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ABSTRACT

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

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 industry wage premiums, adjusting for a series of worker, job, and firm characteristics. We find that Colombian industry wage premiums exhibit remarkably less persistence over time than US wage premiums. Similarly, tariffs are less correlated over time than in the US data, indicating that trade liberalization has changed the structure of protection. We next relate wage premiums to trade policy 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 labour is immobile across sectors, or, alternatively, with the existence of industry rents that are reduced by trade liberalization. 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. These problems are particularly severe when quantity measures are used. As has been pointed out before, in general equilibrium trade models, trade affects wages through prices that are set on the margin, and not through quantities. The use of price data on the other hand raises other issues: prices are plagued by measurement problems, and are simultaneously determined with wages. As Freeman (1995) points out, "perhaps the biggest problem with these studies is that they ignore potential determinants of sectoral prices …. save for trade".[2] Similarly, Haskel and Slaughter (2001) argue that relying on product prices could be problematic since little is known about "how much domestic price variation is caused by international trade, such as changes in trade barriers".[3]

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. 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

[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] Freeman, R. (1995), p. 29.
[3] Haskel, J. and M. Slaughter (2001), p. 164.

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.[4]

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 of drastic tariff reductions to levels comparable to those in developed countries.[5] Tariffs are both well measured and -- contrary to NTB measures -- comparable across time. In addition, the period 1985-1994 includes multiple tariff reduction episodes that affected not only the *average* tariff, but also the *structure* of protection across industries. Figure 1 plots tariffs in 1984 against tariffs in 1998 and nicely portrays why the Colombian trade liberalization provides an excellent setting to address the impact of trade on labor markets. Not only do tariffs exhibit large variation 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. Hence, our data provides ample variation to identify the effects of trade policy on wages.

A further advantage of focusing on a country like Colombia that was not a GATT or WTO member prior to the trade reforms, is that the government's objective when reducing tariff rates was dictated by the WTO negotiations. In particular, this objective was to achieve a uniform tariff rate of 13% across industries. Policy makers had accordingly less room to cater to special lobby interests; from an individual industry's perspective, the target tariff rate was exogenously predetermined, implying that tariff declines in each industry were proportional to the industry's pre-reform tariff level in 1983. This is illustrated in Figure 2 that shows a strong positive relationship between the 1983-1998 tariff changes and the initial, pre-reform tariff rates. We exploit this particular feature of the reforms to construct instruments for the annual tariff changes based on interactions of the pre-reform tariff rates with macroeconomic variables.

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

[4] A notable exception to this pattern is the paper by Gaston and Trefler (1994) that we refer to in more detail below.
[5] 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.

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. Moreover, they assume perfect competition. 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, in which industry rents may be passed on to workers as higher wages. These models seem particularly relevant in developing economies (like Colombia) where labor market rigidities obstruct labor mobility across sectors, and where markets have been (at least until recently) highly protected. 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 experience declines in their relative incomes. This effect is conceptually distinct from the potential effect of trade liberalization on the skill premium. In this sense, less-skilled 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.. 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 (2001), and several papers on Chile and Colombia by Robbins, to name only a

few).[6] 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.*

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. The implications of our estimates for changes in the income distribution are discussed in detail in Section 6.3, and the concluding section of the paper.

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

[6] 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.

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 imply a different relationship between protection and wages than the one 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 (2002), 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 ($\ln(w_{ijt})$) on a vector of worker i's characteristics (H_{ijt}) such as education, age, gender, dummies for formality of employment, geographic location, and a set of industry indicators (I_{ijt}) reflecting worker i's industry affiliation:

$$\ln(w_{ijt}) = H_{ijt}\beta_H + I_{ijt} * wp_{jt} + \varepsilon_{ijt} \qquad (1)$$

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_{jt}).[7] 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.[8] 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.

$$wp_{jt} = T_{jt}\beta_T + D_{jt}\beta_D + u_{jt} \qquad (2)$$

We interpret (2) as a reduced form relationship, consistent with the alternative theoretical interpretations outlined in Section 2 (e.g., specific factors-, Ricardo-Viner model, or a model with imperfect competition giving rise to industry rents). We are not interested in testing among these models. Instead, it is the reduced form relationship that is of interest here – that is, the response of relative wages to a trade policy change. 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 later in 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, industry capital accumulation, NTB measures, and interactions of a subset of the above variables with exchange rates. The vector D_{jt} consists of a set of industry and

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

[8] 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.

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, to obtain:

$$\Delta wp_{jt} = \eta * \Delta t_{jt} + \Delta T'_{jt}\beta'_T + D'_{jt}\beta'_{D'} + u'_{jt} \qquad (2')$$

where Δwp_{jt} denotes the change in industry wage premium for industry j between t-1 and t, Δt_{jt} denotes the change in tariffs in industry j between t-1 and t, $\Delta T'_{jt}$ denotes the one-period change in trade-related variables other than tariffs, and D'_{jt} denotes a set of other controls, such as year effects.

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. These characteristics control for compositional differences across industries. 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 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. 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 may seem less critical, since industry fixed effects can capture differences in composition across industries. However, this strategy would fail if industry composition, or returns to particular characteristics (such as education) shifted over time.

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 (or first-differencing) approach requires is that time varying unobserved characteristics that affect earnings are uncorrelated with trade policy. This assumption is relaxed in the next subsection where we instrument for tariff rate changes.

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, industry capital accumulation, and most importantly, exchange rates. Second, we instrument for tariff changes, exploiting information on pre-sample protection measures, world coffee prices and exchange rates. Our instrumental variable strategy is described in more detail in the next subsection.

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 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.

3.1 Instrumenting for Trade Protection

While the fixed-effects or first-difference estimation controls for time invariant unobserved industry heterogeneity, two empirical concerns remain. First, as discussed above, there could be unobserved time-varying political economy factors, which simultaneously affect tariff formation and industry wages. More generally, despite our best efforts to control for other sector specific factors that may have affected relative wages during this period (see also empirical section), we cannot completely eliminate the possibility that some omitted variable that is correlated with tariff changes induces spurious correlation. The second related concern is that there could be time-varying selection into industries, based on unobserved worker characteristics. The bias introduced by this selection could go either way. In particular, if trade liberalization causes the more able (or more productive) workers to leave sectors that experience large tariff cuts, so that the remaining workers represent a less able (in terms of unobserved characteristics) sample, we would expect the estimated tariff coefficient to be biased upwards. In contrast, if firms respond by laying-off the less motivated, or less productive workers, so that the remaining workers represent a more able sample, our estimated tariff coefficient will understate the true effect of trade liberalization on wages. Put differently, our tariff coefficient in equations (2) or (2') captures both the "pure" effect of trade liberalization on relative wages, and a potential compositional effect in terms of unobserved characteristics.

To address the above concerns we instrument for trade policy changes. 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 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. The trade balance in Colombia has in turn always been heavily influenced by world coffee prices (see Roberts and Tybout (1997)), since coffee

is a major export of this country. This indicates that at the macroeconomic level, exchange rates and world coffee prices are some of the factors responsible for the trade policy changes. However, exchange rates or coffee prices 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 (see Figure 5); sectors with a high share of unskilled workers (where unskilled is defined as having at most primary education) had higher tariffs.[9] Second, because the tariff reductions were implemented as part of Colombia's entry process into the WTO, the target level for the final tariff rate was set at a uniform rate of 13%, implying that there was little (if any at all) room for industry lobbying[10]; from an individual industry's point of view, the tariff rate at the end of the trade liberalization period was exogenously predetermined. These two facts together imply that tariff reductions were proportionately larger in sectors that had historically higher tariff levels. This is best demonstrated in Figure 2 that 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.

The above discussion suggests that the pre-reform tariff rates are powerful instruments for the annual tariff changes in each sector. We interact these 1983 tariff levels with annual exchange rates, or, alternatively, world coffee prices to create industry-specific, time-varying instruments. Equation (2') is then estimated using 2SLS. The construction of the instruments is discussed in more detail in the empirical section.

In sum, our choice of instruments is based on two important features of the reforms: that tariff reductions in each sector were proportional to the initial, pre-reform tariff levels since the goal was to achieve a predetermined, uniform across sectors, tariff rate; and that the pace of the tariff cuts in each year was influenced by macroeconomic factors, such as exchange rates and world coffee

[9] 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.

[10] In reality, some dispersion in tariff rates remained even after the trade reforms, but this dispersion is substantially smaller than the pre-reform tariff rate dispersion. See Figure 1.

prices. The underlying identification assumption is that – *after purging sector-specific effects through first-differencing* - the pre-reform tariff levels (interacted with exchange rates or coffee prices) affect year-to-year changes in wage premiums *only through* the effect that these initial tariff levels have on annual tariff reductions.

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.[11] 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. Tariff levels declined throughout the period, but the most radical reforms took place in 1985 and 1990-1991. The 1985 tariff cuts almost 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.

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.[12] The

[11] 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)).

[12] 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

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. Table 1b reports tariff correlations over time and confirms that the structure of protection has changed during our sample period. 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. As is the case with tariffs, NTB protection varies widely across industries, with textiles and apparel industry and the manufacturing of wood and wood products enjoying the highest level of protection. Because of the aforementioned measurement problems associated with NTBs, and because these measures are at any rate available only for three years, we do not include NTBs in the estimation. In the three years in which we have NTB data, tariffs and NTBs are however positively correlated. Thus, it is not the case that tariff levels get reduced only to be replaced by less transparent NTBs, as it happened in the U.S. in the mid-1980's.

From a theoretical point of view, it would be desirable to employ effective rather than nominal rates of protection, since the former account for intermediate inputs, and thus measure protection more accurately. Unfortunately, data on effective rates of protection are not readily available for our sample period. Fortunately for us, previous studies suggest that nominal tariffs and effective rates of 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)). Based on these correlations, we believe that the results for effective rates are likely to be similar to the ones obtained with nominal rates.

The shifts in Colombia's trading environment are reflected in the import and export flows. Figure 3 shows the evolution of aggregate imports and export (and manufacturing exports and

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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.

imports) from 1980 and 1998 measured in real 1995 millions of pesos.[13] 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 a year.[14] 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.[15] 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,

[13] 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).

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

[15] 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.

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, Edwards (1999) and anecdotal evidence suggests that unions are ineffective in most industries. The only exception is the union in the petroleum industry, 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 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.[16] 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

[16] This information is not available in 1984.

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 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 two specifications. Both 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}. Specification 1 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 2 adds workplace characteristics (informal sector indicator, size of the establishment indicators, and type of establishment indicator) to specification 1. In section 6, we refer to wage differentials from these three specifications as WP1 and WP2, 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 4 plots the relationship between hourly and weekly industry wage premiums based on specification 1. 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 from the first stage 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. As mentioned above, these characteristics control, among other things, for potential general equilibrium effects of trade liberalization. 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.[17]

We next check how much of the variation in log hourly wages the different specifications of equation (1) explain. The R^2 in specification that only includes industry indicators and no worker characteristics 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 2. 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 the raw data, to about 7 to 9 percent in specification 2. 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 correlation between wage premiums from specification 1 and wage premiums for specification 2 is .90. 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

[17] 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 for 1994, the correlation of the wage premiums with these additional controls with the wage premiums from specification 2 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, we computed year-to-year correlations in wage premiums based on specifications 1 and 2. These correlations range from .14 to .94. For example, for specification 1, 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 2. 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 0.9.[18] 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.

[18] Krueger and Summers (1988) report a correlation of 0.91 between the 1974 and 1984 wage premiums. Robertson (1999) reports a correlation of 0.92 between the 1987 and 1997 U.S. wage premiums.

6. Trade and Wage Premiums

6.1 Main Results

Our main results concerning the relationship between trade policy and industry wages, based on estimation of equations (2) or (2'), are contained in Table 3a. Our sample consists of all industries with available tariff information, including those with relatively little trade exposure such as wholesale trade, electricity, real estate and business services. We include these industries both to avoid introducing potential selection bias by focusing only on a subset of sectors with "high" tariff rates, and to exploit the additional cross-sectional variation arising from the fact that tariff rates (and changes) in these sectors are relatively low. The left panel of the table corresponds to the specification of the wage premium (WP1) that is conditional on worker demographic characteristics, while the right hand side of the panel reports the results based on the second specification of wage premium (WP2) that is conditional on worker demographic characteristics, firm attributes, and informality. Because the firm and informality information is missing in the first year of our sample, 1984, we are forced to drop 1984 from the estimation when we use WP2. Excluding 1984 from the estimation is costly given that tariff rates were substantially reduced between 1984 and 1985. In subsequent specifications we therefore prefer to use the WP1 definition that allows us to exploit the full sample. The regressions in Table 3a based on WP2 serve as a robustness check to ensure that the results do not change substantially when we control for firm characteristics and informality in the computation of wage premiums.

All specifications in Table 3a and subsequent tables include year indicators. Year indicators allow for the average wage premium to change over time 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 addition, year indicators control for the potential effects of the labor reform on wage premiums. Previous work (see Kugler (1999)) finds no evidence that the 1990 Colombian labor market reform affected different industries differentially, so that the labor reform effects can be adequately captured by year indicators.

We start by estimating equation (2) without industry indicators, and without first differencing (columns (1) and (4)). The reason we do this, is that this specification is the closest

analog to earlier work that has estimated (2) exploiting only cross-sectional data (we elaborate on this point below). The tariff coefficient is negative and insignificant. However, there are good reasons to believe that this coefficient could be biased. By conditioning the industry wage differentials on worker characteristics such as education, age, and occupation in the first stage of the estimation, we partially control for the spurious correlation between protection and relative wages (i.e., industries with less-skilled workers may receive higher protection). But to the extent that protection depends not only on observable worker characteristics, but also on unobserved worker and industry attributes, spurious correlation could still be present. Previous work based on cross-sectional analysis 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 yielded 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. Columns 2 and 5 of Table 3a report the results from specifications that include, in addition to year, industry indicators.

The remarkable feature of the results in columns 2 and 5 is that the inclusion of the fixed effects reverses the sign of the tariff coefficient, which is now positive and significant. An alternative to using industry 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 (equation 2'). The results from this "first-difference" specification are reported in columns 3 and 6 of table 3a. The estimated tariff coefficients are again positive and significant. This implies that increasing protection in a particular sector raises wages in that sector. The magnitude of the effect is economically significant. Suppose for example that the tariff in a sector with an average level of protection in 1984 (50% tariff rate) is reduced to zero. According to our estimates in column 3, this would translate to a 6% (0.12 x .5) decrease in the wage premium in this sector. For the most protected sectors (91% tariff) this effect increases to 11% (0.12 x .91).[19]

In interpreting the results of Table 3a, it is also interesting to note that the tariff coefficient estimates in the right panel of the tables (WP1) do not differ significantly from the estimates

[19] In our data, a tariff value of .20 denotes an ad-valorem tariff of 20 percent.

reported in the left panel (WP2). Wage premiums based on specification WP1 do not condition on firm characteristics and informality; to the extent that these characteristics affect tariffs and wages independently, the results based on WP1 could be biased. Yet, as evidenced by columns (4)-(6), the tariff coefficients are insensitive to the inclusion of additional controls in the computation of WP2, when industry fixed effect, or first-difference regressions are employed. This is intuitive, and supports the hypothesis that the negative correlation between tariffs and relative wages in columns 1 and 4 is driven by unobserved industry characteristics; once we account for these characteristics through industry fixed effects or first differencing, it becomes less important to control for observable worker and firm attributes.

The positive relationship between wage premiums and tariffs contrasts with the results of earlier work on the U.S. (i.e., Gaston and Trefler (1994)) that found a negative relationship between protection and relative wages employing cross-sectional data. Given that these earlier results were obtained using data for manufacturing only, we also estimate equation (2) on a subsample of manufacturing industries, to examine whether our differences with previous work do not stem from sample differences. Table 3b presents the results. Two noteworthy features emerge. First, without controlling for unobserved industry characteristics, the effect of tariffs on relative wages is estimated to be negative, and now highly significant (column 1). Workers in industries with high tariffs receive lower wages than workers with identical observable characteristics in industries with low tariffs. Moreover, the implied tariff effects are large. Suppose that we conducted the conceptual experiment of shifting a worker from an industry with 50% tariff in 1984 to one with no tariffs. Then the estimated coefficient in column 1 implies that this worker's wage would rise by 12% (0.24 x .5). These results are consistent, both in sign and magnitude, with what Gaston and Trefler (1994) report for the U.S. However, controlling for unobserved industry characteristics through first differencing (column 2) reverses the sign of the tariff coefficient from negative to positive. The second noteworthy feature in Table 3b is that the magnitude of the tariff coefficient based on the manufacturing sample only, is similar to the magnitude of the coefficient based on all industries in table 3a. In particular, the coefficient in the first difference specification in column 2 (.14) suggests that a 50-percentage point tariff decline is associated with a 7% decline in the wage premium in this industry.

The reversal of the tariff coefficient sign from negative to positive when we condition on industry fixed effects, or first-difference, demonstrates the importance of unobserved sector

heterogeneity, and provides indirect support for political economy theories of protection. The positive association between industry wage premiums and tariffs is consistent with the existence of industry rents that are reduced by trade liberalization, or, alternatively, with the predictions of the short- and medium-run models of trade, in which labor is immobile across sectors. Both explanations seem plausible in the context of the Colombian trade liberalization. In particular, the notion that trade protection had generated industry rents is supported both by economic theory and by related empirical work on the effects of regulation on rent-sharing (see for example Rose's (1985, 1987) work on the effects of deregulation in the trucking industry, or Budd and Slaughter's (2000) work on international rent sharing). On the other hand, the existence of labor market rigidities also seems a-priori relevant in Colombia, a country characterized by one of the most restrictive labor market regimes in Latin America. Indicatively, Heckman and Pages (2000) report that the cost of dismissing a worker in Colombia is approximately 6 times the monthly wage at the end of the 1980's, and 3.5 times the monthly wage at the end of the 1990's (after the labor market reform). Kugler (1999) reports similar findings on the costs of firing workers in Colombia.

Though we do not attempt a formal investigation of the role of labor market rigidities on relative wages, we examined in a different paper (Attanasio, Goldberg and Pavcnik (2002)) the response of sectoral employment shares to trade liberalization. Normally, one expects big labor reallocations in the aftermath of a major trade reform, from sectors that experienced large protection declines to sectors that were less affected by liberalization. Yet, the employment shares are remarkably stable during this period, while regressions of changes in sectoral employment shares on tariff changes fail to detect any relationship between trade liberalization and sectoral employment. This stability of employment shares is consistent with the hypothesis of constrained labor mobility. Still, the lack of labor reallocation seems rather surprising given the existence of a large informal sector in Colombia that does not comply with labor market regulation and thus provides an additional margin of adjustment. Along these lines, Marcouiller et al (1997) document significant wage gaps between the formal and informal workers for El Salvador, Mexico and Peru. In an attempt to investigate whether the effects of trade on relative wages stem from constrained labor mobility, we estimated equations (2) or (2') separately for the formal and informal workers in our sample, but failed to find any significant differences between the two sectors. One possible explanation for the lack of any differences between the formal and informal sectors is that labor is more mobile across the formal and informal sectors, than across industries. Indeed, in a related

paper (Goldberg and Pavcnik (2003), p. 21), we find, that while the share of informal workers increased in Colombia in the aftermath of the trade reforms, the entire increase is accounted for by within-industry changes from the formal to the informal sector, rather than between industry shifts of informal workers.

To summarize, our findings indicate that trade liberalization has had a significant impact on relative wages in Colombia; whether this impact stems from the presence of industry rents, or the existence of constraints on labor mobility is however a question we cannot convincingly answer at this point. A-priori, we consider both hypotheses to be plausible, and suspect that the decrease in wage premiums in the sectors with large tariff reductions most likely reflects a combination of the two mechanisms.

6.2 Sensitivity Analysis

Our main measure of trade policy in this paper is tariffs, and for the reasons we laid out above, we consider this to be one of the strengths of our approach. However, apart from tariffs, there may be other channels through which trade affects wages. For example, industries may have faced differential changes in transportation and communication costs, informal trade barriers, and exchange rates over time. In this section we investigate the robustness of our results to controlling for 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 a 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 value of imports and exports 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. The specification with lagged imports and exports is reported in column 1 of table 4. The tariff coefficient is robust to the inclusion of the additional trade controls and continues to suggest a positive association between tariffs and wage premiums.

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 (column 2).[20] 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 (note also that the *aggregate* effects of exchange rates are already controlled for through the time indicators). 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.[21] 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 column 2 indicate, the tariff coefficient is robust to the inclusion of exchange rates – in fact, the magnitude hardly changes compared to our base specification in Table 3a.

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 could occur 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). However, the positive correlation between tariffs and NTB measures reported in section 4.1 indicates that this was not the case: that is, sectors with proportionately larger tariff cuts also experienced large reductions in NTBs. Furthermore, we indirectly capture the effect of NTB changes indirectly through the effect these changes may have had on industry import and export measures. Nevertheless, since we have some limited information on NTBs for three years only, we also attempted a more direct investigation of their effects.[22] Tables for these specifications

[20] The exchange rate we use is the nominal effective rate (source: IMF) that is computed taking into account Colombia's major trade partners.

[21] 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.

[22] 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

can be found in our NBER Working Paper. 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. 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.

Finally, our tariff coefficient would be biased if there were other time-variant, industry-specific factors that affected wage premiums, which were correlated with tariff changes but are not controlled for in the estimation. Three such factors that come to mind are sector-specific capital, unionization, and minimum wage. Industry-specific capital is particularly relevant, if one interprets the results with the medium-run model in mind, in which case capital is mobile across sectors; on the other hand, inclusion of capital on the right hand side presents the problem that capital formation itself responds endogenously to changes in factor costs (e.g., wages). Nevertheless, to check the robustness of our results, we included a measure of sectoral capital accumulation in our first difference regressions; given the aforementioned simultaneity bias, we are not interested in the capital coefficient per se – rather, we interpret capital as a conditioning variable. Our measure of capital accumulation is based on UNIDO's industrial statistics on gross fixed capital formation. Unfortunately, this measure is available only for manufacturing industries, and it is not available in 1997 and 1998. Table 5 reports the results. Given that the manufacturing sectors experienced the largest changes in tariffs, one would expect the omitted variable bias (if it exists) to most likely affect these sectors. However, the comparison of the tariff coefficients in columns 1 and 2 that do not control for capital accumulation, with those in columns 3 and 4 suggests that the inclusion of industry capital accumulation hardly affects the tariff coefficient. Thus, the positive correlation between tariffs and wage premiums is not driven by capital accumulation.

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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.

Regarding unionization, our individual level data do not provide information on the union membership of each worker. Unfortunately, detailed industry-level information on union membership is also not available. If tariff changes were correlated with changes in the union strength in each industry, our results would again be biased. While in the absence of industry-level union data we cannot formally address this issue, we believe that changes in unionization are unlikely to be of concern during this period. Anecdotal evidence suggests that unions do not have significant power in most Colombian industries (public sector and the petroleum industry are the exception). In his book on Colombian reforms, Edwards (1999) confirms these anecdotal reports. More importantly, there is no evidence (or even a claim) in the literature that union strength changed during the period of trade liberalization. We therefore believe that changes in unionization are unlikely to be driving our results. Similarly, we believe that minimum wages are of secondary importance during this period in Colombia. The most significant increases in the minimum wage took place in the late 1970's and early 1980's (see Bell (1997), Table 2). The changes in the late 1980's and 1990's were in comparison small. More importantly, the minimum wage is set in Colombia at the national level, so that minimum wages do not vary by industry. (Note that any effects minimum wage changes may have had on industry wages through compositional channels, for example because some industries employ more unskilled workers than others, are already controlled for in our approach, since the first-stage regressions control for industry composition in each year, and allow the returns to various educational and professional categories to change from year to year.)

Any remaining concerns about omitted variable, or more generally, simultaneity bias, can be addressed by instrumenting for tariff policy changes using the approach described in section 3. The next subsection reports the results from that exercise.

6.3 Results from Instrumenting for Trade Policy Changes

To instrument for tariff changes we exploit the close link between the magnitude of tariff reductions and the initial level of protection in 1983 (a year prior to our sample). This link was discussed in section 3.1 and demonstrated clearly in Figure 2. We start our instruments discussion in this section be exploring the determinants of annual tariff changes from 1985 to 1998 more rigorously. In table 6a, we relate the annual change in tariffs from 1985 to 1998 to the various variables discussed in section 3.1. Column (1) demonstrates that tariff reductions are largest in

sectors with a high share of unskilled workers (a tariff reduction corresponds to a negative change). In column (2) the tariff changes are regressed against the 1983 tariff level, year indicators, and a constant. The coefficient on the 1983 tariff level is -.152, and the R^2 is .31. These results do not change when we add the industry's 1984 share of the unskilled workers as a regressor (unreported), since the initial tariff level and the share of unskilled workers are highly correlated. We therefore focus on the 1983 tariff levels (rather than proportion of unskilled workers) as the main determinants of tariff reductions. Columns (3) to (5) report the results from regressing tariff changes on interactions of the 1983 tariff levels with world coffee prices and exchange rates.[23] These interactions yield potential industry-specific, time-varying instruments. The joint explanatory power of these regressors remains high in all specifications.

Table 6b contains the 2SLS estimates for equation (2'). Column 1 reports the first-difference results when we do not instrument for tariff changes as a baseline. As discussed earlier, the tariff coefficient is positive and significant. Columns (2)-(6) report the 2SLS results using alternative sets of instruments. Note that the coefficient estimates seem robust to interacting pre-reform tariff levels with exchange rates versus coffee prices. Although the magnitude of the tariff coefficient changes compared to the baseline specification in (1), 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 .12 in column 1, to .05 in column 2, and between .04 and .05 in columns (3) to (6). The coefficient of .05 implies that a 50-point tariff reduction would lead to a 2.5 percent decline in wage premiums. To take a concrete example, in textiles, where the tariff rate dropped from ca. 91% in 1984 to ca. 18% in 1998, the implied decline in the relative wage is 3.7%. While this decline may not seem large, note that it affects sectors that have lower relative wages at the onset of trade liberalization. The cross-sectional estimates on page 24 imply that the adjusted relative wages in a highly protected sector like textiles (91% tariff) are approximately 9.8% (0.24*(91%-50%)) higher than the relative wages in a sector with an average rate of protection in 1984 (50% tariff), and 14.6% (0.24*(91%-30%)) higher than the wages in a sector with a low rate of protection (30% tariff). The estimated 3.7% decline in the wage premium widens this gap even further.

[23] 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.

In sum, our results demonstrate the importance of accounting for unobserved sector heterogeneity 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, and conditioning on capital accumulation in each industry. More importantly, the positive relationship was robust to using instrumental variables to account for time-varying political economy factors affecting trade policy changes and time-varying selection (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 (or medium) run. Alternatively, they could be interpreted as evidence that trade liberalization reduced existing industry rents. Whatever interpretation one adopts, our findings suggest an additional channel through which income inequality in developing countries may have been affected during this period. Since the tariff cuts were concentrated in sectors with a high proportion of unskilled workers (see Figure 5), such workers may have been hit by the reforms twice: 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.

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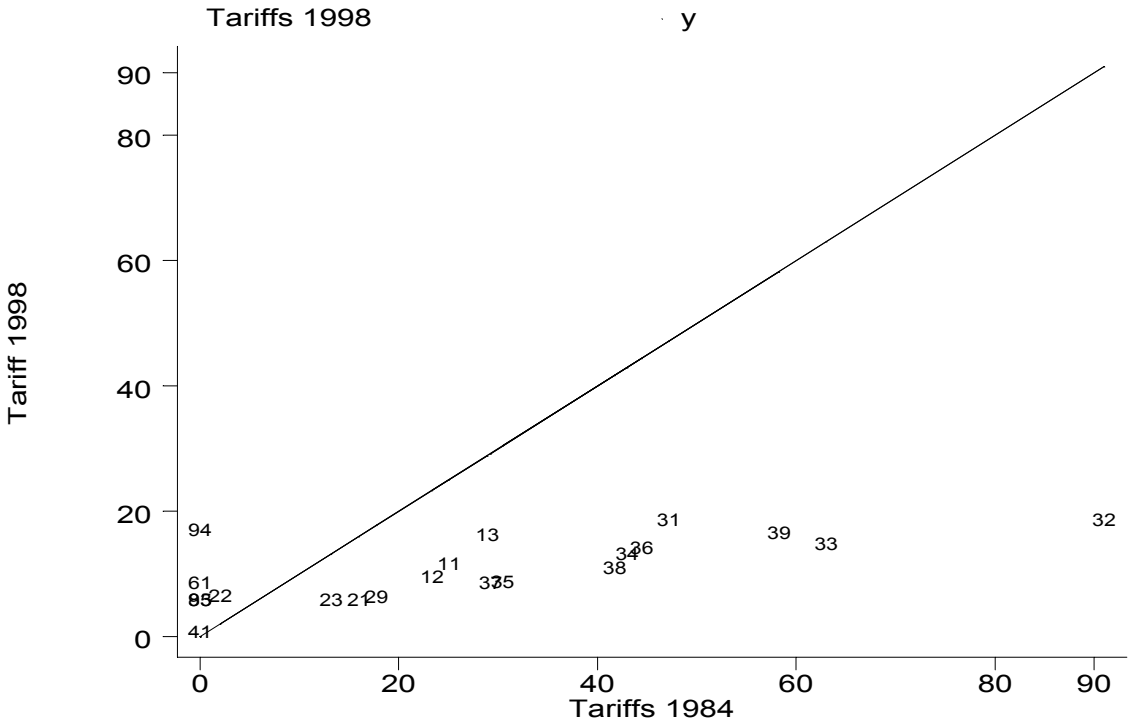
Figure 1—Industry Tariffs in 1984 and 1998



Note: the line is a 45-degree line.

Note: Numbers denote 2-digit ISIC sectors.

31: Manufacture of Food, Beverages and Tobacco
32: Textile, Wearing Apparel and Leather Industries
33: Manufacture of Wood and Wood Products
39: Other Manufacturing Industries

Figure 2--Tariff Decline 1998-1984 and Tariffs in 1983

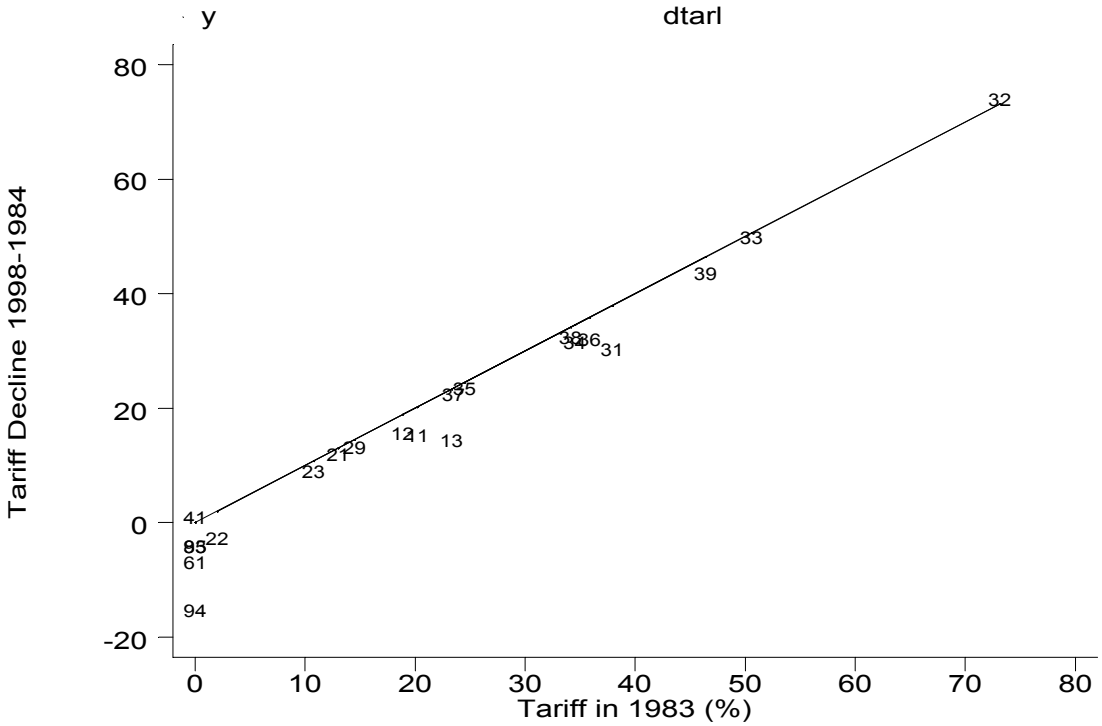
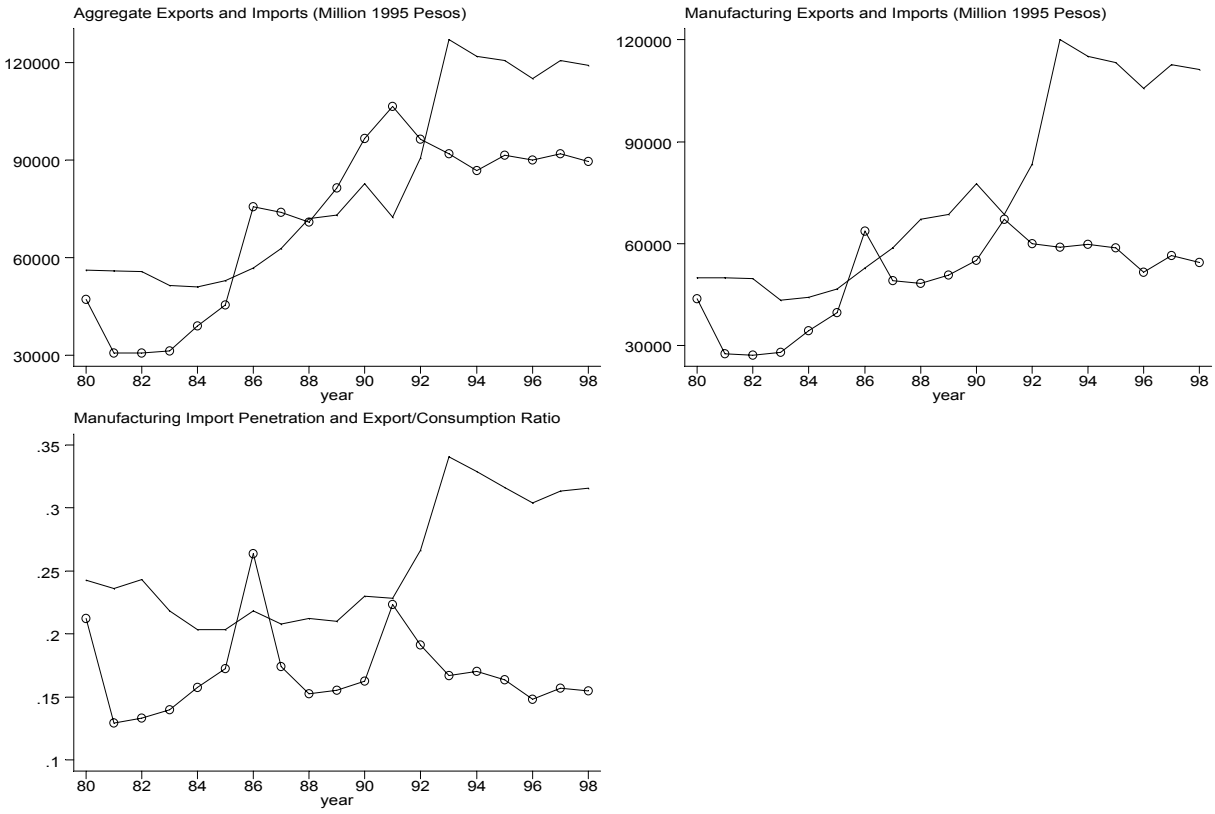
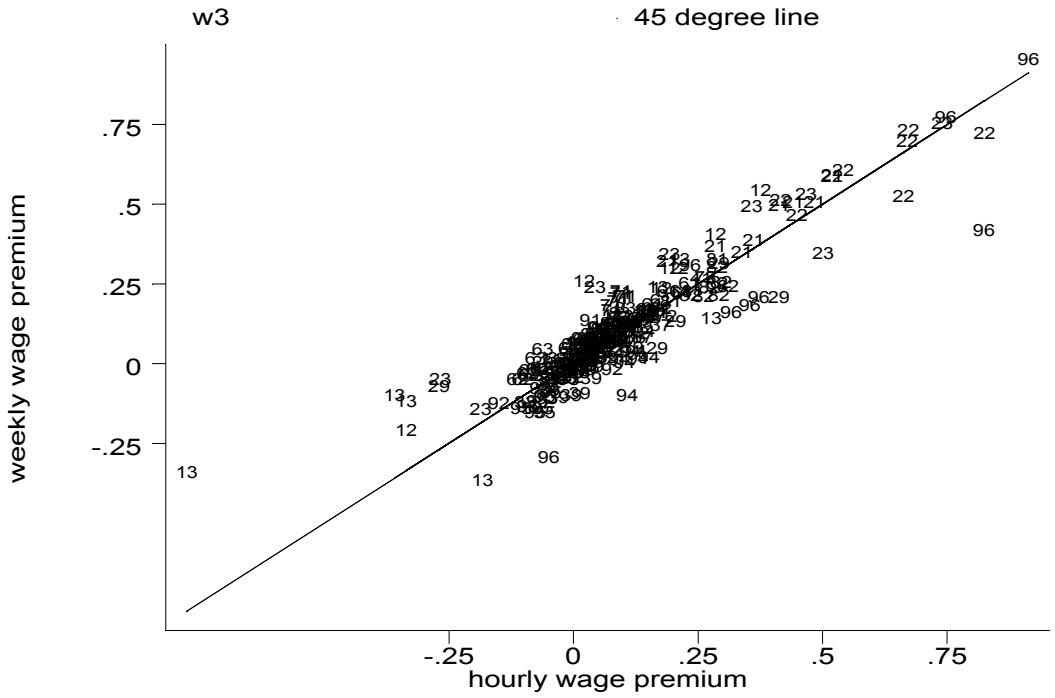


Figure 3—Various Measures of Trade flows 1980 – 1998



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

Figure 4—Hourly and Weekly wage premiums (based on specification 1)



Note: the line is a 45-degree line.

Figure 5—Tariff Reductions and Share of Unskilled Workers

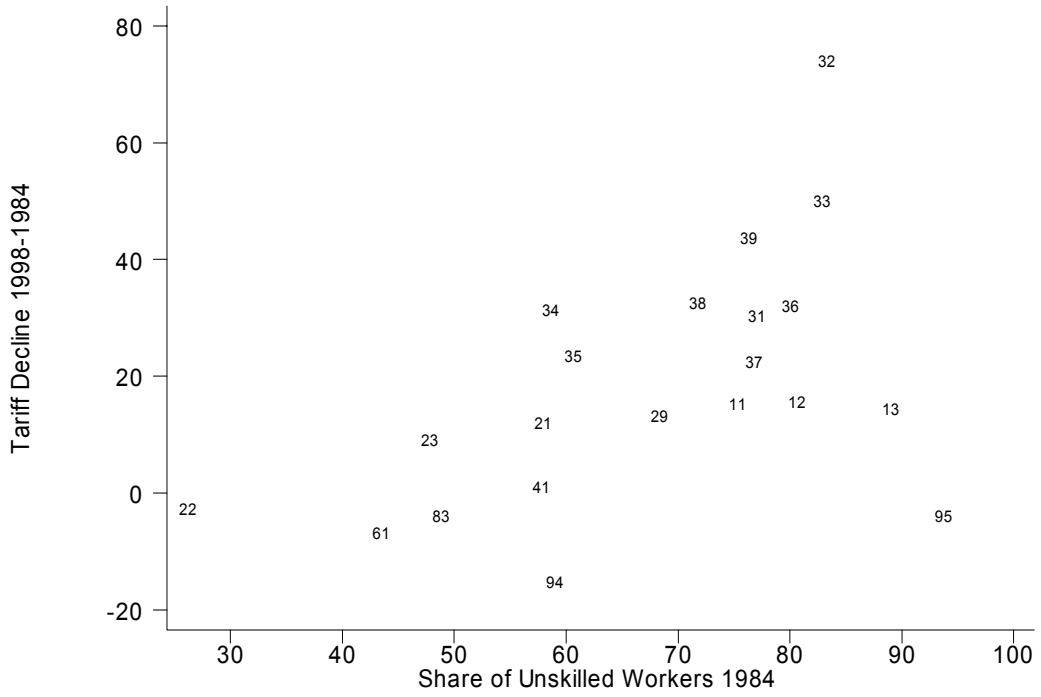


Table 1a--Summary statistics for Tariffs 1984-1998

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
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 3a--Industry Wage Premiums and tariffs

	(1)	(2)	(3)	(4)	(5)	(6)
		WP1			WP2	
Tariff	-0.0114	0.0660***	0.1191***	-0.1117	0.0908***	0.1405***
	[0.926]	[0.000]	[0.000]	[0.458]	[0.001]	[0.009]
First differencing	no	no	yes	no	no	yes
Year Indicators	yes	yes	yes	yes	yes	yes
Industry Indicators	no	yes	no	no	yes	no

Note: P values based on standard errors that are clustered on industry are reported in parethesis. ***, **, * indicate significance at a 1%, 5%, and 10% level, respectively. N is 168 columns 1-2, 147 in columns 4-6, and 126 in column 6.

Table 3b--Industry Wage Premiums and tariffs in Manufacturing

	(1)	(2)
Tariff	-0.2418**	0.1435***
	[0.013]	[0.005]
First differencing	no	yes
Year Indicators	yes	yes

Note: P values based on standard errors that are clustered on industry are reported in parethesis. ***, **, * indicate significance at a 1%, 5%, and 10% level, respectively. Dependent variable is WP1. N is 72 in column 1, 63 in column 3.

Table 4--Industry Wage Premiums and trade exposure measures

	(1)	(2)
Tariff	0.1300***	0.1356***
	[0.000]	[0.000]
Lagged Imports (I)	0.00003	0.00002
	[0.545]	[0.762]
Lagged Exports (E)	0.00014	0.00007
	[0.499]	[0.684]
I*Exchange rate		0.0000001
		[0.861]
E*Exchange rate		0.0000015*
		[0.079]
First differencing	yes	yes
Year Indicators	yes	yes

Note: P values based on standard errors that are clustered on industry are reported in parethesis. ***, **, * indicate significance at a 1%, 5%, and 10% level, respectively. Dependent variable is WP1. N is in 147 columns.

Table 5--Industry Wage Premiums and trade exposure measures in manufacturing

	(1)	(2)	(3)	(4)
Tariff	0.1392***	0.2404***	0.1381**	0.2432***
	[0.009]	[0.004]	[0.012]	[0.003]
Lagged Imports (I)	-0.00001	-0.0002	-.000003	-.000168
	[0.850]	[0.100]	[0.968]	[0.134]
Lagged Exports (E)	0.00015	-0.00011	0.00021	-0.00008
	[0.593]	[0.530]	[0.520]	[0.687]
I*Exchange rate		0.0000015		0.0000013
		[0.129]		[0.175]
E*Exchange rate		0.0000042**		0.0000046***
		[0.015]		[0.008]
Gross Capital Accumulation			-0.0000335*	-.00003
			[0.060]	[0.122]
First differencing	yes	yes	yes	yes
Year Indicators	yes	yes	yes	yes

Note: P values based on standard errors that are clustered on industry are reported in parethesis. ***, **, * indicate significance at a 1%, 5%, and 10% level, respectively. Dependent variable is WP1. N is in 63 in columns 1-2 and 54 in columns 3-4.

Table 6a: Determinants of Trade Policy Changes
(dependent variable is annual change in tariffs)

	(1)	(2)	(3)	(4)	(5)
Tariff in 83		-0.1521***		.2839	-.2523
		[0.000]		[0.109]	[0.126]
Exchange Rate*Tariff in 83			-0.0015***	-0.0041**	
			[0.000]	[0.018]	
Coffee Prices*Tariff in 83					.0012
					[0.534]
Share of Unskilled Workers in 1984	-0.0921**				
	[0.028]				
R^2	.179	.313	.350	.376	.321
Year Indicators	yes	yes	yes	yes	yes

Note: P values based on standard errors that are clustered on industry are reported in parethesis. ***, **, * indicate significance at a 1%, 5%, and 10% level, respectively. The mean of the exchange rate is 107 for 1986-1998. N is 147.

Table 6b: Industry Wage premiums and tariffs, First Differences, 2SLS results

	(1)	(2)	(3)	(4)	(5)	(6)
Nominal tariff	0.1191***	0.0462**	0.0444***	.0416	0.0362*	0.0496*
	[0.000]	[0.021]	[0.001]	[0.104]	[0.087]	[0.053]
Instrument	none	tariffs 83	exchange rate* tariffs 83	tariffs 83, exchange rate* tariffs 83	coffee price* tariffs 83	tariffs 83, coffee price* tariffs 83
Year Indicators	yes	yes	yes	yes	yes	yes

Note: P values based on standard errors that are clustered on industry are reported in parethesis. ** and * indicate 5 and 10 % significance, respecitively. WP1 is the dependent variable. Reported standard errors are robust and clustered by industry. N is 147.