see Figure 3a) is the most recent of the set of reforms to be initiated in Latin America. It also is among the more difficult reforms to measure. In the first two lines of Table 3 we present the coefficients for the labor index obtained by adding this index to relation (2), where the other reform indices are also used. We present the results for this index separately because they refer to a smaller sample of 49 household surveys to which a labor reform index (available only for 1985-1995) can be attached. The labor reform index itself has a positive significant effect on the wage differential between higher and secondary and higher and primary school graduates in all eight random effects regressions.

We already have stressed that one important advantage of using the reform indices developed by Lora (1997) and Morley *et al.* (1999) is that their main purpose is to measure reform efforts by focusing on changes in policy variables while abstracting from behavioral responses to those policy changes and from other sources of change. Other measures, such as trade flows as a proxy for trade liberalization, can be modified by changes in terms of trade or other factors that are independent from domestic policies, which is one major reason why we do not focus on them. But because exports plus imports as a share of GDP is a widely used proxy for trade openness, we test the sensitivity of our results to estimating relation (2) with random effects, substituting the conventional trade flow variable

for our index of trade reform.²⁶ We report the coefficient for the trade variable in Table 3. As with the labor reform regressions, for brevity we do not present the coefficients for the other indices or other statistics. For both dependent variables, the conclusion is that trade flows do not significantly expand the wage differential. In fact, with a lag of 4 to 5 years, this variable has a negative and significant effect at the 10 per cent level.

4.2 Effects of Reforms in Different Contexts

Another interesting question is whether reforms have differential effects in different economic, policy and technological environments. Have reforms had a larger disequalizing effect in countries that are more integrated into the world economy through trade, and do reforms have different effects depending on the extent to which technological progress has taken place? Table 4 provides estimates of the effect of the average reform index in which we also control for trade flows (imports plus exports over GDP) and the value of high technology exports as a proportion of GDP, as well for interactions between these two variables and the average reform index.²⁷ All regressions presented in this table are country fixed effect estimates because random effects fail to pass the Hausman test. We interpret these results with caution for at least two reasons. First, one of the channels through which technology is transmitted across countries is trade.²⁸ Second, technology exports as a share of GDP has its limitations as a proxy that we use for technological change, but is the only variable available to us with sufficient coverage of the countries in our sample.²⁹

Subject to such caveats, the results are quite interesting. Incorporating these variables into relation (2) improves the fit of the regression considerably, as compared to the results in Table 1.³⁰ Once we control for reforms, a higher proportion of trade in total economic activity appears to have

²⁶ An additional reason why this substitution is of interest is that trade reforms, as they are characterized by the trade reform index, do not necessarily result in greater trade flows if, for instance, the real exchange rate is overvalued. French-Davis (2000), for example, argues that the potential positive effects of trade liberalization on growth were vitiated in some countries in the region because inflation fears prevented nominal devaluations.

²⁷ These variables were calculated from data in World Bank (1999).

²⁸ We estimated the same regressions with, in addition, an interaction term between trade flows and technological progress, but the coefficient estimates for this interaction was never statistically significant, so these regressions are not presented.

²⁹ Other variables that may be considered better suited for capturing the effects of technology, such as the number of computers per inhabitant, are only available for few countries and years, so using these variables implies an excessive loss of information. For instance, the number of computers per inhabitant is available from the World Bank (1999), but using this data reduces the number of observations for the econometric estimates by about one half.

³⁰ The Wald tests in Table 4 show that all the variables are jointly significant.

the effect of *reducing* earnings differentials. At the mean of the trade flow variable, the net effect of an increase in trade flows by one standard deviation is negative (-0.88). Moreover, the coefficient estimates of the reform-trade interaction term, though positive, are not statistically significant, so it seems that reforms do *not* have larger disequalizing effects in countries that are more integrated into the world economy through trade. In contrast, the positive effect on earnings differentials of high technology exports is clearly increased by reform; in this case the coefficient estimates of the interaction terms are always significant, and tend to increase the greater the lag. At the mean for the variable measuring technology exports as a percentage of GDP, the net effect of an increase of one standard deviation is positive but smaller in absolute terms (0.24) than the trade interaction.

These results suggest that in Latin American countries that have implemented structural reforms, including trade liberalization, it is not increases in trade but changes in technology that are associated with growing wage gaps. Indeed it is likely that increases in trade are partially offsetting other factors and reducing wage differentials. This net effect (in Table 4) is also consistent with the statistically insignificant effect of the trade reform indices in Table 2, and of the trade flow variable in Table 3. Of course the picture is complicated by the likelihood that changes in the export of high technology products reflect the increased overall openness of economies, including in the capital account, leading to greater foreign direct investment and greater domestic investment in new technologies.³¹

4.3 Effect of Reforms on Wage Levels

Tables 5 and 6 show estimates of the effect of the average reform index (Table 5) and the separate indices (Table 6) on the average (log) wage *level*, with the reform indices lagged as in the tables above. For these specifications we only present the results for the country fixed effects estimates to control for country fixed characteristics because random effects estimates fail to pass the Hausman test. These estimates provide insight into the question whether the average *level* of wages increases or declines with reform (independent of whether wage *differentials* for different schooling groups

³¹ The trend to “deep integration,” in which increased trade between countries leads to increased emphasis on harmonization of inside-the-border regulatory standards, reflects the likelihood that increased trade flows reflect and reinforce increased capital flows and for developing countries increased foreign direct investment (Birdsall and Lawrence, 1998). In 1998 net foreign direct investment flows comprised more than 90 percent of all net capital inflows to Latin America (Hausmann and Fernández Arias, 2000).

increase or decline). However, because we are unable to control for time-varying characteristics that may be correlated with reforms, interpreting the results as reflecting the causal effects of reforms requires stronger assumptions than are required above.³²

The results in the first part of Table 5 suggest that average reforms have a positive significant effect on average wage levels, with the effect being smaller and less significant when the index is lagged for more than four years. Among the individual reforms, capital account reforms and tax reforms have a positive significant effect after six and seven years, while trade has a positive effect with a five and six year lag (Table 6). Financial sector reform is negatively associated with wage levels after four years, although the effect is significant only for a six-year lag. Privatization does not have a significant effect on wage levels.³³

A positive effect of reforms on wage *levels* combined with the result above of a strong positive effect on wage *differentials* raises the question whether less-schooled workers are better or worse off due to the reforms. To address this question we present three additional sets of country fixed effects regressions (subject to the same caveats), using in turn the first set of the average (log) wage of individuals with higher, secondary and then primary schooling. The effects of reforms on wages of individuals at the top of the schooling distribution are positive and much stronger than those obtained for overall average wages. However, they fade away faster than the effects on the overall average wage. The effects for those with secondary schooling are positive and stronger than for the average wage, and smaller than for individuals with higher schooling. Individuals with secondary schooling, thus, benefited *absolutely* from the reforms even if they fell *relatively* behind those with higher schooling. The results for individuals with primary schooling are striking. For all regressions that use lags of one to seven years, the average reform index is negative and highly significant. Reforms contribute to the wage gap both because they raise the wages of relatively more-schooled individuals (enough to raise the average), and because they reduce the wages of those with the lowest

³² We transform all wages to constant PPP adjusted 1987 international dollars using the deflators in the World Bank (2000).

³³ To address the question of whether reforms have affected the share of wages as compared to profits, we used IMF data on the wage share reported in the National Accounts, by country and year, to estimate the effect of the reform indexes on the wage share (the ratio of wages to GDP) under a country fixed effects specification. Because we are not confident in the quality and comparability of the wage share data and we are not able to control for time-varying country characteristics, we do not wish to overstate the importance of these results, and so discuss them only in this footnote. The estimates suggest that the average reform index is associated with a reduction in the wage share with the index lagged for one through five years; after five years the negative effect is no longer statistically significant. Among the separate indexes it appears to be capital and financial sector reforms that are reducing the wage share.

schooling levels.

5. Conclusions

This paper develops and applies a new approach to the estimation of the impact of economy-wide reforms on wage differentials using a new data set on wage differentials by schooling level for 18 Latin American countries for the period 1980-1998. The wage data are merged with reform indices that characterize the pace of different types of economic reforms in the region. The data set represents a significant advance over previous data used for similar purposes because it includes information for many countries and for all urban productive sectors, allowing an assessment of the overall impact of reforms as opposed to the partial effects in specific industries or regions. The comparability of the data across countries assures that we are observing genuine changes in wage differentials between and within countries.

We use the data first to characterize the evolution of wage differentials. We find that the gap between workers with higher education and those with secondary and primary education has widened considerably, especially in the 1990s. We then explore the relation between the reforms and wage differentials, a topic on which very limited prior empirical evidence exists. We find that, on average, reforms have had a strong positive effect on wage differentials, but that the effect tends to become smaller over time. The positive effect of reforms appears to be due to the strong effects on wage differentials of domestic financial market reform, capital account liberalization and tax reform. Labor market reform also appears to raise wage differentials, though this result is less solid because the period covered is more limited. Privatization has a negative effect, but it is not enough to offset the positive effects of other reforms. Trade openness has no overall effect on wage differentials, perhaps because it triggers many countervailing forces that cancel each other out.

We also explore whether reforms have been more disequalizing in countries that are more integrated into the world economy through trade or in countries in which the technological exports are greater. Because we are not able to characterize in a totally satisfactory way the environment in which reforms are implemented, the interpretation of these results must be qualified. These estimates suggest that technological progress rather than trade has been the important mechanism through which the disequalizing effects have been operating. We further explore whether reforms have had

an effect on wage levels, independent of their effect on inequality, though for this question our data are not as well suited to address this issue because there may be unobserved time-varying variables that are correlated with the reforms that are not controlled. Our results suggest that capital account liberalization, tax reform, and trade openness have had positive effects on average wage levels. The impact on average wages is only due to a positive impact on wages of workers with tertiary and secondary schooling, since reforms have contributed to lower the wages of less-schooled individuals.

Do our results suggest that reforms have been bad for Latin America—a “class act” favoring the relatively highly schooled upper classes because their net effect has been to exacerbate earnings differentials? Though the net effects of reforms has been to increase wage differentials, our answer is “not necessarily” because: (i) Reforms do raise earnings differentials in the short run, but the effect fades away rapidly. (ii) The composition of reforms matters. Even in the short run, privatization *reduces* differentials, as does more trade in the presence of trade liberalization and other reforms. (iii) Reform measures were badly needed to improve efficiency and did in fact contribute to growth in the region—as is implied by our results showing that the reforms raised average wage levels and wages for individuals with secondary and higher schooling levels. (iv) The effects of reforms on the distribution of non-labor income may have been more immediately favorable—if, for example, trade liberalization or financial sector reform reduced rents to large firms and raised profits of small businesses. In addition, the short-run effect of reforms -- an increase in the wage returns for better-schooled workers -- raises the private demand for additional schooling. That should raise private investment in schooling. Over time, the increase in the proportion of workers with more schooling not only can contribute to higher overall growth; it also can offset the effect of high schooling returns on overall wage inequality.³⁴

We conclude not that the reforms should be eliminated because our results show negative short-run disequalizing effects, but that consideration should be given to policies and programs that might mitigate their short-run side effects as part of the larger reform agenda.

34. See footnote 5.

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Figure 1

Marginal Returns to Education in Latin America in the 1990s

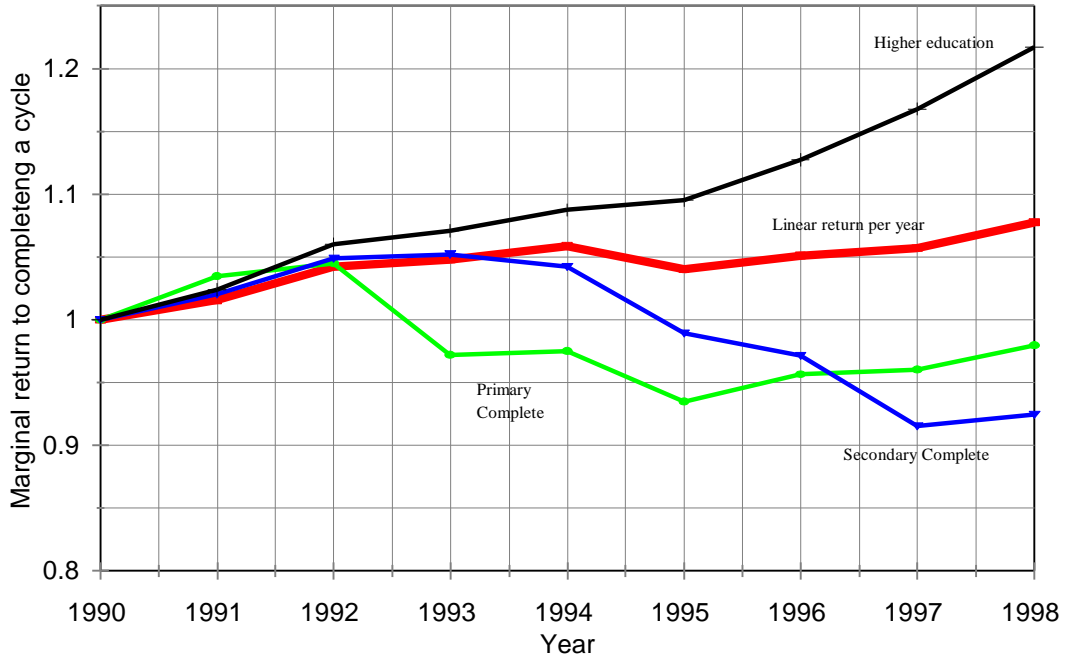


Figure 2

Wage Differentials in Latin America in the 1990s

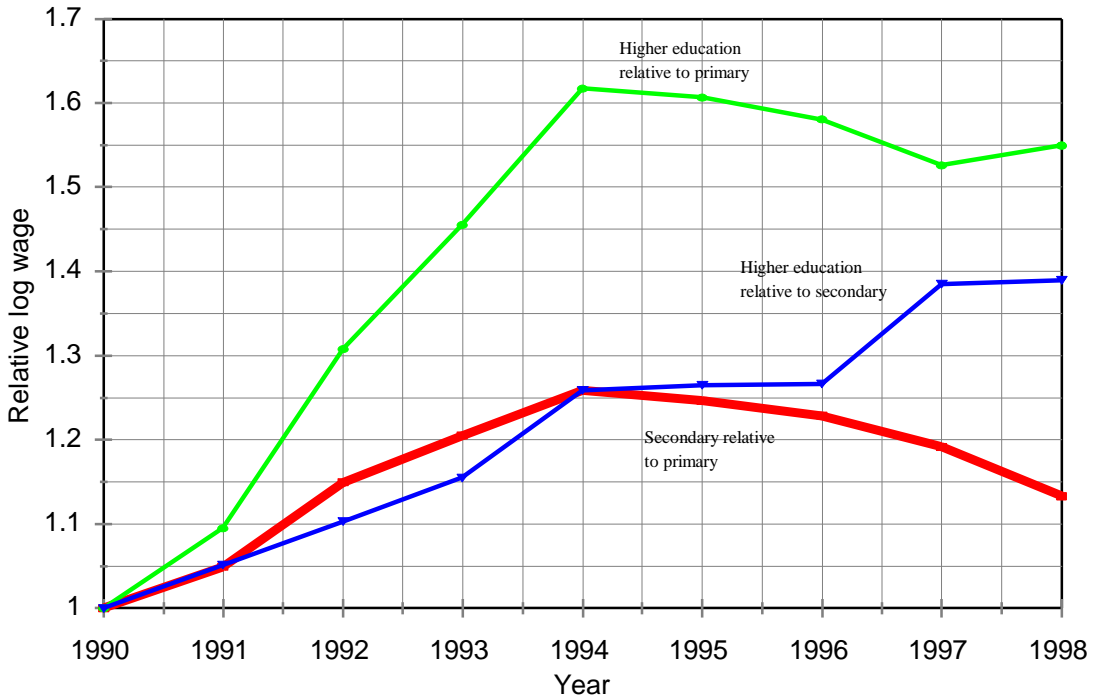


Figure 3a
Average Value of Reform Indices
 Latin America, 1970-1995

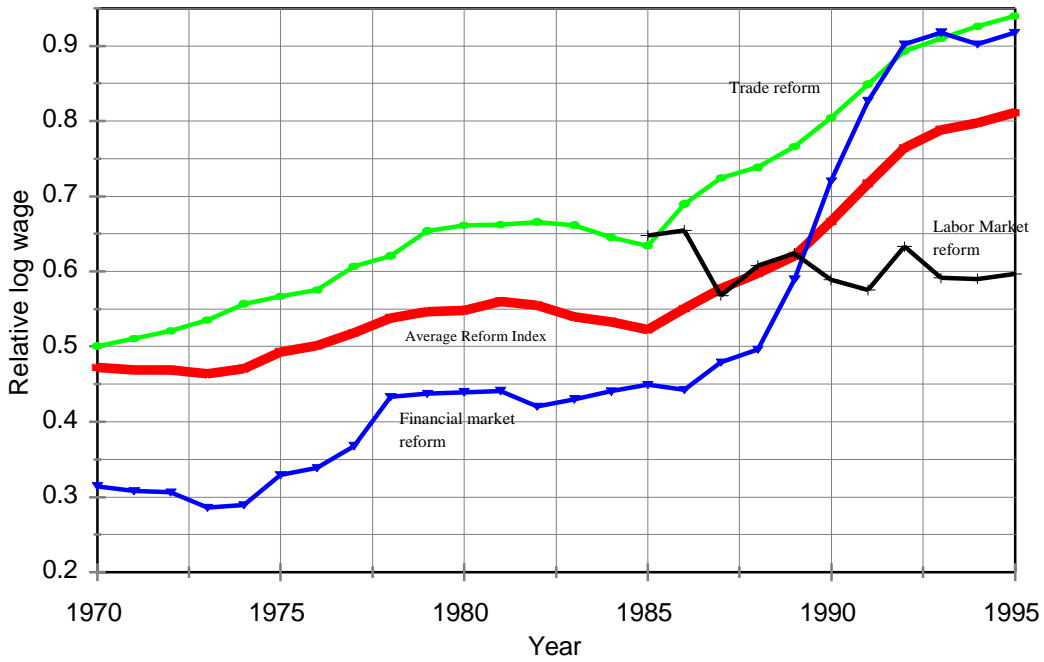
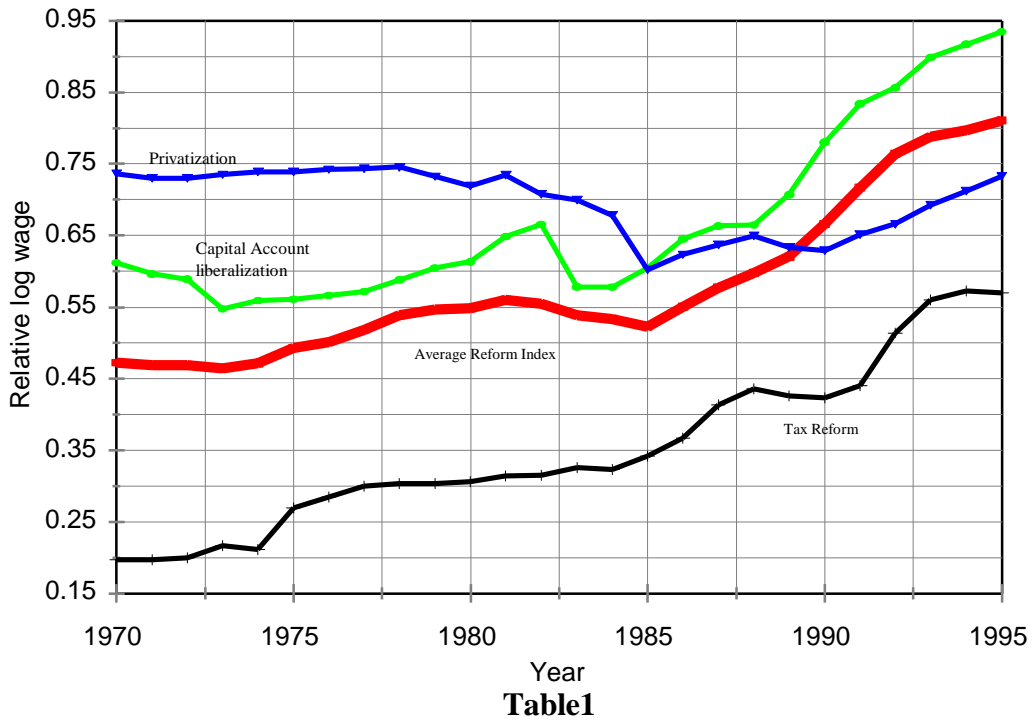


Figure 3b
Average Value of Reform Indices
 Latin America, 1970-1995



Wage Differentials and Average Reform Index

Independent Variable	Estimation in Difference	Lag for Independent Variable							
		1-year	2-years	3-years	4-years	5-years	6-years	7-years	8-years
<i>Dependent variable: high/secondary</i>									
Average index	0.57	0.45	0.43	0.43	0.44	0.40	0.37	0.32	0.29
	5.13	2.59	2.62	2.66	2.87	2.67	2.60	2.36	2.15
Constant	-0.04	0.45	0.47	0.47	0.48	0.51	0.51	0.53	0.54
	-0.95	3.63	4.01	4.12	4.53	5.07	4.90	5.07	4.99
R-sq. Overall	0.075	0.054	0.048	0.036	0.042	0.036	0.035	0.030	0.025
Num. Obs.	304	290	340	395	390	395	390	385	390
No. Household Surveys	79	58	68	79	78	79	78	77	78
Avg Obs. per country	17.9	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0
Wald chi2(1)	26.27	6.7	6.86	7.06	8.22	7.15	6.78	5.56	4.6
Prob > chi2	0.00	0.01	0.01	0.01	0.00	0.01	0.01	0.02	0.03
Hausman Test		0.80	0.59	0.61	0.84	0.77	0.60	0.60	0.63
<i>Dependent variable: high/primary</i>									
Average index	0.35	0.47	0.39	0.38	0.39	0.37	0.29	0.25	0.23
	2.51	2.82	2.52	2.54	2.64	2.83	2.22	2.31	2.15
Constant	0.01	0.83	0.87	0.88	0.88	0.87	0.80	0.79	0.54
	0.39	4.55	4.75	4.98	5.43	5.66	5.13	4.99	4.99
R-sq. Overall	0.021	0.037	0.023	0.019	0.022	0.026	0.039	0.042	0.025
Num. Obs.	304	290	340	395	390	395	390	385	390
No. Household Surveys	17	58	68	79	78	79	78	77	78
Avg Obs. per country	17.9	5	5	5	5	5	5	5	5
Wald chi2(1)	6.32	3.31	2.32	2.37	2.7	3.36	4.93	5.33	4.6
Prob > chi2	0.01	0.07	0.13	0.12	0.10	0.07	0.03	0.02	0.03
Hausman Test		0.85	0.99	0.92	0.65	0.70	0.69	0.65	0.56

Source: Authors' calculations. 'z' Statistics are presented below each coefficient.

Table 2

Wage Differentials and Individual Reform Indices

Independent Variable	Estimation in Difference	Lag for Independent Variable							
		1-year	2-years	3-years	4-years	5-years	6-years	7-years	8-years
<i>Dependent variable: high/secondary</i>									
Trade openness	0.09	0.21	0.08	0.10	-0.02	-0.05	0.07	0.07	0.12
	0.72	0.98	0.44	0.69	-0.13	-0.38	0.59	0.68	1.09
Financial market reform	0.11	0.21	0.28	0.23	0.29	0.28	0.21	0.22	0.17
	2.29	2.54	2.47	2.19	3.31	2.99	2.61	3.08	2.22
Capital account lib.	0.09	0.49	0.48	0.38	0.39	0.33	0.32	0.27	0.21
	1.94	4.04	4.56	4.13	4.66	3.84	4.82	5.27	4.53
Privatization	-0.16	-0.10	-0.22	-0.17	-0.17	-0.15	-0.17	-0.24	-0.16
	-1.99	-0.92	-2.29	-1.91	-1.97	-1.93	-2.21	-3.01	-2.01
Tax reform	0.38	0.24	0.27	0.31	0.37	0.32	0.34	0.30	0.29
	2.06	1.43	1.95	2.31	3.21	2.80	3.03	2.97	2.56
Constant	-0.05	0.77	0.88	0.78	0.81	0.82	0.86	0.96	0.87
	-1.01	4.28	5.89	5.74	6.40	6.02	7.20	8.30	7.55
R-sq. Overall	0.037	0.210	0.218	0.157	0.191	0.160	0.188	0.200	0.164
Num. Obs.	304	290	340	395	390	395	390	385	390
No. Household Surveys	79	58	68	79	78	79	78	77	78
Avg Obs. per country	17.90	5.00	5.00	5.00	5.00	5.00	5.00	5.00	5.00
Wald chi2(1)	30.24	36.65	47.29	42.31	57.40	43.38	56.28	62.44	44.47
Prob > chi2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Hausman Test		0.77	0.62	0.54	0.66	0.69	0.67	0.56	0.66
<i>Dependent variable: high/primary</i>									
Trade openness	0.12	-0.12	-0.30	-0.11	-0.09	-0.15	0.01	0.03	0.11
	0.82	-0.37	-0.95	-0.46	-0.41	-0.68	0.04	0.18	0.59
Financial market reform	0.27	0.35	0.39	0.32	0.27	0.27	0.18	0.21	0.11
	2.37	1.99	2.09	2.00	1.98	2.08	2.09	1.93	1.93
Capital account lib.	0.17	0.63	0.63	0.59	0.56	0.41	0.40	0.33	0.27
	2.17	3.39	3.55	4.15	4.13	2.94	3.57	3.63	3.27
Privatization	-0.43	-0.06	-0.24	-0.20	-0.29	-0.26	-0.26	-0.29	-0.28
	-2.15	-1.94	-1.92	-1.98	-1.83	-1.72	-1.45	-0.90	-0.46
Tax reform	0.27	0.31	0.45	0.44	0.56	0.52	0.61	0.58	0.59
	1.86	2.21	1.96	2.15	3.01	2.86	3.36	3.44	3.30
Constant	0.01	1.38	1.52	1.37	1.24	1.18	1.16	1.22	1.14
	0.38	5.01	6.12	6.51	6.00	5.38	6.02	6.39	6.20
R-sq. Overall	0.094	0.216	0.219	0.200	0.216	0.175	0.218	0.223	0.188
Num. Obs.	304	290	340	395	390	395	390	385	390
No. Household Surveys	79	58	68	79	78	79	78	77	78
Avg Obs. per country	17.90	5.00	5.00	5.00	5.00	5.00	5.00	5.00	5.00
Wald chi2(1)	11.59	25.17	29.15	33.92	37.10	27.92	37.98	38.23	30.46
Prob > chi2	0.04	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Hausman Test		0.61	0.66	0.61	0.58	0.53	0.59	0.57	0.52

Source: Authors' calculations. 'z' Statistics are presented below each coefficient.

Table 3
Wage Differentials, Labor Market Reform and Trade Flows
(All regressions include Reform Indices as controls)

Dependent Variable	Lag for Independent Variable							
	1-year	2-years	3-years	4-years	5-years	6-years	7-years	8-years
<i>Coefficients for Labor Market Reform Index</i>								
Higher-Secondary	0.47	0.46	0.38	0.33	0.45	0.57	0.61	0.53
	3.22	3.56	2.70	2.31	2.79	3.90	4.46	3.27
Higher-Primary	0.53	0.66	0.60	0.55	0.70	0.79	0.81	0.94
	2.20	2.83	2.78	2.40	2.73	3.39	3.46	3.58
<i>Coefficients for Trade Flows</i>								
Higher-Secondary	0.00	0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	0.46	0.02	-1.17	-1.74	-1.50	-0.97	-0.39	-0.31
Higher-Primary	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	-0.64	-0.90	-0.97	-1.66	-1.94	-1.29	-1.36	-0.86

Source: Authors' calculations. 'z' Statistics are presented below each coefficient.

Table 4
Wage Differentials, Reform Index, Trade Flows and Tech Exports
Including Interaction Terms
(Fixed Effects Regressions)

Independent Variable	Lag for Independent Variable							
	1-year	2-years	3-years	4-years	5-years	6-years	7-years	8-years
<i>Dependent variable: high/secondary</i>								
Average index	0.18	0.09	0.10	0.46	0.35	0.22	0.10	0.07
	2.18	1.94	1.93	2.20	2.67	2.15	1.84	1.14
Trade flows	-0.02	-0.02	-0.01	-0.01	-0.01	-0.01	-0.02	-0.02
	-2.79	-2.85	-2.36	-1.58	-1.31	-2.00	-2.83	-2.58
Interaction (Index*flow)	0.02	0.02	0.01	0.00	0.00	0.01	0.02	0.02
	1.57	1.48	1.23	0.91	0.72	1.42	1.26	1.13
Tech Exports/GDP	34.23	50.44	34.16	45.04	29.21	39.58	82.25	82.30
	1.49	1.77	1.73	1.78	1.18	1.60	1.11	0.97
Interaction (Index*tech)	42.39	59.99	38.91	53.81	33.49	55.00	122.35	124.09
	2.67	3.01	2.71	2.75	2.12	2.61	4.07	3.86
Constant	0.95	0.89	0.79	0.54	0.63	0.76	0.82	0.90
	3.39	3.47	3.29	2.64	2.82	3.13	3.20	3.14
R-sq. Overall	0.129	0.137	0.104	0.106	0.094	0.105	0.126	0.119
Num. Obs.	290	340	395	390	395	390	385	390
No. Household Surveys	58	68	79	78	79	78	77	78
Avg Obs. per country	5.00	5.00	5.00	5.00	5.00	5.00	5.00	5.00
Wald chi2(1)	17.75	23.10	23.53	23.65	20.45	23.20	29.75	27.71
Prob > chi2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
<i>Dependent variable: high/primary</i>								
Average index	0.28	0.26	0.35	0.08	0.02	0.29	0.22	0.32
	2.21	2.17	1.89	2.78	1.94	2.11	1.97	1.48
Trade flows	-0.02	-0.02	-0.02	-0.01	-0.01	-0.02	-0.02	-0.03
	-2.73	-2.49	-3.02	-2.47	-2.26	-1.97	-2.92	-3.36
Interaction (Index*flow)	0.03	0.02	0.02	0.01	0.01	0.01	0.03	0.04
	1.32	0.96	1.40	0.80	1.49	1.17	1.02	1.35
Tech Exports/GDP	58.13	64.84	53.71	64.67	42.06	44.77	104.85	122.03
	1.78	1.45	1.80	1.72	1.17	1.27	0.86	1.26
Interaction (Index*tech)	69.00	71.51	62.30	72.48	44.50	52.44	144.30	175.75
	2.88	2.60	2.80	2.60	2.04	2.14	3.65	3.98
Constant	1.47	1.46	1.55	1.23	1.31	1.19	1.25	1.52
	3.70	3.70	4.25	4.04	4.02	3.44	3.50	3.92
R-sq. Overall	0.176	0.175	0.141	0.161	0.165	0.196	0.256	0.256
Num. Obs.	290	340	395	390	395	390	385	390
No. Household Surveys	58	68	79	78	79	78	77	78
Avg Obs. per country	5.00	5.00	5.00	5.00	5.00	5.00	5.00	5.00
Wald chi2(1)	18.81	21.38	20.98	24.39	25.88	32.36	47.67	49.24
Prob > chi2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Source: Authors' calculations. 'z' Statistics are presented below each coefficient.

Table 5
Wage Levels and Average Reform Index

Independent Variable	Lag for Independent Variable							
	1-year	2-years	3-years	4-years	5-years	6-years	7-years	8-years
<i>Dependent variable: log wages all schooling levels</i>								
Average index	0.48	0.45	0.57	0.56	0.44	0.36	0.38	0.32
	1.78	1.62	2.08	1.99	1.78	1.37	1.60	1.56
Constant	1.22	1.23	1.16	1.18	1.28	1.34	1.27	1.19
	4.60	5.07	5.03	5.17	6.48	6.12	5.66	4.83
R-sq. within	0.042	0.036	0.053	0.050	0.037	0.020	0.030	0.042
Num. Obs.	290	340	395	390	395	390	385	390
Countries	17	17	18	18	18	18	18	18
F-Test	1.63	1.74	3.17	2.87	2.19	1.15	1.70	2.45
Prob > chi2	0.21	0.19	0.08	0.10	0.14	0.29	0.20	0.12
<i>Dependent variable: log wages individuals with Higher schooling</i>								
Average index	4.39	3.97	3.00	3.34	2.06	1.84	0.48	0.21
	4.26	4.24	3.58	3.92	2.69	2.11	1.30	1.27
Constant	-4.97	-4.54	-3.87	-4.11	-3.11	-2.99	-1.27	-0.95
	-6.80	-6.79	-6.43	-6.95	-6.03	-5.30	5.66	-1.47
R-sq. within	0.066	0.055	0.034	0.042	0.020	0.013	0.030	0.005
Num. Obs.	290	340	395	390	395	390	385	390
Countries	17	17	18	18	18	18	18	18
F-Test	18.15	18.00	12.78	15.39	7.21	4.47	1.70	1.62
Prob > chi2	0.00	0.00	0.00	0.00	0.01	0.04	0.20	0.20
<i>Dependent variable: log wages individuals with Secondary schooling</i>								
Average index	3.41	3.16	1.99	2.14	0.67	0.49	0.47	0.10
	3.70	3.76	3.02	3.20	1.79	1.25	0.48	0.37
Constant	-4.19	-3.86	-3.04	-3.12	-1.99	-1.89	-1.51	-1.19
	-7.63	-7.66	-7.25	-7.67	-6.66	-5.88	-4.95	-1.82
R-sq. within	0.081	0.070	0.043	0.049	0.022	0.014	0.006	0.005
Num. Obs.	290	340	395	390	395	390	385	390
Countries	17	17	18	18	18	18	18	18
F-Test	22.05	22.69	16.12	17.67	7.81	5.04	2.20	1.87
Prob > chi2	0.00	0.00	0.00	0.00	0.01	0.03	0.14	0.17
<i>Dependent variable: log wages individuals with primary schooling</i>								
Average index	-0.97	-0.89	-0.73	-0.77	-0.50	-0.55	-0.47	-0.16
	-8.93	-8.44	-7.68	-7.98	-5.50	-5.39	-3.96	-1.25
Constant	1.12	1.03	0.89	0.90	0.65	0.68	0.60	0.35
	11.60	11.38	10.76	11.14	8.98	8.63	6.99	3.76
R-sq. within	0.204	0.170	0.127	0.139	0.071	0.071	0.041	0.004
Num. Obs.	290	340	395	390	395	390	385	390
Countries	17	17	18	18	18	18	18	18
F-Test	1.14	1.09	1.06	1.14	1.68	1.62	1.91	1.56
Prob > chi2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.21

Source: Authors' calculations. 'z' Statistics are presented below each coefficient.

Table 6

Wage Levels and Individual Reform Indices

Independent Variable	Lag for Independent Variable							
	1-year	2-years	3-years	4-years	5-years	6-years	7-years	8-years
Trade openness	0.38	0.47	0.55	0.46	0.67	0.66	0.20	0.09
	0.55	0.28	1.51	1.26	1.95	2.16	0.56	0.24
Financial market reform	0.11	0.09	0.03	-0.10	-0.33	-0.56	-0.26	-0.25
	0.39	0.35	0.12	-0.46	-1.41	-2.74	-1.28	-0.96
Capital account lib.	0.37	0.28	0.19	0.22	0.26	0.27	0.29	0.22
	0.48	0.06	0.41	0.42	0.88	1.90	1.76	2.00
Privatization	0.45	0.50	0.54	0.42	0.29	0.25	0.20	0.22
	0.75	0.49	1.04	1.06	0.63	0.13	0.48	0.39
Tax reform	0.56	0.55	0.55	0.39	0.23	0.60	0.96	1.22
	0.66	0.76	0.21	0.13	0.59	1.24	1.87	2.54
Constant	1.03	1.13	0.94	0.72	0.79	0.66	0.85	0.87
	2.23	2.64	2.81	1.74	2.17	1.74	2.25	2.16
R-sq. within	0.071	0.049	0.085	0.096	0.106	0.199	0.116	0.140
Num. Obs.	55	65	76	74	76	74	74	75
Countries	17	17	18	18	18	18	18	18
Wald chi2(1)	0.51	0.45	0.99	1.08	1.26	2.53	1.34	1.70
Prob > chi2	0.77	0.81	0.43	0.38	0.30	0.04	0.26	0.15

Source: Authors' calculations. 'z' Statistics are presented below each coefficient.

Appendix

Table A1

Household Surveys			
Country	# Surveys	Years	Survey
Argentina	2	1980, 96	Encuesta Permanente de Hogares
Bolivia	6	1986 1990, 93, 95 1996, 97	Encuesta Permanente de Hogares Encuesta Integrada de Hogares Encuesta Nacional de Empleo
Brazil	9	1981, 83, 86, 88 1992, 93, 95, 96, 97	Pesquisa Nacional por Amostra de Domicilios Pesquisa Nacional por Amostra de Domicilios
Chile	6	1987, 90, 92, 94, 96, 98	Encuesta de Caracterización Socioeconómica Nacional
Colombia	5	1991, 93, 95, 97, 98	Encuesta Nacional de Hogares - Fuerza de Trabajo
Costa Rica	10	1981, 83, 85 1987, 89, 91, 93, 95, 97, 98	Encuesta Nacional de Hogares - Empleo y Desempleo Encuesta de Hogares de Propósitos Múltiples
Dominican Republic	2	1996 1998	Encuesta Nacional de Fuerza de Trabajo Encuesta Nacional Sobre Gastos e Ingresos de los Hogares
Ecuador	2	1995, 98	Encuesta de Condiciones de Vida
El Salvador	3	1995, 97, 98	Encuesta de Hogares de Propósitos Múltiples
Guatemala	1	1998	Encuesta Nacional de Ingresos y Gastos Familiares
Honduras	5	1989, 92, 96, 97, 98	Encuesta Permanente de Hogares de Propósitos Múltiples
Mexico	5	1984, 89, 92, 94, 96	Encuesta Nacional de Ingreso Gasto de los Hogares
Nicaragua	2	1993, 98	Encuesta Nacional de Hogares Sobre Medicion de Niveles de Vida
Panama	4	1991, 95, 97, 98	Encuesta Continua de Hogares
Paraguay	2	1995 1998	Encuesta Nacional de Empleo Encuesta Integrada de Hogares
Peru	4	1985, 91, 94, 97	Encuesta Nacional de Hogares sobre Medicion de Niveles de Vida
Uruguay	5	1981, 89 1992, 95, 97	Encuesta Nacional de Hogares Encuesta Continua de Hogares
Venezuela	6	1981, 86, 89, 93, 95, 97	Encuesta de Hogares por Muestra

Appendix Table A2

Characteristics of the Sample of Urban Males 30-55 Years of Age

Country	Employed Urban Males 30-55 as share of			Labor force participation Urb Males 30-55	Unemployment Rate of Urb Males 30-55	Wages of Urban Males 30-55 as share of		
	Total Employment	Urban Employment	Male Employment			All wages	Urban Wages	Male Wages
Average LAC	20.3	30.4	31.7	94.2	3.8	33.6	41.9	48.7
Argentina	35.4	35.4	54.7	95.3	6.1	41.9	41.9	62.3
Bolivia	20.5	30.9	35.8	94.2	3.5	33.6	42.4	50.2
Brazil	22.3	29.7	35.1	92.9	3.6	42.4	46.8	58.9
Chile	30.3	35.9	45.7	94.4	4.8	44.2	48.3	61.0
Costa Rica	15.1	32.3	21.3	94.6	2.7	23.2	39.7	32.8
Colombia	19.0	30.6	30.3	96.0	4.9	31.9	41.5	47.9
Dominican Republic	18.7	31.6	28.2	94.8	3.7	30.1	43.5	41.8
Ecuador	15.6	27.7	25.8	96.1	2.6	30.6	39.8	45.5
El Salvador	16.0	26.6	26.2	90.4	0.4	30.1	37.4	47.4
Guatemala	11.2	25.9	17.6	95.6	2.2	26.6	41.2	38.8
Honduras	12.0	26.9	17.7	95.5	3.8	23.1	39.1	31.9
Mexico	20.2	32.6	29.0	94.2	2.1	37.1	45.5	50.9
Paraguay	14.6	27.1	23.5	96.2	2.4	28.0	39.1	41.0
Panama	20.0	32.5	29.8	92.9	5.1	34.8	42.5	52.7
Peru	17.9	28.5	31.1	94.4	2.2	35.2	41.6	51.3
Nicaragua	15.6	27.5	23.3	86.8	10.2	30.9	39.6	46.6
Uruguay	30.7	30.7	52.3	95.5	2.8	40.1	40.1	60.9
Venezuela	29.7	34.7	42.8	95.3	5.2	40.7	44.8	55.1

Source: Authos' calculations from household survey data.

Appendix Table A3

Summary Statistics for Wage Differentials

Variable	No. Obs.	Mean	Std. Dev.	Min	Max
<i>High/secondary Wage Differential</i>					
Whole Sample	395	0.77	0.29	0.30	2.03
Argentina	10	0.58	0.09	0.46	0.68
Bolivia	30	0.73	0.28	0.27	1.27
Brazil	45	0.98	0.08	0.74	1.17
Chile	30	1.09	0.22	0.79	1.73
Costa Rica	50	0.66	0.21	0.34	1.47
Colombia	25	0.99	0.22	0.60	1.45
Dominican Republic	10	0.73	0.33	0.29	1.02
Ecuador	10	0.59	0.16	0.06	1.00
El Salvador	15	0.81	0.58	0.61	1.17
Guatemala	5	0.65	0.30	0.30	1.28
Honduras	25	0.61	0.32	0.18	1.28
Mexico	25	0.77	0.67	0.03	1.48
Paraguay	10	0.87	0.20	0.09	2.03
Panama	20	0.73	0.22	0.30	1.09
Peru	20	0.58	0.44	0.26	1.20
Nicaragua	10	0.87	0.18	0.31	1.47
Uruguay	25	0.69	0.16	0.29	1.06
Venezuela	30	0.62	0.62	0.43	1.19
<i>High/Primary Wage Differential</i>					
Whole Sample	395	1.14	0.09	0.15	2.48
Argentina	10	1.01	0.34	0.90	1.17
Bolivia	30	0.99	0.08	0.18	1.58
Brazil	45	1.59	0.23	1.42	1.79
Chile	30	1.48	0.32	0.65	2.19
Costa Rica	50	0.96	0.22	0.33	1.65
Colombia	25	1.33	0.19	0.36	1.93
Dominican Republic	10	1.00	0.24	0.70	1.41
Ecuador	10	0.93	0.17	0.73	1.32
El Salvador	15	1.08	0.45	0.60	1.34
Guatemala	5	1.34	0.35	1.13	1.58
Honduras	25	0.91	0.61	0.15	1.50
Mexico	25	1.26	0.26	0.53	1.97
Paraguay	10	1.33	0.26	0.63	2.48
Panama	20	1.16	0.33	0.36	1.73
Peru	20	0.89	0.19	0.50	1.44
Nicaragua	10	1.06	0.15	0.61	1.62
Uruguay	25	1.11	1.11	0.76	1.46
Venezuela	30	0.86	0.86	0.54	1.18

Source: Authors' calculations from household surveys.

Table A4

Correlation Coefficients of Reform Indices

Reform Index	Average Index	Capital Account Lib.	Trade Lib.	Financial Market Reform	Privatization	Tax Reform
Average Index	1					
Capital Account Liberalization	0.5297	1				
Trade Liberalization	0.7076	0.4087	1			
Financial Market Reform	0.859	0.3692	0.5878	1		
Privatization	0.2336	-0.2298	-0.1983	0.0408	1	
Tax Reform	0.7612	0.2899	0.4292	0.6116	0.0809	1

Source: Calculated from the original reform indices.

Table A5

Correlations of Reform Indexes and Macro Variables

Variable	Average Reform Index	Capital Account Index	Trade Reform Index	Financial Market Reform	Privatization Index	Tax Reform Index
Coef. Var. GDP growth	-0.159	-0.079	-0.036	-0.115	-0.099	-0.204
Inflation (bounded)	-0.301	-0.158	0.059	-0.225	-0.448	-0.057
Real Exchange Rate index	-0.286	-0.259	-0.144	-0.282	-0.080	-0.254
Trade flows (X+M/GDP)	0.072	-0.158	-0.039	-0.111	0.153	-0.007
Capital Flows as % of GDP	-0.286	0.131	0.054	-0.077	-0.480	-0.120
High-tech exports as % of GDP	0.286	0.216	0.008	0.292	0.257	0.138

Source: Authors' calculations using World Development Indicators, WB (1999), and Morley, et.al. reform indexes.