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Economic Policy and Wage Differentials in Latin America

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Abstract:

This paper applies a new approach to the estimation of the impact of policy, both the levels and the changes, on wage differentials using a new high-quality data set on wage differentials by schooling level for 18 Latin American countries for the period 1977-1998. The results indicate that liberalizing policy changes overall have had a short-run disequalizing effect of expanding wage differentials, although this effect tends to fade away 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 significant effect on wage differentials. Technological progress, rather than trade flows, appears to be a channel through which policy changes are affecting inequality.

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Economic Policy and Wage Differentials in Latin America

by

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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 trade with low-wage developing countries has been blamed, with more imports from low-wage developing countries reducing the demand for and wages of the less-schooled, and with the threat of more imports and of employers investing overseas undermining 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 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 1990s, the discussion has shifted to emphasizing 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 policy changes 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]

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[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), 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. Robbins (1995) and Feliciano (1995) are two early contributions to this empirical literature for Latin America.

[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 liberalization were lost in many countries of the region because exchange rates remained overvalued (often due to their use as anti-inflation anchors). See also

This paper assesses the effects of various economic policies on wage differentials in Latin America during the last two decades. We focus on the set of market-oriented policies, and their increasing intensity of application, that have come to be labeled the Washington Consensus – trade and financial sector liberalization, privatization, the opening of capital markets, the reduction of high-income tax rates in favor of broad-based taxes on consumption, and the deregulation of labor markets. The set of six policies (often called market reforms or structural adjustment reforms) have been widely implemented throughout the region over the last three decades, though at different times and with different degrees of intensity in different countries. We generally refer below to the policy shifts as "policy changes," but we also at times refer to liberalizing policy changes as "reforms", as is common in much of the literature.

Our objective is to investigate whether these policies and changes in these policies, by country and period, had immediate and/or lasting effects on relative wages over the past two decades. If the policies and changes in them have increased wage differentials, then 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 and the suspicion that stabilization and these structural reforms have contributed to that inequality.[6]

To undertake this assessment we use a rich new data set and develop and apply a new estimation strategy for this type of study. Our new high quality data set on Latin America includes comparable information on urban wages and education for 18 Latin American countries over the period 1977-1998,

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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. Most counties in South America actually lost market share in the industrialized countries to other developing countries in the 1990s.

[5] Székely and Hilgert (2001) show that changes in the distribution of labor income have been the main reason why overall income inequality failed to decline in Latin America during the 1990s.

[6] For the effects of policy reforms on growth in Latin America, see IDB (1997) and Lora (1997). Morley, *et.al.* (1999) report that despite policy 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.

which we compute directly from 71 household surveys, merged with annual country-specific indices of the intensity of the six policies. The combination of household-level survey-based wage and schooling data for many countries and years, with country and year-specific information on polices, constitutes a significant advance in itself.[7] The lack of such data in the past meant that previous studies of the effect of policy changes on wage differentials have had to focus on specific industries or small regions within a country, thereby having but limited variation in aggregate polices and policy changes and in responses to those policies and policy changes on which to base their analyses. Studies focusing only on specific industries are likely to miss an important part of the picture. One of the major effects of policy changes may be 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 policy changes, wage differentials in some manufacturing sub-sectors may decline, but due to the same policy changes, the differences among manufacturing sub-sectors, or the wage differentials in other sectors of the economy may expand. Analysts looking at a subset of industries observe only partial effects, yet the magnitude of and direction in which wage differentials change overall may be very different from such partial effects.

Our new estimation strategy allows us to estimate the differential impact of policies and policy changes on labor market returns to workers with different schooling levels, while controlling for all fixed and time-varying country characteristics -- the effects of which otherwise could be confounded with the

[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 suitable 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 makes 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 of UNIDO (2000) refer to a small sample of manufacturing industries that only could be used to capture partial effects of reforms, which, as discussed in the text, may be very different from the overall effects.

effects of the policies and the policy changes.[8]

The question also arises whether 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 make a preliminary effort to address this question, focusing on the long-standing issue of the relative impact of trade liberalization versus technological charge on wage inequality. But we present our results as suggestive only, since there are limits on how well we can characterize the different economic environments in different countries.

The paper is divided into five sections. Section 1 presents the data and provides up-to-date evidence on the evolution of wage differentials and on the pace of policy reform in Latin America. Section 2 discusses estimation issues. Section 3 presents our empirical results, first with respect to the estimated effects of policy *levels* and then with respect to the estimated effects of policy *changes*. Section 4 reports on a set of robustness tests for our main results. Section 5 presents our conclusions.

1. Data Construction and Patterns in the Data

To explore empirically the relationships between both policies and changes in policies on the one hand and the relative returns to schooling on the other hand we need: (a) data that characterizes wages by schooling levels over time and (b) indicators that summarize the nature of policies and changes in policies in each country over time. These data requirements are considerable, and up to now have limited analysis

[8] There have been studies on the impact of supply and demand factors on the wage premium for higher schooling in the U.S. at least dating back to Katz and Murphy (1992), who use what they characterize as "a simple supply and demand framework." Katz and Murphy focus on wage differentials and therefore effectively control for all unobserved variables that affect ln wage differentials, similarly to the present study. But their study differs from the present study in that it focuses exclusively on one country with relatively limited variation in policy changes and does not include explicit direct representations of these policy changes. The approach that we use combines data from a number of countries at different points of time, uses explicit representations of policy levels and changes and controls for all country factors that may determine wage levels, as well as tests for the impact of some prominent aggregate country variables directly on wage premia.

of the type we perform. 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 policy indices and other economy-wide changes of interest.

1.1 Data on Wages

Data Sources

The best sources of information on wages at the individual level are employment surveys and household surveys. In the case of Latin America, there are three reasons why household surveys are a better source for this study than would be employment surveys. First employment surveys in most countries in Latin America only cover the major cities in each country rather than all urban areas.[9] Second, employment surveys tend to oversample industrial sectors and the formal economy, while generally household surveys represent all sectors of activity including both formal and informal employment. Third, unlike household surveys, employment surveys in Latin America do not include a detailed breakdown of income sources in their questionnaires, but rather ask only about labor incomes. They are therefore more prone to measurement error than household surveys. When asked only about one income source, respondents have difficulty distinguishing between income they obtain strictly as payment for their labor (wages), and income they obtain as returns to capital, rents, gifts, or others. For self-employed individuals, the probability of confounding wages and return to capital is larger if they are asked only a generic question on "labor incomes", than if they are asked for a detailed breakdown of

[9] Some countries such as Peru or Colombia recently introduced employment surveys with complete urban coverage, but series with this kind of information have only been made available in the 1990s. In most other countries, employment surveys continue to be restricted to a few major cities.

wages, profits, rent, etc. Household surveys normally do ask explicitly for a detailed breakdown of income by source.[10]

Many countries in Latin America have household surveys with information on incomes, but for this work we impose four conditions in order to improve data consistency and quality. First, the household survey has to be representative of the entire urban population of the country.[11] Second, the survey questionnaire has to include a breakdown of income by source, with at least three separate questions on income, that identify labor income, profits, and capital rents separately. Third, the recall period for incomes has to be the previous month in each survey.[12] Fourth, the central purpose of the survey must be to collect information on the standard of living of the population to assure that obtaining accurate information on incomes is an important objective of the survey.

We are able to access the micro data from 71 household surveys fulfilling these requirements, for various years between 1977 and 1998 for 18 Latin America countries that include about 95 percent of the total population of the region. Surveys are listed by country and year in Appendix Table A1. Altogether, the 71 surveys include 2.22 million household and 9.6 million individual records. The average number of households and individuals surveyed across all data sets is 23,953 and 103,329, respectively per survey. Previous efforts of data compilation have been much more limited in country, sector, and population coverage.

Previous work on this topic has been plagued by comparability problems (e.g., see note 7). To assure comparability of our data we use the survey questionnaire to identify the following specific item in

[10] To document these differences we compare hourly wages for the same geographic area and reference period using employment and household surveys in Mexico, where the Urban Employment Survey and the National Household Income and Expenditure surveys are both available for 1984, 1989, 1992, 1994, 1996 and 1998. We find that the employment surveys systematically yield wages that are around 35 percent above those in household surveys. The only difference between the two questionnaires is that the household survey includes much more detailed disaggregation of income sources, while the employment survey includes only questions about labor incomes.
[11] In fact, most of the surveys we include in our data base are nationally representative. The only surveys that have urban rather than national coverage are those for Argentina and Uruguay, and the earlier surveys for Bolivia.
[12] The Mexican household survey questionnaire asks about income in each of the previous six months, but we only use information on the previous month for consistency with the other countries.

each household survey: "*during the past month, how much did you receive as net income from remuneration to your labor*". The income obtained in response to this question is then divided over the number of self-reported worked hours and deflated by the consumer price index to compute the real hourly wage rate. When an individual has more than one job, we compute real hourly wage rates from all jobs. The procedure is applied to all labor-income earners regardless of whether they are employees or self-employed. For the self-employed, having a breakdown of other income sources in the questionnaires reassures us that measurement error in hourly wage rates is limited. As a further check, in Section 4.3 we report robustness checks to verify if our conclusions change when we exclude the self-employed from our sample.

Definition of the Sample

We restrict our sample to employed urban males aged 30 to 55, which controls for three individual characteristics: age, gender and urban location.[13] These urban areas include all urban areas, which is more extensive coverage than the major cities to which, as noted, some of the employment surveys are limited. As shown in Appendix Table A2, after imposing these restrictions we are left with an average sample size across the 71 household surveys of 7,424 individuals; as expected, countries with smaller populations tend to have fewer observations. The smallest sample is for Nicaragua, and the largest is for Brazil, respectively one of the least populated and the most populated country in the region. Even so, all sample sizes are over 1,000 individuals, and sample sizes are quite stable for different years within countries, with relatively large changes in sample sizes over time only in Argentina, Chile, and Venezuela.

Our samples of urban males 30-55 years of age represent about one fifth of the total population employed, 30 percent of the population employed in urban areas, and 32 percent of all males employed

13 In addition this has the advantage of ensuring that our assessment of the effect of reforms on wage differentials is less a function of one-time shifts in demographic composition of the population that are reflected in the labor force.

(columns 1, 2 and 3 in Appendix Table A3).[14] The samples account for one third, almost 42 percent, and almost 50 percent of all wages, urban wages, and male wages, respectively (columns 6, 7 and 8 in Appendix Table A3).

The gender and age restrictions minimize 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 policy levels and changes, and not to changes in labor force participation decisions that affect labor supplies. The labor force participation rates of this group (across all years for which data are available) are about 95 percent on average, and unemployment rates are only about 3.8 percent (columns 4 and 5 in Appendix Table A3). High participation and low unemployment rates guarantee that in restricting the analysis to wage differentials, we are *not* missing other potentially important effects of policies and policy changes for urban males in the 30-55 year age range, such as changes in employment levels. The restriction to urban areas is because data quality on labor incomes is higher for urban than for rural areas, in part because rural activities (such as agricultural self-employment) involve the use of own labor, land and capital simultaneously, which makes it very difficult to obtain a pure measure of income from labor net of payments to land and physical capital. At the same time, by considering urban areas as a whole, we are able to examine the effect of policies and policy changes over most production sectors in these economies because GDP from agriculture -the prime activity in rural areas- accounts for only about 15 percent of total GDP in Latin America.[15]

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[14] The age restriction is the main element reducing sample sizes. Our sample includes relatively larger shares of the total population in countries in which the demographic transition started earlier so that they now have relatively large shares of the population in the 30-55 age range (e.g., Argentina, Chile, Uruguay and Venezuela) than in countries in which the demographic transition started later (e.g., Costa Rica, Ecuador, Guatemala, Honduras, Paraguay and Nicaragua). Restricting the sample to urban areas also reduces sample sizes, but the differences across countries due to this restriction are smaller than are those due to the age restriction.

[15] See IDB (1999).

Characterization of Wage Differentials

We characterize information on changes in wage differentials over time in three ways. The first is based on the standard Mincer-type semi-log wage regression, where the dependent variable is the log of hourly wages, and the right-side variables are dummies for completed years of schooling, potential work experience (age minus six minus years of schooling) and potential work experience squared.[16] The estimated coefficients for the dummy variables are normally interpreted as the returns to schooling.[17]

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.[18] The generally positive slope for the linear return in Figure 1 reflects that the return to an extra year of schooling in Latin America has increased by about 7 percent during the 1990s. The disaggregation by schooling levels 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 after the early 1990s though with partial recovery in the late 1990s.[19]

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[16] 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.

[17] As discussed 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). If there are such biases but their magnitude is constant over time, then our estimates still correctly portray the movements over time.

[18] 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 we have relatively few household surveys for previous years. Countries with only one observation are excluded from this estimation.

[19] Attanasio and Székely (2001) present a detailed account of the evolution of returns to schooling by country.

Our second approach to characterizing changes in wage differentials over time, which we use in the econometric estimates in Section 3, is based on comparisons of the difference in (log) hourly wages across three schooling categories: primary complete or less; some secondary but no post-secondary; and at least some post –secondary (hereafter “higher”).[20] 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 A4 presents some summary statistics by country for the difference between the ln wage of individuals with higher education relative to those with secondary and primary schooling, respectively, averaged over age groups and years. Figure 2 presents regional patterns for these ratios, smoothed in the same way as in Figure 1, and normalized to their 1990s values. This figure reveals that the wage gap between individuals with higher schooling and those with primary or secondary schooling has widened considerably during the 1990s in Latin America, though with some closure for the higher-to-primary gap after 1994. The ratio between the wages of those with secondary and the wages of those with primary schooling increased in the early 1990s to a peak of 26 percent above the 1990 ratio in 1994, and then it declined after 1994 to about 13 percent in 1998.[21]

Our third approach is to estimate OLS regressions for each country separately in which the dependent variables are the differences in ln hourly wages between two schooling categories, and the independent variable is a year trend. The first two columns of Table 1 give the coefficient estimates for the trend variable for each country. Countries are ranked according to the change in the ln wage gap between individuals with higher education and primary. As can be seen in the next-to-last line, the

[20] Because we split the samples into five-year age groups, we are always comparing the wages within age groups (i.e. cohorts), so that secular increases in average schooling over time (reflected in higher schooling on average for younger males) are not a problem. For males younger than 35 in the first survey, and that enter our sample in subsequent surveys, there is some chance that the rate of change in average years of schooling changes over time. However, the rate of change is likely to be lower in later years (going from 10 to 11 years vs. 3 to 4 years for example), reducing (not expanding) wage differentials, and thus if anything weakening our results.

[21] For striking evidence of the phenomenon in Brazil, see Bloom and Velez, 2001.

average change has been slightly greater for the higher-primary wage gap than for the higher-secondary gap. This is in line with the evidence in Figure 2.

The countries where the higher-primary wage gap (second column) increased the most are Paraguay, El Salvador and Colombia. The only three countries where the gap narrowed are Argentina, Venezuela and Honduras. Paraguay and El Salvador are also the countries where the higher-secondary wage gap increased the most, while Colombia registered a more moderate increase than in the higher-primary gap. Peru and Argentina are the two countries where the increase in the higher-secondary gap was smallest, but there is no country where the gap narrowed. The correlation between the coefficients in columns 1 and 2 in Table 1 is .86, which indicates a high (although not perfect) correspondence between changes in the higher-primary and higher-secondary wage gaps.

1.2 Characterization of Policies and Policy Changes

To characterize the levels of and changes in different types of policies, we use the five policy indices presented by the Inter-American Development Bank (IDB (1997)). Lora (1997) performed the background research for the indices for the 1985-1995 period, with an extension of the series for 1970-1984 by Morley, *et al.* (1999). The five indices for 1970-1995 characterize trade policy, financial policy, tax policy, external capital transactions policy, and privatization policy. A labor policy index, also developed by Lora (1997), is available for the shorter period from 1985 to 1995. The country-specific sources of underlying data and the methodology for constructing each policy index are set out in detail in IDB (1997).[22]

Unlike proxies commonly used in the literature, these policy indices have the advantage that they are based on direct indicators of governmental policies, so that they measure directly only policy efforts.

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[22] The IDB report, the original indices, and a detailed explanation of each item included in their calculation can be accessed electronically at http://www.iadb.org/oce/ipes/5b.htm. The indices in the original source previously have been used mainly for exploring the relation between aggregate economic growth and policies.

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. A major problem with these proxy variables is that they reflect not only or necessarily policies, but reactions to policies by individuals and entities in both the private and the public sectors. As representations of policies, they are contaminated by responses to the policies and do not necessarily represent the policies *per se*.

The idea behind each index is to measure the extent to which policies grant space to market forces and eliminate distortions, under the assumption that by doing so, greater efficiency in the allocation of resources is achieved. Each index is calculated as follows. The trade policy 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: sector controls of foreign investment, limits on profits and interest repatriation, controls on external credits by national borrowers and capital outflows. The tax policy index also 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 tax policy index is higher, the lower is 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 policy index considers firing costs (severance payments are legislatively mandated in most countries of Latin America) after one and ten years of work, mandatory costs for overtime work, restrictions on temporary contracts and the value of contributions to social security.

Each policy index is the arithmetic average of the specified components.[23] 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. The interpretation of the coefficients below, of course, is what would be the impact of a change from the least to the most liberal policy in Latin America during the time period we use. This range is defined by the range observed in the region, not by what has been observed in other parts of the world or by what hypothetically might occur. For example, for trade reforms we do not consider what would happen if there were an uniform zero tariff because that was not observed in the region during the period of our analysis.

Figures 3a and 3b present the evolution of each policy index, plus the average for the first five indices (that for labor market policy 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 policy index nearly doubled between 1970 and 1995, illustrating the diffusion of policy reforms across the region and their tendencies towards increasing depth over time. Second, beginning in 1985 the pace of overall policy reform accelerated. Third, there are substantial differences across the individual indices

[23] We realize that indices can be sensitive to specific choices about how to weigh each of their components. Unfortunately, because we do not have access to the underlying data for constructing the indices we are not able to test for the sensitivity of our estimates to the use of different weights, nor can we document which components of each index account for most of the variation during the period under study. However, we note that extensive earlier work relating these indices to growth patterns (e.g. IDB, 1997) yielded reasonable and robust results. In addition, the values of the indices by country and year accord with general impressions of timing of reforms across countries. For example, the lows and highs respectively of the indices are as follows: trade, Uruguay, 1970-77; Chile 1995 (latest year of our data for this country); capital account, Nicaragua 1998, Argentina 1995; financial sector, Argentina 1973-74 and Nicaragua 1988, Mexico, Chile and Argentina 1995; privatization, Nicaragua 1987, Dominican Republic 1994 and Argentina 1995; tax reform, Dominican Republic 1970s, Bolivia 1994 and Uruguay 1995; labor, Mexico 1995 and Panama 1995.

averaged across countries.[24] Over the 1970-1995 period, the financial market policy index about tripled, the trade and tax policy indices doubled, and the capital account liberalization index increased by half. In sharp contrast the privatization and labor market indices varied much less than the others and for most of the 1990-5 period the latter was below previous peak levels.

Lora (1997) and Morley, *et al.* (1999) discuss the evolution of each policy index by country and the synchronicity of policy reforms. To summarize some of this information we present correlation coefficients among all policy indices in Appendix Table A5. The highest correlations are found between the index of trade liberalization and the indices of financial market and tax reform. But even in these cases the correlation coefficients of .58 and .61 suggest that although some reforms had some degree of synchronization, the reforms generally varied a fair amount in their pace and intensity over time and across countries.

Columns 3 to 7 in Table 1 summarize the speed of policy reforms by country. Similarly to the description of the evolution of wages gaps by country in the first two columns, we estimate an OLS regression where the dependent variable is each of the policy indices entered separately, and the independent variable is a year trend. Therefore, the speed of reforms corresponds to the same time period as the changes in the wage gaps documented earlier. For each index, we divide countries into three groups according to the size of the coefficient estimate for the year trend. The five countries with the highest coefficient estimates are classified as "high-speed reformers", the next five as "medium-speed reformers", and the remaining six countries as "low-speed reformers". The correlation coefficients between changes in each policy index and changes in the higher-primary wage gap are presented in the last line of the table. The domestic financial policy index, the capital account liberalization policy index and the tax policy

[24] Of course, we are describing the figure, given the normalization of the indices set out above. An increase in the average value of an index in effect reflects that an increasing number of countries are approaching the maximum level of reform experienced in some country in the region in the period.

index all have fairly strong positive correlations, while the other two indices have negative (and weaker) correlations.

No clear pattern relating trade policy reforms to changing wage gaps is apparent. While the country with the largest increase in wage gaps (Paraguay) is a high-speed reformer, the next three countries in terms of increases in wage gaps are all low-speed reformers. On the other hand, three of the four countries that have the smallest increases in the wage gap are medium-speed reformers. For the domestic financial market liberalization index there is a somewhat clearer pattern. The five countries with the largest increases in higher-primary wage gaps are either medium or high-speed reformers, while two of the countries where the gap narrowed, are low-speed reformers. Nevertheless, there are still countries with relatively small expansions of the wage gap that are high-speed reformers (Bolivia, Brazil and Argentina).

The clearest patterns are for the privatization, capital account liberalization, and tax reforms. For privatization, four of the five countries with the largest expansion of the higher-primary wage gap are low-speed reformers, while five out of six with the smallest widening of the gap are either high or medium-speed reformers (Argentina is the only high-speed reformer with small wage gap expansions). The capital account liberalization and the tax policy reform have the opposite relations with changes in the wage gaps. Among the six countries with the greatest widening of the wage gap, three are high-speed and two are medium-speed in these two reform areas. Of the six countries at the bottom of the table at least four are low-speed reformers, and the rest are medium-speed.

The final column follows IDB (1997) and classifies countries into four groups according to the relative timing and pace of changes in the average policy index between 1985 and 1995. The early and sustained reformers are those that were above average in 1985 and 1995. The gradual reformers were above average in 1985, but fell behind during the course of the following 10 years. Intense reformers are those that were below average in 1985, but accelerated the process and were above average by 1995.

Countries below average at the beginning and end of the 10-year period are classified as slow reformers. Interestingly, the two countries experiencing the largest widening in wage gaps (Paraguay and El Salvador) are both intense reformers, while the two countries where the higher-primary wage gap narrowed the most, are slow reformers. Four out of the six countries with the smallest expansions of the higher-primary wage gap are also slow reformers. But apart from these cases, there are few other distinguishable patterns in these data.

Figures 4a and 4b provide further perspective on the relation between polices and wage differentials. Figure 4a shows the relation between the average policy index and relative wages for the higher-secondary wage differential, and Figure 4b for the higher-primary differential. In both cases the relation is positive, indicating that higher values of the policy indices are associated with larger wage differentials, with the association a little stronger for the higher-primary wage gap. In both cases there also is substantial dispersion around the regression lines. To be able to go beyond discussing associations, we need to address several methodological issues. The following subsection and Section 2 of the paper are devoted to this task.

1.3 Other Changes at the Country Level Correlated with Policies

The correlations between the wage differentials by schooling levels and the changes in policy indices that are discussed at the end of Section 1.2 suggest some general patterns of associations. But one major problem in identifying the effect of policies on wage differentials is that policies (as well as changes in the policies, which are our principal interest) may be correlated with other country characteristics that also may affect wages and that are not controlled for in these simple correlations. If such variables are included in estimates of the effects of policies or of policy changes, their inclusion reduces the limited degrees of freedom and increases possible multicollinearity problems. But if they are excluded and are correlated with the policy indices or with the changes in policies, their exclusion may cause unobserved variable bias in the estimated coefficients of the effects of policies and of policy changes.

We help to characterize the possible extent of this problem in Table A6 in the appendix, which presents correlation coefficients for the policy 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 may have direct effects on wages.[25] As expected, with very few exceptions the policy indices are inversely correlated with volatility, inflation and the level of the real exchange rate, and 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 policy index. The relation appears to be driven by the negative relation between capital flows and the privatization and tax policy indices. Also surprisingly, the variable with the smallest correlation with the average policy 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 policy changes 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, at least in our estimates of the impact of policy *levels*. (Not surprisingly, the correlations are much closer to zero and not significant for policy *changes*.). In Section 2 we explain how we ensure such controls.

2. Estimation Issues

2.1 Empirical Specification

[25] All macro variables are taken (or calculated) from the World Bank (1999).

We explore, as noted, the empirical relations between policy *levels* and the wage differential between schooling levels (Section 3.1) and between policy *changes* and the wage differential between schooling levels (Section 3.2). We use a similar specification in both cases, but for the exploration of the impact of policy levels we include only the (lagged) policy levels and for the exploration of policy changes we include a distributed lag in policy changes. If all adjustments to policies occurred within one year, the two specifications would lead to the same results. But if adjustments to policy changes are distributed over several years, the estimates with the distributed lag in policy changes are informative about the adjustment process. We begin in Section 3.1 with some discussion of the estimates of the impact of policy levels because they more concisely summarize the policy effects. But we explore somewhat more extensively the estimates of the impact of policy changes distributed over time in Section 3.2 because they suggest that lags are important.

For our estimates of the impact of policy *levels* 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) policies. To describe the estimation approach we extend the basic semi-log wage relation to include possible effects of policies that may differ by schooling levels, along with possible effects on wages independent of schooling:

(1) $$\ln W = (\alpha_p + \beta_p L)P + (\alpha_s + \beta_s L)S + (\alpha_h + \beta_h L)H + \alpha + \beta_L L + \delta I + \gamma Z + \varepsilon$$

where P, S, and H are dichotomous variables that refer to the highest completed schooling being primary (P), secondary (S) and higher (H) schooling; L is a vector of the *level* of policies (i.e., the relative degree of regulation or liberalization at a point of time); I is a vector of individual variables (e.g., age); Z is a vector of country variables (e.g., capital per worker, state of technology);[26] and ε is a stochastic shock. All of the variables could have subscripts for time and country and the individual variables also could have

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

subscripts for individuals, but these are suppressed to reduce clutter. In this specification the impact of primary schooling on ln wages is ($\alpha_p+\beta_pL$), the impact of secondary schooling on ln wages is ($\alpha_s + \beta_sL$), and the impact of higher education on ln wages is ($\alpha_h + \beta_hL$). Thus, policy levels 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_L), all controlling for individual and country characteristics. Our primary interest is in obtaining estimates of the coefficients of the differential effects of policy levels by schooling levels -- that is of the relative magnitudes of the coefficient vectors β_p, β_s and β_h. Estimates of the impact of other individual characteristics (the coefficient vector δ), of the impact of policy levels independent of schooling (the coefficient vector β_L) 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 estimation of relation (1). Four of these are:

(i) There are a large number of parameters. With five policy 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 71 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 policy levels. While this does not in itself cause biases, it is likely to lead to limited precision for the coefficient estimates of the policy levels and other economy-wide variables.

(ii) 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.

(iii) Not all of the possibly relevant country-level variables are observed in our (or any other) data. If the unobserved variables are correlated with the interaction between the policy levels and schooling, the result is unobserved variable bias in the estimated effects of policy levels on the returns to different schooling levels. One possible partial resolution for this problem is to control for country fixed effects with country dummy variables in relation (1). But this strategy (i) results in loss of degrees of freedom for estimating the country-wide effects and (ii) 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).

(iv) The country-wide factors that affect lnW 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, training and schooling) and the nature of options of the individual in the labor market.[27] 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

[27] 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; Behrman, Birdsall and Kaplan 1996).

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 therefore adopt an estimation strategy that reduces or eliminates all four of these problems but permits the estimation of the relative impact of policy levels on schooling returns in relation (1). We sum relation (1) by averaging it over quinquinia 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) $$\ln WS - \ln WP = (\alpha_s - \alpha_p) + (\beta_s - \beta_p)L + (\varepsilon_s - \varepsilon_p)$$

(2b) $$\ln WH - \ln WS = (\alpha_h - \alpha_s) + (\beta_h - \beta_s)L + (\varepsilon_h - \varepsilon_s)$$

(2c) $$\ln WH - \ln WP = (\alpha_h - \alpha_p) + (\beta_h - \beta_p)L + (\varepsilon_h - \varepsilon_p)$$

where lnW*i* (for *i* = P, S, H) is the average for a birth cohort over a quinquinium of lnW for the schooling level *i* and ε_j (for *j* = p, s, h) is the stochastic disturbance term for a birth cohort of a quinquinium 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 differences of principal interest, i.e. whether the impact of policy levels differs by the schooling level (i.e., $(\beta_p - \beta_s)$, $(\beta_h - \beta_s)$, $(\beta_h - \beta_p)$), and direct tests of the statistical significance of these differences.

These estimates have a number of advantages over efforts to estimate relation (1) directly with regard to the question of primary interest for this paper - are there differential effects by schooling levels of the impact of different policy levels on workers' wages? These can be seen by reconsidering each of the four problems discussed above. (i) For estimating each relation in (2) there are only six parameters (one for each policy level index plus one for the difference independent of the policy level indices) rather than at least five times as many for estimates of relation (1). (ii) With many fewer variables for estimating relation (2) than relation (1), the problems of collinearity are reduced. (iii) This specification controls for all unobserved country characteristics whether fixed over time or time varying (including, for example, endogenous policies that are in the vector Z) so, conditional on the specification in relation (1), there are not problems with omitted variable bias. (iv) This approach controls for the entire history of country-wide effects since the time of the marginal schooling/labor force entry decisions for each birth cohort because relation (2) is estimated within a (five-year) birth cohort.

For our estimates of the impact of policy *changes* on the returns to different schooling levels we adopt a parallel specification except that we include a vector of policy changes (C) instead of policy levels (L):

(3) $$\ln W = (\alpha_p' + \beta_p' C)P + (\alpha_s' + \beta_s' C)S + (\alpha_h' + \beta_h' C)H + \alpha' + \beta_C' C + \delta' I + \gamma' Z + \varepsilon'$$

where C refers to distributed lags in policy *changes* and the primes are used to distinguish the parameters in relation (3) from those in relation (1). Differencing relation (3) between pairs of schooling levels for each age group yields:

(4a) $$\ln WS - \ln WP = (\alpha_s' - \alpha_p') + (\beta_s' - \beta_p')C + (\varepsilon_s' - \varepsilon_p')$$

(4b) $$\ln WH - \ln WS = (\alpha_h' - \alpha_s') + (\beta_h' - \beta_s')C + (\varepsilon_h' - \varepsilon_s')$$

(4c) $$\ln WH - \ln WP = (\alpha_h' - \alpha_p') + (\beta_h' - \beta_p')C + (\varepsilon_h' - \varepsilon_p')$$

Estimating these relations has the same advantages as estimating relations (2), with an added advantage regarding cointegration for the policy change variables of primary interest (Section 2.2).

2.2 Estimation Strategy

In the estimation of relations (2a-2c) L represents the policy *levels* in the relevant years. In the estimation of relations (4a-4b) C embodies policy *change*s in the relevant years, the effects of which may differ with different lags. As already mentioned, the nature of the lag structure may be crucial because policy changes lead to economic restructuring through resource reallocations that can have differential effects over time. For instance, the main short-term effect of a policy change 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. To explore the dynamic effects of policy changes requires including lagged right-side variables. Because analytical frameworks do not provide specific guidance regarding the timing of the effects of policy changes, we estimate the lag structure from the data by including policy changes with lags of up to four and up to seven years (depending in part on the number of parameters in the specification), with the lagged level of the policy index prior to these lagged policy changes to summarize the policy regime prior to the recent policy changes.

Because the policy level indices (lagged one year for the level estimates and to the period prior to the policy changes for the change estimates) are right-side variables that are likely to have unit roots, we need to test whether wage differentials and policy level indices are cointegrated. Appendix Table A7 presents the test statistics for the Augmented Dickey-Fuller test for each policy level index independently and for the average of the five policies by using the whole series spanning 1970-1995 for each country. For all countries and all policy indices we do not reject the null hypothesis that the series has a unit root. However, we are not able to test whether wage differentials (the dependent variable in our estimation) have a unit root because of the limited number of observations per country. Therefore, it is not possible to test for cointegration, and this raises the concern that the standard errors of the coefficients of the right-side variables will be incorrect. We therefore estimate our base regressions in first differences.

In our policy change estimates, this should not be a problem for the coefficient estimates of the policy changes because if policy *levels* are *I(1)* series the policy *changes* should be *I(0)*. But even when we use lagged policy changes as right-side variables it is desirable to include the lagged level of policies prior to those changes to control for the fact that countries that are starting out with low levels of policy liberalization may be more likely to register greater policy changes than countries which in the earlier years already had had relatively high levels of policy liberalization, and which therefore have more limited scope for additional liberalization. Even though the lagged levels of policies are introduced as control variables that are not of central interest, and where having robust standard errors is not crucial, we estimate our base regressions of changes also in first differences, to avoid any concern with the possibility of zero cointegration.[28]

Of course there may be cohort effects related to changing labor force composition by schooling levels due, for example, to supply-side schooling changes or rural-urban migration. To the extent that such effects enter additively in relation (1), they are controlled perfectly in relation (2). If they enter in relation (1) interactively with the schooling levels, then in relation (2) they enter in differenced form (i.e., so there is an additional term in relation (2a) equal to $g_s M_s - g_p M_p$, where g_i is the cohort effect on the rate of return to the ith schooling level and M_i is the cohort variable for the ith schooling level). If $g_s M_s - g_p$

[28] All the regressions we present are estimated with (robust) Huber-White corrected standard errors. Additionally, we do all differenced estimates by using the Huber iteration to reduce the potential effect of outlier observations. Though the majority of the differences between survey rounds in our data are gaps of two years, there are some cases in which the gaps are different than two years (with 80 percent of the cases being 1-3 years, and the remaining ones larger). Under the standard assumption that the coefficients in relation (1) have the same expected values over time and with the correction for the standard errors noted next, that the gaps vary somewhat does not cause estimation problems. We also explore the robustness of our estimates to dropping countries (including those with relatively large gaps between the surveys) from the sample in Section 4.3. Additionally, we perform our base estimates by dropping Argentina (which has the largest gap between surveys), as well as the observation with the largest gap for Uruguay, Peru and Mexico, and our results hold.

M_p and L are orthogonal, this causes no bias in our estimates. We see no reason to think that $g_s M_s - g_p M_p$ and L are correlated. But there may be a time trend in $g_s M_s - g_p M_p$, so we add a secular trend to our specification to control in part for such a possibility.

The time trend also assures that we are not just capturing a spurious correlation of policies and wage gaps simply moving in the same direction over time. This concern arises because increased demand for skills may simply correspond to a secular worldwide trend of rising wages that could be observed even without the policy changes. Policies also have secular trends towards liberalization, though not all the policy changes are in this direction (Figure 3). Including a time trend guarantees that the coefficient estimates can be interpreted as a real correlation between the two variables of interest net of any such secular trends. Because of the limited number of degrees of freedom, we are not able to include year effects.

Apart from estimating in first differences, we also estimate some of the regressions using fixed and random effects at the country level. We do this to explore whether our estimates are robust to these alternatives. If random and fixed effects yield similar coefficients to those of the estimates in first differences, this would reassure us that the right-side and dependent variables (in levels) are in fact cointegrated.[29]

3. Empirical Results

The signs of the effects of policies and of policy changes on wage differentials cannot be predicted unambiguously from theory because of the multiplicity of the effects, some working in

[29] An additional element of interest is that the random effects regressions use the information on the time-variation within countries, and even though specification (2) controls for country effects, the coefficient estimates also are identified from the between country variation, which uses the information on reforms more effectively if the right-side variables are orthogonal to the unobserved country effects. We conduct Hausman tests to see if our estimates satisfy the latter conditions and only present random effects estimates below when they do pass this test.

opposing directions.[30] We alluded in our introduction to the debate regarding the effects of trade liberalization on wage differentials; the expectation using the standard models that the opening of markets in Latin America would increase the demand for the region's apparently plentiful factor, unskilled labor, has not been borne out, perhaps due to shifts in relative availability of factors of production elsewhere in a global trading system, perhaps due to the dominance of technological change biased toward use of skilled labor, perhaps due to reductions in the cost of capital and the complementarity of physical capital and skilled labor .[31] This is only one example of the multiplicity of possibly offsetting effects.

We do posit that the trend of increasing liberalization across the board should have made the region's economies in general more flexible and more efficient; that would suggest that over long periods of time, whatever shifts in the relative returns to schooling and thus in wage differentials arise due to the "shock" of policy changes, they should gradually erode, as the resulting changes in relative prices lead to adjustments in agents' behavior. The obvious example is the likelihood that increasing returns to higher education will create incentives for more schooling, leading eventually to a reduction in the differential returns. However, we do not know the equilibrium "returns differential" and thus cannot discern whether any differences or reductions in the differential are due to natural adjustments of a flexible economy vs. structural changes in response to new "shocks".

Moreover, because the effects of the individual policies and policy changes are not clear *a priori,* it is obvious that their combined effects are not clear either. In short, the direction and magnitude of the effects of policies and how those effects vary with time are fundamentally empirical questions, which we explore in this section. As noted as the start of Section 2, we begin in Section 3.1 with estimates in which

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[30] In a similar spirit, Heckman and Pagés (2000, pp. 9-11) summarize the partial- and general-equilibrium theoretical implications of one labor market change, job security. They conclude that: "Given the ambiguity of theoretical models, the magnitude and direction of the impact of job security on employment has to be resolved empirically." The ambiguities increase with our concern about the effects of multiple policy changes with general equilibrium feedbacks.

[31] Spilembergo et al (1999) show that unskilled labor is not the region's scarce factor relative for example to China and Asia in general.

the (lagged one year) policy *levels* are the right-side variables because these yield a more concise summary of the policy effects. In Section 3.2 we turn to estimates in which policy *changes* distributed over a number of years are the right-side variables.

3.1 The Effect of Policy on Wage Differentials in Latin America

The first row in each panel of Table 2 shows the results of estimates of (2b) (higher minus secondary schooling) and (2c) (higher minus primary schooling) using the average level of the five policy indices as the dependent variable. Although this specification is somewhat more vulnerable to the concern regarding lack of cointegration discussed in Section 2.2 and discuss not incorporate the possibility of distributed lagged responses to policy changes, we start with these results because this is the simplest specification that we discuss in Section 2, and is therefore a useful reference point.

Because the individual policy indices are normalized across all country-year observations, the average of the five indices in each year represents the cumulative policy reforms of each country relative to others during the current year and all years in the sample. 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.[32] 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.

We regress each of the two dependent variables on the average policy index using a one-year lag. We use the one-year lag rather than the contemporaneous policy index because household surveys are carried out throughout the year, and are sometimes held during the first quarter. The policy indices span

[32] See IDB (1999) and Székely and Hilgert (2001). As noted in Section 2.1, 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).

the period 1970-1995, so using a one-year lag enables us to incorporate surveys for 1996, which would otherwise be dropped from the sample.[33]

The results in Table 2 are striking. First, the coefficient estimates for the average policy index are consistently positive and statistically significant for both dependent variables. This is the case for the regression estimated in differences (first column), as well as for the country fixed effects and random effect regressions (second and third columns). Thus, there is evidence that reforms have played a role in expanding the skilled-unskilled wage differentials in Latin America during the past two decades.[34] Table 3 and Figure 5 show parallel estimates to those in Table 2, but now we include the five different policy indices separately. Different reforms have differential estimated effects, but they are always jointly significant. The overall effect of policies on increasing wage differentials appears to result from the significant effects of capital market, financial sector, and tax policies – the combination of which more than offsets the opposite effect of privatization. The trade policy index alone is insignificant (though positive). The disequalizing effect of liberalizing policies is stronger for the higher-primary wage differential than for the higher-secondary wage differential, particularly for domestic financial market

[33] There are some countries, such as Nicaragua and Paraguay, where household surveys are available to us for 1998 or 1997 rather than 1996. To increase our sample size and the precision of the coefficient estimates, we use the 1998 or 1997 data for these countries as proxies for wage differentials in 1996. We performed all the regressions reported in the paper by excluding data sets beyond 1996, and all our conclusions hold.

[34] To check whether the average *level* of wages increases or declines with reform (independent of whether wage *differentials* for different schooling groups increase or decline), we estimate fixed effects regressions where the dependent variable is the absolute level of wages (PPP adjusted), and the independent variable is the average policy index. The results are not presented here (but are available from the authors) because in such estimates we are unable to control for time-varying characteristics that may be correlated with reforms. Therefore, interpreting the results as reflecting the causal effects of reforms requires stronger assumptions than are required for the analysis of wage differentials. The estimates indicate that average reforms have a positive significant effect on average wage levels. We perform similar regressions (subject to the same caveat), using in turn the average (log) wage of individuals with primary, secondary and higher schooling. The effects of reforms on wages of individuals with higher schooling are positive and much stronger than those obtained for overall average wages. The effects for those with secondary schooling are positive and more similar to those for the average wage, and smaller than for individuals with higher schooling. For individuals with primary schooling the average policy index has a negative coefficient estimate that in almost all cases is significant. Thus, these estimates suggest that 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 schooling levels.

changes and privatization (Figure 5c). These conclusions are robust to differenced estimates, estimation with inclusion of the indices separately, fixed effects, and random effects.

The magnitude of the estimated effects of policy on the wage differentials is not so small as to be irrelevant. Consider the differenced regression in Table 2 for illustration; these estimates imply that an increase in the average policy index by 0.4, which is the change observed between 1970 and 1995, would increase the ln wage differential between higher and secondary school workers by about 17 percentage points (22 percent of its mean). The same increase would raise the ln wage differential between higher and primary school workers by 19 percentage points (17 per cent of its mean).[35] The individual estimated effects in Table 3 for some policies are also fairly substantial. The financial market and tax policy indices alone, for example, increase by 0.6 and 0.4 points respectively during 1970-1995, which would expand the ln 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

Another interesting question is whether policies have differential effects in different economic, policy and technological environments. For instance, have policies had a larger disequalizing effect in countries that are more integrated into the world economy through trade, and do policies have different effects depending on the extent to which technological progress has taken place? Table 4 provides estimates of the effect of the average policy 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 each of these two variables and the average policy index.[36] We interpret these results with caution because of the concern with zero cointegration noted in Section 2.2 and for at least two additional reasons. First, one of the channels through which technology is transmitted across countries is

[35] I.e. an increase of .4 (.42) in the ln differential of higher to secondary (mean is .78) ans of 4. (.47) in the ln differential of higher to primary (mean is 1.13).

[36] These variables were calculated from data in World Bank (1999).

trade.[37] Second, technology exports as a share of GDP has its limitations as a proxy for technological change, but is the only variable available to us with sufficient coverage of the countries in our sample.[38]

Subject to such caveats, the results are quite interesting. Incorporating these variables into relation (2) improves the fit of the regression for all three estimation methods, as compared to the results in Table 2.[39] Once we control for policies, a higher proportion of trade in total economic activity appears if anything to have the effect of *reducing* earnings differentials, though the estimates are not very precise and are not significantly nonzero by the usual criterion. At the mean of the trade flow variable, the net effect of an increase in trade flows by one standard deviation is negative. Moreover, the coefficient estimates of the policy-trade interaction term, though positive, are not statistically significant, suggesting that policies do *not* necessarily 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 more liberal policies; in this case the coefficient estimates of the interaction terms are always significant.

These results suggest that in the 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 possible 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 policy indices in Table 3, and of the trade flow variable in Table 7. Of course the picture is complicated by the likelihood that increases in the export of high

[37] We estimated the same regressions with, in addition, an interaction term between trade flows and technological progress, but the coefficient estimates for this interaction are never statistically significant, so these regressions are not presented.
[38] Other variables that may be considered better suited for capturing the effects of technology are only available for a few countries and years. For instance, the number of computers per inhabitant is available from the World Bank (1999), but using these data reduces the number of observations for the econometric estimates by about half.
[39] The Wald tests in Table 7 show that all the variables are jointly significant.

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.[40]

3.2 The Effect of Policy Changes on Wage Differentials

In Table 5 and Figure 6 we present our preferred estimates of equations (4b) and (4c), in which we test the effects of policy *change*. We assess the effects of policy change in each of the past seven years controlling for the *level* of policies – and thus accumulated prior policy changes -- eight years before.

These results are also quite striking. They reveal that for both dependent variables and regardless of the estimation method, policy reforms have an initial effect of increasing wage differentials, but that this effect erodes considerably in magnitude by the second year and continues to fade over the subsequent years. By the seventh year the effect of reforms is small and no longer statistically significant, though there remains a significant positive – though relatively small -- effect of the prior policy history on the higher/primary school wage differential. The explanatory powers of the regressions are not very high, though in most cases higher than for the parallel estimates with policy levels in Table 2. Thus, although policy changes have a statistically significant effect on wage differentials with lags of several years that differ across policies, they are only a limited part of the reason why such differentials have changed.

As explained above, these regressions and the ones reported in the following tables include a time trend to assure that the policy change coefficient estimates are not capturing secular trends in increases in the demand for skills or in the relative supplies of those less schooled. Interestingly, the time trend

[40] 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)).

coefficient is not statistically significant in any of these or the following regressions. Moreover, our conclusions do not change when performing the estimates without the trend variable (the results are not presented for brevity).

Table 6 and Figure 7 explore the differential effect of each policy change over time. Because the inclusion of the policy change in the previous seven years for each of the five indices would entail a significant loss of degrees of freedom, we run five separate regressions, one per index. In each regression we include one of the policy change indices separately (coefficient estimates appear in the first four lines), and compute the average of the remaining four policy changes and introduce it as a control (presented in lines 5 to 8). For both variables (the individual policy change and the average of the other four policy changes), we include the policy change in the previous three years, as well as the value of the policy index lagged four years as right-side variables. We include shorter lags in these regressions than in those for the overall policy changes because in these regressions we include twice as many policy coefficients (i.e., for the policy change of focus in each regression and for the combined index for the other four policies). Only the results from the differenced estimates are presented for brevity, but estimation with fixed and random effects leads to the same conclusions.

In the first column of Table 6 and Figure 7a we present the results for changes in trade policy. The coefficient estimates are not significant. So, conditional on this specification, trade liberalization *per se* has not significantly widened wage gaps, which is a notable finding given the concern that it is the opening of economies that has exacerbated those gaps. One interpretation is that this result may be because of strong countervailing forces that this trade policy change induces. On one hand, liberalization reduces wage differentials if product market changes shift production towards a country's comparative advantage, which within the assumptions of the classical framework would seem to benefit less-schooled workers relative to more-schooled workers in most developing countries. But a number of possible counter-effects could widen wage differentials. For instance the pre-liberalization framework might have protected unskilled workers. Or, intermediate inputs of a given quality may become cheaper with low-schooled workers being (at least relatively) substitutes for intermediate inputs. Capital goods of a given quality are also likely to become cheaper and more-schooled workers tend to be relatively complements with physical capital. Additionally, new, more-schooled-worker intensive technologies may become available through trade and increase the demand for skills. The gains from learning about new markets and new technologies may also increase due to more rapid changes in markets and technologies, for which schooling may have high returns. Finally, Latin America may no longer have a comparative advantage in low-wage, less-schooled labor, due to the expansion of China and other low-wage Asian economies into global markets.

Increased privatization (second column in Table 6, Figure 7b) has a significant negative effect on wage differentials that decreases in absolute magnitude with greater lags. This is consistent with a situation in which before privatization state enterprises had relatively large numbers of managers (with more schooling) per production worker (with less schooling) than privatized firms in the same sector. Eventually, 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 labor in general – including both skilled and unskilled workers.

Capital account opening (third column, Figure 7c) raises wage gaps -- again the effect is reduced with greater lags, but in this case with a relatively big effect of the policy regime four years earlier before the included distributed lag of policy changes.

Financial sector liberalization (fourth column, Figure 7d) also has a consistently positive effect in increasing wage gaps, that also declines in magnitude over time for the higher/secondary wage differential. There is a large (but insignificant) effect of the policy regime prior to the distributed lags for the higher-primary wage differential. A lower cost of borrowing or improved access to financing apparently favors skilled labor, possibly because skilled labor is complementary to capital, and effective domestic capital market liberalization is likely to facilitate financing of both current production and of longer-run investments in capital and technology.

Tax reform (fifth column, Figure 7e) raises wage gaps significantly, again with a V-shaped effect; the effect of the accumulated policy reforms prior to the distributed lags is as large as the effect of policy changes lagged one year (though not significant). Except for the first year, the estimated effects are almost identical for the higher/secondary as for the higher/primary wage differentials, so in this case more than in any of the others the impact is to increase wages of those with higher schooling relative to all of those with less schooling. The reasons for these persistent effects 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 more intensively, which reduces the demand for less-schooled workers.

Labor reform (see Figure 3a) is the most recent of the reforms initiated in Latin America. It also is among the more difficult reforms to measure due to data limitations. In the first two columns of Table 7 we present coefficient estimates for labor market policy changes, which are analogous to those in Table 6, but with the average of the other policy reforms in Table 6 referring to the other five indices. We present the results for the labor policy change separately because they refer to a smaller sample of 42 household surveys to which a labor policy index (available only for 1985-1995) can be attached. The change in labor policy has a positive but not always significant effect on the wage differential between higher and secondary and higher and primary school graduates, but the effect rapidly fades away.

We stress in Section 1 that an important advantage of using the policy indices is that they permit the measurement of policy changes, while abstracting from behavioral responses to these 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 the terms of trade or other factors that are independent of domestic policies and by behavioral responses of importers and exporters to policy changes, which are two major reasons 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 this conventional trade flow variable instead of the indicator of changes in trade policies.[41] We report the estimates for the specification with this trade variable in the last two columns in Table 7. These are also analogous to the results in Table 6, the only difference being the change in the indicators of trade reform. The use of this alternative indicator does not alter the result -- that trade flows do not significantly expand the wage differential.

4. Robustness Tests

In this section we perform a series of robustness test to check whether our central result -which is that reforms other than privatization and perhaps trade policy have the effect of widening wage differentials significantly, but with the impact substantially fading away over time- holds under different specifications, estimation methods, and samples.

4.1 Alternative Specifications

As discussed in Section 1, policy changes can be influenced by economic growth or by other aggregate factors. This raises the concern that the coefficients in Tables 5-7 could be "contaminated" by the correlation between policy changes and these types of aggregate variables. Although our empirical specification accounts for this possibility in terms of correlations between policy changes and other variables that determine log wages independent of the schooling level, it does not control for the

[41] An additional reason why this alternative specification is of interest is that trade reforms, as they are characterized by changes in the trade policy 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 devaluations.

possibility of correlations between policy changes and other variables that differentially affect returns to different schooling levels. To explore the latter possibility, we present in Table 8 two sets of three additional regressions in which macro variables are introduced as controls for differential effects on relative returns between schooling levels. The first three columns refer to estimates in which the higher/secondary wage gap is used as the dependent variable, while the last three use the higher/primary wage gap. All regressions refer to differenced estimates.[42]

Columns (1) and (1a) in Table 8 present regressions (of equation 4) analogous to those in the first column of Table 4, where we estimate the impact of policy changes over the past seven years, on wage gaps. As in Table 4, we control for the policy level eight years before, and include a year trend. However, in Table 8 we add the rate of growth of GDP per capita (adjusted using PPP based exchange rates) as a control variable to address the concern that it is possible that countries with lower growth rates face stronger pressures to change their policies. For both dependent variables we find that introducing this variable has some effect on the magnitude of the coefficient estimates for the first two lags, but certainly does not modify our central conclusions from Table 4. Interestingly, the rate of growth has an insignificant effect on the wage gap.

Columns (2) and (2a) are analogous to (1) and (1a) in Table 8, but we add the unemployment rate as a right-side variable, under the argument that policy changes can also be triggered or decelerated by social pressure, fueled for instance, by high unemployment rates. Our conclusions are also robust to including this variable. Finally, regressions (3) and (3a) include an index of the real exchange rates as a control. The argument for including this variable is that devaluation or appreciation of the exchange rate may provide a more or less favorable context for policy change, so the policy change indices could be capturing some of the effects of this variable on wage differentials. However, as in the previous two sets

[42] The GDP figures refer to PPP adjusted GDP per capita. GDP and the index of real exchange rate are taken from World Bank (1999). Unemployment figures are computed directly from each household survey and refer to the same sample of individuals as used for the wage data.

of regressions, including this control variable does not modify any of our conclusions and has little effect on the magnitude of the coefficient estimates. Additionally, none of the three-macro variables included as controls in Table 8 is statistically significant.

We also estimate a set of regressions similar to those just discussed but including either the rate of unemployment or the index of the real exchange rate as the only controls (rather than combined with the other aggregate variables). The results are similar to those in Table 8 and lead to the same conclusions. We also experimented with introducing measures of inflation, terms of trade, and GDP volatility in a parallel fashion. These variables had little effect on the coefficients of interest and were insignificant in most cases. These results are not presented for brevity.

4.2 Alternative Estimation Methods

Estimating equations (4a) and (4c) with lagged policy changes lessens endogeneity concerns. The possibility that our estimates are contaminated by contemporaneous high wage gaps creating pressures for reform in a country is basically ruled out by the use of right-side variables that measure lagged policy changes. The same applies to the variable measuring the level of policy liberalization from eight years ago (used as a control), which could hardly be influenced by current wage gaps. Moreover, even if there is a systematic relationship between inequality and policy change -for instance if high inequality countries are more or less prone to adopt reforms- this is controlled for in the fixed effect regressions and in the differenced estimates. However, to reassure us further that we can interpret our results causally, we present in the first two columns of Table 9 a regression analogous to the differenced estimates in the first column of Table 4, but using instrumental variables for each of the seven policy changes. The data on policy changes for the five years preceding each policy change are used as instruments. We also instrument the policy level eight years before (used as a control), by using the average level of policies during the five preceding years. This procedure avoids a common problem with using lagged values as instruments -- that is, that the lagged values are not independent of unobserved fixed characteristics -- because relation (4) controls for the unobserved fixed effects in the vector Z so they are not in the disturbance term of the relation estimated.

The first column of Table 9 presents the regressions using the higher-secondary wage gap as the dependent variable, while the second column uses the higher-primary wage gap. As can be verified, our conclusions hold under these specifications also.

One potential concern with the sample of countries used for our analysis is that we are mixing countries with large populations, such as Brazil, with others that are much smaller. As is standard in the literature we have implicitly assigned equal weight to each country. To reassure us that a small country is not driving the results we estimate our base regression by using the population of each country and year as weights in the regression. Columns 3 and 4 in Table 9 report the results. For both dependent variables our conclusions are unmodified with this change. If anything, the short-run effects of reforms on the wage differential appear to be stronger.

4.3 Different Samples

Finally, we test whether changing the country composition of our sample or changing the sample of individuals over which wages are computed, has any implications for our analysis. Columns 5 and 6 in Table 9 summarize the results of 18 regressions, all using the higher-secondary differential as the dependent variable, and where each regression excludes one of the countries at a time. This exploration is in the same spirit as the regressions using population weights because it assesses whether any particular country drives the results. Rather than presenting the 18 regressions, we present in Column 5 the average value of the coefficient from the 18 estimates, as well as the average 't' statistic, R^2, sample size, and so on. Column 6 presents the standard deviation of each coefficient, 't' statistic, etc. to assess the spread of results obtained across estimates. Columns 7 and 8 perform the same exercise, using the higher-primary wage differential as the dependent variable.

The conclusions from this exercise are, first, that on average, the coefficient estimates and the results on the lagged effects of policy changes are the same as in our base estimates. Second, the variability across the 18 regressions is rather low; as judged by the size of the standard deviations reported in columns 6 and 8 of Table 9. These conclusions apply for both dependent variables.

The last robustness test that we present pertains to the sample of individuals over which wages are computed. As discussed in Section 1, the sample over which average wages by schooling level are computed includes urban males in a restricted age range, without distinguishing whether the individual is self-employed or not. Even though we have imposed strict conditions on the household surveys included in our sample –such as that survey questionnaires have to include at least three separate questions on three different income sources- to attempt to insure that measurement error in wages is low, it is still possible that self-employed income has relatively large and possibly systematic measurement error. To address this issue, we go back to each household survey and reconstruct our wage data, using the individual records as before but further restricting the sample to individuals that are *not* self-employed and that are *not* working in the informal sector. This modified data set is used to re-estimate our base regression in first differences.

The last two columns of Table 9 present the results. First, the conclusions about the effect of policy changes are unmodified by the change in the sample. Second, the magnitude of the coefficients is larger than in Table 5. The latter result suggests that the effects of policy changes are greater for individuals who are not self-employed.

5. Conclusions

This paper develops and applies a new approach to the estimation of the impact of economy-wide policies and changes in those policies on wage differentials using a new data set on wage differentials by schooling levels for 18 Latin American countries for the period 1977-1998. The wage data are merged with policy indices, changes in which 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 policy changes 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 policies and policy changes on one hand and wage differentials on the other, a topic on which very limited prior empirical evidence exists. We find that on average, liberalizing policies and policy changes have had a strong positive effect on wage differentials, but that the overall effect tends to become smaller over time (though there seems to be a fair amount of persistency for some of the individual policies). The disequalizing effect of liberalizing 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 and the estimated effects fade away relatively fast. Privatization reduces wage differentials, but not enough to offset the increases in wage differential due to other reforms. Trade openness has no overall effect on wage differentials, perhaps because it triggers countervailing forces that offset each other.

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 high technology 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 more qualified than for our other results. These estimates suggest that technological progress rather than trade has been the mechanism through which the disequalizing effects have been operating.

Do our results suggest that policy liberalization has been bad for equality concerns in Latin America -- a "class act" favoring the relatively highly schooled upper classes because their net effect has been to exacerbate earnings differentials? Our answer is a qualified yes. It is yes because we present what we consider to be strong evidence that overall reforms increased wage inequality across schooling levels. But this positive answer is qualified for several reasons. First, though reforms in the aggregate initially raise earnings differentials, the effect for the most part fades fairly rapidly. Second, the composition of policy reform also matters. Even in the short run, privatization *reduces* differentials, as does more trade in the presence of trade liberalization and other reforms. Domestic financial liberalization, opening of capital markets and tax reforms are the policy changes most clearly implicated in higher wage differentials. Third, in any event reforms probably were needed to improve efficiency – other evidence suggests they have in fact contributed to growth in the region.[43] Fourth, we do not know the effects of reforms on the distribution of non-labor income. They may be favorable, especially in the medium term, if for example trade liberalization or financial sector reform reduces rents to large firms and raises profits of small businesses. They may be unfavorable if they encourage higher concentrations of wealth because of reduced tax burdens or less costly access to safer external financial markets. We conclude not that the reforms should be avoided because our results show negative disequalizing effects particularly in the short run, but that their design and implementation should be fine-tuned, their

[43] The wages of those with low schooling levels could have increased due to this growth at the same time as wage dispersion by schooling levels increased. We explored this possibility. Our results are subject to caveats because we are unable to control for unobserved time-varying characteristics. However, they suggest that the reforms not only increased the wages of those with higher levels of schooling but also reduced the wages of those with low levels of schooling in absolute terms (see note 34).

sequencing understood, and consideration given to policies and programs to mitigate their shorter-run effects on inequality.

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Figure 1

Changes in Marginal Returns to Education in
Latin America in the 1990s

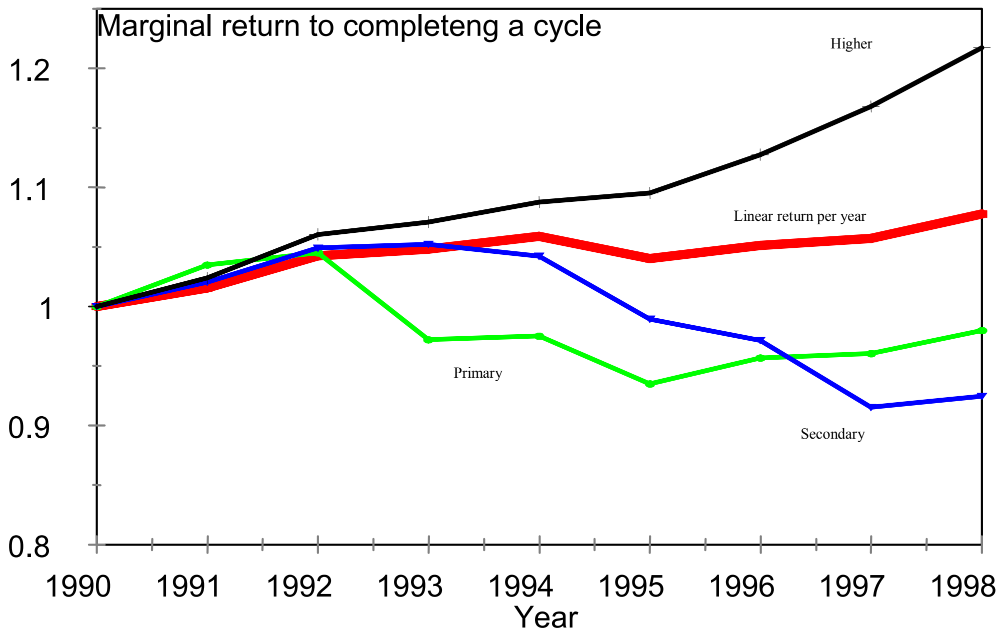

Figure 2

Changes in Wage Differentials in
Latin America in the 1990s

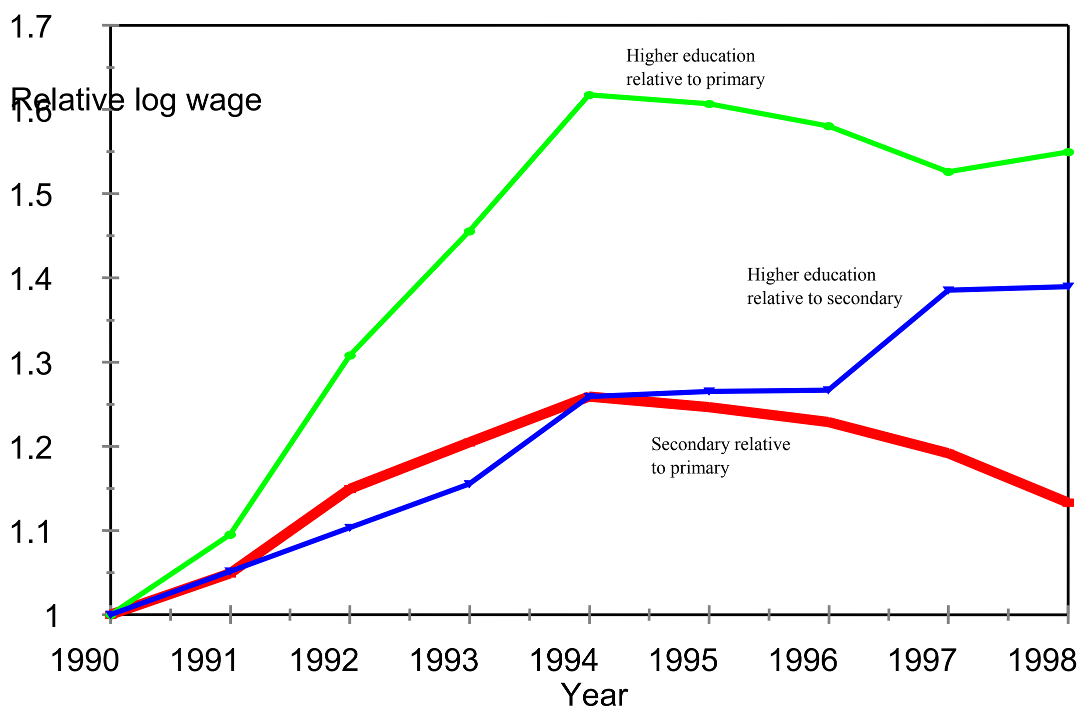

Figure 3a

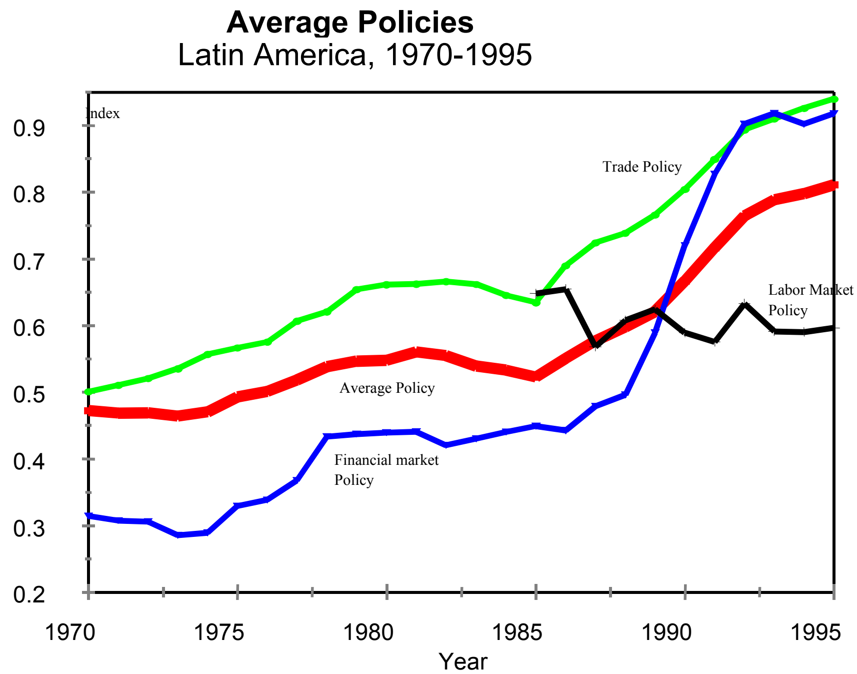

Figure 3b

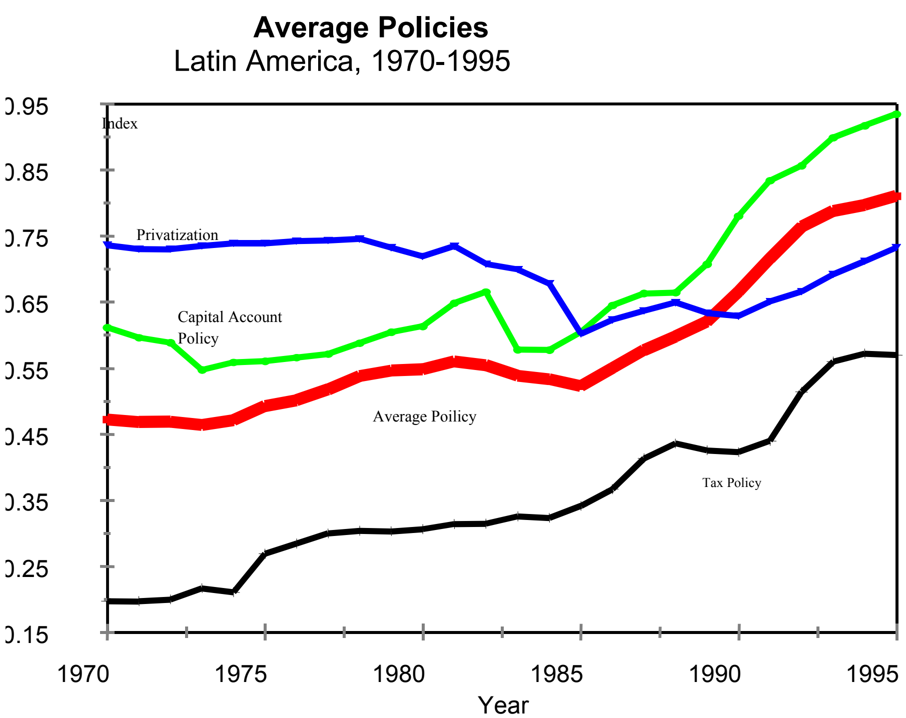

Figure 4a

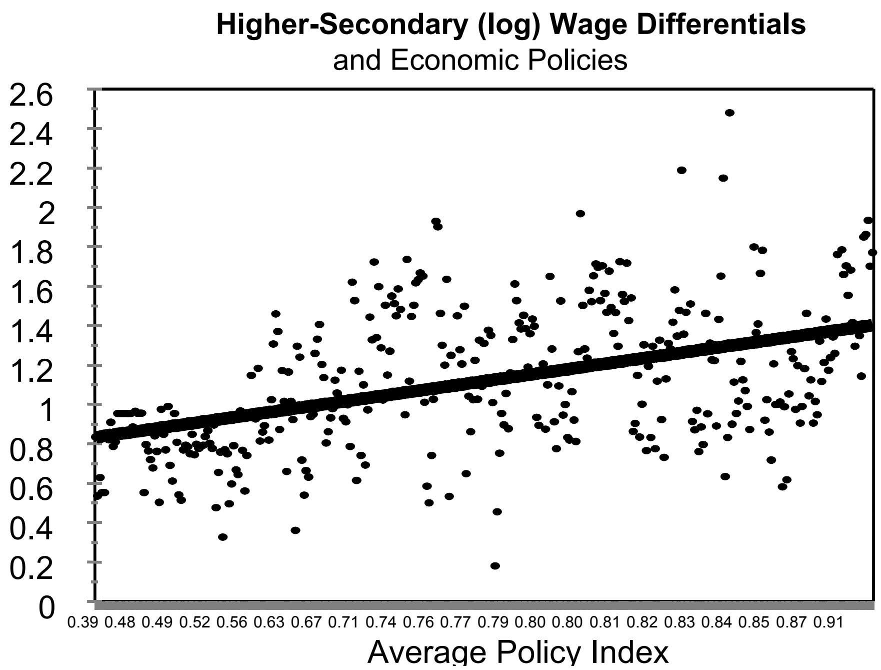

Figure 4b

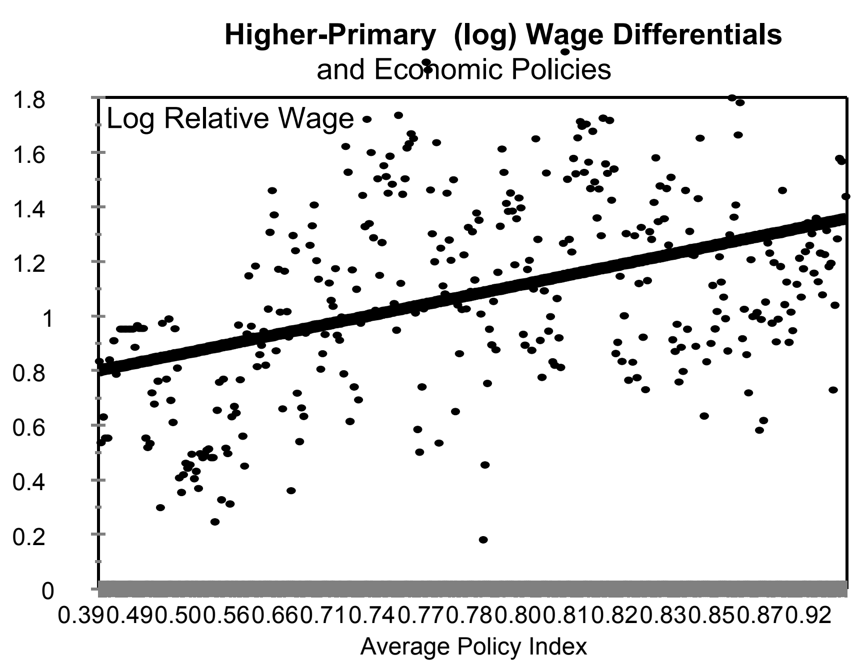

Figure 5a

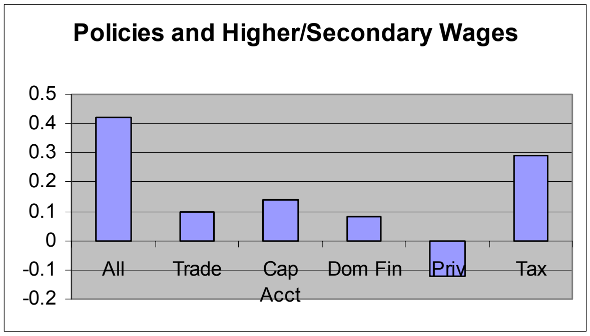

Figure 5b

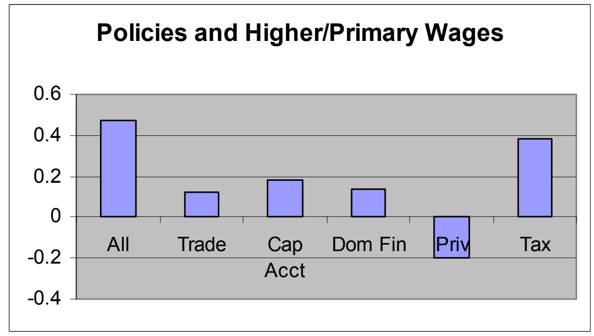

Figure 5c

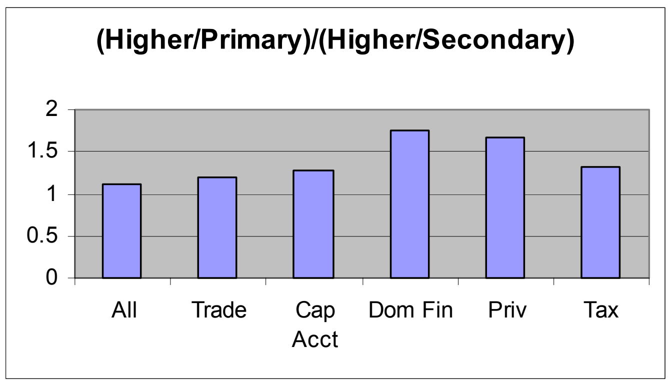

Figure 6

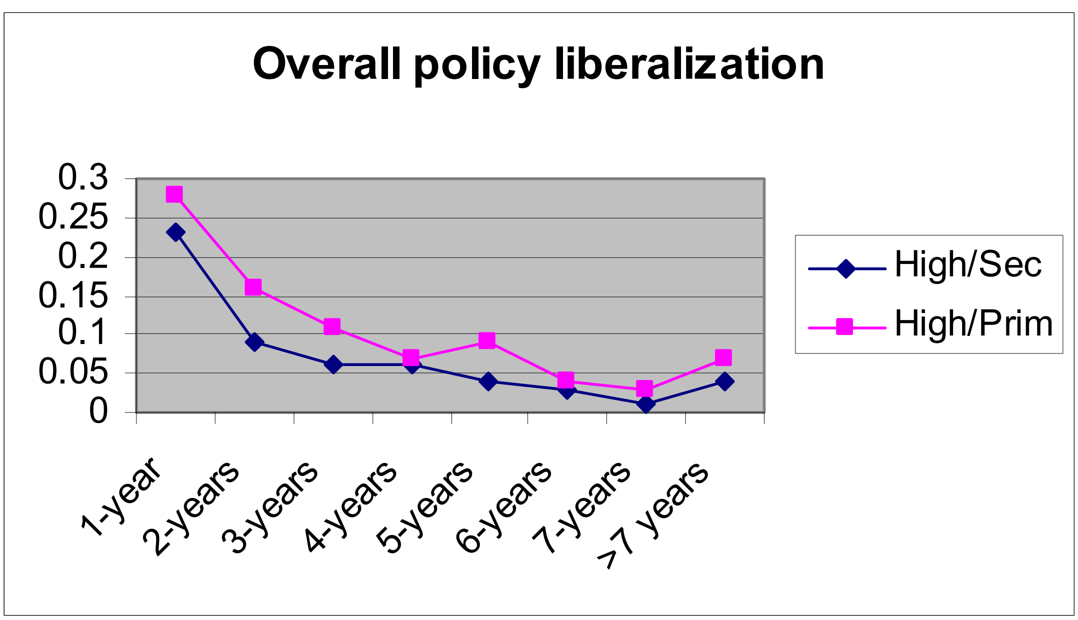

Figure 7a

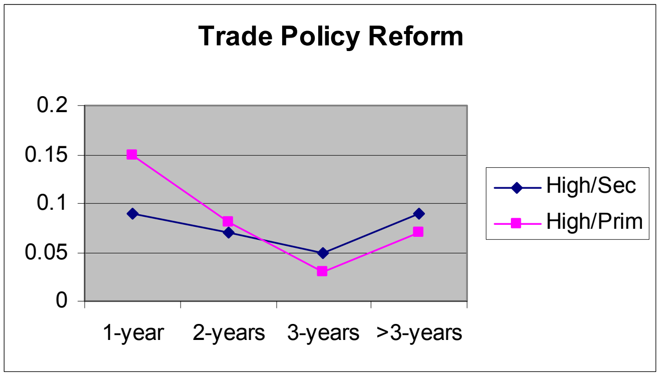

Figure 7b

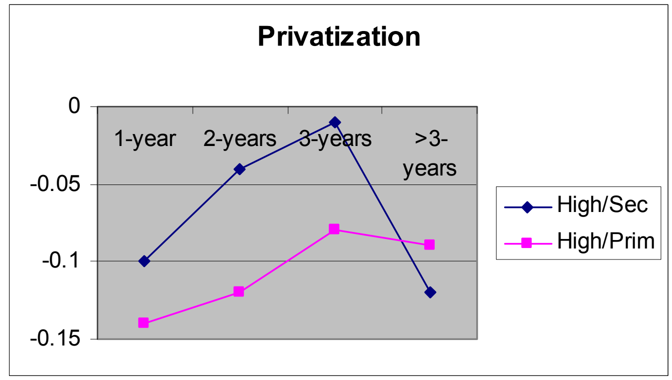

Figure 7c

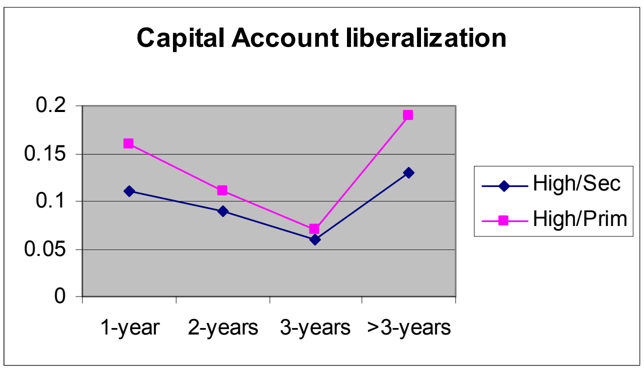

Figure 7d

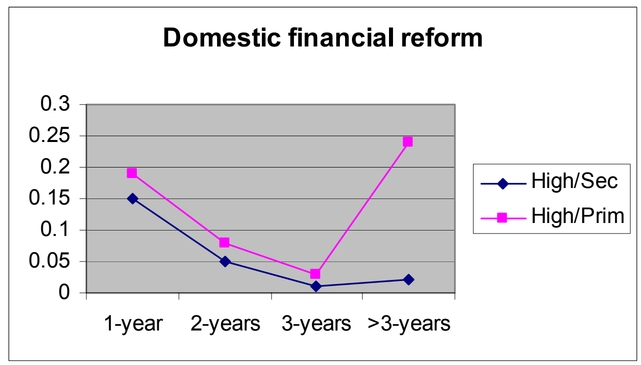

Figure 7e

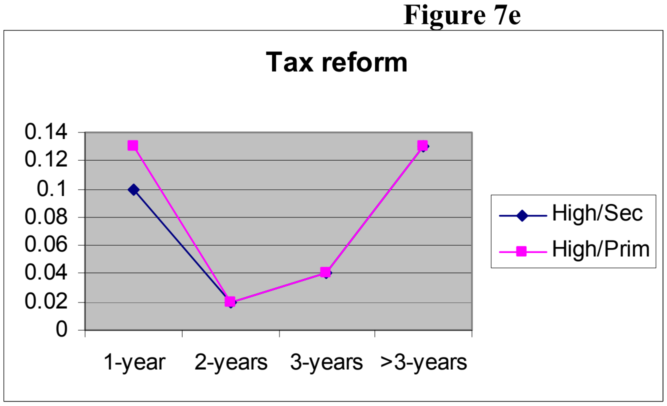

Table 1

Country Trends for Wage Gaps and Overall Policy Indices and Speed and Pace of Reforms

Country	Higher to Secondary Wage Gap	Higher to Primary Wage Gap	Speed of Individual Reforms: Trade Liberalization	Domestic Financial	Privatization	Capital Account	Tax Reform	Timing & Pace of Reforms*
Paraguay	0.1008	0.1482	High	Medium	Low	Medium	High	Intense
El Salvador	0.0736	0.0942	Low	Medium	Low	High	High	Intense
Colombia	0.0181	0.0661	Low	High	Low	High	Medium	Gradual
Mexico	0.0152	0.0287	Low	Medium	Medium	Low	Medium	Slow
Ecuador	0.0177	0.0283	Medium	Medium	Low	High	Low	Slow
Chile	0.0146	0.0241	Medium	Low	High	Medium	High	Early
Nicaragua	0.0561	0.0209	High	High	High	High	Medium	Intense
Costa Rica	0.0198	0.0198	High	Low	Low	Medium	High	Slow
Peru	0.0061	0.0170	Low	Medium	Medium	High	High	Intense
Uruguay	0.0074	0.0129	High	Low	Medium	Low	Medium	Gradual
Panama	0.0103	0.0082	High	Low	High	Low	Low	Slow
Bolivia	0.0097	0.0065	Low	High	High	Low	Low	Intense
Brazil	0.0093	0.0012	Medium	High	Medium	Low	Low	Slow
Argentina	0.0064	-0.0009	Medium	High	High	Medium	Low	Early
Venezuela	0.0102	-0.0017	Medium	Low	Medium	Low	Medium	Slow
Honduras	0.0155	-0.0134	Low	Low	Low	Medium	Low	Slow
Average all LAC	0.0244	0.0288	0.0303	0.0265	0.0051	0.0117	0.0131	
Correlation with Higher-Primary Wage Gap			-0.1236	0.3435	-0.2263	0.3900	0.4919	

Source: Authors' calculations from household survey data.
*Taken from Table 4 in IDB (1997), pg. 50.

Table 2

Wage Differentials and Average Policy Index

Independent Variable	One-Year Lag Estimation in Difference	One-Year Lag Fixed Effects	One-Year Lag Random Effects
Dependent variable: higher/secondary			
Average Policy index (t-1)	0.42	0.26	0.20
	2.72	2.21	2.15
Year Trend	0.001	0.007	0.010
	0.24	1.44	1.89
Constant	-2.4	-13.9	-15.3
	-0.24	-1.4	1.84
R-sq. Overall	0.0131	0.091	0.091
Number of Observations	265	355	355
No. Household Surveys	53	71	71
Avg Obs. per country	3.3	3.9	3.9
Wald chi2(1)	13.05	13.17	26.7
Prob > chi2	0	0	0
Dependent variable: higher/primary			
Average Policy index (t-1)	0.47	0.29	0.24
	2.49	2.54	3.31
Year Trend	0.0007	0.0101	0.0100
	0.14	1.85	1.45
Constant	-1.4	-19.0	-12.2
	-0.13	-1.77	1.36
R-sq. Overall	0.014	0.0769	0.076
Number of Observations	265	355	355
No. Household Surveys	53	71	71
Avg Obs. per country	3.3	3.9	3.9
Wald chi2(1)	10.27	10.96	21.8
Prob > chi2	0	0	0

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

Table 3

Wage Differentials and Individual Policy Indices

Independent Variable	Dependent Variable: Higher/Secondary			Dependent Variable: Higher/Primary		
	Estimation in Differences	Fixed Effects	Random Effects	Estimation in Differences	Fixed Effects	Random Effects
Trade index (t-1)	**0.10**	**0.07**	**0.06**	**0.12**	**0.10**	**0.08**
	0.93	0.69	0.24	1.09	1.31	0.80
Financial index (t-1)	**0.08**	**0.10**	**0.12**	**0.14**	**0.28**	**0.33**
	2.82	3.16	3.55	2.28	2.61	3.27
Capital Acct. index (t-1)	**0.14**	**0.23**	**0.25**	**0.18**	**0.28**	**0.30**
	2.54	3.41	4.58	2.03	3.36	2.94
Privatization index (t-1)	**-0.12**	**-0.04**	**-0.02**	**-0.20**	**-0.16**	**-0.14**
	-1.53	-1.48	-0.24	-2.66	-2.84	-3.25
Tax reform index (t-1)	**0.29**	**0.33**	**0.35**	**0.38**	**0.42**	**0.41**
	2.16	2.69	3.98	2.33	2.20	2.00
Year Trend	**0.00**	**0.01**	**0.01**	**0.00**	**0.01**	**0.01**
	-0.35	1.34	1.77	-0.13	1.10	1.21
Constant	**3.81**	**-14.80**	**-15.13**	**1.53**	**-24.87**	**-15.53**
	0.35	-1.31	-1.71	0.14	-2.04	-1.62
R-sq. Overall	0.073	0.099	0.078	0.030	0.087	0.060
Number of Observations	265	355	355	265	355	355
No. Household Surveys	53	71	71	53	71	71
Avg Obs. per country	3.3	3.9	3.9	3.3	3.9	3.9
Wald chi2(1)	4.2	4.7	35.1	3.9	4.1	30.5
Prob > chi2	0.003	0.000	0.000	0.007	0.001	0.000

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

Table 4

Wage Differentials, Av erage Policy Index, Trade Flows and Technological Exports
Including Interaction Terms

Independent Variable	Dependent Variable: Higher/Secondary			Dependent Variable: Higher/Primary		
	Estimation in Differences	Fixed Effects	Random Effects	Estimation in Differences	Fixed Effects	Random Effects
Average Policy index (t-1)	**0.30**	**0.18**	**0.15**	**0.32**	**0.36**	**0.34**
	2.71	2.35	2.19	2.29	2.88	2.59
Trade flows as (%) GDP (t-1) (thousands)	**-3.97**	**-5.61**	**-4.45**	**-2.46**	**-3.89**	**-1.87**
	-0.71	-1.70	-1.57	-0.43	-1.10	-0.60
Policy Index*Trade flows/GDP (t-1)	**-0.03**	**0.05**	**0.04**	**-0.01**	**0.03**	**0.02**
	-0.40	1.18	1.14	-0.10	0.68	0.43
Technological exports as % of GDP (t-1)	**15.53**	**14.75**	**13.99**	**14.42**	**38.38**	**32.92**
	1.74	2.67	2.00	3.35	3.90	2.69
Policy Index*Tech. exports/GDP (t-1)	**21.80**	**19.39**	**16.78**	**30.21**	**52.00**	**52.55**
	3.79	2.93	2.06	2.47	4.33	2.47
Time Trend	**0.00**	**0.01**	**0.01**	**-0.02**	**0.01**	**0.00**
	0.25	1.49	1.48	-0.27	1.36	0.94
Constant	**-2.79**	**-15.32**	**-12.09**	**-3.14**	**-18.03**	**-7.83**
	-0.25	-1.42	-1.40	-0.27	-1.56	-0.84
R-sq. Overall	0.036	0.110	0.107	0.029	0.113	0.098
Number of Observations	265	265	265	265	265	265
No. Household Surveys	53	53	53	53	53	53
Avg Obs. per country	3.3	3.3	3.3	3.3	3.3	3.3
Wald chi2(1)	3.6	5.3	33.4	2.7	5.5	23.8
Prob > chi2	0.003	0.000	0.000	0.008	0.000	0.001

Source: Authors' calculations. 't' and 'z' statistics are presented below each coefficient.

Table 5

Wage Differentials and Overall Policy Change

Independent Variable	Dependent Variable: Higher/Secondary Differential			Dependent Variable: Higher/Primary Differential		
	Estimation in Difference	Fixed Effects	Random Effects	Estimation in Difference	Fixed Effects	Random Effects
Policy Change (t-1)	**0.23**	**0.35**	**0.32**	**0.28**	**0.37**	**0.34**
	2.41	3.43	2.29	2.21	2.87	3.39
Policy Change (t-2)	**0.09**	**0.16**	**0.19**	**0.16**	**0.25**	**0.19**
	2.36	2.32	2.79	2.33	2.34	2.73
Policy Change (t-3)	**0.06**	**0.10**	**0.10**	**0.11**	**0.19**	**0.17**
	2.04	2.30	2.66	2.41	2.37	2.21
Policy Change (t-4)	**0.06**	**0.09**	**0.07**	**0.07**	**0.12**	**0.11**
	2.00	3.37	2.45	1.97	2.34	2.49
Policy Change (t-5)	**0.04**	**0.08**	**0.07**	**0.09**	**0.09**	**0.08**
	1.97	1.90	1.61	1.87	1.75	1.19
Policy Change (t-6)	**0.03**	**0.04**	**0.05**	**0.04**	**0.06**	**0.02**
	2.44	1.97	1.98	0.93	1.24	1.43
Policy Change (t-7)	**0.01**	**0.03**	**0.01**	**0.03**	**0.04**	**0.02**
	0.37	1.56	1.45	0.80	0.36	0.63
Average Policy Index (t-8)	**0.04**	**0.10**	**0.10**	**0.07**	**0.09**	**0.06**
	1.28	0.28	0.95	2.56	2.24	2.81
Year Trend	**0.00**	**0.01**	**0.01**	**0.00**	**0.01**	**0.00**
	-0.33	1.24	1.38	-0.45	1.34	0.26
Constant	**3.75**	**-17.06**	**-13.57**	**5.28**	**-19.42**	**-2.13**
	0.33	-1.21	-1.34	0.45	-1.29	-0.20
R-sq. Overall	0.048	0.108	0.103	0.077	0.097	0.076
Number of Observations	260	350	350	260	350	350
No. Household Surveys	52	70	70	52	70	70
Avg Obs. per country	3.2	3.9	3.9	3.2	3.9	3.9
Wald chi2(1)	13.7	3.39	29.02	15.790	2.980	28.820
Prob > chi2	0.006	0.001	0.001	0.001	0.002	0.001

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

Table 6

Wage Differentials and Overall Policy Change
(All regressions are Estimated in First Differences)

Independent Variable	Individual Index Entered Separately				
	Trade Reform	Privatization	Capital Account Lib.	Domestic Financial Reform	Tax Reform
	Dependent Variable: Higher/Secondary				
Policy Change (t-1)	**0.09**	**-0.10**	**0.11**	**0.15**	**0.10**
	0.19	-2.60	3.35	2.39	2.11
Policy Change (t-2)	**0.07**	**-0.04**	**0.09**	**0.05**	**0.02**
	0.17	-2.04	3.76	3.16	1.98
Policy Change (t-3)	**0.05**	**-0.01**	**0.06**	**0.01**	**0.04**
	0.88	-1.87	2.65	0.72	1.32
Prior Policy Level (t-4)	**0.09**	**-0.12**	**0.13**	**0.02**	**0.13**
	0.84	-1.30	1.82	0.10	0.94
Average of other Policy Changes (t-1)	**0.16**	**0.40**	**0.14**	**0.23**	**0.33**
	2.35	2.06	1.72	2.55	2.71
Average of other Policy Changes (t-2)	**0.14**	**0.18**	**0.06**	**0.09**	**0.16**
	2.61	2.09	1.80	2.01	2.69
Average of other Policy Changes (t-3)	**0.08**	**0.19**	**0.23**	**0.05**	**0.10**
	2.74	1.95	0.88	1.43	1.24
Prior Level of Other Policies (t-4)	**0.35**	**0.02**	**0.22**	**0.18**	**0.11**
	2.52	1.96	1.09	1.83	2.53
Year Trend	**0.01**	**0.00**	**0.00**	**0.00**	**0.00**
	1.00	0.49	0.70	0.27	0.46
Constant	**-11.93**	**-5.70**	**-8.34**	**-3.07**	**-9.05**
	-0.97	-0.46	-0.67	-0.24	-0.44
R-sq. Overall	0.111	0.100	0.099	0.114	0.125
Number of Observations	355	355	355	355	355
No. Household Surveys	71	71	71	71	71
Avg Obs. per country	3.9	3.9	3.9	3.9	3.9
Wald chi2(1)	3.6	3.1	3.1	3.7	2.8
Prob > chi2	0.000	0.001	0.001	0.000	0.004
	Dependent Variable: Higher/Primary				
Policy Change (t-1)	**0.15**	**-0.14**	**0.16**	**0.19**	**0.13**
	0.61	-3.23	2.97	2.96	2.22
Policy Change (t-2)	**0.08**	**-0.12**	**0.11**	**0.08**	**0.02**
	1.52	-2.26	2.10	2.36	2.03
Policy Change (t-3)	**0.03**	**-0.08**	**0.07**	**0.03**	**0.04**
	1.51	2.03	2.70	2.16	2.79
Prior Policy Level (t-4)	**0.07**	**-0.09**	**0.19**	**0.24**	**0.13**
	0.61	0.88	2.02	1.14	0.83
Average of other Policy Changes (t-1)	**0.26**	**0.18**	**0.15**	**0.22**	**0.33**
	2.50	2.71	2.80	2.40	2.52
Average of other Policy Changes (t-2)	**0.14**	**0.08**	**0.15**	**0.10**	**0.16**
	2.31	2.09	1.94	2.07	2.06
Average of other Policy Changes (t-3)	**0.03**	**0.04**	**0.04**	**0.06**	**0.10**
	2.92	2.45	1.95	1.04	1.96
Prior Level of Other Policies (t-4)	**0.21**	**0.02**	**0.07**	**0.18**	**0.11**
	2.84	2.14	1.65	2.12	1.63
Year Trend	**0.01**	**0.01**	**0.01**	**0.01**	**0.02**
	1.07	0.99	1.11	1.43	1.52
Constant	**-26.49**	**-12.45**	**-27.48**	**-20.38**	**-30.67**
	-2.01	-0.93	-2.06	-1.48	-1.48
R-sq. Overall	0.118	0.113	0.106	0.115	0.142
Number of Observations	355	355	355	355	355
No. Household Surveys	71	71	71	71	71
Avg Obs. per country	3.9	3.9	3.9	3.9	3.9
Wald chi2(1)	3.8	3.6	3.4	3.7	3.2
Prob > chi2	0.000	0.000	0.001	0.000	0.001

Source: Authors' calculations. 't' Statistics are presented below each coefficient.

Table 7

Wage Differentials, Labor Market Policy Change and Trade Flows
(All regressions are estimated in first differences)

Independent Variable	Independent Variable: Labor Market Index — Dependent Variable: Higher-Secondary Wage Gap	Labor Market Index — Higher-Primary Wage Gap	Trade Flows/GDP — Dependent Variable: Higher-Secondary Wage Gap	Trade Flows/GDP — Higher-Primary Wage Gap
Policy Change (t-1)	**0.18**	**0.21**	**0.002**	**-0.003**
	1.62	2.56	0.43	-0.60
Policy Change (t-2)	**0.11**	**0.10**	**0.000**	**0.001**
	1.53	1.85	-0.12	0.40
Policy Change (t-3)	**0.03**	**0.07**	**-0.003**	**-0.003**
	0.42	1.28	-0.88	-0.74
Prior Policy Level (t-4)	**0.04**	**0.01**	**-0.001**	**-0.001**
	0.25	0.73	-1.35	-0.67
Average of other Policy Changes (t-1)	**0.54**	**0.53**	**0.31**	**0.36**
	2.71	2.11	2.31	2.13
Average of other Policy Changes (t-2)	**0.15**	**0.28**	**0.19**	**0.21**
	2.64	1.90	2.43	3.43
Average of other Policy Changes (t-3)	**0.06**	**0.10**	**0.05**	**0.05**
	2.24	1.96	2.41	1.83
Prior Level of other policies (t-4)	**0.07**	**0.14**	**0.08**	**0.15**
	2.31	2.18	3.07	2.63
Time Trend	**0.00**	**0.02**	**0.01**	**0.01**
	-0.24	1.28	0.85	2.25
Constant	**8.51**	**-47.23**	**-16.84**	**-21.87**
	0.24	-1.28	-1.81	-2.17
R-sq. Overall	0.080	0.277	0.106	0.096
Number of Observations	210	210	355	355
No. Household Surveys	42	42	71	71
Avg Obs. per country	2.5	2.5	3.9	3.9
Wald chi2(1)	3.1	4.9	3.4	3.0
Prob > chi2	0.001	0.000	0.001	0.002

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

Table 8

Wage Differentials and Average Policy Change Index, with Macro Variables as Controls
(All regressions are Estimated in First Differences)

Independent Variable	*Dep. Variable: Higher/Secondary Gap* Including GDP Growth (1)	Including Growth & Unempl. (2)	Including Growth & Unempl. & Exchg. Rate (3)	*Dep. Variable: Higher/Primary Gap* Including GDP Growth (1a)	Including Growth & Unempl. (2a)	Including Growth & Unempl. & Exchg. Rate (3a)
Policy Change (t-1)	**0.32**	**0.26**	**0.27**	**0.36**	**0.27**	**0.29**
	3.48	3.75	4.59	3.13	3.33	3.81
Policy Change (t-2)	**0.11**	**0.10**	**0.12**	**0.11**	**0.11**	**0.10**
	2.40	2.39	2.21	1.97	1.99	2.70
Policy Change (t-3)	**0.08**	**0.09**	**0.08**	**0.12**	**0.14**	**0.12**
	2.23	2.28	2.42	2.08	2.14	1.28
Policy Change(t-4)	**0.06**	**0.07**	**0.09**	**0.11**	**0.07**	**0.06**
	3.43	3.50	3.73	2.05	1.97	1.62
Policy Change (t-5)	**0.04**	**0.06**	**0.04**	**0.05**	**0.06**	**0.06**
	1.81	1.85	2.11	1.39	1.44	1.36
Policy Change (t-6)	**0.06**	**0.06**	**0.04**	**0.03**	**0.03**	**0.05**
	2.00	2.02	1.87	1.42	1.44	1.27
Policy Change (t-7)	**0.04**	**0.05**	**0.04**	**0.03**	**0.02**	**0.07**
	1.54	1.41	1.69	0.30	0.15	0.32
Prior Policy Level (t-8)	**0.11**	**0.13**	**0.13**	**0.11**	**0.14**	**0.10**
	0.30	0.37	0.43	2.30	2.37	1.99
GDP per capita growth (x100)	**0.03**	**0.04**	**-0.02**	**0.13**	**0.14**	**0.04**
	0.76	0.96	-0.34	1.08	1.11	0.18
Unemployment (x100)		**0.38**	**0.94**		**0.42**	**1.03**
		0.78	1.27		0.83	1.26
Real exchange rate index (x100)			**0.12**			**0.18**
			0.93			1.24
Year Trend	**0.01**	**0.01**	**0.01**	**0.01**	**0.01**	**0.02**
	1.15	1.04	0.96	1.34	0.88	1.32
Constant	**-15.97**	**-14.56**	**-16.93**	**-19.42**	**-12.95**	**-18.80**
	-1.13	-1.02	-0.96	-1.29	-0.86	-1.33
R-sq. Overall	0.110	0.113	0.140	0.133	0.135	0.170
Number of Observations	350	350	350	350	350	350
No. Household Surveys	70	70	70	70	70	70
Avg Obs. per country	3.9	3.9	3.9	3.9	3.9	3.9
Wald chi2(1)	3.1	2.87	1.95	3.83	3.54	2.44
Prob > chi2	0.001	0.001	0.034	0.000	0.000	0.007

Source: Authors' estimates.

Table 9

Wage Differentials and Average Policy Change with Different Estimation Strategies and Samples

(All regressions are estimated in first differences)

Independent Variable	*Instrumental Variables*		*Using Population Weights*		*Regressions Excluding One Country at the time*				*Excluding Self-Employed from Sample*	
					Higher-Secondary		Higher-Primary			
	High-Sec. As Dep. Variable	High-Prim. As Dep. Variable	High-Sec. As Dep. Variable	High-Prim. As Dep. Variable	Average Value	Standard Deviation	Average Value	Standard Deviation	High-Sec. As Dep. Variable	High-Prim. As Dep. Variable
Policy Change (t-1)	**0.38**	**0.41**	**0.28**	**0.29**	**0.38**	**-0.002**	**0.44**	**0.007**	**0.41**	**0.43**
	2.06	2.54	1.98	2.40	3.43	0.00	4.82	0.01	3.35	2.40
Policy Change(t-2)	**0.18**	**0.20**	**0.14**	**0.17**	**0.17**	**-0.001**	**0.19**	**-0.015**	**0.26**	**0.25**
	2.30	2.08	2.50	2.56	2.71	0.00	3.10	-0.03	1.93	2.62
Policy Change (t-3)	**0.08**	**0.13**	**0.05**	**0.08**	**0.10**	**0.001**	**0.16**	**0.006**	**0.11**	**0.18**
	1.83	1.83	1.75	2.34	2.28	0.00	3.31	0.02	2.31	2.39
Policy Change (t-4)	**0.08**	**0.07**	**0.04**	**0.06**	**0.08**	**0.006**	**0.09**	**0.000**	**0.13**	**0.10**
	1.67	2.42	1.70	1.89	3.31	-0.01	2.07	0.00	2.81	2.41
Policy Change (t-5)	**0.04**	**0.06**	**0.02**	**0.06**	**0.05**	**0.003**	**0.04**	**0.012**	**0.08**	**0.10**
	1.98	1.96	1.63	1.78	1.88	-0.03	1.73	0.03	2.17	1.77
Policy Change (t-6)	**0.05**	**0.02**	**0.02**	**0.03**	**0.04**	**-0.011**	**0.03**	**0.001**	**0.04**	**0.07**
	1.59	1.03	1.00	1.45	1.90	0.00	1.88	0.01	1.87	1.78
Policy Change (t-7)	**0.02**	**0.03**	**0.00**	**0.01**	**0.02**	**0.001**	**0.02**	**0.008**	**0.03**	**0.04**
	1.08	1.33	0.97	0.56	1.56	0.00	1.55	0.02	1.60	0.85
Prior Policy Level (t-8)	**0.12**	**0.03**	**0.14**	**0.09**	**0.10**	**0.001**	**0.08**	**0.007**	**0.06**	**0.07**
	0.73	0.16	1.89	2.35	1.34	0.00	1.39	0.02	1.77	2.47
Year Trend	**0.01**	**0.01**	**0.02**	**0.01**	**0.01**	**0.000**	**0.01**	**0.000**	**0.01**	**0.01**
	1.40	0.93	1.34	0.98	1.22	0.00	1.32	-0.01	1.58	1.12
Constant	**-24.01**	**-18.93**	**-40.74**	**44.90**	**-17.26**	**-0.641**	**-19.92**	**0.366**	**-23.11**	**-19.56**
	-2.31	-1.73	3.82	3.02	-1.19	0.02	-1.27	0.01	-1.55	-1.08
R-sq. Overall	0.105	0.129	0.555	0.693	0.110	0.000	0.101	0.000	0.114	0.097
Number of Observations	350	350	350	350	331	-0.253	331	-0.447	350	350
No. Household Surveys	70	70	70	70	66	66	66	66	70	70
Avg Obs. per country	3.9	3.9	3.9	3.9	3.7	3.7	3.7	3.7	3.9	3.9
Wald chi2(1)	2.87	3.64	2.89	3.23	3.27	0.00	2.96	0.00	3.59	3.010
Prob > chi2	0.003	0.000	0.002	0.001	0.002	0.000	0.003	0.000	0.000	0.002

Source: Auhtors' estimates.

Appendix

Table A1

Household Surveys

Country	# Surveys	Years	Survey
Argentina	2	1980, 96	Encuesta Permanente de Hogares
Bolivia	5	1986	Encuesta Permanente de Hogares
		1990, 93, 95	Encuesta Integrada de Hogares
		1996	Encuesta Nacional de Hogares
Brazil	8	1981, 83, 86, 88	Pesquisa Nacional por Amostra de Domicilios
		1992, 93, 95, 96	Pesquisa Nacional por Amostra de Domicilios
Chile	5	1987, 90, 92, 94, 96	Encuesta de Caracterización Socioeconómica Nacional
Colombia	4	1991, 93, 95, 97	Encuesta Nacional de Hogares - Fuerza de Trabajo
Costa Rica	9	1981, 83, 85	Encuesta Nacional de Hogares - Empleo y Desempleo
		1987, 89, 91, 93, 95, 97	Encuesta de Hogares de Propósitos Múltiples
Dominican Republic	2	1996	Encuesta Nacional de Hogares
		1998	Encuesta Nacional Sobre Gastos e Ingresos de los Hogares
Ecuador	2	1995, 98	Encuesta de Condiciones de Vida
El Salvador	2	1995, 97	Encuesta de Hogares de Propósitos Múltiples
Guatemala	1	1998	Encuesta Naional de Ingresos y Gastos Familiares
Honduras	3	1989, 92, 96,	Encuesta Permanente de Hogares de Propósitos Múltiples
Mexico	6	1977, 84, 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	3	1991, 95, 97	Encuesta Continua de Hogares
Paraguay	2	1995	Encuesta Nacional de Hogares y Empleo
		1998	Encuesta Integrada de Hogares
Peru	4	1985, 91, 94, 97	Encuesta Nacional de Hogares sobre Medición de Niveles de Vida
Uruguay	5	1981, 89	Encuesta Nacional de Hogares
		1992, 95, 97	Encuesta Continua de Hogares
Venezuela	6	1981, 86, 89, 93, 95, 97	Encuesta de Hogares por Muestra

Appendix Table A2

Sample Sizes from Household Surveys

Country	No. Surveys	Average Sample Size	St. Deviation Sample Size	Minimum Sample Size	Maximum Sample Size
Argentina	2	8,875	9,939	1,847	15,903
Bolivia	5	2,760	772	1,596	3,628
Brazil	8	38,144	7,182	28,382	49,548
Chile	5	12,928	3,191	9,373	17,039
Costa Rica	9	1,805	165	1,613	2,071
Colombia	4	12,179	1,327	11,160	14,087
Dominican Republic	2	1,855	260	1,671	2,038
Ecuador	2	1,917	36	1,891	1,942
El Salvador	2	2,465	64	2,420	2,510
Guatemala	1	2,884	0	2,884	2,884
Honduras	3	1,224	352	1,341	1,995
Mexico	6	4,011	1,626	1,696	6,634
Paraguay	2	1,392	134	1,297	1,486
Panama	3	2,727	455	2,201	2,993
Peru	4	1,500	302	1,093	1,805
Nicaragua	2	1,345	78	1,290	1,400
Uruguay	5	6,315	2,600	4,168	9,248
Venezuela	6	29,302	20,340	10,529	65,493
Average	72	7,424	2,873	4,803	11,261

Source: Calculations from household surveys in Appendix Table A1.

Appendix Table A3

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

Country	Employed Urban Males 30-55 as share of Total Employment	Employed Urban Males 30-55 as share of Urban Employment	Employed Urban Males 30-55 as share of Male Employment	Labor force participation Urb Males 30-55	Unemployment Rate of Urb Males 30-55	Wages of Urban Males 30-55 as share of All wages	Wages of Urban Males 30-55 as share of Urban Wages	Wages of Urban Males 30-55 as share of 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.

Table A4

Summary Statistics for Log Wage Differentials

Variable	No. Obs.	Mean	Std. Dev.	Min	Max
		Higher/Secondary log Wage Differential			
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
		Higher/Primary log Wage Differential			
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 A5

Correlation Coefficients of Policy Indices

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

Source: Calculated from the original policy

Table A6

Correlations of Policy Indexes and Macro Variables

Variable	Average Policy Level	Capital Account Index	Trade Policy Index	Financial Market Policy	Privatization Index	Tax Policy 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. policy indices.

Table A7

Augmented Dickey-Fuller Unit Root Test
(ADF Test Statistics)

Country	Policy Index					
	Average Index	Trade Liberalization	Capital Account	Financial Market	Privatization	Tax Reform
Argentina	-2.95	-2.58	-2.55	-2.93	-1.62	-2.41
Bolivia	-1.16	-3.14	-2.64	-1.47	-1.59	-2.13
Brazil	-1.39	-1.58	1.75	-1.98	-2.41	-2.22
Chile	-2.77	-2.77	-1.97	-2.22	-1.58	-1.77
Colombia	-2.88	-1.97	-0.29	-1.89	-1.16	-2.34
Costa Rica	-1.70	-1.90	-2.60	-2.03	-1.01	-1.84
Dominican Republic	0.64	0.26	-2.10	-0.90	-2.71	-0.58
Ecuador	-2.09	-0.17	-4.19	-0.54	-2.22	-1.83
El Salvador	-0.74	-2.88	-1.40	-1.38	-3.03	-0.20
Guatemala	-1.30	-1.93	-2.18	-1.24	0.21	-2.08
Honduras	-2.14	-2.82	1.88	-1.03	-2.27	-1.59
Mexico	-1.87	-2.32	-1.14	-1.92	-1.35	-2.31
Nicaragua	-1.30	-1.93	-2.18	-1.24	0.21	-2.08
Panama	-1.46	-2.24	-2.98	-1.44	-2.98	-1.35
Paraguay	-0.95	-1.46	-2.85	-1.44	-1.62	-1.87
Peru	-1.06	-2.51	-1.15	-0.99	-1.60	-2.26
Uruguay	-1.26	-1.75	-5.43	-1.68	-2.39	-1.90
Venezuela	0.91	-2.36	-0.31	-1.64	-2.97	0.00

Source: Authors'computations using the policy indices by Lora (1997) and Morley (1999).
1% Critical Value*-4.4415
5% Critical Value -3.6330
10% Critical Value -3.2535

Table A8

Summary Statistics for the Policy Indexes

Variable	No. Obs.	Mean	Std. Dev.	Min	Max
Capital Account Policy	464	0.67	0.24	0.16	1.58
Trade Policy	463	0.69	0.21	0	0.99
Financial Market Policy	464	0.52	0.3	0	1
Privatization	464	0.7	0.24	0	1
Tax Policy	462	0.36	0.19	0.03	0.78
Labor	286	0.66	0.2	0.29	0.99
Total Index	461	0.59	0.15	0.18	0.89

Source: Authors' calculation using the policy indices by Lora (1997) and Morley (1999)..