Working Paper Number 29
April 2003 (updated July 2006)

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Keywords: reform, inequality, wages, trade, distribution, schooling returns, Latin America, liberalization.
JEL Classification: D31, J31.

4 July 2006

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Introduction

Growing wage differentials between less-schooled and more-schooled workers are evident in a number of emerging markets, including Latin America.[2] 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 shifted to emphasizing major changes taking place on the demand side, due mainly to economic restructuring and opening to international markets undertaken by most countries. Many analysts and policymakers had assumed that these policy reform 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]

This paper assesses the effects of various economic policy changes on wage differentials in Latin America during the last two decades. We focus on the set of market-oriented policy changes or reforms that often are labeled the Washington Consensus – trade and financial sector liberalization, privatization, opening of capital markets, reduction of high-income tax rates in favor of broad-based taxes on consumption, and deregulation of labor markets. This set of six policy changes has been widely implemented throughout the region over the last three decades, though at different times and with different degrees of intensity from one country to another. Their aim has been to foster efficiency in the allocation of resources through the elimination of distortions and governmental regulations and interventions in order to attain higher growth and better-functioning economies.

Our objective is to investigate whether these policy changes have had immediate and/or lasting effects on relative wages in Latin America. If these policy changes have increased wage differentials, then income inequality has increased (or decreased less than it might have) because the distribution of labor income primarily governs the overall distribution of income in the region (Székely and Hilgert 2002). The question is important because of long-standing concerns about high inequality in the region and the suspicion that these policy changes have contributed to that inequality.[5]

To undertake this assessment we use a rich new data set and a new estimation strategy for the study of wage differentials related to schooling levels. Our data set includes comparable information on urban wages and education for 18 Latin American countries over the period 1977-1998, which we

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2. 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 (2004) 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 Ffrench-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).

5 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.

compute directly from 71 household surveys, merged with annual country-specific indices of the intensity of the six sets of 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.[6] Previous studies of effects of policy changes on wage differentials generally have had to focus on specific industries or small regions within a country or on one country, thereby having but limited variation in aggregate policy changes and in responses to those policy changes for the analyses. Studies focusing only on specific industries also are likely to miss an important part of the picture because policy changes may trigger resource reallocations throughout the economy. 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 estimation strategy allows us to estimate the differential impact of 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 effects of the policy changes or of the factors that determine the policy changes.[7]

The paper is divided into five sections. Section 1 presents the data and provides evidence on the evolution of wage differentials and of policy changes in Latin America. Section 2 discusses our expectations from theory for the effects of policy changes on differential schooling returns, and then turns to estimation issues. Section 3 presents our basic empirical results. Section 4 reports on a set of robustness tests for our main results. Section 5 presents our conclusions.

[6] To our knowledge, this panel data set is the most comprehensive on wage differentials for Latin America. 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 policy 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 "noise" due to 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 may be very different from the overall effects (see text).

[7] Somewhat related strategies date at least back to Katz and Murphy (1992), who focus on wage differentials in the U.S. 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 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.

1. Data Construction and Patterns in the Data

We need: (a) data that characterizes wages by schooling levels over time and (b) indicators that summarize the nature of changes in policies in each country over time. Such data requirements are considerable, and up to now there has been limited analysis of the type we perform here, at least concerning the evolution of wage differentials with regard to schooling levels. This section describes our data set and provides some background about the evolution of the critical data.

1.1 Data on Wages

Data Sources: The best sources of information on wages at the individual level are employment surveys and household surveys. For Latin America, household surveys are a better source for this study than are employment surveys. First, employment surveys in many countries in Latin America only cover major cities in each country rather than all urban areas.[8] Second, employment surveys tend to oversample industrial sectors and the formal economy, while household surveys represent all sectors of activity including both formal and informal employment. Third, 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. If 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 wages, profits, rent, etc. Household surveys normally do ask explicitly for a detailed breakdown of income by source.[9]

[8] Some countries such as Peru or Colombia recently introduced employment surveys with complete national 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.

[9] To illustrate 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 apparent relevant difference between the two questionnaires is that the household survey includes much more detailed disaggregation of income sources.

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.[10] Second, the survey questionnaire has to include a breakdown of income by source, with at least three questions on income that identify labor income, profits, and capital rents separately. Third, the recall period for incomes has to be the previous month.[11] 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 have been able to access 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.

Previous work on this topic has been plagued by comparability problems (e.g., note 6). To assure comparability of our data we identify the following specific item in 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 hours worked 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. All urban areas are included, which is, as already noted, a more extensive coverage than the major cities to which are limited some of the employment surveys. 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. 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

[10] Most of the surveys we include 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.

[11] 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.

are over 1,000 individuals, and sample sizes are fairly 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 (columns 1, 2 and 3 in Appendix Table A3).[12] 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 so that changes observed in wage differentials are due primarily to changes in labor demands induced by the policy changes, and not to changes in labor force participation decisions. 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 policy changes. The restriction to urban areas is justified because data quality on labor incomes is higher for urban than for rural areas, partly 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 (largely implicit) 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 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 (IDB 1999).

<u>Characterization of Wage Differentials</u>: We characterize the changes in wage differentials over time in two 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 grades of schooling, potential work experience (age minus six minus

[12] 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.

years of schooling) and potential work experience squared.[13] The estimated coefficients for the dummy variables are normally interpreted as the returns to schooling.[14]

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.[15] The generally positive slope for the linear return in Figure 1 reflects that the return to an extra grade 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.[16]

Our second 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 (defined to be some post-secondary schooling) and primary schooling. As can be seen in the next-to-last line, the average change has been slightly greater for the

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[13] 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.

[14] This interpretation is only correct under certain conditions (e.g., Willis 1986). One of the problems with the standard interpretation is that schooling and ability (as well as other factors often not observed, such as motivation, parents' connections, and so on) are highly correlated, and it is difficult to disentangle the effect of each of these elements (e.g., Behrman and Rosenzweig 1999, 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.

[15]Specifically, to smooth out the profiles we first estimate the ln wage regression for each household survey and then put together a panel for each of the three coefficients that represents 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 earlier household surveys. Countries with only one observation are excluded from this estimation.

T-tests for the estimates underlying Figure 1 indicate that the overall returns to schooling are significantly higher at the 10% level in 1995 and 1996 than in 1990 and at the 5% level in 1997 and 1998 than in 1990. Underlying these aggregate estimates are no significant differences in the returns to primary schooling over the 1990s compared with 1990 and no significant differences in the returns to secondary schooling over 1991-96 compared with 1990, followed by significant declines at the 5% level in 1997 and 1998 relative to 1990. In sharp contrast, there are significant increases in the returns to tertiary schooling for 1992-1998 (at the 10% level for 1992 and the 5% level for other years). See Behrman, Birdsall and Székely (2005, Appendix Table A4).

[16] Attanasio and Székely (2002) present a detailed account of the evolution of returns to schooling by country.

higher-primary wage gap than for the higher-secondary gap. In this table we also provide information on the speed of the individual reforms, reflecting the judgment summarized in IDB (1997). There is no obvious relationship between the speed of reforms and the size of the ln wage gap; this is not surprising since some countries, such as Mexico in the case of trade liberalization, have had gradual processes that resulted in substantial changes.

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 Policy Changes

To characterize changes in policies, we use four of the five policy indices presented in IDB (1997). Lora (1997) and Lora (2001) performed the background research for the indices for 1985-1995, with an extension of the series for 1970-1984 by Morley *et al.* (1999), which also is the source for the privatization index that we use. 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), which is the basis for the following summary.[17]

These policy indices are based primarily on direct indicators of governmental policies, so that they measure policy efforts rather than responses to them by economic actors or indirect macroeconomic outcomes. 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 private and public sectors. As representations of policies, they are contaminated by responses to the policies.

[17] 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 idea behind each index is to measure the extent to which policies grant space to market forces and eliminate distortions. To avoid any misinterpretation, we emphasize that the relative 'absence' of public interventions or regulations should not be interpreted as a neutral phenomenon (i.e. absence of 'policy'), but on the contrary as a public action aimed at getting certain results, which is one of the main characteristics of what is usually referred to as a 'policy'. In other words, these indices intend to reflect the structural set of governmental 'rules of the game' faced by economic actors, rather than actual responses to those rules. The indices are calculated as follows. The trade policy index is the average of the average level of tariffs and the average dispersion of tariffs, and tries to sum up the level and intensity of controls over international trade faced by foreign and domestic economic actors. 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; this index intends to show the relative presence of governmental controls over domestic financial activity. The index for international financial liberalization averages four components: sector controls of foreign investment, limits on profits and interest repatriation, controls both on external credits by national borrowers and controls on capital outflows. This indicator illustrates the level and evolution of domestic controls and regulations over international financial activity, and especially the degree of freedom granted to cross-border financial flows. 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. It shows the relative weight and neutrality of the tax system over economic activity. 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 relative value of social security contributions paid both by companies and employees as a share of total salary cost.[18]

[18] Some elaboration regarding the particular characteristics of labor markets in Latin America is useful. In the absence of a universal welfare system, formal labor regulations (i.e. associated social benefits and hiring/firing regulations) have acted as partial substitutes for those individuals working in the formal sector, resulting in a very inflexible (formal) labor market that has been said to foster informality. Of course, a significant difference between the formal and the informal labor markets lies in wage levels, since there is usually a 'formal' minimum wage that doesn't exist in the informal sector. However, our index focuses not on the 'formal' minimum wage but instead on the formal labor regulations mentioned because their relative absence or maintenance constitutes a better indicator of the extent to which public policies have (or have not) granted wider space to market forces in the labor market.

Each policy index is the arithmetic average of the specified components.[19] 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, 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. The interpretation of the coefficients below 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 consider..

Figures 2a and 2b 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 averaged across countries. 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 1990-95 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. The highest correlations are found between the index of trade liberalization and the indices of financial market and tax reform (Behrman, Birdsall and Székely 2005, Table A6). But even in these cases the correlations of about 0.6 suggest that the reforms generally varied a fair amount in their pace and intensity over time and across countries.

Table 1 shows country-specific wage gaps, overall policy indices and a summary measure of the speed and pace of reforms for the period 1970-1995. 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

[19] Indices can be sensitive to the weights for 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.

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”.

Perhaps surprisingly, Mexico shows up as a low-speed reformer in terms of trade liberalization, primarily because for most of the period concerned, 1970-1995, Mexico had a closed and protected economy.[20] A fall in foreign trade controls took place in 1985-1986 when Mexico unilaterally joined the eral Agreement on Tariffs and Trade (GATT), and lowered its external tariffs. That opening to foreign trade was then completed and considerably deepened only in 1994-1995 when the North American Free Trade Agreement (NAFTA) came into force.

The correlation coefficients between changes in each policy index and changes in the higher-primary wage gap are given in the last row of the table. The domestic financial policy index, the capital account liberalization policy index and the tax policy index all have fairly strong positive correlations, while the other two indices have negative (and weaker in terms of absolute magnitude) 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 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

[20] In 1978 the Mexican Government undertook a series of internal consultations with national entrepreneurs and labor unions in order to determine if Mexico would benefit from joining the GATT. However, in the face of rising international oil prices, the Government finally decided that the country should first take advantage of its oil producer status in order to foster a quick (and protectionist) industrialization, and only then expose the Mexican economy to foreign competition and trade.

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.

2. Theory and Estimation Issues

2.1 Expected Effects of Policy Changes on Differential Schooling Returns

We allude in our introduction to the debate regarding the effects of trade liberalization on wage differentials. We summarize very briefly here relevant theory about the impacts of the various forms of liberalization that we consider and our resulting expectations of how the policy changes that we have outlined are likely to affect relative returns to schooling and thus wage differentials for different schooling levels.

<u>Trade liberalization</u>: Standard models suggest that the opening of markets in Latin America should increase demands for the region's apparently plentiful factor, unskilled labor, and thus reduce wage differentials between less and more schooled. However, a combination of other factors is likely to offset the standard prediction, so that the total effect is uncertain. Spilimbergo *et al.* (1999) show that unskilled labor is not the region's scarce factor compared to China and Asia in general, and that as a result the region may have a comparative advantage in production that exploits its relatively plentiful land and natural resources. It is also possible that in some countries, relatively unskilled workers benefited from protection prior to liberalization and thus are relatively worse off following liberalization (Hanson and

Harrioson 1999, Goldberg and Pavcnik 2004). And with trade liberalization, the production of intermediate goods that are unskilled-labor intensive in developed countries, shifts to developing countries where they are skilled-labor intensive (Feenstra and Hanson, 2001).

Capital account liberalization: Similarly theory suggests that the opening capital markets may trigger offsetting effects. On one hand, such opening should reduce the cost of capital, encouraging its use and raising investment rates leading to job creation benefiting plentiful unskilled labor. On the other hand, higher investment -- particularly in capital equipment -- may embody technologies that are biased toward more skilled labor, reflecting scarcities in the more advanced economies in which the majority of new technologies are developed. That implies that skilled labor and capital are complementary not only in the advanced economies but in developing countries (Cragg and Epelbaum, 1996).

Domestic financial sector liberalization: Financial sector liberalization in Latin America has generally led to higher borrowing rates and, all else equal, may have reduced domestic investment, particularly in the small and medium enterprise sector. That could reduce demands for unskilled labor relative to skilled labor, particularly if the latter is more likely to be employed in larger firms with better access to dollar borrowings and higher shares of own-financed investment. Over time, however, the liberalized sector should reduce the cost of borrowing for firms across the board.

Privatization's effect in the short run depends on the ratio of skilled to unskilled workers in state enterprises compared to the privatized firms that replace them. We do not know these ratios so we have no priors about the short-run effect. In the longer run, there should be positive spillover effects for labor if privatization leads to expansion of market share and increased employment. However, there is no reason to have strong expectations about how that increased employment will be shared in terms of wage gains between skilled and unskilled labor.

Tax reforms that reduce marginal rates while broadening the tax base have effects that depend on a number of factors that again could be offsetting. Reduced marginal tax rates on capital could increase the demand for skilled labor if skilled labor and capital are complements in production. Tax reform in most Latin American countries has involved shifts from direct income to value-added taxes; the effects on demands for skilled versus unskilled labor depend on the extent to which the consumer prices of goods that use the different types of labor more effectively are affected.

Labor market reform, by reducing the cost of firing workers, should increase job creation and access to employment in the formal sector for unskilled workers more than skilled workers, on the assumption that labor market rigidities have encouraged more capital-intensive production in general, which is more likely to have favored skilled workers.

In summary the signs of the effects of policy changes on wage differentials cannot be predicted unambiguously from theory because of the multiplicity of effects, some working in opposing directions.[21] But we do posit that over time any initial shifts in relative returns to schooling due to the initial “shocks” of policy changes are likely to erode, as the resulting changes in relative prices lead to adjustments in economic entities’ behavior. Thus the trend of increasing liberalization in Latin America across the board should have made the region’s economies in general more flexible and more efficient over the period in question, and we should see erosion of the initial effects of policy changes. An obvious example is the likelihood that increasing returns to higher schooling creates incentives for more schooling, leading eventually to a reduction in the differential returns. However the direction and magnitude of how those effects vary with time are fundamentally empirical questions, to which we turn in Section 3.

2.2. Estimation Issues:

To describe the estimation approach we extend the basic semi-log wage relation to include possible effects of policy changes that may differ by schooling levels, along with possible effects on wages independent of schooling:

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

where P, S, and H are dichotomous variables that refer to the highest completed schooling being primary (P), secondary (S) and higher (H) schooling; C is a vector of policy changes; 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);[22] and ε is a stochastic shock. All of the variables conceptually have subscripts for time and country and the individual variables also could have subscripts for individuals, but these are suppressed to reduce clutter. In this specification the impact of primary schooling on ln wages is $(\alpha_p+\beta_p C)$, the impact of secondary schooling on ln wages is $(\alpha_s + \beta_s C)$, and the impact of higher education on ln wages is $(\alpha_h + \beta_h C)$. Thus, policy changes 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_C), all controlling for individual and country characteristics. Our primary interest is in obtaining estimates of the

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[21] 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 multiple policy changes with general equilibrium feedbacks.

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

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 impacts of other individual characteristics (the coefficient vector δ), impacts of policy changes independent of schooling (the coefficient vector β_C) 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 change 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. While this does not in itself cause biases, it is likely to lead to limited precision for the coefficient estimates of the policy changes 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 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 changes and schooling, the result is unobserved variable bias in the estimated effects of policy changes 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) that may affect both the policy changes themselves and the returns to differing schooling levels..

(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 labor markets.[23] This raises the question for observed countrywide characteristics of how to

[23] 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).

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

We therefore adopt an estimation strategy that reduces all four of these problems but allows the estimation of the relative impact of policy changes on schooling returns in relation (1). We sum relation (1) by averaging it over quinquennia of birth cohorts and by school levels. We aggregate by birth cohorts in order to control for differential time between the marginal schooling/labor force entry decisions and the time of the survey for different birth cohorts.[24] Then we difference relation (1) between pairs of schooling levels for each age group to obtain:

(2a) $$\ln WS - \ln WP = (\alpha_s - \alpha_p) + (\beta_s - \beta_p)C + (\varepsilon_s - \varepsilon_p)$$

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

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

where lnWi (for i = P, S, H) is the average for a birth cohort over a quinquennium 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 quinquennium 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 parameter differences of principal interest, i.e. whether the impact of policy changes differ by schooling levels (i.e., (β_p - β_s), (β_h - β_s), (β_h - β_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 changes 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

[24] This procedure averages out stochastic variations across individuals. If, alternatively, estimates were to be made on the individual level, it would be important to recognize in the estimation that the information about policy changes is at the country-time period level, so that the "cluster" (country) or "design effects" of the sample would have to be incorporated into the estimation as Kish (1965), Moulton (1990), Deaton (1997) and others have emphasized in order to avoid what Moulton refers to in his title as an important "pitfall in estimating the effects of aggregate variables on micro units."

(one for each policy change index plus one for the difference independent of the policy change 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 and the history of country-level factors that determine endogenous policies) 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.

Though we think that our approach has definite advantages for investigating the impact of policy changes on relative wages in Latin America, of course it is not without limitations. Despite the strengths of the policy change indicators, for example, there may be important measurement issues. If the indicators are noisy measures of the underlying concepts, as some certainly are (e.g., privatization), they tend to lead to biases towards zero in our estimates of policy effects. If there are unobserved policy changes that are correlated with the policy change indices that we use, then our policy indices proxy in part for these unobserved policies. If, for example, there were changes in effective minimum wage policies that were correlated with the policy change indicators that we use (that do not include explicitly changes in minimum wages though we are not aware of evidence that minimum wages have been effective in the region, which is not surprising given the large informal sectors), then our estimated coefficients of our observed policy indicators include the correlated impact of the minimum wage changes. This would mean that our estimates are capturing a wider range of policy changes than just the underlying changes used to construct the policy change indicators that we use and that identification of exactly what specific policy changes are relevant would be less confident than were there not any unobserved policy changes that are correlated with our policy change indicators – but that would not seem to have major effects on our interpretation of our general policy change indicators as representing the package of policies associated with policy liberalization.

2.3 Empirical Issues: Our Estimation Strategy

In the estimation of relations (2a-2c) C embodies policy changes, the effects of which may differ with different lags. As mentioned, the lag structures 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 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.

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 in terms of changes in first differences, to avoid any concern with the possibility of zero cointegration.[25]

Another possible concern is that 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

[25] 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.

ith schooling level). If $g_s M_s - g_p M_p$ and C 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 C 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 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 2). Including a time trend increases the probabilities that the coefficient estimates can be interpreted as real correlations 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 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.[26] This is indeed what we find (see Table 2 below for an example).

3. Empirical Results

In Table 2 we present our preferred estimates of equations (2b) and (2c) for overall policy changes. 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 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. Thus, although policy changes have a statistically significant effect on wage differentials with lags of

[26] 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 that do pass this test.

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 time trends to assure that the policy change coefficient estimates are not capturing secular trends in increases in demands for skills or relative supplies of those less schooled. Interestingly, the time trend 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 (results are not presented for brevity).

Table 3 explores 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 rows), and compute the average of the remaining four policy changes and introduce it as a control (presented in rows 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 overall policy changes in Table 2 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.

The first column of Table 3 presents 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 as discussed in Section 2.1.

Increased privatization (second column in Table 3) 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) 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) also has a consistently positive effect in increasing wage gaps, which 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) 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 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 that 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 is the most recent of the reforms initiated in Latin America. In the first two columns of Table 4 we present coefficient estimates for labor market policy changes, which are analogous to those in Table 3, but with the average of the other policy reforms in Table 4 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 allow 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 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.[27] We report the estimates for the specification with this trade variable in the last two columns in Table 4. These are also analogous to the results in Table 3, the only difference being the change in the indicators of trade reform. The use of this alternative indicator does not alter the result -- trade flows do not significantly expand wage differentials.

4. Robustness Tests

In this section we perform a series of robustness tests to check whether our central result -- 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 may raise the concern that the coefficients in Tables 2-4 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 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 5 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

[27] 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. Ffrench-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.

higher/secondary wage gap is used as the dependent variable, while the last three use the higher/primary wage gap.[28]

Columns (1) and (1a) in Table 5 present regressions (analogous to those in the first column of Table 2. As in Table 2, we control for the policy level eight years before, and include a year trend. However, in Table 5 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 in ways that have differential effects on schooling returns. 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 2. 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 5, 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 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 5 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 5 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

The possibility that our estimates are contaminated by contemporaneous high wage gaps creating pressures for reform in a country is reduced considerably by the use of right-side variables that measure

[28] 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.

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-effects regressions and in the differenced estimates. However, to reassure us further that we can interpret our results as identifying causal effects, we present in the first two columns of Table 6 a regression analogous to the differenced estimates in the first column of Table 2, 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 (2) 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 6 presents the regressions using the higher-secondary wage gap as the dependent variable, while the second column uses the higher-primary wage gap. 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 most of 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 6 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 6 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 6. 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 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 6 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 2. The latter result suggests that the effects of policy changes are greater for individuals who are not self-employed.

5. Conclusions

This paper applies a new approach to the estimation of the impact of economy-wide changes in policies on wage differentials using a new data set on wage differentials by schooling levels for 18 Latin American countries for 1977-1998. The wage data are merged with policy indices that characterize the pace of different types of economic liberalization 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 gaps between workers with higher education and those with secondary and primary education have widened considerably, especially in the 1990s. We then explore the relation between policy changes on the one hand and wage differentials across schooling levels on the other, a topic on which very limited prior empirical evidence exists. We find that on average, liberalizing 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.[29] Fourth, we do not know the effects of

[29] 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 in Behrman, Birdsall and Székely (2005). 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

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 sequencing understood, and that more consideration should be given to programs to mitigate their shorter-run effects on inequality.

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the wages of those with low levels of schooling in absolute terms .

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Figure 1. Changes in Marginal Returns to Schooling In Latin America in the 1990s

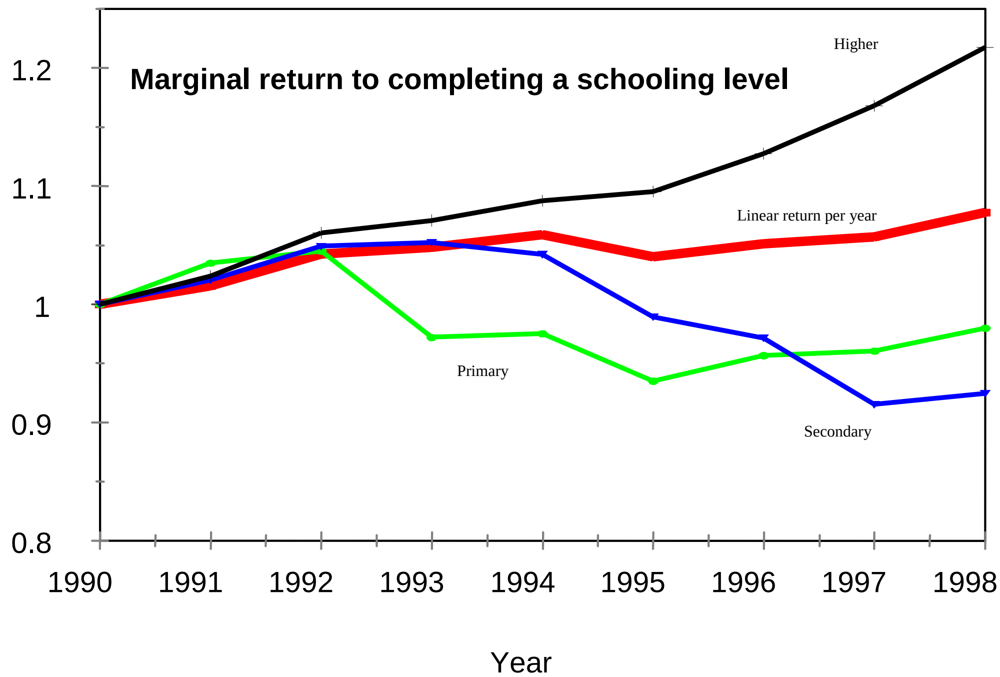

Figure 2A. Average Policies in Latin America, 1970-95

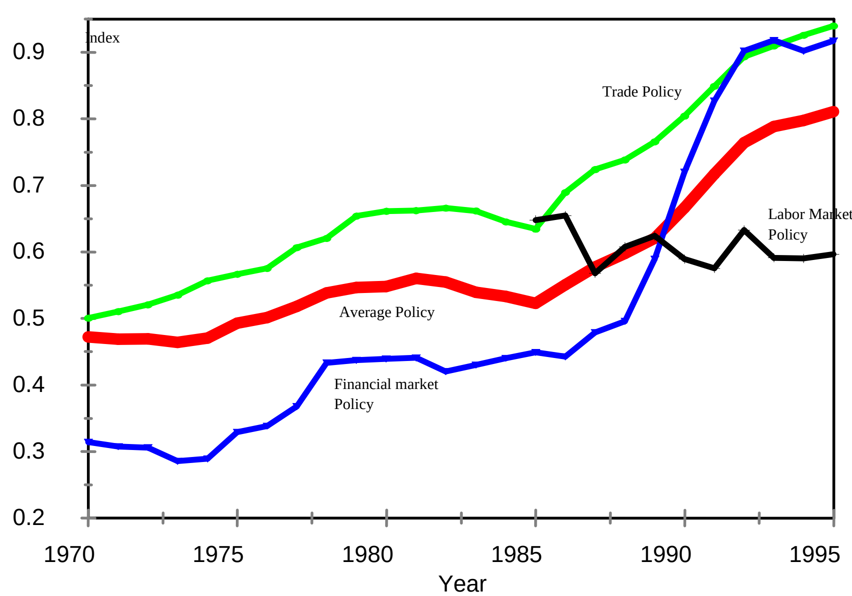

Figure 2B. Average Policies in Latin America,1970-95

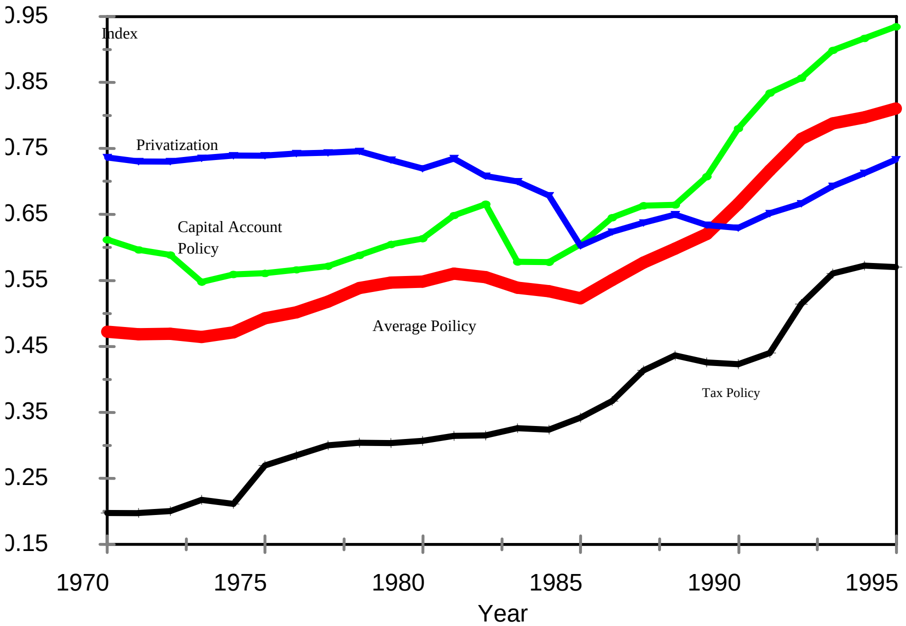

Table 1

Country Wage Gaps, Overall Policy Indices and Speed and Pace of Reforms 1970-1995

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 Overall Policy Change

Independent Variable	Dependent Variable: Higher/Secondary Differential — Estimation in Difference	Fixed Effects	Random Effects	Dependent Variable: Higher/Primary Differential — 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 3

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 4

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

Independent Variable	Independent Variable			
	Labor Market Index		Trade Flows/GDP	
	Dependent Variable		Dependent Variable	
	Higher-Secondary Wage Gap	Higher-Primary Wage Gap	Higher-Secondary Wage Gap	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 5

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			Dep. Variable: Higher/Primary Gap		
	Including GDP Growth	Including Growth & Unempl.	Including Growth & Unempl. & Exchg. Rate	Including GDP Growth	Including Growth & Unempl.	Including Growth & Unempl. & Exchg. Rate
	(1)	(2)	(3)	(1a)	(2a)	(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 6

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.