Trade, Wages, and the Political Economy of Trade Protection: Evidence from the Colombian Trade Reforms[*]

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Abstract

Worker industry affiliation plays a crucial role in how trade policy affects wages in many trade models. Yet, most research has focused on how trade policy affects wages by altering the economy-wide returns to a specific worker characteristic (i.e., skill or education) rather than through worker industry affiliation. This paper exploits drastic trade liberalizations in Colombia in the 1980s and 1990s to investigate the relationship between protection and industry wages. Using the Colombian National Household Survey we first compute wage premiums, adjusting for a series of worker characteristics, job and firm attributes, and informality. We find that Colombian industry wage premiums exhibit remarkably less persistence over time than U.S. wage premiums. Similarly, measures of trade protection are less correlated over time than in the U.S. data, indicating that trade liberalization has changed the structure of protection. We next relate wage premiums to trade policy measures in a framework that accounts for the political economy of trade protection. Accounting for time-invariant political economy factors is critical. When we do not control for unobserved time-invariant industry characteristics, we find that workers in protected sectors earn less than workers with similar observable characteristics in unprotected sectors. Allowing for industry fixed effects reverses the result: trade protection increases relative wages. This positive relationship persists when we instrument for tariff changes. Our results are in line with short- and medium-run models of trade where labor is immobile across sectors. In the context of the current debate on the rising income inequality in developing countries, our findings point to a source of disparity beyond the well-documented rise in the economy-wide skill premium: because tariff reductions were proportionately larger in sectors employing a high fraction of less-skilled workers, the decrease in the wage premiums in these sectors affected such workers disproportionately.

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1. Introduction

The public debate on the merits and perils of trade liberalization often centers on the question of how trade reforms will affect labor markets. But despite the prominence of this question in public policy, empirical research to date has offered no conclusive evidence on the effects of trade liberalization on employment and wages. This state of affairs reflects two main difficulties associated with empirical work in the area. The first one is a measurement issue: in recent years, trade protection in developed countries has taken the form of non-tariff barriers (NTBs) that are inherently hard, if not impossible, to measure.[1] Accordingly, while one might hope to use recent waves of trade liberalization as a testing ground to identify the effects of trade on wages, inference is limited by the lack of proper measures of this liberalization. The measures of international integration usually employed in the literature (imports, exports, import and export growth, import price indices, or product prices when available) are highly contentious, as they are associated with conceptual problems in their interpretation, while regressions employing them as explanatory variables suffer from simultaneity biases.[2]

A second limitation is that the political economy of trade protection, while having made inroads in trade theory and empirical studies of import penetration, has remained a second-order concern in studies of the effects of trade reform on wages.[3] Trade liberalization is usually treated as exogenous. Yet, both political economy theories of trade protection and casual empiricism suggest that trade policy is endogenous, both in the economic and econometric sense: labor market concerns are often a consideration in the formulation of trade policy; moreover, unobserved factors affecting trade protection (e.g., industry lobbying) are likely to simultaneously affect wages.

This paper hopes to make progress on these two issues by exploiting the Colombian trade liberalization between 1985 and 1994. The main advantage of this liberalization episode is that Colombia, like other developing countries, had not participated in the tariff reducing rounds of the GATT, so that tariff levels were high prior to the reforms. Trade reform consisted primarily

[1] The common wisdom in the field is that the agencies collecting NTB data take great care in making the data comparable across sectors and across countries in any given year, but are less concerned with consistency of the numbers across years. This makes the use of time series data on NTBs troublesome.
[2] These problems are particularly severe when quantity measures are used. As has been pointed out before, in the general equilibrium, trade affects wages through prices that are set on the margin, and not through quantities. The use of price data on the other hand presents other problems: prices are simultaneously determined with wages, and are often poorly measured.
[3] A notable exception to this pattern is the paper by Gaston and Trefler (1994) that we refer to in more detail below.

of drastic tariff reductions to levels comparable to those in developed countries.[4] Tariffs are both well measured and -- contrary to NTB measures -- comparable across time. A further advantage of the Colombian trade reform is that the period 1985-1994 includes multiple tariff reduction episodes that affected not only the *average* tariff, but also the *structure* of protection across industries. Hence, our data provides ample variation to identify the effects of trade policy on wages.

Our particular focus is on the effect of liberalization on industry wage premiums. Industry wage premiums are defined as the portion of individual wages that cannot be explained by worker, firm, or job characteristics, but can be explained by the worker's industry affiliation. Our approach contrasts with the previous literature, which has concentrated on the effects of trade policy changes on the returns to particular worker characteristics (most prominently, returns to skill and education). These studies consider the consequences of trade reforms in the long run, when workers can plausibly be considered mobile across sectors so that their industry affiliation does not matter. However, industry affiliation is crucial in predicting the impact of trade reforms in short- and medium-run models of trade, and in trade models with imperfect competition. These models seem particularly relevant in developing economies (like Colombia) where labor market rigidities obstruct labor mobility across sectors. Whether wage premiums represent returns to industry-specific skills that are not transferable in the short run, or industry rents, trade liberalization is expected to affect them through the channels we indicate in Section 2.

Although we do not attempt a general analysis of the sources of income inequality in this paper, our results on the effects of trade reform on wage premiums have important implications for the impact of trade liberalization on income distribution. To the extent that different industries employ different proportions of educated and skilled workers, changes in wage premiums translate to changes in the relative incomes of skilled and unskilled workers. If tariff reductions are proportionately larger in sectors employing less-skilled workers, and if these sectors experience a decline in their relative wages as a result of trade liberalization, then less-skilled workers will see their relative incomes decline. This effect is conceptually distinct from the potential effect of trade liberalization on the skill premium. In this sense, less-skilled

[4] Trade liberalization in Colombia also reduced NTBs; still, tariffs remain the primary trade policy instrument. Despite measurement problems we make an attempt at examining NTB effects in the empirical section.

workers may be “hit’’ twice: first the average return to their skill may decrease; second, the industry specific return in the sectors they are employed may decline.

We conduct our empirical analysis in two steps: first, we compute industry wage premiums for Colombia for the period 1984-1998; then, we relate them to the reduction of trade barriers. We use data from the June waves of the Colombian National Household Survey (NHS) that cover the urban sector (approximately 85% of the labor force) and contain detailed information on informality. It is estimated that 50 to 60 percent of employment in Colombia takes place in the informal sector. Accordingly, we thought it particularly important to account for informality, especially since the trade reforms in Colombia coincide chronologically with major labor reforms that caused reallocation across the formal and informal sectors (see Kugler, 1999). The significance of the informal sector in developing countries is discussed extensively in Harrison and Leamer (1997), who show that in the presence of an informal sector, labor market adjustment to trade and/or labor reform may be different from what was originally intended by policy makers.

Our work is related to two different strands of the literature. The first one consists of the voluminous literature on industry wage premiums (Dickens and Katz (1986), Krueger and Summers (1987) and (1988), Katz and Summers (1989)). This literature that has focused mainly on the U.S. has established that industry effects explain a substantial amount of individual wage variation. But while the importance of industry effects is uncontroversial, the reasons for their existence have been harder to establish. To our knowledge only one paper, by Gaston and Trefler (1994), has related U.S. wage premiums to trade protection. Focusing on cross-sectional data from the 1984 CPS Gaston and Trefler find a negative correlation between wage premiums and tariff protection. This correlation is robust to various specification tests, and most importantly, to treating protection as endogenous. Though the cross-sectional data do not lend themselves to an analysis of policy changes such as tariff reductions, Gaston and Trefler argue convincingly that there is little reason for focusing on time-series data in the U.S.: wage premiums are highly correlated across time (year-to-year correlations are reported in several studies to be 0.9 or higher), while the GATT rounds affected the level but not the structure of protection. This implies equally high year-to-year correlations for tariffs (e.g., the correlation between the 1972 and 1988 tariffs is reported to be 0.98).

This argument however does not apply to developing countries. As we show below, the year-to-year correlations for our estimated wage premiums in Colombia are substantially lower than the ones estimated for the U.S., taking values as low as 0.14 for individual years. Similarly, year-to-year correlations for tariffs lie below those computed for developed countries. Cragg and Epelbaum (1996) and Robertson (1999) report similar magnitudes for year-to-year correlations of wage premiums in Mexico. Thus it seems that wage premiums in these countries exhibit more volatility than in the U.S. Given that both countries experienced major trade liberalization in the last two decades, there is, at least in principle, room for establishing a connection between trade protection and industry wage determination.

The second part of the literature our paper is related to, is the newly emerging literature on the effects of trade reform on wage inequality in Latin American countries (Cragg and Epelbaum (1996), Johnston (1996), Revenga (1997), Harrison and Hanson (1999), Robertson (1999), Feliciano (2001), Pavcnik (2001b), and several papers on Chile and Colombia by Robbins, to name only a few).[5] Several papers have documented an increase in the skill premium or the returns to education over the last two decades, and have attributed them to an increase in demand for labor, though establishing a link to trade policy has been more tenuous. Since our focus in this work is on the short- and medium-run adjustments to trade liberalization, we do not attempt to estimate returns to worker specific characteristics. Instead, we focus on industry effects.

In our study, we take special care to account for political economy determinants of tariff protection that may also affect industry wage premiums independently, inducing spurious correlation between industry protection and wages. To this end, we first exploit the strengths of our data (disaggregate information and panel structure) to account for time-invariant political economy factors that could explain industry protection, and subsequently turn to instrumental variable estimation to account for the potential endogeneity of protection *changes.* The disaggregate household level data allow us to control for worker characteristics that may explain

[5] Among these papers, Feliciano (2001) is most closely related to our work. Feliciano relates wage premiums in Mexico to trade protection measures, but focuses primarily on import license coverage as a measure of trade protection and a single trade liberalization episode. The main problem with import license coverage is, like with other NTBs, that the percentage of domestic output covered by licenses that is used as a measure of protection has no relation to the equivalent tariff, the right measure of trade restrictiveness. Robertson (1999) provides many interesting facts concerning wage premiums and rankings of sectors by wage premium size in the U.S. and Mexico (see our discussion in section 5), but does not relate them to trade protection measures. Neither paper deals with the political economy of protection.

inter-industry variation in wages. Many earlier studies on the effects of trade protection on wages have utilized industry- or plant-level data that offer information only on average industry or plant wages. As pointed out by Gaston and Trefler, such studies may overstate the effect of trade related measures on wages, since import competing industries, at least in the U.S., tend to employ a higher fraction of less-skilled workers. By including information on worker attributes we aim at addressing this deficiency. The information on informality and other workplace characteristics represents an additional improvement in this direction.

When industry panel data are available (as is the case here) and industry composition does not change over time, the use of individual worker characteristics is less crucial, since industry fixed effects can capture differences in composition across industries. However, this strategy fails when industry composition shifts over time. Moreover, previous empirical work on Latin American countries suggests that the returns to individuals' skills and characteristics have changed concurrently with tariffs. In particular, the growing return to schooling (i.e., skill premium) in many developing economies during the 1980s and 1990s coincides with large tariff reductions. If we relied on aggregate industry data only, we might falsely conclude that tariff cuts that were concentrated in sectors with a high proportion of skilled workers led to an increase in industry wage premiums, even without any change in industry composition (although compositional shifts are likely to occur in response to changes in relative factor prices in the longer run).

A further advantage of using industry panel data is suggested by the political economy theories of protection. Even if there are no differences in the composition of workers across industries (or we have successfully controlled for them), industries differ in unobservable characteristics that simultaneously affect tariff formation and inter-industry wage differentials. Such characteristics could involve the ability to lobby the government for trade protection, or government's targeting of industries with specific characteristics. For example, some industries may easily organize and lobby for protection, while workers employed in these industries have the ability to bargain for higher wages than workers with the same observable attributes in other industries. Alternatively, policymakers may protect capital-intensive (or less productive) industries, and these industries also pay higher (lower) wages. Or, workers in some industries may be willing to accept lower wages in return for higher job security. These workers are in turn protected by higher tariffs. The bias generated by such factors could have either sign. In general, it is difficult to control for such industry characteristics or find variables that satisfy the necessary exclusion restriction of being correlated with tariffs but not having a direct effect on wages to instrument for tariffs in a cross-section. The advantage of industry panel data in this context is that industry fixed effects can capture the effect of political economy factors as long as these do not vary substantially across years.

Assuming that political economy determinants do not vary much over relatively short time periods seems a reasonable identification assumption in many cases, but it still leaves the question open of why trade reform was instituted in the first place. To address this concern, we exploit information on the institutional details of the Colombian trade reforms and use pre-reform tariff levels and exchange rate variation to instrument for tariff changes.

Our results suggest that it is crucial to account for political economy factors in the analysis of the effect of protection on industry wages. In particular, controlling for time-invariant unobserved heterogeneity alone is sufficient to flip the sign of our results. Before controlling for unobserved time-invariant, industry specific factors we find that trade protection is negatively correlated with wages. Conditioning on industry fixed effects reverses this result. We find that tariffs have an economically significant, positive effect on relative wages. This positive effect is robust (though smaller in magnitude) to instrumenting for time-variant political economy factors. Our findings support the predictions of the short-run models of trade, where labor mobility across industries is constrained. The implications of our estimates for changes in the income distribution are discussed in detail in the concluding section.

The remainder of the paper is organized as follows. In the next section we examine the predictions of theoretical models regarding the effects of trade policy on relative wages. Section 3 describes our empirical strategy. Section 4 discusses the data and provides a brief overview of the trade policy in Colombia during our sample period. In Section 5 we describe in detail our results from the wage premium estimation and examine the sensitivity of our estimates to various specifications. Section 6 considers the relationship between our wage premiums estimates and trade liberalization, and Section 7 concludes.

2. Trade Protection and Relative Wages: Theoretical Background

Before embarking on the empirical analysis it is worth laying out what our expectations are with regard to the effects of trade reform on relative industry wages, based on existing theoretical models.

Perhaps the most natural point of departure for thinking about relative wages and trade is the specific factors model. This model is short-run by nature as it considers factors of production immobile across sectors. The model predicts a positive relationship between protection and industry wages; in the context of our trade liberalization experiment this implies that sectors that experienced proportionately larger tariff reductions should be associated with a decrease of wage premiums. The medium-run Ricardo-Viner model that considers labor immobile, but capital mobile across sectors, yields similar predictions. In a well known paper, Magee (1982) presents indirect evidence in favor of the short-run model based on the attitudes of capital and labor representatives from various industries towards liberalization. The popular notion that trade reform is going to make workers poorer in the previously protected sectors is also consistent with this model.

In contrast, the long-run Hecksher-Ohlin model predicts that trade reform should affect only economy-wide returns to the factors of production, but not industry specific returns, since all factors of productions are mobile across uses. In particular, the model predicts that liberalization concentrating on labor-intensive industries should reduce the average wage, as it decreases the overall demand for labor, while relative wages should remain unchanged given that wages are assumed to be equalized across industries. The problem with adopting this framework for our analysis is that it is hard to reconcile with the considerable inter-industry variation in wages for observationally equivalent individuals. Nevertheless, a failure of our results to establish a link between trade policy and relative wages could be indicative of adjustments along the lines of the Hecksher-Ohlin model, namely reallocation of labor across sectors.

The above trade models assume perfectly competitive product and factor markets. Introducing imperfect competition opens up additional channels through which trade policy may impact wages. In the presence of unionization, it is possible that unions extract the rents associated with protection in the form of employment guarantees rather than wages. Grossman (1984) develops this idea in the context of a model in which seniority-based layoff rules are important; these induce senior workers to push of higher wages while younger workers are more interested in preventing layoffs. Such rules may break the simple link between protection and wages implied by the specific factors model. This model also suggests a closer examination of the seniority structure of each industry and the employment responses to liberalization.

Liberalization induced productivity changes may further impact relative wages. There is by now a voluminous literature on the effects of trade reform on firm productivity. While in theory the effects of liberalization on productivity are ambiguous (see Rodrik (1991) and Roberts and Tybout (1991, 1996) for a discussion), most empirical work to date has established a positive link between liberalization and productivity (Harrison for Cote d' Ivoire (1994), Krishna and Mitra for India (1998), Kim for Korea (2000), Pavcnik for Chile (2001a), Fernandes for Colombia (2001)). The productivity enhancements can occur either through exit of old inefficient plants and entry of new more efficient plants, or through better allocation of resources within existing plants. In either case, to the extent that productivity enhancements are passed through onto industry wages, we would expect wages to increase in the industries with the highest productivity gains. If these occur in the industries with the highest trade barrier reductions, relative wages would be positively correlated with trade liberalization.

The above discussion suggests that, based on theoretical considerations alone, it is not possible to unambiguously predict the sign of the expected trade liberalization effect on wages. The question is one that needs to be resolved empirically. Nevertheless, the theoretical arguments we outlined in this section can serve as guides in our specification search, and help us interpret our results.

3. Empirical Strategy

As noted above, our approach in investigating the effects of trade policy on wages follows the industry wage premium methodology of the labor literature. The estimation has two stages. In the first stage we regress the log of worker i's wages ($\ln(w_{ij})$) on a vector of worker i's characteristics (H_{ij}) such as education, age, gender, dummies for formality of employment, geographic location, and a set of industry indicators (I_{ij}) reflecting worker i's industry affiliation:

$$\ln(w_{ij}) = H_{ij}\beta_H + I_{ij} * wp_j + \varepsilon_{ij} \qquad (1)$$

The coefficient on the industry dummy, the wage premium, captures the part of the variation in wages that cannot be explained by worker characteristics, but can be explained by the workers' industry affiliation. Following Krueger and Summers (1988) we assume that the omitted industry (retail trade in our case) has zero wage premium. We then express the estimated wage premiums as deviations from the employment-weighted average wage premium (wp_j).[6] This normalized wage premium can be interpreted as the proportional difference in wages for a worker in a given industry relative to an average worker in all industries with the same observable characteristics. The normalized wage differentials and their exact standard errors are calculated using the Haisken-DeNew and Schmidt (1997) two-step restricted least squares procedure provided to us by John P. Haisken-DeNew and Christoph M. Schmidt.[7] The first stage regressions are estimated separately for each year in our sample. In the second stage, we pool the industry wage premiums wp_j over time and regress them on trade related industry characteristics.

$$wp_{jt} = T_{jt}\beta_T + D_{jt}\beta_D + u_{jt} \tag{2}$$

The primary variable we include in T_{jt}, the vector of trade related industry characteristics, is tariffs. We consider our use of tariffs to be an advantage over previous studies that have used quantity measures such as imports and exports, or price indices. Since we are interested in the effects of policy changes on relative wages, tariffs are conceptually the right measure, they can be more plausibly considered as exogenous (though we relax this assumption in a later part of the paper), and they exhibit substantial variation over our sample period. Nevertheless, to see how our results compare to the ones of earlier studies, we also experiment with other controls in T_{jt} such as imports, exports, import and export ratios, NTB measures, and interactions of the above variables with exchange rates. The vector D_{jt} consists of a set of industry and time indicators, which we include in our more complete specifications. As an alternative to using industry fixed effects, we also estimate equation (2) in first-differences.

Before presenting our empirical results it is worth discussing some particular features of our estimation. First, we consider the use of individual wage data and worker characteristics a plus. As Gaston and Trefler (1994) point out, average industry wages might vary across industries because different industries employ workers with varying characteristics. As a result, industries with a large share of unskilled workers are likely to have lower average wages. If

[6] The sum of the employment weighted normalized wage premiums is zero.

[7] Although Krueger and Summers (1988) express wage differentials as deviations from the employment-share weighted mean, they approximate the standard errors of these normalized coefficients by the standard errors of the first stage coefficients on industry indicators. Haisken DeNew and Schmidt (1997) adjust the variance covariance matrix of the normalized industry indicators to yield an exact standard error for the normalized coefficients. The adjustment of the variance covariance matrix occurs by taking into account the linear restriction that the employment- share weighted sum of the normalized coefficients is zero.

these industries also have high tariffs, one could falsely predict that higher tariffs induce lower industry wages. By conditioning our industry wage premium estimates on individual characteristics in the first stage, the relationship between tariffs and wages in the second stage cannot be driven by differences in worker composition across industries. Of course, unobserved worker characteristics (for example, ability, desire for good working conditions, etc.) could still affect both worker wages and their industry choice. To the extent that industry composition based on such unobserved characteristics does not respond to trade liberalization, we can account for the effect of unobserved ability on wages in the second stage of the estimation through industry fixed effects. Thus, the only identification assumption that the industry-fixed effects approach requires is that time varying unobserved characteristics that affect earnings are uncorrelated with trade policy (albeit we relax this assumption when we focus on political economy of changes in tariffs).

A similar identification assumption is needed in the context of the usual concern about the endogeneity of protection. The literature on the political economy of trade protection suggests that policymakers consider industry characteristics when deciding whether or not, and how much to protect an industry. If some industries systematically receive more protection because of their characteristics (e.g. proportion of unskilled workers), this effect is captured in the second stage of the estimation through industry fixed effects. Put differently, we rely solely on the within-industry variation to identify the effect of tariffs on wages. This should mitigate the expected bias in the tariff coefficient if political economy factors that do not change much over time (e.g., average education of workers, average skill level, seller concentration, geographic concentration of the industry, etc.) are indeed important. However, potential bias arising from the role of time-variant political economy factors still remains unaccounted for. Given that the structure of protection changes over our sample period, such time-variant political economy considerations are expected to be important. For example, if protection responds to exchange rate pressures, and exchange rates also have a direct effect on wages, one would expect the tariff coefficient to be biased. We address this concern in two ways. First, in our regressions we try to control for several additional variables in equation (2), in an effort to eliminate potential omitted variable bias. As indicated above, such variables are lagged imports and exports, NTBs, and most importantly, exchange rates. Second, we instrument for tariff changes, exploiting information on pre-sample protection measures. Our instrumental variable strategy is described in more detail in the empirical section.

Finally, the dependent variable in the second stage is estimated, so it is measured with error. This does not affect the consistency of our second-stage coefficients (as long as this measurement error is uncorrelated with the independent variables), but it introduces additional noise in the second-stage regression model so that the second stage estimator has a larger variance. The noise in the industry wage premiums likely differs across industries and depends on the variance of the estimated coefficients on industry indicators in the first stage. We thus estimate (2) with LS and weighted least squares (WLS), using the inverse of the variance of the wage premium estimates from the first stage as weights. This puts more weight on industries with smaller variance in industry premiums. We also account for general forms of heteroskedasticity and serial correlation in the error term in (2) by computing robust (Huber-White) standard errors clustered by industry.

4. Data

4.1 Trade Policy

Colombia's trade policy underwent significant changes during the past three decades. Although Colombia considerably liberalized its trading environment during the late 1970s, the government increased protection during the early 1980s in an attempt to combat the impact of the exchange rate appreciation and intensified foreign competition.[8] As a result, the average tariff level increased to 27 percent in 1984. The level of protection varied widely across industries. Manufacturing industries enjoyed especially high levels of protection with an average tariff of 50 percent. Imports from the two most protected sectors, textiles and apparel, and wood and wood product manufacturing, faced tariffs of over 90 percent and 60 percent respectively. This suggests that Colombia protected relatively unskilled, labor-intensive sectors, which conforms to a finding by Hanson and Harrison (1999) for Mexico. From 1985 to 1994, Colombia gradually liberalized its trading regime by reducing the tariff levels and virtually eliminating the nontariff barriers to trade. Although the tariff levels declined throughout the period, the most radical reforms took place in 1985 and 1990-1991. The 1985 tariff cuts almost

[8] High world prices of coffee, significant foreign borrowing by Colombia, and illegal exports all contributed to the large appreciation of the peso during the late 1970s and early 1980s (Roberts and Tybout (1997)).

reversed the protection measures implemented during the early 1980s, while the 1990-91 reforms resulted in the historically lowest levels of protection, and a very liberal trade regime. The 1990-91 *apertura* trade reforms aimed to expose domestic producers to international competition, increase efficiency, accelerate output growth, and provide lower prices for consumers (Rajapatirana (1998)). While the Gaviria government initially planned to gradually lower tariffs and NTBs from 1990 to 1994, the government, faced with the current account surplus, accelerated and completed the reforms by 1992.

Table 1a provides the average tariff across all industries, across agriculture, mining, and manufacturing, and for manufacturing alone from 1984 to 1998, the period of our study.[9] The average tariff declined from 27 to about 10 percent from 1984 to 1998. The average tariff level in manufacturing dropped from 50 to 13 percent during the same period. Figure 1 plots tariffs in 1984 and 1998 and nicely portrays why Colombian trade liberalization provides an excellent setting to address the impact of trade on labor markets. Not only do tariffs exhibit large variations over time and across sectors, but also the relatively low correlation between the tariffs in 1984 and 1998 suggests that the structure of protection has changed over time. Table 1b reports tariff correlations over time and confirms this. The correlations range from .94 to .54 between various year pairs. The intertemporal correlation of Colombian tariffs is significantly lower than the intertemporal correlation in the U.S. tariffs, where the correlation between post-Kennedy GATT Round tariffs (1972) and post Tokyo GATT round tariffs (1988) is .98.

In addition to tariffs, Colombia reduced NTBs between 1990 and 1992. Information on NTBs is available for three years only: 1986, 1988, and 1992.[10] In 1986, the average coverage ratio was 72.2 percent. As is the case with tariffs, NTB protection varies widely across

[9] The source of tariff information is the Colombian National Planning Department (DNP). The original data provide tariff levels and the number of tariff lines at the 3-digit ISIC level from 1984 to 1998. This information is missing in 1986. However, 4-digit ISIC tariffs on agriculture, mining, and manufacturing from the World Bank that cover the period up to 1988 indicate that almost no tariff changes occur between 1985 and 1986 at the 4-digit ISIC level. The tariff means in 1985 and 1986 are not statistically different from each other and the correlation in tariffs across the two years is .999. We thus use the 1985 tariff information from DNP for 1986. We aggregate tariffs to the 2-digit level, so that they correspond to the level of industry aggregation in the household survey. To aggregate to the 2-digit level, we weight 3-digit tariffs by the number of tariff lines they represent. We have also used 3-digit imports as weights, which yielded similar 2-digit ISIC tariff means. Tariff data are available for 2-digit agricultural sectors, mining sectors, manufacturing, as well as ISIC codes 41 (electricity), 83 (real estate and business services), 94 (recreational and cultural services), and 95 (personal and household services). For most of the latter categories, tariffs are usually zero, except for some years in the 1990s. This yields a total of 21 industries with tariff data.
[10] The source of NTB information is the United Nation's publication Directory of Import Regimes. NTBs are measured as coverage ratios. They are available for 2-digit ISIC sectors in agriculture, mining, and manufacturing, as well as ISIC 61 (wholesale trade).

industries, with textiles and apparel industry and the manufacturing of wood and wood products enjoying the highest level of protection. Between 1990 and 1992, the average coverage ratio dropped to 1.1 percent. In addition, the structure of NTB protection has changed: the correlation in NTBs between 1986 and 1992 is not significantly different from zero (.10 with a p-value of .69).

The above shifts in Colombia's trading environment are reflected in the import and export flows. Figure 2 shows the evolution of aggregate imports and export (and manufacturing exports and imports) from 1980 and 1998 measured in real 1995 millions of pesos.[11] For manufacturing industries we have also computed the import penetration (import/(output+net imports)), and the export to domestic consumption ratio (exports/(output+net imports)) depicted in the bottom graph in figure 2. While import flows increased significantly since 1984, they surge after 1991. Between 1984 and 1993, the aggregate (as well as manufacturing) import flows more than double. Manufacturing import penetration also follows a similar pattern: import penetration increases from about 20 percent in 1984 to 23 percent in 1990, and surpasses 25 percent in 1992. Manufacturing exports and aggregate exports also increase over time. However, the export to consumption ratio in manufacturing is quite volatile over time, which likely reflects exchange rate fluctuations.

4.2 National Household Survey

We relate the trade policy measures to household survey data from the 1984, 1986, 1988, 1990, 1992, 1994, 1996, and 1998 June waves of the Colombian National Household Survey (NHS) administered and provided by the Colombian National Statistical Agency (DANE). The data is a repeated cross-section and covers urban areas. The data provide information on earnings, number of hours worked in a week, demographic characteristics (age, gender, marital status, family background, educational attainment, literacy, occupation, job type), sector of employment, and region. The survey includes information on about 18,000 to 36,000 workers in

[11] We use data on imports and exports from the United Nations COMTRADE database provided to us by the World Bank. The data only include sectors in which either exports or imports were greater than zero. As a result, no trade flows were reported for SITC categories that map into one-digit ISIC codes 4, 5, 6, 7, 8, and 9 in years with no trade flow. Since these categories are very likely to have zero imports and exports, we replaced the missing values with zero. Note also that trade flows for 41 are reported in the original data for years they exceed zero. Since trade flows for 61 always exceed zero, they are always reported. Data on industry output and other industry characteristics are only available for manufacturing sectors from the UNIDO's Industrial Statistics Database (3-digit ISIC level).

a year.[12] The industry of employment is reported at the 2-digit ISIC level, which gives us 33 industries per year.

We use the household survey to create several variables. We construct an hourly wage based on the reported earnings and the number of hours worked normally in a week.[13] Using the information on the highest completed grade, we define four education indicators: no completed education, completed primary school, completed secondary school, completed college (university degree). We distinguish between seven occupation categories: professional/technical, management, personnel, sales, service workers and servants, blue-collar workers in agriculture/forest, blue-collar industry workers. In addition, we control for whether an individual works for a private company, government, a private household, or whether a worker is an employer or is self-employed. Descriptive statistics for each year of the data are provided in Table 2.

The data on worker's characteristics have several shortcomings. First, although the union status is often an important determinant of individual earnings, our data do not provide information on unionization. However, anecdotal evidence suggests that unions are ineffective in most industries. The only exception is the union in the petroleum industry, USO (Union Syndical Obrera), whose power stems from its close ties to the Colombian guerrillas. Second, our data do not provide information on the number of years since a worker has entered the workforce. We try to control for tenure by including age and age squared in our specification (in addition to controlling for education). Moreover, the survey provides information on how long a worker has been employed at the current job, and an indicator for whether or not the worker has been previously employed. This information is not available in 1984, a year preceding a large trade liberalization. We have compared whether the inclusion of time at current job (and its square), and of an indicator for whether a worker has been previously employed affect our estimates of wage premiums relative to the wage premiums obtained when we control for age and age squared only. Although these variables enter positively and significantly in the first stage regression, they hardly change the estimates of wage premiums. The correlation between the premiums based on this specification and the wage premiums conditional on age and age

[12] We have excluded all workers for which one or more variables were not reported.

[13] The survey allows the worker to report monthly, weekly, biweekly, daily, hourly, or ten-day earnings. For workers who receive room and board on a monthly basis, we incorporated the self-reported value of room and board into their earnings. For self-employed workers, we use their monthly net earnings from their business to calculate their hourly wage.

squared only is .99. As a result, we continue to control for tenure using only age and age squared so that we can include 1984 in our sample. Finally, the information on the sector of employment is reported only at the 2-digit ISIC level, which enables us to distinguish between 33 sectors of employment in a given year. If changes in tariffs at the 3 or 4-digit levels lead to large adjustments within 2-digit ISIC industry groups, our level of aggregation will ignore such effects.

While our data suffer from the above shortcomings, they provide detailed information on informality and workplace characteristics that are not available in many other labor force surveys. First, the survey asks each worker whether a worker's employer pays social security taxes.[14] The employer's compliance with social security tax (and thus labor market legislation) provides a good indicator that a worker is employed in the formal sector. Given that between 50 to 60 percent of Colombian workers work in the informal sector, the inclusion of information on informality is important. Moreover, Colombia implemented large labor market reforms in 1990 that increased the flexibility of the labor market by decreasing the cost of hiring and firing a worker (see Kugler (1999) for details). These reforms likely affected the incentives of firms to comply with labor legislation, their hiring and firing decisions, and workers' choice between formal and informal employment. Descriptive statistics suggest that about 57 percent of workers worked in informal sector prior to 1992. This is also the share of informal workers in 1992, however the share fluctuates significantly thereafter from .51 in 1994 to about .6 in 1996 and 1998. The survey also provides several workplace characteristics. We create four indicator variables to capture whether a worker works alone, whether the worker works in an establishment with 2 to 5 people, 6 to 10 people, or 11 or more people. We also use an indicator for whether a worker works in a permanent establishment in a building (as opposed to outdoors, kiosk, home, etc.).

These workplace characteristics potentially control for differences in the quality of the workplace across industries and should thus be included as controls in equation (1). In 1994 we can check this interpretation of our workplace controls by correlating them with particular measures of workplace quality that are available in a special module for 1994 only. Using the 1994 quality of work survey, we create an indicator for whether a worker has received job training at the current job, an indicator for whether a worker finds employee relations excellent

[14] This information is not available in 1984.

or good, an indicator for whether a worker grades physical, mental, and social conditions at a workplace as excellent or good, and an indicator that is one when a worker finds his job excellent or good. Working in a larger firm or working in a permanent building/establishment is positively correlated with job training, satisfaction with workplace conditions, employee relations, and general job satisfaction. Working in the informal sector is negatively correlated with job satisfaction, good workplace conditions, good employee relations, and job training.

5. Estimation of Wage Premiums

In the first stage of our estimation, we estimate equation (1) for each cross section of the household survey using three specifications. All three specifications include a full set of industry indicators (retail trade industry is the omitted group), but they differ in the set of individual characteristics included in vector H_{ij}. The most parsimonious specification, specification 1, does not control for any individual characteristics. Thus, the wage premiums in this specification are equivalent to raw relative wages. Specification 2 includes demographic characteristics (age, age squared, gender, marital status, head of the household indicator, education indicators, literacy, location indicator, occupational indicators, and job type indicators). Specification 3 adds workplace characteristics (informal sector indicator, size of the establishment indicators, and type of establishment indicator) to specification 2. In section 6, we refer to wage differentials from these three specifications as WP1, WP2, and WP3, respectively. In order to check if the estimates of wage premiums are sensitive to whether we express earnings per hour or per week, we estimated all of the above specifications using both the log of hourly earnings and the log of weekly earnings as dependent variables. Figure 3 plots the relationship between hourly and weekly industry wage premiums based on specification 3. Most observations are located on or close to the 45 degree line, which indicates a high correlation between wage premiums based on weekly and hourly earnings. We thus focus our discussion on hourly wage premiums only.

In general, the signs and magnitudes of the coefficients on individual characteristics are similar to those obtained in previous studies. Older workers, men, married workers, head of the households, and people living in Bogota earn relatively more. The signs on the occupation indicators are also intuitive—except for managers, other occupation categories earn relatively less than the professionals and technical workers (the omitted category). Employees earn less than employers (the omitted category). Unlike previous studies, we also control for workplace characteristics. People working in bigger establishments earn more, as do people working in permanent buildings or establishments. People working in the informal sector earn less that people with the same observable characteristics in the formal sectors. More detail on the results from this stage (including additional tables) can be found in the NBER Working Paper version of our work.

A comparison of the coefficients across years suggests that the returns to several worker characteristics have changed over time. Most importantly, the returns to education and the returns to working in the informal sector seem to vary substantially over time. Our results on the return to a college degree are consistent with the patterns documented in other studies of Latin American countries; in particular, we find that the return to higher education has increased, peaking in 1994 and 1998. With respect to informality, we find that while workers in the informal sector earn about 4 to 5.6% less than workers with the same observable characteristics in the formal sector prior to 1990, this wage difference gradually declines between 1990 and 1994, but increases dramatically afterwards. This probably reflects changes induced by the labor market reform. The changes in the returns to various worker characteristics over time further substantiate the importance of conditioning on worker characteristics to compute wage premiums.[15]

We next check how much of the variation in log hourly wages the different specifications of equation (1) explain. The R^2 in specification 1 ranges between .10 and .15 in various years, which implies that industry indicators alone can explain up to 15 percent of the variation in log hourly wages. As we condition on more worker characteristics, the R^2 increases to a range of .37 to .42 (across various years) in specification 3. When we estimate this specification without industry indicators, the new R^2 ranges from .36 to .40, suggesting that conditional on worker and firm characteristics, industry indicators explain about 2 percent of the variation in log hourly wages. The conditioning on worker and firm characteristics also significantly reduces the variation in industry wage differentials. The employment weighted standard deviation of industry wage differentials drops from about 25 to 35 percent in

[15] There is a large literature in labor economics that has tried to estimate returns to education controlling for worker ability. This literature has emphasized that estimates obtained without controls for workplace ability may be biased, since education is likely to be correlated with unobserved ability. Our results on the returns to education may suffer from such bias. Nevertheless, we should point out that we are not interested in the returns to schooling per se, but rather in how these evolved during the period of trade reforms. To the extent that the trade reforms did not affect the sign or magnitude of the bias (and we have no compelling reason to believe that they did), the statement that the returns to schooling have increased in the 1990s is valid even in the existence of simultaneity bias.

specification 1 to about 7 to 9 percent in specification 3. While Katz and Summers (1989) report similar variation in unconditional wage differentials for the U.S. in 1984, the dispersion in wage differentials conditional on individual characteristics is lower in the Colombian data. Moreover, while the variation in unconditional wage differentials is higher in Colombia than the variation in Mexico, as reported by Robertson (1999), the variation in the conditional wage differentials is actually lower. This could be due to the fact that we account for some demographic variables that are not included in the study for Mexico, and for workplace characteristics.

The wage premiums we compute based on the different specifications tend to be highly correlated with each other. When we pool industry wages across time, the correlations between wage premiums from specification 1 and wage premiums for specifications 2 and 3 are .91 and .90 respectively. Moreover, previous studies have suggested that differences in the quality of workplace across industries could account for differences in industry wage differentials. Quality of workplace is often unobserved. While, like in previous studies, information on the quality of work is not available to us in most years, the special "Quality of Work" module in 1994 provides answers to questions about job training and job satisfaction, as we explained in the data section. When this additional information is used to estimate an extended specification for 1994, the correlation of the wage premiums with these additional controls with the wage premiums from specification 3 is .99. This seems to suggest that either other characteristics of the workplace (for example, firm size and type of establishment) are already controlling for job quality, or that workplace quality does not vary across industries in a systematic fashion.

Wage premium correlations are substantially lower when we focus on year-to-year correlations. While a few industries have persistently high or low wage premiums in all time periods, the ranking of most sectors shifts significantly over time. Sectors with persistently high wage premiums are coal mining, crude petroleum and national gas production, and metal ore mining; insurance, wholesale trade, transport and storage, and communication also fare quite well. Retail trade and personal and household services exhibit persistently low wage premiums. Among the manufacturing industries, textiles and apparel, food processing, and wood and wood products tend to have lower wage premiums, while the manufacturing of basic metal products exhibits the highest wage premium. However, their rankings in the economy as a whole change over time. While Katz and Summers (1988), Robertson (1999) and Helwege (1992) find that the ranking of U.S. wage differentials is stable over time, Robertson (1999) finds that the ranking of

Mexico's wage differentials also fluctuates substantially over time. In order to check more formally how wage premiums vary over time, Table 3 presents year-to-year correlations in wage premiums based on specifications 2 and 3. These correlations range from .14 to .94. For example, for specification 2, the correlation between the 1984 premiums and the premiums in 1986, a year after a large trade liberalization episode, is .71. The correlation between the 1984 and 1992 relative wages is .58 -- 1992 is again a year that follows a major trade liberalization. Similar patterns are observed for the wage premiums based on specification 3. Colombian wage premiums are much less correlated over time than wage premiums in the United States, where the year-to-year correlation in general exceeds .9.[16] Given that our sample spans a period of major trade reforms, changes in trade policy could potentially provide an explanation for the variation of relative industry wages over time. We thus relate industry wage premiums to trade policy changes in the next section of the paper.

6. Trade and Wage Premiums

6.1 Main Results

Our main results concerning the relationship between trade policy and relative wages are contained in Table 4a. Equation (2) was estimated using both WLS and LS. Since the two sets of results were similar, we focus our discussion on the WLS alone. Table 4a has three panels. The first one contains all industries in our sample with available tariff information, including those with little trade exposure such as wholesale trade, electricity, real estate and business services. Since these industries were by nature hardly affected by changes in trade policy during our sample period, they effectively act as a control group in the estimation. The second panel focuses on the three sectors (manufacturing, mining and agriculture) that were exposed to trade protection measures. The last panel reports results for manufacturing only, to provide a standard of comparison for subsequent specifications, in which we utilize variables that are available only for the manufacturing sector. The three rows in each panel correspond to the three specifications of the wage premium equation; note that the third specification (WP3) in which we include measures of firm size and informality, has fewer observations, as the information on informality and other workplace characteristics was not available in 1984.

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[16] Krueger and Summers (1988) report a correlation of .91 between the 1974 and 1984 wage premiums. Robertson (1999) reports a correlation of .92 between the 1987 and 1997 U.S. wage premiums.

The first two columns (column 1 and column 2) of the results refer to specifications that do not include industry fixed effects. These specifications are the closest analog to earlier work that has estimated similar equations exploiting only cross-sectional data. Consider the third panel (manufacturing) in particular, that is the most comparable to previous work that has typically focused on the manufacturing sector only. Three noteworthy features emerge. First, in almost all specifications the effect of tariffs on relative wages is negative and significant. Workers in industries with high tariffs receive lower wages than workers with identical observable characteristics in industries with low tariffs. Second, the estimated tariff effects are large. To interpret the size of the tariff coefficients, consider an industry from the manufacturing sector with an average level of tariffs in 1998 (13%). Suppose that we conducted the conceptual experiment of shifting a worker from this industry to one with no tariffs. Then the estimated coefficient in the specification with year indicators and WP2 (controls for worker characteristics) implies that this worker's wage would rise by 3.12% (0.24 x 13%).[17] The corresponding effect in 1984, when the average tariff was 50%, would be 0.24 x 50% = 12%. Controlling for firm characteristics and informality in the wage premium definitions (WP3) makes the effects even larger: 5.46% (0.42 x 13%) for 1984, and 21% (0.42 x 50%) for 1998. These are economically significant effects.

The third noteworthy feature of the results in the first two columns is that the magnitude of the correlations between tariffs and wage premiums depends on the extent to which we have controlled for worker characteristics. The coefficients based on industry wage premiums based on the first-stage specification 1 (WP1) that does not control for any worker or firm characteristics are substantially larger in absolute value than the coefficients based on WP2. Interestingly, the additional controls for firm size and informality in WP3 do not seem to have as big of an impact on the results. These findings are consistent with the results in Gaston and Trefler (1994) for the U.S., and so is their interpretation: The large negative tariff coefficients in the WP1 regressions reflect the sorting of workers based on observable characteristics such as education, age, and occupation, and the political economy of protection (industries with less-skilled workers may receive higher protection). By conditioning the industry wage differentials on worker characteristics in the first stage of the estimation we partially control for this spurious

[17] A tariff value of 20 denotes an ad-valorem tariff of 20 percent.

correlation between protection and relative wages; hence, the estimated tariff coefficient declines in absolute value.

Of course to the extent that protection depends not only on observable worker characteristics, but also on unobserved worker and industry attributes, concern about spurious correlation remains. Previous work has tried to eliminate simultaneity bias by including additional industry characteristics in the estimation and by instrumenting for tariffs using sector characteristics (such as capital intensity, employment, unemployment, concentration indices, etc.) and worker characteristics as instruments. In Gaston and Trefler's work the simultaneity bias correction yields an even more negative tariff coefficient. The nature of our data allows us to deal with potential simultaneity bias in a more straightforward manner: to the extent that political economy factors and sorting based on unobserved worker attributes are time-invariant, we can control for them through industry fixed effects. Column 3 in Table 4a reports results from various specifications in all of which industry indicators are included.

The remarkable feature of the results in column 3 is that the inclusion of the fixed effects reverses the sign of the tariff coefficient, which is now positive and significant in every single specification. This implies that increasing protection in a particular sector raises wages in that sector. The magnitude of the effect is significant. Suppose for example that in a manufacturing sector with an average level of protection in 1984 (50% tariff) the tariff level were reduced to zero. According to our estimates, this would translate to a 4% (0.08 x 50%) decrease in the wage premium in this sector. For the most protected sectors (91% tariff) this effect increases to 7.3% (0.08 x 91%). It is interesting to note that – contrary to the results without fixed effects – the magnitude of the tariff coefficient is now less sensitive to the particular definition of the wage premium. This is intuitive and supports the hypothesis that the negative correlation between tariffs and relative wages in columns 1 and 2 was driven by unobserved industry characteristics; once we account for these characteristics through industry fixed effects, it becomes less important to control for observable worker and firm attributes.

An alternative to using fixed effects to control for unobserved industry heterogeneity is to estimate a specification in which changes of wage premiums are regressed against changes in tariffs. The results from this "first-difference" specification are reported in Table 4b. Most of the estimated tariff coefficients are again positive and significant, and have magnitudes similar to the ones reported in the previous table.

The last specification in column 4 of Table 4a reports results from specifications with both industry and year indicators. Year indicators allow for the average wage premium to change over time. This is particularly important in order to capture business cycle effects that may otherwise lead to spurious correlation between tariffs and wage premiums. Suppose, for example, that as a result of a recession wage premiums decrease, while the government responds to lower domestic demand by increasing tariffs. In the absence of any controls for the business cycle our framework would attribute the decrease of wage premiums to the higher tariffs. In practice, it seems unlikely that such macroeconomic effects drive the sign of the tariff coefficient in column 3, since they suggest a *negative* bias in the tariff coefficient, however the coefficient is estimated positive in all our specifications with industry effects. Moreover, the estimated coefficients in column 2 (that conditions on year, but not industry indicators) are in general more negative than the coefficients in column 1, where we did not control for year effects. Nevertheless, column 4 can give us a more accurate idea of the potential importance of macroeconomic effects. The results in the first panel that includes all industries are almost unchanged compared to the specification in column 3. Tariffs enter again positive and significant. The results in the other two panels are similar in magnitude, but the standard errors are larger, so that the tariff effect, while positive, is statistically insignificant in some specifications. This is not surprising given that the combined time and fixed effects absorb most of the variation in our data, while decreasing the number of industries included in the estimation further reduces the variation in the data used to identify the trade policy effect.

The reversal of the tariff coefficient sign once we condition on industry fixed effects demonstrates the importance of unobserved sector heterogeneity, and provides indirect support for political economy theories of protection. Of course, to the extent that political economy factors influencing protection are time-variant, the tariff coefficient may still be biased. We take up this issue in section 6.3.

6.2 Sensitivity Analysis

6.2.1 Additional Trade Exposure Controls

Apart from tariffs, there may be other channels through which trade affects wages. Industries could face differential changes in transportation and communication costs, informal trade barriers, and exchange rates over time. In this section we investigate the relevance of some of these factors.

We start by estimating a specification in which, in addition to tariffs, we include measures of industry imports and exports in the estimation. This approach is not motivated by a particular theoretical model; accordingly, we do not attempt to interpret the estimated coefficients in light of particular theory. Rather, we treat imports and exports as conditioning variables in order to investigate the robustness of our tariff coefficients. To the extent that the trade factors mentioned above affect trade flows, industry imports and exports capture the combined effect of all trade related channels, other than trade policy, on relative wages. Because trade flows are arguably endogenous (they depend on factor costs), we include the first lags of import and export measures in the estimation rather than their current values. Of course, to the extent that these variables are serially correlated, this approach does not completely eliminate simultaneity bias.

Tables 5a and 5b report results from two sets of specifications. In Table 5a we include lagged values of imports and exports, and estimate (2) for all industries with available tariff data. In Table 5b we include lags of the industry import penetration (defined as imports/consumption) and export/consumption ratios, for the manufacturing sector only. While our preferred set of controls involves ratios rather than absolute values, since ratios account for the size of the sector, industry output measures are available only for manufacturing. Hence we use lagged imports and exports when we utilize data from all sectors. The results in the two tables are similar.

There are two things to be learnt from Tables 5a and 5b. First, the tariff coefficients seem robust to the inclusion of the additional trade controls in all specifications. Second, the signs of the import and export variables are interesting in their own right. Consider columns 1 and 2 of both tables that do not include industry fixed effects. Note that the import variable consistently enters positive, while the export coefficients are negative or insignificant for most part. These results suggest that sectors with high imports (or import penetration) have on average higher wages, and they are in sharp contrast with what is usually reported for the U.S. A possible interpretation is that while in the U.S. sectors with high import penetration tend to employ a higher proportion of unskilled workers, the opposite is true in Colombia: the sectors with the highest imports are the ones that employ skilled labor. This interpretation is also supported by the fact that the conditional correlation between imports and industry wage differentials is substantially larger in specification WP1 (that does not condition industry wage differentials on workers' education, occupation, and demographic characteristics) than in specifications WP2 and WP3. Together with our earlier findings concerning the cross-sectional pattern of protection, these numbers suggest that in Colombia high tariffs are associated with low imports (or import penetration), relatively low wages, and unskilled labor. We should emphasize that the negative correlation between import penetration and protection is a correlation referring to the cross-sectional pattern of protection, and as such it is consistent with theoretical models of cross-sectional trade protection (e.g., Grossman and Helpman (1994)) and empirical evidence on these models based on cross-sectional data (Goldberg and Maggi (1999)).

Now consider columns 3 and 4 where industry indicators account for unobserved industry attributes. The trade flow coefficients experience the opposite effect from the tariff coefficients. While conditioning on industry fixed effects turns tariff coefficients from negative to positive, the import coefficients turn from positive to negative. Exports become now positive and significant. These signs are intuitive, suggesting that an increase in imports leads to lower relative wages (presumably through the channel of a decrease in labor demand) while higher exports translate to higher wages. Once again, these patterns demonstrate how important accounting for unobserved heterogeneity is, and how different the interpretation of the coefficients is when within industry rather than cross-sectional variation is used to identify the effects of interest.

One could object that lagged import and export measures do not capture the contemporaneous effects of trade factors, and hence our estimates still suffer from omitted variable bias. This is more likely to be the case in years with large exchange rate fluctuations. To investigate whether our results are robust to controlling for currency fluctuations we also estimated specifications in which the exchange rate is interacted with lagged values of import and export measures (Table 6). The exchange rate we use is the nominal effective rate (source: IMF) that is computed taking into account Colombia's major trade partners. We interact the exchange rate with lagged trade flows because a-priori we would expect the effects of currency fluctuations to vary depending on the trade exposure of the sector. Furthermore, the inclusion of the exchange rate may alleviate concerns that time-variant political economy factors generate spurious correlation in the estimation. Specifically, the time pattern of trade liberalization in Colombia suggests that import barriers are often adjusted to mitigate the effects of exchange rate movements. The major liberalization in the late 1970s, for example, is often attributed to the peso devaluation, while its reversal in the early 1980s is believed to have occurred in response to the peso appreciation during that time. To the extent that exchange rates also impact relative wages directly (via their impact on current imports and exports) their omission from the estimation would result in a biased tariff coefficient.

This concern is however not borne out. As our results in Table 6 indicate, the results are robust to the inclusion of exchange rates – in fact, the magnitude hardly changes compared to our base specification in Table 4a. Surprisingly, the exchange rate interactions are not significant in most specifications, and when they are, their signs seem to vary depending on the exact specification.

6.2.2 Non-tariff Barriers

Our main measure of trade policy in this paper is tariffs, and for the reasons we laid out above, we consider this to be a strength of our approach. However, trade liberalization in Colombia was not confined to tariff reductions, but extended to the decrease of NTBs. This raises the concern that omission of NTBs may lead to a bias in the estimation of tariff effects. This would be, for example, the case, if policy makers attempted to alleviate the effects of the trade reform by replacing tariffs in sectors that experienced large tariff reductions with less transparent, but potentially more restrictive, non-tariff barriers (as it had happened in the past in developed countries). A simple look at correlations between tariffs and NTB measures indicates that this was not the case: the two sets of trade instruments are highly correlated - that is, sectors with proportionately larger tariff cuts also experienced large reductions in NTBs. In addition, the extended specifications we have considered in the course of the sensitivity analysis of the previous subsection provide an indirect way of controlling for NTB effects: specifically, we capture the effect of NTB changes indirectly through the effect these changes may have had on industry import and export measures, and through time dummies. Nevertheless, since we have some limited information on NTBs, we attempt a more direct investigation of their effects in this subsection.

This investigation poses several challenges. First, NTBs are measured as coverage ratios (i.e., the percent of trade flows affected by a non-tariff barrier); this is a notoriously bad measure of protection that is especially difficult to compare over time. Second, NTB data are available only for three years in our sample (1986, 1988 and 1992) and they do not cover all industries. Using only three years substantially reduces the time variation in our data, which we rely on to identify the effect of policy changes on wage premiums. Still, to obtain a rough idea of how NTBs might affect our conclusions we estimated specifications that include NTBs as an additional independent variable for the three years using all industries with available NTB data (tables for these specifications can be found in our NBER Working Paper).[18] We should emphasize that we view these regressions as a rough check on the robustness of our tariff effect estimates, rather than a serious empirical examination of NTB effects.

The main conclusion from these regressions is that the tariff coefficient is robust to including NTB measures, at least in terms of its sign. The standard errors are however larger now, which is not surprising given that we utilize a significantly smaller number of observations. What is perhaps more surprising is that in almost all specifications the magnitude of the tariff coefficient is larger than before. The NTB coefficients on the other hand are very sensitive to the particular specification, and often insignificant. Since our NTB measures are plagued with measurement problems and the number of observations we use in this part of the estimation is limited, the lack of robust results for NTBs is not that surprising. Overall, we consider our results to tentatively support the claim that the estimated tariff effects are robust to the inclusion of NTBs, but not to be particularly informative on the role of NTBs in determining wage premiums.

6.3 The Political Economy of Trade Protection

So far our discussion of the political economy of protection has focused on the role of time-invariant factors that are captured in our framework through industry fixed effects. This section addresses the role of time-varying shocks, which may simultaneously affect tariff formation and industry wages, in an instrumental variable setting. Ideally, we would like to base our empirical analysis on a theoretical model of the dynamics of the political economy of protection that would identify the determinants of trade policy changes and suggest appropriate instruments for tariff changes. Unfortunately, all political economy models to date explain the

[18] Because the elimination of NTBs was concentrated in the period 1990-92 and NTBs did not change much after 1992, we also experimented with specifications that include 1986, 1988, 1992, and all years after 1992. This assumes that NTB levels remained constant after 1992. It turns out that the results in the limited and extended sample are similar.

cross-sectional patterns of protection in a static setting, and not the dynamics of protection *changes*. We therefore turn to the history of protection in Colombia and the institutional details of the reforms for guidance. A close examination of the determinants of tariff levels and tariff changes during our sample period is a crucial piece of our analysis at this stage, as it motivates our choice of instruments.

We start by asking the basic question why trade reform was instituted in the first place, and what factors account for the differential pattern of liberalization across sectors. Anecdotal evidence and World Bank reports suggest that the Colombian government initiated liberalization in response to exchange rate fluctuations and the trade balance. This indicates that at the macroeconomic level, exchange rates are one of the factors responsible for the trade policy changes. However, exchange rates alone cannot explain why some sectors experienced larger tariff reductions than others. In explaining the latter, two facts seem of importance. First, before the onset of trade liberalization, there was substantial tariff dispersion across sectors. In examining the cross-sectional pattern of protection we find that the single most important determinant of tariff levels was the share of unskilled workers; sectors with a high share of unskilled workers (where unskilled is defined as having at most primary education) had higher tariffs.[19] Second, the Gaviria government was committed to economy-wide liberalization for the purpose of exposing domestic producers to international competition among other things. This goal translated to proportionately larger tariff reductions in sectors that had historically higher tariff levels. This is evident from Table 1a and Figure 1 that clearly show that liberalization reduced not only the average tariff, but also tariff dispersion.

The close link between the magnitude of tariff reductions and the initial level of protection in 1983 (a year prior to our sample) can be demonstrated in several ways. Figure 4 pictures the relationship between the 1998-1984 decline in industry tariffs and the 1983 industry tariff level; it illustrates a strong positive correlation between tariff declines and the 1983 tariff level. A regression that relates the 1998-1984 tariff reductions to the 1983 tariff levels yields a coefficient on the 1983 tariff of 1.06 (with a T-statistic of 26.3) and an R^2 of .97. This again demonstrates that the 1998-1984 tariff declines were higher in industries with historically high tariff levels. This finding also applies to annual changes in tariffs from 1985 to 1998. In table

[19] Note that this pattern is consistent with the Grossman-Helpman political economy model of protection that predicts a negative correlation between import penetration and protection for organized sectors. In Colombia, sectors with a high share of unskilled workers have low import penetration and receive more protection.

7a, we relate the annual change in tariffs from 1985 to 1998 to the 1983 tariff level, year indicators, and a constant. A tariff reduction corresponds to a negative change. The coefficient on the 1983 tariff level is -.152, and the R^2 is .31. The results do not change much when we add the industry's 1984 share of the unskilled workers as a regressor (column 3), since the initial tariff level and the share of unskilled workers are highly correlated. We therefore continue to focus on the 1983 tariff levels as the main determinant of tariff reductions.

Given that the decision to liberalize the economy as a whole was influenced by exchange rate fluctuations, we interact the 1983 tariff with the nominal effective exchange rate in column 4 of table 7a.[20] This yields an industry-specific, time-varying variable that reflects that industries with lower tariffs are likely more exposed to exchange rate fluctuations than industries with high tariffs. The negative coefficient on the variable confirms the anecdotal evidence that industries with historically high tariffs experienced bigger tariff reductions during times of exchange rate devaluations. In column 5, we allow the tariff changes to vary with both the 1983 tariff level and the interaction of the tariff level with the exchange rate. Once again, the negative coefficient on the interaction term confirms that industries with historically higher tariff levels experienced larger tariff declines during exchange rate depreciation.

Overall, our findings suggest that the 1983 industry tariff levels, and their interaction with exchange rates, are highly correlated with the industry tariff reductions and may provide good instruments for the tariff changes. Consider the following industry-level first-difference regression framework for industry j:

$$\Delta wp_{jt} = \alpha + \eta * \Delta t_{jt} + u_{jt}. \tag{3}$$

Δwp_{jt} denotes the change in industry wage premium for industry j between t-1 and t, and Δt_{jt} denotes the change in tariffs in industry j between t-1 and t. The error term u may include variables omitted from the specification that drive changes in industry wages. Assuming that such omitted factors are uncorrelated with tariff levels in 1983, we can use the tariff levels in 1983 as instruments for tariff changes, and estimate (3) using two-stage least squares (2SLS).

Table 7b contains the estimates of equation (3) for all industries using the wage premiums from specification 2 (WP2). Column 1 reports the first difference results when we do

[20] An increase in the exchange rate implies an exchange rate appreciation. We do not include exchange rates without interacting them with other variables in the regression, because the year indicators already control for macroeconomic variables that affect tariff changes.

not instrument for tariff changes as a baseline. As discussed in section 6.1, the coefficient is positive and significant. Columns 2-4 report 2SLS results. Although the magnitude of the tariff coefficient changes, the positive (and statistically significant) relationship between tariff reductions and declines in industry wage premiums is robust. The estimated effect of liberalization on wages drops however from .0012 in column 1, to .0005 in column 2. The coefficient of .0005 implies that a 50-point tariff reduction would lead to a 2.5 percent decline in wage premiums. The coefficient estimates hardly change when we instrument for the change in tariffs with the interaction of the 1983-tariff with the exchange rate (column 3), or with both the 1983-tariff, and its interaction with the exchange rate (column 4).

In sum, our results demonstrate the importance of accounting for the political economy of protection when estimating the effects of trade liberalization on wages. Controlling for time-invariant political economy factors reversed the estimated relationship between wages and protection. Controlling for time-varying, industry-specific effects did not have quite as dramatic an effect (the positive relation between tariffs and wages remained robust), but it substantially reduced the estimated effect of protection on wages.

7. Conclusions

This paper set out to exploit the Colombian trade liberalization experiment to investigate the relationship between trade policy and industry wage premiums. Our main finding is that in sectors with larger tariff reductions wages declined relative to the economy-wide average. To obtain this finding we utilized detailed information on worker and firm characteristics that allowed us to control for observed industry heterogeneity of workers across industries, and the panel nature of our industry-level data that allowed us to control for unobserved heterogeneity and political economy factors through industry fixed effects. Conditioning on time-invariant industry attributes reversed the sign of the relationship between tariffs and industry wage differentials from negative (the sign found in previous work) to positive. These results were robust to the inclusion of trade flow variables, and their interactions with exchange rates. More importantly, the positive relationship was robust to using instrumental variables to account for time-varying political economy factors affecting trade policy changes (albeit the magnitude of the effect decreased).

Our results are in line with trade models in which labor mobility across sectors is constrained in the short run. Moreover, since the tariff cuts were concentrated in sectors with a high proportion of unskilled workers, our findings suggest an additional channel through which income inequality may have been affected: not only was the skill premium rising in the 1980s and 1990s, less-skilled workers experienced an additional decrease in their relative incomes because the industries in which they were employed experienced a decline in their wage premiums relative to industries with more skilled workers.

Our work can be extended in several directions to offer a more complete picture of effects of trade liberalization on the labor market. For example, it would be desirable to check the robustness of our results to measures of effective protection. Unfortunately, data on effective rates of protection are not readily available for our sample period. Previous studies suggest that tariffs and effective protection are highly correlated before and after the major trade liberalization of 1990. Fernandes (2001) reports a correlation of .91 for 1983, 1984, 1989, and 1990. The correlation coefficient between the effective protection and tariff measures computed for 1995 is .93 (Echavarria, Gamboa, Guerrero (2000)). This makes it likely that the results for effective rates will be similar. A further extension could involve examination of productivity effects on wages. The empirical evidence to date suggests that trade reform leads to productivity increases. If these increases translate to higher wages, then the tariff coefficients in the second stage may understate the pure trade protection effect on wages. In other words, the trade liberalization in Colombia might have led to larger decreases in wage premiums, were it not for reform induced productivity increases that were passed on to workers in form of higher wages. Controlling for sectoral productivity changes in the estimation would allow us to decompose the estimated effect into a "pure trade barrier", and a "productivity" effect.

Finally, it would be desirable to investigate the employment responses in each industry. In particular, one could examine whether there is any evidence of labor reallocation across sectors differentially affected by the reforms along the lines suggested by the Hecksher-Ohlin model. In the same vein, it would be interesting to examine more thoroughly the differences in the response to trade liberalization across the formal and informal sectors. One straightforward extension is to consider how wage premiums were affected by trade reform in the formal and informal sectors separately. A more difficult question is whether trade reform led to movements from one sector to the other, and had thus allocative effects across the two sectors of the economy. These are questions we leave up to future research.

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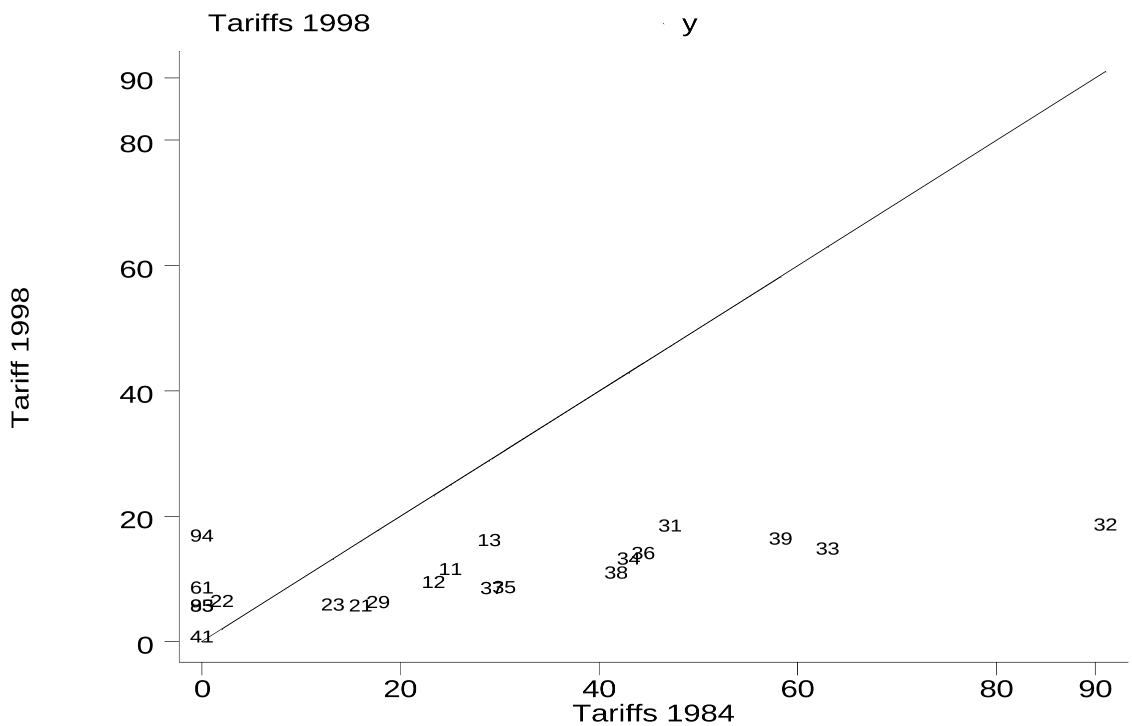
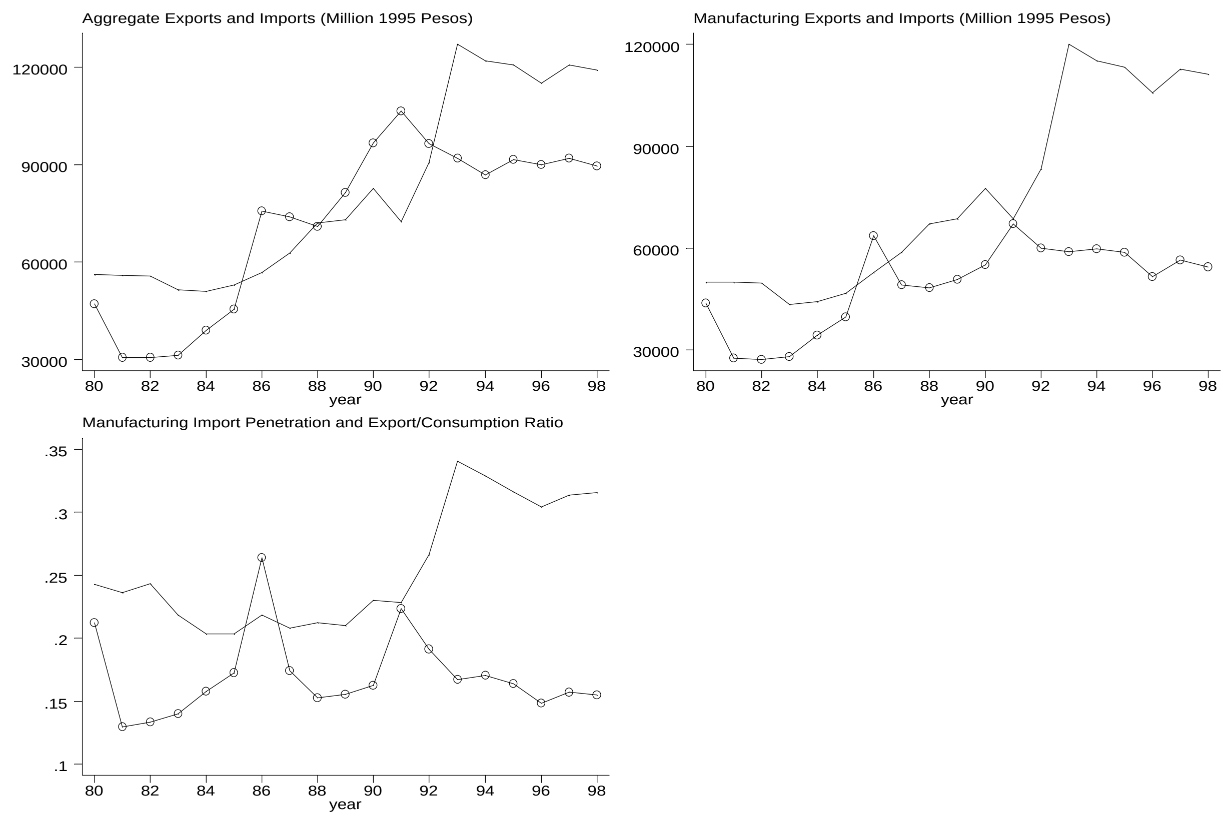
Figure 1—Industry Tariffs in 1984 and 1998



Note: the line is a 45 degree line.

Figure 2—Various Measures of Trade flows 1980 – 1998



Note: exports (line with circles), imports (solid line).

Figure 3—Hourly and Weekly wage premiums (based on specification 2)

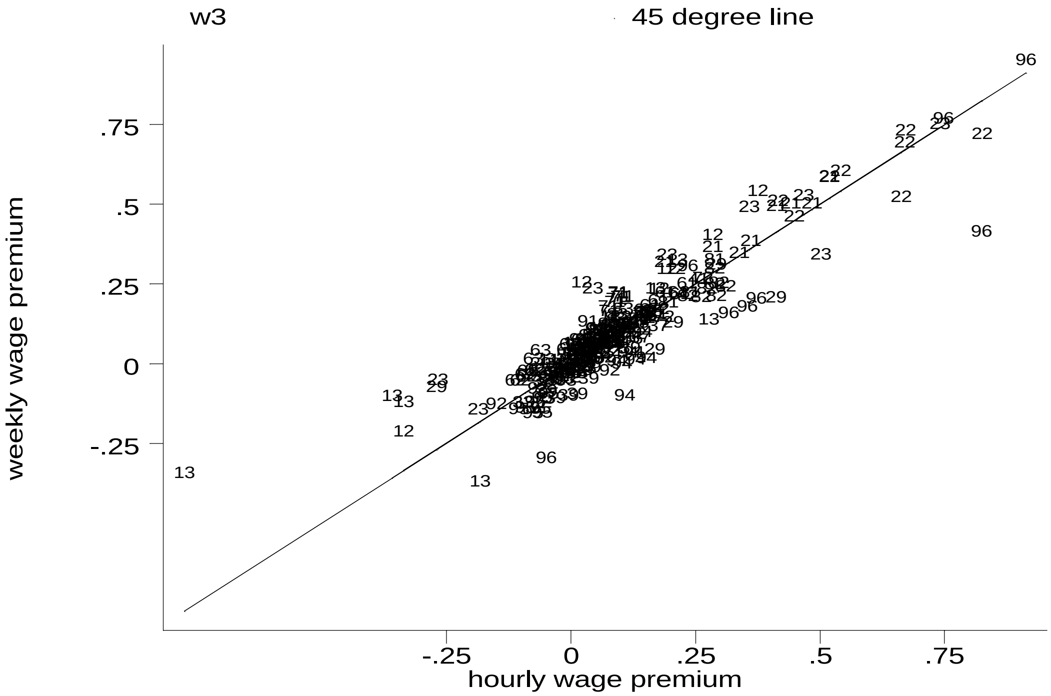
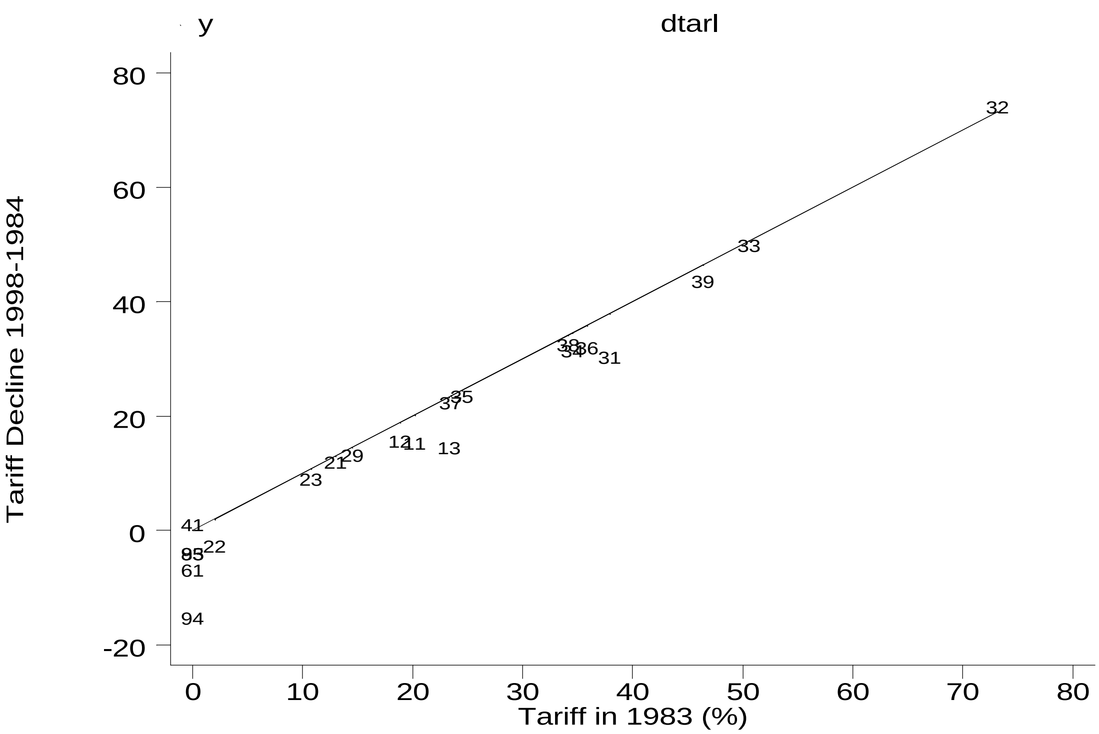


Figure 4--Tariff Decline 1998-1984 and Tariffs in 1983



Note: the line is a 45 degree line.

Table 1a--Summary statistics for Tariffs 1984-1998

Year	N	Mean	S.D.	Min	Max
All Industries					
1984	21	27.4	24.8	0.0	91.0
1985	21	22.2	16.7	0.0	50.1
1988	21	20.7	16.0	0.0	48.7
1990	21	17.5	14.0	0.0	38.7
1992	21	10.6	4.1	5.0	17.7
1994	21	9.7	4.8	0.0	17.8
1996	21	9.8	5.1	0.0	17.9
1998	21	9.9	5.1	0.0	17.9
Agriculture, Mining, Manufacturing					
1984	16	35.9	22.1	2.0	91.0
1985	16	29.2	12.6	10.0	50.1
1988	16	27.2	12.4	10.0	48.7
1990	16	22.9	11.3	5.0	38.7
1992	16	10.4	4.2	5.0	17.7
1994	16	10.7	4.4	5.0	17.8
1996	16	10.8	4.5	5.0	17.9
1998	16	10.9	4.5	5.0	17.9
Manufacturing					
1984	9	49.8	19.0	29.2	91.0
1985	9	36.6	9.5	22.5	50.1
1988	9	33.5	11.1	17.1	48.7
1990	9	29.1	9.1	15.2	38.7
1992	9	12.9	3.4	8.4	17.7
1994	9	12.9	3.6	8.0	17.8
1996	9	13.0	3.9	7.5	17.9
1998	9	13.1	3.8	7.8	17.9

Note: N stands for number of industries in a given year. Source: Authors' calculations based on tariff data provided by DNP.

Table 1b--Correlation of Tariffs over Time

	1984	1985	1988	1990	1992	1994	1996	1998
1984	1.000							
1985	.943	1.000						
1988	.929	.992	1.000					
1990	.918	.981	.984	1.000				
1992	.548	.456	.461	.489	1.000			
1994	.774	.811	.819	.827	.734	1.000		
1996	.713	.745	.759	.766	.702	.810	1.000	
1998	.716	.749	.761	.768	.700	.810	1.000	1.000

Source: Authors' calculations based on tariff data provided by DNP.

Table 2--National Household Survey Summary Statistics

	1984	1986	1988	1990	1992	1994	1996	1998
Hourly wage (current pesos)	115.4	168.7	259.1	430.5	686.9	1337.6	1850.6	2725.0
log hourly wage	4.4	4.8	5.2	5.7	6.1	6.7	7.0	7.4
Weekly wage (current pesos)	5109.0	7158.4	11396.0	18787.2	30000.1	59260.2	79884.4	112281.7
log weekly wage	8.2	8.5	9.0	9.5	9.9	10.5	10.8	11.2
Male	.622	.619	.601	.606	.587	.591	.589	.553
Age	33.7	33.8	33.9	34.3	34.3	34.7	35.2	35.6
Married	.427	.413	.385	.411	.392	.357	.358	.356
Head of the household	.471	.468	.453	.474	.459	.462	.464	.457
Literate	.970	.973	.978	.980	.978	.985	.982	.981
No complete schooling	.218	.197	.178	.155	.144	.121	.118	.119
Elementary school complete	.489	.479	.480	.479	.473	.465	.434	.393
Secondary school complete	.218	.238	.250	.264	.282	.304	.326	.350
University complete*	.076	.087	.092	.102	.101	.109	.121	.137
Lives in Bogota	.434	.435	.424	.429	.402	.524	.439	.386
Occupation Indicators								
Professional/Technical	.103	.103	.107	.109	.113	.111	.121	.135
Management	.012	.013	.013	.018	.020	.020	.016	.021
Personnel	.138	.133	.128	.126	.124	.137	.130	.132
Sales	.180	.186	.195	.192	.190	.191	.201	.196
Servant	.194	.196	.188	.185	.191	.172	.174	.194
Agricultural/Forest	.013	.013	.015	.016	.013	.009	.010	.010
Manual Manufacturing	.360	.356	.354	.353	.348	.360	.347	.312
Job Type Indicators								
Private Employee	.530	.550	.551	.546	.564	.585	.569	.523
Government Employee	.118	.116	.107	.108	.099	.080	.085	.089
Private Household Employee	.064	.067	.058	.054	.050	.035	.032	.047
Self-employed	.242	.220	.227	.227	.224	.234	.261	.282
Employer	.046	.047	.056	.065	.064	.066	.053	.059
Place of work characteristics								
Work in single-person establishment		.250	.244	.253	.247	.252	.263	.311
Work in 2 to 5 person establishment		.218	.223	.192	.215	.193	.205	.196
Work in 6-10 person establishment		.080	.093	.063	.083	.085	.078	.073
Work in 11 or more person establishment		.451	.440	.492	.455	.470	.454	.420
Work in a building		.597	.600	.674	.608	.615	.616	.597
Work in informal sector		.577	.568	.574	.564	.516	.609	.590
Number of years at current job		5.7	5.8	5.8	5.9	6.3	6.5	6.2
Employed Prior to current job		.547	.592	.451	.555	.518	.552	.607
Number of observations	36,717	28,481	31,006	25,950	27,521	18,070	27,365	30,092

Note: The reported means are weighted using survey weights. We define complete university if a person completes 5 or more years of post secondary education. The number of observations for number of years at current job and employed prior to current job is lower than the reported one. However, we don't eliminate observations with those missing variables because we do not use them in most of the paper.

Table 3--Correlation of Wage Premiums Across Years

Wage premiums based on Specification 2

	1984	1986	1988	1990	1992	1994	1996	1998
1984	1.00							
1986	0.71	1.00						
1988	0.94	0.76	1.00					
1990	0.80	0.62	0.76	1.00				
1992	0.58	0.64	0.60	0.84	1.00			
1994	0.51	0.48	0.48	0.82	0.89	1.00		
1996	0.67	0.16	0.58	0.62	0.36	0.38	1.00	
1998	0.62	0.89	0.73	0.40	0.40	0.21	0.14	1.00

Wage premiums based on Specification 3

	1984	1986	1988	1990	1992	1994	1996	1998
1984								
1986		1.00						
1988		0.75	1.00					
1990		0.61	0.73	1.00				
1992		0.64	0.59	0.83	1.00			
1994		0.52	0.52	0.86	0.90	1.00		
1996		0.05	0.52	0.58	0.34	0.40	1.00	
1998		0.88	0.74	0.39	0.42	0.26	0.06	1.00

Note: All correlations are statistically significant.

Table 4a--Industry Wage premiums and tariffs

Dept Var.	Independent Var.	(1)	(2)	(3)	(4)
All (N=168, 21 industries)					
WP1	Nominal tariff	.0017	.0019	.0010 **	.0008 **
		(.0038)	(.0044)	(.0002)	(.0003)
WP2	Nominal tariff	.0001	-.0001	.0008 **	.0007 **
		(.0009)	(.0012)	(.0001)	(.0001)
WP3 (N=147)	Nominal tariff	-.0009	-.0011	.0009 **	.0009 **
		(.0011)	(.0015)	(.0002)	(.0003)
Manufacturing, Mining, Agriculture (N=128, 16 industries)					
WP1	Nominal tariff	-.0022 **	-.0082 **	.0011 **	.0004
		(.0010)	(.0016)	(.0003)	(.0004)
WP2	Nominal tariff	-.0005	-.0031 **	.0008 **	.0007 **
		(.0005)	(.0009)	(.0001)	(.0003)
WP3 (N=112)	Nominal tariff	-.0011 *	-.0058 **	.0008 **	-.0002
		(.0006)	(.0015)	(.0002)	(.0006)
Manufacturing (N=72, 9 industries)					
WP1	Nominal tariff	-.0016 *	-.0080 **	.0011 **	.0001
		(.0010)	(.0013)	(.0003)	(.0004)
WP2	Nominal tariff	-.0002	-.0024 **	.0008 **	.0006
		(.0004)	(.0008)	(.0001)	(.0004)
WP3 (N=63)	Nominal tariff	-.0005	-.0042 **	.0008 **	-.0003
		(.0004)	(.0006)	(.0002)	(.0006)
	Year Indicators	no	yes	no	yes
	Industry Indicators	no	no	yes	yes

Note: ** and * indicate 5 and 10 % significance, respecitively. Reported standard errors are robust and clustered on industry. Rows indicate separate regressions using different industry samples (see bold headings) and different wage premiums (wp1-wp3) as a dependent variable. The information about the inclusion of year and industry indicators at the bottom of each column applies to all specifications reported in a given column.

Table 4b--Wage premia and tariffs, first difference specification

Dept Var.	Independent Var.	(1)	(2)
All (N=147)			
WP1	Nominal tariff	.0006 **	.0009 **
		(.0002)	(.0003)
WP2	Nominal tariff	.0010 **	.0012 **
		(.0002)	(.0003)
WP3 (N=126)	Nominal tariff	.0013 **	.0014 **
		(.0005)	(.0005)
Manufacturing, Mining, Agriculture (N=112)			
WP1	Nominal tariff	.0006 **	.0019 **
		(.0002)	(.0004)
WP2	Nominal tariff	.0012 **	.0016 **
		(.0003)	(.0003)
WP3 (N=96)	Nominal tariff	.00132 *	.0008
		(.00077)	(.0014)
Manufacturing (N=63)			
WP1	Nominal tariff	.0005 **	.0016 **
		(.0002)	(.0004)
WP2	Nominal tariff	.0011 **	.0014 **
		(.0003)	(.0004)
WP3 (N=54)	Nominal tariff	.0013	.0001
		(.0008)	(.0013)
	Year Indicators	no	yes
	Industry Indicators	no	no

Note: ** and * indicate 5 and 10 % significance, respecitively. Reported standard errors are robust and clustered on industry. Rows indicate separate regressions using different industry samples (see bold headings) and different wage premiums (wp1-wp3) as a dependent variable.

Table 5a--Industry wage premiums and trade exposure measures (All Industries)

Dept Var.	Independent Var.	(1)	(2)	(3)	(4)
WP1	Nominal tariff	.0013	.0012	.0010 **	.0010 **
		(.0029)	(.0034)	(.0003)	(.0004)
	Lagged Imports	.00073 *	.00075 *	-.00029 **	-.00025 **
		(.00040)	(.00039)	(.00006)	(.00009)
	Lagged Exports	.00041	.00044	.00025	.00036
		(.00055)	(.00052)	(.00020)	(.00023)
WP2	Nominal tariff	.0001	-.0002	.0007 **	.0007 **
		(.0008)	(.0010)	(.0001)	(.0002)
	Lagged Imports	.00017	.00018	-.00008 **	-.00007 **
		(.00012)	(.00012)	(.00002)	(.00003)
	Lagged Exports	-.00003	.00001	.00002	.00007
		(.00015)	(.00014)	(.00012)	(.00011)
WP3	Nominal tariff	-.0008	-.0010	.0009 **	.0010 **
		(.0010)	(.0014)	(.0002)	(.0003)
	Lagged Imports	.00003	.00004	-.00003	-.00004
		(.00009)	(.00009)	(.00002)	(.00002)
	Lagged Exports	-.00008	-.00006	.00008	.00010
		(.00011)	(.00011)	(.00013)	(.00012)
	Year Indicators	no	yes	no	yes
	Industry Indicators	no	no	yes	yes

Note: ** and * indicate 5 and 10 % significance, respecitively. Reported standard errors are robust and clustered on industry. The four sections indicate separate regressions using different wage premiums (wp1-wp3) as a dependent variable. The information about the inclusion of year and industry indicators at the bottom of each column applies to all specifications reported in a given column. Imports and exports are expressed in real 1995 100 millions of pesos. N is 168. For WP3, N=147.

Table 5b--Manufacturing Wage premiums and trade exposure measures

Dept Var.	Independent Var.	(1)	(2)	(3)	(4)
WP1	Nominal tariff	-.0004	-.0061 **	.0009 **	.0001
		(.0007)	(.0014)	(.0003)	(.0004)
	Lagged Imp. Penetration	.3872 **	.2349 **	-.1924 **	-.1420 **
		(.1316)	(.1142)	(.0859)	(.0478)
	Lagged Export/Consumption	-.0737 **	-.0447 *	.0412 **	.0304 **
		(.0306)	(.0258)	(.0187)	(.0109)
WP2	Nominal tariff	.0003	-.0017 **	.0007 **	.0006
		(.0003)	(.0008)	(.0001)	(.0004)
	Lagged Imp. Penetration	.1429 **	.0918 *	-.0751 **	-.0719 **
		(.0492)	(.0489)	(.0235)	(.0197)
	Lagged Export/Consumption	-.0275 **	-.0184 *	.0167 **	.0152 **
		(.0106)	(.0101)	(.0050)	(.0047)
WP3	Nominal tariff	.0001	-.0045 **	.0007 **	-.0002
		(.0004)	(.0006)	(.0002)	(.0007)
	Lagged Imp. Penetration	.0877 **	-.0224	-.0566 **	-.0640 **
		(.0434)	(.0209)	(.0192)	(.0172)
	Lagged Export/Consumption	-.0188 **	.0026	.0131 **	.0138 **
		(.0096)	(.0043)	(.0043)	(.0041)
	Year Indicators	no	yes	no	yes
	Industry Indicators	no	no	yes	yes

Note: ** and * indicate 5 and 10 % significance, respecitively. Reported standard errors are robust and clustered on industry. The four sections indicate separate regressions using different wage premiums (wp1-wp3) as a dependent variable. The information about the inclusion of year and industry indicators at the bottom of each column applies to all specifications reported in a given column. N is 72. For WP3, N=63.

Table 6a-- Industry wage premiums and exchange rates (All Industries)

Dept Var.	Independent Var.	(1)	(2)	(3)	(4)
WP1	Nominal tariff	.0009	.0010	.0010 **	.0010 **
		(.0030)	(.0033)	(.0003)	(.0004)
	Lagged Imports	-.00005	-.00007	-.00030 **	-.00026 **
		(.00028)	(.00021)	(.00005)	(.00007)
	Lagged Export	.00031	.00026	.00018	.00031 *
		(.00097)	(.00091)	(.00014)	(.00019)
	Lagged Imports*Ex.Rate	.0000073 **	.0000075 **	.0000004	.0000003
		(.0000022)	(.0000024)	(.0000003)	(.0000006)
	Lagged Exports*Ex.Rate	.0000014	.0000017	.0000013 **	.0000009 **
		(.0000040)	(.0000035)	(.0000004)	(.0000004)
WP2	Nominal tariff	-.0001	-.0002	.0007 **	.0007 **
		(.0008)	(.0010)	(.0001)	(.0002)
	Lagged Imports	-.00010	-.00006	-.00009 **	-.00008 **
		(.00009)	(.00007)	(.00001)	(.00002)
	Lagged Export	-.00013	-.00007	-.00001	.00004
		(.00026)	(.00023)	(.00009)	(.00011)
	Lagged Imports*Ex.Rate	.0000025 **	.0000022 **	.0000004 **	.0000003
		(.0000007)	(.0000007)	(.0000002)	(.0000004)
	Lagged Exports*Ex.Rate	.0000011	.0000007	.0000006 **	.0000005 **
		(.0000011)	(.0000008)	(.0000001)	(.0000002)
WP3	Nominal tariff	-.0011	-.0011	.0009 **	.0009 **
		(.0011)	(.0013)	(.0003)	(.0003)
	Lagged Imports	-.00039 **	-.00041 **	-.00022 **	-.00023 **
		(.0001)	(.0001)	(.0001)	(.0001)
	Lagged Export	-.00047	-.00049	.00007	.00009
		(.0004)	(.0004)	(.0002)	(.0002)
	Lagged Imports*Ex.Rate	.0000044	.0000046 **	.0000024 **	.0000026 **
		(.0000016)	(.0000017)	(.0000009)	(.0000010)
	Lagged Exports*Ex.Rate	.0000039	.0000041	.0000002	.0000002
		(.0000029)	(.0000027)	(.0000009)	(.0000009)
	Year Indicators	no	yes	no	yes
	Industry Indicators	no	no	yes	yes

Note: ** and * indicate 5 and 10 % significance, respecitively. Reported standard errors are robust and clustered by industry. The four sections indicate separate regressions using different wage premiums (wp1-wp3) as a dependent variable. The information about the inclusion of industry indicators at the bottom of each column applies to all specifications reported in a given column. Number of observations is 168, except for WP3, where the number of observations is 147 due to lack of informal sector information in 1984.

Table 7a: Determinants of Trade Policy Changes
(dependent variable is annual change in tariffs)

	(1)	(2)	(3)	(4)	(5)	(6)
Tariff in 83	-.1521 **		-.1543 **		.2839 *	.2817
	(.0056)		(.0063)		(.1694)	(.1700)
Exchange Rate*Tariff in 83				-.0015 **	-.0041 **	-.0041 **
				(.0001)	(.0016)	(.0016)
Share of Unskilled Workers in 1984		-9.2122 **	.4954			.4954
		(3.8775)	(.5430)			(.5450)
R^2	.313	.179	.313	.350	.376	.376
Year Indicators	yes	yes	yes	yes	yes	yes

Note: ** and * indicate 5 and 10 % significance, respecitively. Reported standard errors are robust and clustered by industry. The mean of the exchange rate is 107 for 1986-1998. N is 147. The mean exchange rate is 107 for 1986-1998.

Table 7b: Industry Wage premiums and tariffs, First Differences, 2SLS results
(WP2 is the dependant variable)

	(1)	(2)	(3)	(4)
Nominal tariff	.0012 **	.0005 **	.0004 **	.0004 *
	(.0003)	(.0002)	(.0001)	(.0002)
Instrument	none	tariffs 83	exchange rate* tariffs 83	tariffs 83, exchange rate* tariffs 83
Year Indicators	yes	yes	yes	yes

Note: ** and * indicate 5 and 10 % significance, respecitively. Reported standard errors are robust and clustered by industry. N is 147.