

Trade Liberalization and Intersectoral Labor Movements*

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Abstract

This paper examines the impact of trade liberalization episodes on movements of labor across sectors. The aim is to assess empirically whether increased trade openness leads to increased structural change, and if so to what extent. Results for a set of 25 liberalization episodes suggest weakly negative effects of liberalization on the extent of intersectoral labor shifts at the economy-wide 1-digit level of disaggregation. We do uncover increased sectoral change after liberalization at the 3-digit level within manufacturing, although the estimated effects are statistically weak and small in magnitude. The effects of liberalization on labor shifts differ across individual countries, in a way related to the scope and depth of reforms as well as the extent of job protection regulations.

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1 Introduction

To what extent is trade liberalization followed by intersectoral labor shifts? Interest groups which favor or oppose liberalization would agree with claims that it leads to intersectoral displacements of labor. Protectionist arguments abound in political discourse pointing to plant closures and the relocation of entire sectors to countries with lower (relative) labor costs. Most discussions of the costs of trade liberalization center on the transitional costs and temporary unemployment associated with such trade-induced structural change. Movements of labor and capital across sectors, however, are precisely what allows countries to reap the benefits of trade openness in classical trade models.

In these models, gains from trade are obtained by moving resources towards sectors in which a country has a comparative advantage. Such comparative advantage can be due to relative technological differences across countries in the Ricardian model, or to varying relative factor endowments in the Heckscher-Ohlin model. Both predict that moving from restricted trade to freer trade should entail structural change as a result of liberalization, and that this should be observable at the sector level. New trade theory also carries predictions about the effect of liberalization on sectoral structure. For instance, in models with increasing returns to scale (IRS) trade liberalization leads to the agglomeration of production in certain geographic locations, which can translate into observable intersectoral shifts at the country level.¹

An entirely different class of theoretical models points to either uniform effects of trade liberalization across sectors or to unobservable movements of labor. For instance, Rivera-Batiz and Romer [1991] demonstrate how economic integration can allow countries to exploit increasing returns in R&D activities, yielding dynamic productivity benefits that need not stem from changes in specialization patterns. Markusen [1981] and Wacziarg [1997] shows how trade liberalization can have procompetitive effects on pricing and output decisions in, respectively, static and dynamic Cournot-Nash imperfect competition models, without necessarily relying on changes in the pattern of comparative advantage. Similarly, the class of IRS models alluded to above may predict shifts that are not observable at existing levels of disaggregation in sectoral labor data. Many of these models focus on intra-industry trade patterns, and as a result may not deliver clear predictions on labor shifts across observable sectoral categories. An important aspect

¹See for instance Krugman [1991], and the references therein.

of these theories is that gains from trade are possible in the absence of intersectoral factor movements.

Despite contrasting theoretical predictions, there has been surprisingly little systematic evidence gathered as to whether or not trade liberalization generates observable structural change at the sector level and, if so, at what level of disaggregation. This paper tries to fill this gap by bringing together the literature on job reallocation and the literature on the macroeconomic effects of trade liberalization. Using internationally comparable sectoral labor data, and focusing on developing or emerging economies, we examine a set of liberalization episodes and assess whether their aftermath was characterized by intersectoral labor shifts in excess of those observed in the absence of liberalization. Our empirical approach draws on the measurement framework of the job reallocation literature.²

We find little evidence that opening up to trade leads to increased intersectoral labor reallocation at the economy-wide 1-digit level of disaggregation. Such an effect, however, is more noticeable at the 3-digit level for manufacturing sectors. However, these effects within manufacturing sectors are relatively small in magnitude and not statistically robust. Overall, our results suggest trade liberalization has far smaller effects on intersectoral labor shifts than conventional wisdom would suggest.

This paper is organized as follows: Section 2 discusses the conceptual framework of this paper, based on contrasting a set of models where the effects of increased trade openness are unbalanced across sectors, with a set of models predicting uniform effects. Section 3 surveys past findings on the effects of trade liberalization on sectoral structure. Section 4 discusses our empirical strategy and data. Section 5 presents our empirical results. Section 6 considers robustness issues and extensions of our estimation framework. Finally, section 7 concludes.

2 Conceptual Framework

2.1 Sectorally Unbalanced Effects

2.1.1 Classical trade models: A Ricardian example

A useful starting point for a discussion of intersectoral labor shifts in the context of classical trade models is Imbs and Wacziarg [2000]. This paper

²See Davis, Haltiwanger and Schuh [1996] for such methods applied to plant-level rather than sector-level data.

presents a Ricardian trade model with a continuum of goods extended to encompass dynamics and trading costs.³ As a simplified, static version of this model, consider a two-country world and assume that demand is Leontief and production for each of a continuum of $z \in [0, 1]$ varieties of consumption goods is Cobb-Douglas in labor and capital:

$$y(z) = a(z)K(z)^\alpha L(z)^{1-\alpha} \quad (1)$$

The sector specific productivity parameter $a(z)$ takes the form $a(z) = z^2$, so that the domestic economy is increasingly productive in z and will therefore tend to produce the goods at the upper end of the $[0, 1]$ segment. There are trading costs ψ satisfying the following pricing relationships:⁴

$$p_1^2(z) = (1 + \psi) p_1^1(z) \quad (2)$$

$$p_2^1(z) = (1 + \psi) p_2^2(z) \equiv (1 + \psi) \Gamma \quad (3)$$

where $p_i^j(z)$ denotes the price of variety z produced in i and consumed in j , and where it is assumed for simplicity that the schedule of foreign price $p_2^2(z) = \Gamma$ (and therefore foreign productivities for each variety) does not depend on z . The threshold variety at which the domestic economy starts to produce varieties of goods is denoted z_1 , and can be solved for by arbitrage: $p_1^1(z_1) = p_2^1(z_1)$ implies $z_1 = [(1 + \psi) \Gamma]^{-\frac{1}{2}}$. Thus, in a general equilibrium the range of goods produced domestically (equal to $1 - z_1$) increases in ψ , all else being equal. Finally, labor input in each sector is given by:

$$L(z) = \frac{L}{z^2} \left(\frac{z_1}{1 - z_1} \right) \quad (4)$$

Therefore, an exogenous reduction of the trading cost ψ , for example induced by trade liberalization, will lead the country to abandon the production of a subset of the varieties, and reallocate the corresponding labor to the varieties remaining in production.⁵

³See also Dornbusch, Fischer and Samuelson [1977] for the initial formulation of the Ricardian model with a continuum of goods.

⁴We assume for simplicity that these trading costs are lost to the economy - they have the character of iceberg costs. This does not preclude interpreting them as policy barriers as long as the proceeds of such barriers are put to relatively unproductive uses by the government.

⁵A Heckscher-Ohlin modification of the same model would involve setting $a(z) = 1$, so that relative productivities are equal across countries, but allowing the implied capital labor ratios derived from optimizing over the production function to differ across sectors within countries. Differences in relative initial (aggregate) endowments of capital and labor across countries would then lead to the determination of trading patterns, and would allow a similar characterization of intersectoral labor transfers resulting from a reduction of trading costs. See Schott (1999) for an empirical application.

2.1.2 New trade theory: an example

Some versions of “new” trade theories would also lead to observable intersectoral reallocation. Suppose for instance that changes in the pattern of comparative advantage result from sector biased technological transmissions, rather than from reduction in trading costs. For example, different sectors in the home country of the Ricardian setup presented above could benefit differently from trade openness. Consider this setup and set $\psi = 0$. Define $\varphi(z)$ as the degree of openness ($\varphi(z) > 0$). Rather than working by reducing trading costs, we consider now that openness works by fostering technological transmissions across countries (for example through the exchange of technologies embedded in traded goods), but that it does so to a varying extent depending on the varieties under consideration.⁶ As a reduced form for this technological transmission mechanism, assume that $a(z) = f(\varphi(z), z)$, where $\varphi'(z) \neq 0$, $f_1 > 0$ and $f_2 > 0$ (as before). For example, we could specify $a(z) = \varphi(z)z^2$. In this case, we obtain:

$$L(z) = \frac{L}{\varphi(z)z^2} \left(\int_{z_1}^1 \frac{1}{\varphi(z)z^2} dz \right)^{-1} \quad (5)$$

which, in general, will depend on $\varphi(z)$. Thus, a reduction in trading costs $\varphi(z)$ will involve increased intersectoral labor shifts because it has different effects on different sectors. This contrasts starkly with the case where technological transmissions would act uniformly on sectoral productivity, for example if $\varphi(z) = \varphi$ (a constant). In this case we get, as before:

$$L(z) = \frac{L}{z^2} \left(\frac{z_1}{1 - z_1} \right) \quad (6)$$

which does not vary with the degree of openness φ . With such uniform technological effects of trade liberalization, no intersectoral labor shifts would be observed after liberalization.

2.2 Sectorally Uniform Effects

2.2.1 Increasing Returns to Scale and Unobservable Reallocation

Other “new” trade models similarly imply that the effects of trade liberalization could be uniform across sectors. For example, Rivera-Batiz and Romer [1991] and Alesina, Spolaore and Wacziarg [2000] present models where the

⁶See Barro and Sala-i-Martin [1997] for a model of technological diffusion which includes microfoundations.

effects of economic integration result purely from the increased scale of economic activity. Rivera-Batiz and Romer [1991] show that scale effects in growth can occur even in the absence of international knowledge spillovers if the generation of blueprints for new product varieties involves increasing returns to scale (so that an integrated world economy can save on paying the fixed costs necessary to develop new blueprints relative to a world where each country would have to incur these costs).⁷ In Alesina, Spolaore and Wacziarg [2000], scale effects result from the fact that integrated economies can save on trading costs. These costs could be policy induced barriers generating fiscal proceeds employed for relatively inefficient uses, or simply waste resulting from burdensome trading procedures and other non-revenue generating legal and political barriers to trade.

Models based on procompetitive effects of trade policy generally carry the implication that the effects of trade need not involve specialization. A simple example of the procompetitive effects of trade policy would entail two identical countries with monopolistic markets. In a move from autarky to free trade, the structure of the market moves from monopoly to oligopoly, so that output expands and prices fall. This occurs in the absence of any observed trade flows in equilibrium (the two countries are identical in every respect).⁸ In more sophisticated, dynamic models, the pattern of specialization is typically held fixed and the main mechanism through which trade affects income levels (or growth) is by increasing the size of the market on which imperfectly competitive firms operate. As market size rises, so do profits for each monopolistic competitor, so more competitors find it affordable to incur the fixed cost of entering the market. As entry takes place, markups fall and output expands.⁹

Furthermore, to the extent that intraindustry trade is prevalent, some forms of specialization might not be noticeable at available levels of data disaggregation. In models with differentiated products and increasing returns

⁷In this type of model, trade in goods embodies technological advances that need to occur in only one of the two countries. Specialization occurs within the research sector rather than across production sectors.

⁸This example is due to Markusen [1981]. The advantage of such an example is that it illustrates neatly the possibility of gains from trade even in the absence of well-defined patterns of specialization. On the other hand, the model involves an exogenously fixed number of firms in each country, irrespective of the trade regime. A more realistic setup would involve trade integration leading to a reduction in the total number of firms (with a concurrent increase in the number of firms operating on any given market). For more on this point see Peretto [1996].

⁹For a formal dynamic model of this process, see Wacziarg [1997].

to scale where increased trade induces shifts of resources within broadly defined sectors of activity, specialization does occur yet may be unobservable.¹⁰ We attempt to measure such effects by using sectoral data at the highest available level of disaggregation for the countries in our sample (4-digit level of disaggregation).¹¹ Internationally comparable plant level data would allow us to better capture intraindustry and interfirm specialization effects, but is not available for our sample. Levinsohn’s [1999] case study of Chile uses that plant level data to show important job creation and destruction effects following changes in the trade policy regime. He argues that such effects cannot be captured using industry-level data. However, most predictions of classical trade theory would apply to industry-level data. Hence, our analysis is distinct from past studies of intra-sectoral, or inter-firm, consequences of trade liberalization. We focus on a different set of predictions of existing trade theories, namely those relating to intersectoral movements.

2.2.2 Factor Mobility

We could find no increases in intersectoral labor shifts following liberalization if some assumptions of classical trade theory were violated. The Heckscher-Ohlin factor abundance theory, for example, relies on the assumption that factors of production are homogenous in quality and costlessly mobile across sectors. A violation of the mobility assumption may limit the extent of post-liberalization intersectoral shifts. Alburquerque and Rebelo [2000], for example, present a model of industry dynamics with irreversible investment where sectoral inertia arises endogenously, preventing an observable structural response to trade reform. High costs of dismissal and restrictions on temporary hiring can also create barriers to the kind of labor market movements that we are studying, an issue we address directly in our empirical work.¹²

¹⁰ A classic reference in this literature is Dixit and Norman [1980], p. 281-295.

¹¹ One disadvantage of this approach is that the quality of the data tend to fall as the level of disaggregation gets finer.

¹² The 1997 Economic and Social Progress Report of the IADB notes for example that labor market reforms have lagged behind trade reform in many Latin American countries. See also Heckman and Pagès [2000] for a discussion of job security regulations in Latin America. We use their data on the costs of dismissal to examine the impact of restrictions on labor mobility on intersectoral labor movements.

2.2.3 Counteractive Policies

Political factors might also affect the propensity towards structural change. Just as other reforms can magnify the observed effects of trade liberalization, governments can react to its potential effects by implementing counteractive policies. For example, they can enact subsidies targeted to sectors that stand to lose from liberalization. This type of counteractive domestic policy would tend to reduce the extent of post-reform intersectoral labor shifts. Case studies in Haggard and Webb [1994] discuss the packaging of trade reforms designed to gain support from firms in import competing industries. Common compensating policies included the expansion of subsidies to producers and, in some cases, wage restraints. We also directly address the issue of counteractive policies in our empirical analysis.¹³

3 Existing Evidence

Previous empirical evidence on the effects of trade liberalization on labor reallocation is mixed, and largely based on individual case studies rather than a broad sample of liberalization episodes. Increased trade does appear to have sector-specific employment effects in the United States and Canada, but the evidence for developing countries suggest more uniform effects. Freeman and Katz [1991], Revenga [1992], Gaston and Trefler [1994, 1997] and Grossman [1986, 1987] found significant employment responses to import competition in some sectors, though smaller effects on wages in the US. Similarly, Gourinchas [1999] uncovered a significant effect of exchange rate fluctuations on movements of jobs across and within sectors in the case of France, using firm-level job creation and destruction data.

Case studies of developing countries in Tybout and Roberts [1996], however, show that industry exit and entry (one indicator of intersectoral reallocation of labor) generally do not increase with import competition, once demand shocks have been controlled for. It is unclear whether this is associated with movements of labor across sectors, but is consistent with the idea that sectoral structure displays a high degree of inertia with respect to trade reform.¹⁴

¹³Appendix 2 discusses the extent of domestic reforms which accompanied trade liberalization.

¹⁴Tybout [1996] finds that more plants were exiting manufacturing than were entering in Chile during 1979-82, despite the growth in productivity. The size of entrants tended to be larger than those exiting, however, so the overall impact on employment is unclear. Overall, the analysis supports the idea that trade liberalization leads to a reallocation of

Papageorgiou et al [1991]’s analysis of nineteen episodes of liberalization in less developed countries uncovered very little relationship between trade liberalization and transitional shifts in employment. Their summary of the case studies outlines evidence for gains from trade without transitional employment effects from movements of jobs between sectors. Overall employment increased after liberalization in nearly all the countries surveyed. There was no statistical relationship between a sector’s imports and its employment in Brazil’s 1965-73 liberalization, except in the textile sector, where employment remained constant under import competition. An intersectoral correlation analysis of employment after the 1978-9 liberalization in Peru revealed no significant relationship between sector employment changes and import shares. Available evidence from the Philippines suggests that increased import ratios in the 1960-65 liberalization could only be linked to a fall in employment in one of the smallest decontrolled sectors. Singaporean data also showed little relationship between changes in employment and manufacturing sectors’ retained-import ratios. There are some exceptions: the impact of liberalization on manufacturing employment in Chile varied by sector (the export sectors expanding and import-competing contracting), though net employment increased.

Several authors have explained such findings as the result of restrictive labor market regulations. Currie and Harrison’s [1997] explanation of the ”sluggish” labor market response to trade liberalization in Morocco focuses on the context of imperfect competition. Their analysis of firm-level employment data from the manufacturing sector ruled out labor market legislation as the culprit and showed that many firms adjusted to trade reform by reducing profit margins and raising productivity rather than laying off workers.¹⁵ The effect of trade liberalization on firm-level employment varied within sectors: parastatal enterprises tended to grow, though real wages decreased after trade liberalization, while private manufacturing firms displayed only small employment effects. Feliciano [1994] attributes her finding that the Mexican trade reform had little impact on employment patterns to labor

investment between sectors in manufacturing, but that it is due more to export sectors attracting investment than to import competition decimating particular sectors. Haddad, et al [1996]’s description of Morocco’s ”gradual” trade liberalization suggests a similar picture: entrance rates in export sectors increased after liberalization, with the rate of entry statistically significantly correlated with a sector’s share in exports.

¹⁵One piece of evidence they cite is that cross-sectoral changes in tariffs and quotas had little discernible impact on wages. Also, while formal restrictions on hiring and firing workers were strict, temporary employment was common and labor inspectors were frequently unable to enforce the rules.

market regulations making it difficult to fire workers. Revenga [1997] also suggests that the small labor market response found in Mexico and Morocco might be due to labor regulations.

In contrast to these case studies, the present paper employs internationally comparable panel data on a wider sample of liberalization episodes. One advantage of using industry-level data is that our measures of structural change and our definition of trade liberalization are comparable across countries and time periods. We also have the advantage of being able to compare labor movements at different levels of disaggregation in the economy.

4 Measurement and Specification

4.1 Measures of Sectoral Labor Shifts

We use sectoral employment data from the United Nations Industrial Development Organization [UNIDO, 1997] and the International Labor Organization [ILO, 1997] to create a panel of sectoral employment shares.¹⁶ International sectoral data for developing countries are notoriously subject to measurement error, which would bias our results toward finding no effect of trade liberalization on employment shifts across sectors. This motivates the use of a variety of sources and levels of disaggregation, and a careful examination of apparent abnormalities in sectoral observations, such as large discrete jumps in employment. In the data we use, most of the suspiciously large year to year changes in employment occur in small sectors which represent a minor percentage of overall employment in any given year.¹⁷

¹⁶The economy-wide ILO data covers nine broad sectors of economic activity from 1969 to 1997. The UNIDO data is available at two levels of disaggregation for the manufacturing sector only: The 3-digit data covers a maximum of 28 sectors of activity over the period 1963 to 1996, while the 4-digit data covers a maximum of 81 categories over the period 1977 to 1997.

¹⁷We identified and examined 31 year to year changes of more than 100% in a sector's level of employment for the 1,628 observations used in the ILO regressions. 12 of these were in sectors with less than 10% of that country's average sectoral employment, where large changes may not be implausible. 115 of 238 large yearly changes in sector size in the UNIDO 3-digit data (which contains 12,482 observations) were in small sectors (less than 10% of manufacturing employment). Only 15 of the sector changes of more than 100% were in sectors that were larger than that country's average. There were 202 yearly changes of more than 100% among 6,338 observations in the UNIDO 4-digit data set, 88 of which were in sectors less than 10% of the average sector size. The UNIDO does not provide any reason to explain these unusually large year to year changes, but these occurrences were relatively rare.

To reflect the conceptual issues discussed in Section 2, we constructed several dependent variables for our empirical analysis. Our first set of dependent variables, labelled *structural change*, captures two effects on the labor market: movements of workers directly from sector to sector as well as sectorally unequal changes in aggregate employment. Our second set of dependent variables focuses strictly on movements of labor across sectors. Specifically, we use a measure of *excess job reallocation* identical to that used by Davis, Haltiwanger and Schuh [1996] for plant level data. Our third set of measures isolates the *net change in aggregate employment* (or, in the case of the UNIDO data, overall manufacturing employment). We focus on these three categories of measures because the welfare consequences of structural change resulting from sectorally unequal changes in aggregate employment are quite different from those resulting from job reallocation across sectors.

Measures of Structural Change Our main set of dependent variables consists of the absolute value of changes in the share S_s^t of each sector s in total employment for each country in any given year t . The rate of structural change is measured by the magnitude of changes in these sectoral employment shares in the pre- and post-liberalization regimes. We use two variants of the measure: differences in shares over two years (CH2) and differences over five years (CH5):

$$CH_{st}(\tau) = |S_s^t - S_s^{t-\tau}| \quad (7)$$

where $\tau = 2, 5$.¹⁸ It is important to note that structural change, measured by CH, has two components: movements of labor across sectors, and sectorally differentiated changes in aggregate employment (resulting from population growth and entry into the labor force). The other measures we use attempt to separate these sources of changes in CH.

Measures of Excess Job Reallocation Our second set of dependent variables attempts to isolate the fraction of jobs that move from sector to sector independently of overall employment gains or losses. Denoting em-

¹⁸We also computed year-to-year changes, i.e. $\tau = 1$. The drawback of such a measure is its sensitivity to measurement error and to low frequency changes in sector shares. The results, which are generally consistent with those obtained using $\tau = 2, 5$, are available upon request.

ployment in sector s at time t by E_s^t ,

$$SH_t(\tau) = \frac{\sum_{s=1}^S |E_s^t - E_s^{t-\tau}| - \left| \sum_{s=1}^S E_s^t - \sum_{s=1}^S E_s^{t-\tau} \right|}{\frac{1}{2} \sum_{s=1}^S (E_s^{t-\tau} + E_s^t)} \quad (8)$$

The changes are computed over $\tau = 2$ and 5 years. In the numerator of equation (8), the term on the left refers to the number of employment changes between t and $t + \tau$. The summation of absolute values counts each job gained or lost as a change in the structure of employment. The term on the right refers to the number of job losses or gains that are not offset by a gain or loss in other sectors. These are the total numbers of uncompensated changes in employment. Subtracting one from the other give the number of compensated changes in the structure of employment, or employment changes resulting from pure shifts of jobs across sectors.¹⁹ It is interesting to note that SH_t will be zero whenever all sectors experience employment changes in the same direction.

Measures based on aggregate employment Lastly, we measure overall employment loss or gain as the percentage change in total employment over $\tau = 2$ and 5 years. We examine the link between liberalization and job loss or gain to provide evidence for our primary findings regarding the extent of sectoral changes after liberalization. This allows us to further differentiate between movements of labor in and out of sectors, and movements of labor in and out of employment.²⁰ This last set of measures can be computed as:

$$EM_t(\tau) = \frac{\sum_{s=1}^S E_s^t - \sum_{s=1}^S E_s^{t-\tau}}{\frac{1}{2} \sum_{s=1}^S (E_s^{t-\tau} + E_s^t)} \quad (9)$$

4.2 Measures of Trade Liberalization

The date of the legal trade reform is less important to us than the date when the effects associated with trade opening are felt. We select liberalization

¹⁹We divide by a measure of total employment for the sectors in consideration (the average employment computed over t and $t - \tau$), to obtain a measure expressed as a rate rather than the number of job reallocations.

²⁰Strictly speaking, this is true only for ILO data, which covers the entire economy. For the UNIDO data, which only covers manufacturing sectors, variations in the EM measures could be due to inflows or outflows of labor between manufacturing and other sectors.

dates based on the following criteria: the country has to have had a *de jure* trade liberalization according to Sachs and Warner [1995] and has to have had a *de facto* trade liberalization demonstrated by a year-to-year increase of 5% or more in the ratio of exports plus imports to GDP.²¹ In our sample of 25 liberalization episodes, the gap between the *de jure* date and the *de facto* date exceeded two years in only two cases (Hungary and Morocco), and the two dates coincided in 14 cases.

We made sure that the *de facto* part of our choice of liberalization years was associated with sustained increases in both imports and exports. To do this, we ran fixed-effects regressions of the level and annual growth rate of imports/GDP and exports/GDP on our liberalization status measures.²² In our sample, trade liberalization appears to have had a significant positive effect on subsequent levels and annual growth of imports and exports as a percentage of GDP. Moreover, we checked that these effects are sustained through time, raising our confidence that they are linked to *de jure* trade liberalization as opposed to temporary factors.

Capturing the correct *de jure* liberalization year is also particularly important for this study. Error in designation of the liberalization year should bias our estimates towards zero. Recently, Rodríguez and Rodrik [2000] have cast serious doubts on the method of classification introduced by Sachs and Warner [1995] to characterize countries as open or closed. In particular, they have criticized the Sachs and Warner (SW) dummy variable for outward orientation by suggesting that it reflects the degree of distortions in domestic economic policies rather than countries' outward orientation.²³

To address potential criticism that we relied too much on the SW dates, we checked each date of liberalization by examining the relevant specific country literatures. With the exception of Spain and Trinidad and Tobago,

²¹This method is a modified version of the dates used by Tornell [1998], except we use a 5% threshold for the post-liberalization increase in trade volumes instead of Tornell's 7%. Our trade volume data are from World Development Indicators [1999]. Appendix 1 contains details on the choice of dates.

²²These results are available upon request.

²³This criticism may be less serious when it comes to the dates of liberalization published by Sachs and Warner, which were established using different criteria than those used to construct their cross-sectional dummy variable of outward orientation: their dates are based on a systematic survey of the literature on countries' individual experiences with trade liberalization, rather than only on the five criteria used to construct their (cross-sectional) dummy variable. For details on these criteria, see Appendix 1. See also Sachs and Warner [1995], page 24, footnote 44 for an explanation of how they determined their dates of liberalization.

we were able to corroborate all of the choices made by SW.²⁴ The sources and results of this systematic search are reported in more detail in Appendix 1, which contains brief summaries of trade liberalizations in our sample and provides background to support our use of the SW *de jure* liberalization years. According to Sachs and Warner, several countries underwent multiple liberalizations during the years in our sample.²⁵ Our primary results are based on the most recent liberalization, as previous liberalizations tended to be shallower or quickly reversed.²⁶

We used these liberalization dates to create three indicators of pre- and post-liberalization periods. The first, LIB2 takes on a value of 1 for the year of liberalization and the following two years, zero otherwise. LIB5 does the same for the year of liberalization and five subsequent years, while LIB indicates the year of liberalization and all subsequent years (without a reversal as of 1995). We selected countries that had at least three years of sectoral employment data before and after their liberalization date. The sample of countries varied across datasets: 13 countries are included in the ILO dataset, 20 in the 3-digit UNIDO dataset and 8 in the 4-digit level UNIDO dataset.²⁷

4.3 Specification and Estimation

We employed the data described above to evaluate the effects of trade liberalization on the extent of structural change and excess labor reallocation. We compared the means of our various outcome measures across subsamples constructed using our various binary liberalization measures by running a fixed-effects regression of our outcome measures on our liberalization indicators. The fixed-effects involved a set of country dummy variables for employment growth (EM) and excess labor reallocation (SH) regressions, and

²⁴The choice of Spain’s liberalization date is explained in Appendix 1. Dropping Spain from our sample did not modify our results. These results are available upon request

²⁵Multiple liberalizations do create the potential for a bias against finding increased shuffling after liberalization. Hence, we reran our estimates using indicators of liberalization around all liberalization attempts in the sample. Appendix 3 lists the alternative liberalization dates that we considered. Again, our estimates were not greatly affected by this modification of the basic specification. These results are available upon request.

²⁶In order to maximize the number of liberalization episodes in our sample, we also included an earlier liberalization episode for Argentina, since we did not have sector-level data for the latest liberalization. Dropping Argentina from our sample did not modify the results.

²⁷Appendix 1 lists these countries.

country×sector dummy variables for structural change (CH) regressions.²⁸ Our estimated equations are as follows:

$$CH_{ist}(\tau) = \alpha_1 + \beta_1 LIB_{it}(\tau') + \nu_{is} + \varepsilon_{ist} \quad (10)$$

$$SH_{it}(\tau) = \alpha_2 + \beta_2 LIB_{it}(\tau') + \mu_i + \xi_{it} \quad (11)$$

$$EM_{it}(\tau) = \alpha_3 + \beta_3 LIB_{it}(\tau') + \gamma_i + \zeta_{it} \quad (12)$$

As is usual in the labor economics literature, for the first set of dependent variables (CH), we present robust standard errors clustered at the country×year level, since the event under study (trade liberalization) is common to all sectors in a given country-year.

The estimated slope coefficient on the liberalization variable can be interpreted as a measure of the mean difference in structural change (CH), excess labor reallocation (SH) or total employment growth (EM) in the liberalizing versus the non-liberalizing regimes.²⁹ The ratio of these coefficients to the non-liberalization mean value of each of these variables (in percentage terms) provides a measure of the economic significance of the effects of liberalization and facilitates comparisons across samples (Table 4).

5 Pooled Sample Estimates

We find that liberalization has either no effect or a negative effect on the magnitude of changes in sectoral employment shares across broad economy-wide sectors (1-digit level), depending on the specification. There is a small increase in structural change after liberalization for the more disaggregated 3-digit data on subsectors within manufacturing, but this effect is generally not statistically significant. Increases in structural change in the manufacturing sector appear to be due both to a sectorally unequal fall in aggregate manufacturing employment growth as well as increased sectoral excess job reallocation.

²⁸For CH , we have one observation per country-sector-year, hence we include country×sector effects. For SH and EM we have one observation per country-year, hence we include country effects only.

²⁹The method has the disadvantage of constraining the slope coefficient on liberalization to being the same for all countries, while intercepts vary across countries (and, where appropriate, sectors-countries) to account for time-invariant country- and country×sector-specific characteristics. Single-country regressions for the country-year-sector CH variables are presented in section 6.

5.1 1-Digit, Economy-wide Results (ILO)

5.1.1 Structural Change

We found no evidence of increased structural change across broad economic sectors as measured in the 1-digit ILO data. A liberalization in the past, in fact, tends to have a negative non-significant effect on changes in sector shares (CH). Table 2 presents conditional means of our various outcome measures, conditioning on our liberalization indicators. For example, in a two-year period with a liberalization in the past five years, a typical sector will experience a 0.646 percentage point absolute value change in its share of total employment. The average change in a two-year period without a prior liberalization is 0.787 percentage points. Hence, the average two-year change in sectoral labor shares falls under liberalization. The comparison of means generally yields similar results for other definitions of CH and liberalization status.

To conduct more precise inferences and to control for country×sector specific effects, we turn to fixed-effects regression results. These are presented in Table 3a. The main results for CH are similar to those obtained from a simple comparison of means. The effects of trade liberalization tend to be stronger when changes in sector shares are computed over longer horizons (CH5). The consideration of longer horizons for the definition of variables may limit the extent of measurement error.

The magnitude of these coefficients is not trivial, although their statistical significance is low: a liberalization in the past two years decreases the 5-year change in sector shares by 0.313 percentage points on average. Table 4 provides a notion of the economic significance of such an effect: a liberalization in the past two years (LIB2) brings about a 22.042% fall in structural change (as measured by the average 5-year change in sectoral employment shares). Similarly, five-year sector changes after a liberalization (LIB) tend to be 12.182% lower than the average non-liberalization sector share change. Our results do not change when we control for country-specific effects only.³⁰

5.1.2 Excess Job Reallocation and Employment

Our results suggest that excess job reallocation also decreases after liberalization at the 1-digit level, in a statistically significant way. The coefficients on our indicators of liberalization in regressions using the measures of excess job reallocation are all negative and highly statistically significant for SH5

³⁰These results are available upon request.

(Table 3a). The magnitudes are quite large: a liberalization in the past two years decreases SH5 by about 2.258 percentage points, or 41.069% of the average excess job reallocation for the years that have not had such a recent liberalization.

The overall growth of employment tends to slow after liberalization, although this effect is not generally significant statistically. Fixed-effects estimates of the coefficients on the liberalization indicators (although often negative) are all insignificant at the 5% level in regressions using the growth in overall employment as a dependent variable (Table 3a). Hence, we found no compelling evidence that trade liberalization affects aggregate employment growth.

5.2 3 and 4-Digit Manufacturing Results (UNIDO)

5.2.1 Structural Change

We found some evidence of increased post-liberalization structural change within the manufacturing sector at the 3-digit level of disaggregation. Both the conditional means presented in the second panel of Table 2 and the regression results presented in Table 3b suggest that the effect of liberalization on changes in sector shares in manufacturing employment (CH) is positive.³¹ This result, like the ILO results, is statistically strongest for changes in sector shares computed over longer time horizons and for liberalization regimes defined over more years. The estimated magnitude of trade liberalization's economic effects is smaller in absolute value for the manufacturing sector than for the economy-wide ILO data: a liberalization in the past two years, for example, increases 5-year sector change by 0.065 percentage points, or 10.484% of the average change in sector shares in years without a recent liberalization.

5.2.2 Excess Job Reallocation and Employment

Increased structural change in the manufacturing sector seems to be accompanied by both a decrease in the overall growth of manufacturing employment (EM) and an increase in excess job reallocation across sectors (SH), especially when using LIB5 and LIB as defining the liberalized regime.

³¹With an increased number of sectors in the UNIDO 3-digit data relative to the ILO data (and in the 4-digit UNIDO data relative to the UNIDO 3-digit data), we would expect smaller estimated coefficients on the effects of liberalization, for a given level of shuffling. One way to make the coefficients comparable is to normalize them by the average shuffling measure under the non liberalization regime, as is done in Table 4.

The effects of liberalization on employment growth is always negative. A liberalization in the past 5 years, for example, reduces the two-year growth of manufacturing employment (EM2) by 4.020 percentage points. For comparison purposes, the average growth of manufacturing employment in non-liberalization regimes is 4.574%. We conclude that the observed increase in the magnitude of changes in sector shares (CH) can be attributed to both sectorally unequal decreases in manufacturing employment growth (EM) and to increases in movements of jobs across sectors (SH).

5.2.3 4-Digit Results

Our results at the 4-digit level of disaggregation within the manufacturing sector are probably the most susceptible to measurement error. They are also characterized by relatively few country episodes, due to limited data availability. We do not find any effects of trade liberalization on changes in sector shares: coefficients are of mixed signs and not statistically significant. The magnitude of the estimated coefficients is also smaller than those obtained from 3-digit data. For example, we estimate that a liberalization in the past five years increases the two-year absolute value change in employment shares by an average of 0.012 percentage points. This represents a 6.522% increase in the extent of structural change relative to non-liberalization years, and it is statistically insignificant. This compares to a 10.281% increase in the rate of sector share change using the corresponding measure for the 3-digit data. The fact that our reported estimates are far less significant, as well as smaller in magnitude, than those reported using 3-digit level data may be due to measurement error, but the overall message is that liberalization does not seem related to the rate of structural change at this level of disaggregation. Similarly, our estimates using the excess job reallocation and overall employment measures uncover no statistically significant pattern.

6 Robustness and Extensions

6.1 The Timing of Intersectoral Labor Shifts

6.1.1 Expectations and Liberalization

Our methodology hinges critically on the assumption that the enactment of liberalizations is not expected (or at least not expected too long in advance of their implementation), else their effects could start to be felt before they occur, biasing our slope estimates downwards. There are two ways in which such an effect could occur in our context. Firstly, if economic agents

expect a *de jure* liberalization, resources may start to move across sectors in anticipation of policy changes. However, Tornell [1998] argues that most liberalizations result from either a political or an economic crisis, or both, rather than from a planned program of reforms. Only 3 of the 25 liberalization episodes we study did not occur after either an economic or a political crisis, as defined by Tornell. This may limit the incidence of expectation effects.

Secondly, in our efforts to identify “real” liberalizations (as opposed to claims of liberalization), we introduced a potential lag between the date of *de jure* liberalizations (as defined by Sachs and Warner) and the date of *de facto* liberalizations. This may once more allow agents to anticipate the policy change and initiate reallocation before we recognize the implementation of liberalization. However, our lag was typically no more than a year, which should limit the size of the potential bias.³²

The expectations problem should be particularly acute in cases where the *de facto* liberalization date occurs several years after the initiation of trade reforms. Indeed, there are cases where the Sachs and Warner date is several years after the trade reforms have been announced and started (Appendix 1, Table A1). In a few cases, trade even started to increase in the years in which trade reforms were announced but before the *de facto* or *de jure* liberalization dates that we use. Hence, in order to minimize the incidence of the expectation problem, we computed our estimates using the earliest possible date of liberalization (Tables 5a, 5b, 5c). In most cases, this is the Sachs and Warner date of *de jure* reform. Exceptions to this rule (due to an early announcement of future reforms, which could have affected intersectoral shifts) are described in Appendix 3.

The use of the earliest possible date of liberalization in our regressions led to several small changes in the results. At the 1-digit, economy wide level (ILO), the negative effect of trade liberalization on structural change (CH) is reinforced both in terms of magnitude and statistical significance. At the 3-digit, manufacturing sectors level, however, the coefficient estimates were smaller in magnitude. Hence, our overall finding of a weak link between trade liberalization and intersectoral labor shifts does not change when we consider the earliest possible date of trade reform.

³²See Appendix 1, Table A1 for a list of *de facto* and *de jure* liberalization dates.

6.1.2 Timing of the labor response

Our measures of structural change may mask some of the dynamics of labor market adjustment to trade reform by granting identical weight to observations at different points in time during a country's liberalization period (as defined by the various liberalization indicators). Our pooled sample results are basically comparisons of the means across the two regimes. Assessing the dynamic path of intersectoral labor shifts, if any, can help account for the findings of the previous section. For example, economic reforms may be implemented slowly. Moreover, as suggested earlier, market rigidities can prevent quick adjustments to policy changes, and the bureaucratic lags may be such that a *de jure* policy change does not immediately become a *de facto* change.

To address this, we analyzed the timing of labor market shifts in the pooled sample, by running regressions of our measure of structural change (CH) on dummy variables representing each of eight years following a liberalization. The coefficient on each year dummy explains how much of the potential post-liberalization difference in structural change is due to differences in the rate of sector change in that year, relative to the pre-liberalization years. Table 6 displays no clear cross-country pattern in sector changes over time. A graphical display tracking the average of countries' deviation from the mean level of CH through time also showed no clear pattern.³³ Thus, the results described in the previous section do not seem attributable to a systematic dynamic pattern of structural change following liberalization.³⁴

6.2 Barriers to Factor Mobility

Barriers to factor mobility, such as restrictions on hiring and firing workers, may impede post-liberalization labor reallocation. To test this hypothesis, we used data from Heckman and Pagès (2000), on severance costs for tenured workers. Their measure of job security is the expected cost, at the time of hiring, of firing a worker in the future due to economic shocks.³⁵ We split

³³These graphs are available upon request.

³⁴This may result from the fact that pooled regressions impose the same pattern of adjustment across time in all countries. Countries may differ in the way the reallocation of labor might occur in the post-liberalization period. Individual country estimates presented below address this issue.

³⁵This measure, while it captures important aspects of labor market rigidities, has several shortcomings. Firstly, contract types might vary within countries, so the severance costs based on a particular kind of contract (long-term workers) might not apply to all jobs. Secondly, it assumes a single common probability of dismissal across countries, although

our sample into two subsamples according to whether a country's level of job security is above or below the sample mean and ran our regressions on the two groups separately. Results are presented in Table 7.

At the economy-wide level (ILO), the countries with lower job security were more likely to have negative and significant coefficients on liberalization in regressions with CH as the dependent variable. The high job security countries had all insignificant coefficients, mostly negative. Within manufacturing, the UNIDO 3-digit data suggests that there is some increase in sectoral change, as measured by CH, in the countries with lower job security. Coefficients on liberalization measures in the low job security sample are positive, though not statistically significant. As expected, there is less sectoral change after liberalization in countries where it is more costly to fire workers. Although these results are only suggestive given the coarse nature of the sample split, they are consistent with the hypothesis that labor market rigidities may offset trade-induced labor reallocation.

6.3 Counteractive Policies

Other economic reforms, besides liberalization, may also have a direct bearing on the extent of intersectoral labor shifts. It is possible, for example, that our findings of small or negative observed increases in structural change are simply the result of counteractive policy measures. We would expect domestic reforms, if they had any effect, to magnify observed structural change after liberalization. For example, deregulation facilitates trade-induced structural change. Privatization can lead previously unprofitable state-owned enterprises to downsize, and this will show up in our structural change measures as long as government ownership is sectorally concentrated. To the extent this is the case, the correct interpretation of the estimates presented earlier relates more to the effects of trade-centered reforms in general than to trade liberalization in isolation.³⁶

We address this possibility by dividing our sample according to the extent of domestic reforms that accompanied trade liberalization. According to our thorough review of the case studies literature on reforms, 14 out of our 25 episodes of trade liberalizations were in *reformer* countries, where they

this is likely to vary with economic conditions. Lastly, their measure only considers one dimension of labor market rigidities, ignoring other factors such as the strength of unions.

³⁶This would be a crucial point if we had found large effects of liberalization, since we would then have to disentangle the impact of trade liberalization from other policy reforms.

were part of a larger package of market-oriented reforms. Trade liberalization was an isolated reform in Bolivia, El Salvador, Ghana, India, Kenya, Morocco, Trinidad and Tobago and Uruguay but these countries did not actively offset the incentives created by the increase in trade with increased subsidies to industries, regulation, or public expenditures (we label these *neutral* countries). Turkey, the Philippines, and Israel were *counteractive* countries where the government attempted shield domestic industries and offset the potential effects of trade liberalization on the labor market. Appendix 2 provides further details on our survey of concurrent reforms in the countries in our sample. Fixed-effects regressions were then run separately on the sample of reformer countries and the sample of non-reformer (i.e. neutral and counteractive) countries.

The results displayed in Table 8 suggest that countries in which trade liberalization was embedded in a larger set of reforms had more marked differences in the pre- and post-liberalization regimes. In the ILO data, the negative effect of liberalization on structural change (CH) is statistically significant for almost all specifications using non-reforming countries, especially when the liberalization regime is defined over longer horizons (LIB5 and LIB). In the UNIDO 3-digit data, countries that are classified as non-reformers display a statistically significant negative impact of liberalization on our measures of structural change, while reformers display a positive impact. Hence, reformer countries likely drove the small overall increase in post-liberalization labor movements in the pooled sample.

These findings are consistent with two non-mutually exclusive hypotheses described above: the first is that domestic counteractive measures can limit the extent to which intersectoral labor shifts are observed, and the second is that observed increases in post-liberalization structural change are partly attributable to accompanying domestic policies of deregulation, privatization, etc., rather than trade liberalization per se. We observe too few examples of countries that liberalized their trade regime without liberalizing domestically to discriminate effectively between these two hypotheses using our data.

6.4 Individual Country Results

Cross-country regressions, by assuming a common coefficient on the liberalization variables, may mask variations across countries in post-liberalization intersectoral labor shifts. To examine possible country-specific differences within our sample, which might provide clues as to the causes of the findings

in Section 5, we ran regressions of changes in sector share on liberalization dummies for individual countries. Since each year of data has 9 or 28 sectors (for each level of disaggregation, respectively), this gives us enough data points for the regressions using CH2 and CH5 as dependent variables.³⁷

Table 9 displays a summary of the results for the various datasets.³⁸ There are significant differences across countries in labor shifts across broad economy-wide sectors: the results of regressions using the ILO sample indicate that Poland and Paraguay (both reformers) had a statistically significant increase in the mean change in sector shares after liberalization, while the Philippines and Turkey (counteractive countries) tended to have statistically significant decreases in structural change. A general feature of these regressions is the high sensitivity of the estimated parameters to the definition of liberalization and CH. Another notable feature is that all of the countries classified as having domestically counteracted the effects of liberalization displayed negative effects on structural change.

Countries are similarly varied in the degree of post-liberalization changes in employment shares of sub-sectors in manufacturing (UNIDO3). Chile, Hungary, Poland, and Spain had statistically significant increased mean change in sector shares after liberalization (these are all "reformer" countries in the sense defined above). Kenya, Turkey, Mexico, and Uruguay, on the other hand, appear to have experienced smaller average structural change after liberalization, although some of these effects vanish when using alternative definitions of the liberalization dummy and CH. As with the ILO results, a notable feature of these findings is that all of the "counteracting" countries displayed zero or negative effects on structural change.

We also plotted simple country-specific graphs of the evolution of the measures of structural change. The regressions of Table 9 focus on whether the mean changes before and after liberalization were different. They do not shed any light on the dynamics of the indicators of labor movement before and after liberalization, such as the timing of the changes in sector shares. Figures 1-3 display the dynamic evolution of structural change, measured by the average value of CH over sectors, for Poland (a reformer), Bolivia (a neutral country) and the Philippines (a counteractive country). Beyond illustrating neatly the results of this paper, these country-specific graphs

³⁷Standard errors were clustered at the year level. We did not run similar regressions for the country-year variables SH and EM, as the data for each country includes just one observations per year for these variables.

³⁸The actual regression results for all definitions of CH and LIB are available upon request.

uncover no systematic timing pattern in the dynamic reaction to a change in the liberalization regime other than the post-reform average differences suggested by the single-country results. The graph for Poland, for example, shows simply that the average change in sector shares (CH2 and CH5 most notably) increased after liberalization, in line with our results for "reformer" countries. Hence, considering the timing of intersectoral shifts country by country provided no insights relative to the pooled results of Table 6.

7 Conclusion

The effects of trade liberalization on the sectoral reallocation of labor are the subject of considerable political debate. Proponents of free trade point to long-term efficiency benefits from trade liberalization through structural change, while its opponents focus on the distributive effects of intersectoral shifts and the potential for increased temporary unemployment during transition. Most of these arguments are based on notions informally derived from classical trade theory, according to which open economies are predicted to specialize according to the pattern of comparative advantage - thereby shifting productive factors across sectors. The actual increase in movements of productive factors across sectors after trade liberalization, however, has not been systematically measured. This paper sought to quantify empirically the extent of post-liberalization intersectoral labor shifts.

We opened this paper by contrasting a set of theories of international trade in which the aftermath of trade liberalization is characterized by increased labor movements, and a set of theories in which labor reallocation is either absent, unobservable or counteracted by other policies. By providing empirical evidence broadly consistent with the second of these sets of theories, this paper has overturned much conventional wisdom about the effects of trade liberalization on labor reallocation. We showed that liberalization episodes were followed, rather unexpectedly, by a reduction in the extent of intersectoral labor shifts at the economy-wide 1-digit level of disaggregation. Liberalization had a weak positive effect at the 3-digit level, and this effect was small in magnitude and sensitive to minor changes in the definition of liberalization or of the measures of sectoral shifts. Moreover, our analysis of cross country variation in the labor market's response to trade liberalization suggests that the policy environment does affect the extent of labor reallocation. Broad-based reforms that include domestic deregulation and privatization have greater effects on intersectoral labor movements than trade reform in isolation, and the extent of post-reform labor shifts appears

positively related to the degree of labor market flexibility.

In summary, claims that trade liberalization generally leads to the absolute declines of entire sectors (broadly defined) are not supported by the data. A weaker statement that trade liberalization may generate, to a limited extent, structural change within sub-categories of the manufacturing sector (but not across broader economic categories), is more consistent with the data. The overall picture that emerges from our results is that significant episodes of liberalization do not appear to be followed by structural upheaval. Finally, it is important to note that our finding that post-liberalization intersectoral labor movements are limited does not preclude the existence of significant intra-sectoral effects, such as those uncovered by some studies using firm-level data. The theoretical implication of this finding are that trade economists should think about comparative advantage more in terms of intrasectoral firm heterogeneity and less in terms of intersectoral differences when discussing the effects of trade liberalization.

Future work should attempt to further uncover the source of these results. We have provided empirical support for a class of theories that includes several models. The welfare gains from trade liberalization are not realized if the limited impact of trade reform on sectoral structure is due to policies that impede structural responses. Welfare gains could be obtained, however, from trade-induced procompetitive effects, intrasectoral reallocation or economy-wide productivity improvements. We have provided empirical evidence in favor of the first of these hypotheses, but this does not preclude a role for the second. This paper was exclusively concerned with the effects of liberalization on intersectoral labor movements, but whether the absence of such movements precludes gains from trade remains an open empirical question.

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Appendix 1: Trade Liberalization Dates

Table A1 contains details of our choice of liberalization dates. As described in the text, we chose our year of liberalization using the following methodology: we use the liberalization dates proposed by Sachs and Warner [1995] as *de jure* liberalization, then choose the *de facto* liberalization as the first year after that with an increase in overall trade of at least 5%.

Sachs and Warner define an economy as closed if it has at least one of the following characteristics: non-tariff barriers covering 40% or more of trade; average tariff rates of 40% or more; a black market exchange rate that is depreciated by 20% or more relative to the official exchange rate, on average, during the 1970s or 1980s, a socialist economic system (as defined by Kornai [1992]); or a state monopoly on exports. An open economy is one in which none of the five conditions applies. The actual date of trade opening is taken to be the year from which the economy is open continuously until the end of their sample, 1994. Earlier liberalization episodes are noted separately.¹

This definition imposes rather stringent conditions for a *de jure* trade opening; perhaps more stringent than are needed for our study. The dates, however, have the advantage of being based on a wide variety of primary sources and a common methodology. With three exceptions, Spain, Argentina and Trinidad and Tobago (as described below), we use the most recent Sachs and Warner liberalization date in the reported results.

Argentina	<p>Argentina is the only country for which we do not use the most recent trade liberalization as reported in Sachs and Warner [1995]. We instead consider an earlier liberalization episode beginning in 1976 and we delete all years after the 1982 reversal to avoid mixing post-liberalization years with those after the reversal. We choose the <i>de facto</i> liberalization year as the year in which the Jose Martinez de Hoz administration came to power and started the series of liberalization reforms by lifting domestic price controls and unifying exchange rates.² Although the Ongania administration (military government, 1967-1970) had made some moves toward liberalization (rationalization of the tariff structure as part of a stabilization package; condensing 60 tariff rates to 16 and reducing the maximum tariff rate from 605% to 140%), quantitative restrictions were increased in many cases, import prohibitions offset much of the tariff liberalization, and liberalizing tendencies were quickly reversed under Peron. The "Martinez de Hoz" liberalization of 1976, beginning with an across-the-board tariff cut and reduction of quantitative restrictions, came as a complete reversal of the Peronist protectionism and is widely considered to be Argentina's first major liberalization. Most quantitative restrictions were eliminated by 1979-80 and nominal tariffs were at their lowest level ever. The <i>de jure</i> liberalization date is also 1976, with an increase in trade of approximately 7%. Trade flows continue to grow from 1976 to 1982, when liberalization was reversed in the midst of a balance-of-payments crisis.</p>
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¹ The Sachs and Warner dates are subject to some error. The authors admit the difficulty of establishing the exact year in which openness was first achieved. The primary data on tariff and non-tariff barriers that they used as a guide is difficult to quantify, and the secondary sources they consulted occasionally contradicted each other.

² Cavallo, Domingo, and Cottani, Joaquin. "Argentina" in Papageorgiou, Demetris, Michael, Michael, and Armeanne Choksi [1991] (hereafter PMC). Vol. 1, pp. 5-167.

Appendix 1: Trade Liberalization Dates

Bolivia	Sachs and Warners' liberalization date of 1985 for Bolivia is based on black market premium data and trade policy information gathered in Sachs and Morales [1988]. The country was previously open from 1956-79. ³ We assign a <i>de jure</i> liberalization date of 1986, the year after liberalization in which there was a sharp increase – 40.3 percent- in exports and imports as a percentage of GDP. Subsequent years show further, but less dramatic increases in trade, with the fastest-growing period being the late 1980s. Trade volumes actually decrease by 2.4% in 1993, but we do not consider this to be a reversal of liberalization policy because growth in trade resumes in 1994 and the average tariff rate in 1995 is still below 10%. ⁴
Brazil	Sachs and Warner record a <i>de facto</i> liberalization date of 1991 for Brazil , focusing on the Collor administration's trade reforms. Coes [1991] reports some earlier attempts at liberalization, but these consisted mainly of export promotion and capital account openings rather than import liberalization. ⁵ Many of the early tariff reductions in the 1960s were simply elimination of redundancy, and later tariff reductions were concentrated in capital and intermediate goods sectors. Even during the most liberal year (1973, according to Papageorgiou et al's index of liberalization) the average effective protection rates were over 40%. ⁶ We follow Sachs and Warner. The <i>de jure</i> liberalization date is also 1991, with a 6.7% increase in trade volume as a percentage of GDP. We find a steady increase 7.8% annualized growth for subsequent years, with particularly large growth of 14% and 11% in 1993 and 1995 respectively.
Chile	Sachs and Warner [1995] select 1976 as the <i>de facto</i> liberalization year for Chile, based on information provided by Dornbusch and Edwards in Bosworth, Dornbusch, and Laban [1994]. De la Cuadra and Hachette [1991] concur, describing 1974-1979 as a period of “major and ambitious” trade liberalization. ⁷ Most quantitative restrictions were eliminated between 1974-76, prior deposits were waived, and official approval for importation dropped in 1976. There were several stages of tariff reductions: 1974-75 when the maximum tariff dropped from 750% to 120% and the simple average dropped from 105 to 57 %; the 1975-77 phase in which a tariff rate of 10-35% and a final stage that led to a uniform tariff rate of 10% by 1979. Trade volume, does increase by 13.9% in 1974, suggesting that some of the impact of liberalization might be felt in that year, but it fell back to the 1973 level in 1975 and did not begin to grow steadily until 1976, the date we use as liberalization year. The <i>de jure</i> liberalization date is also 1976, with an increase in trade volume of 10%. We observe continued rapid growth in trade volume until 1980, a slight decrease in trade volume during the early 1980s and a resumption of annualized growth rate of 4.2% from 1986 to the end of our sample in 1997. We can safely regard the economy as closed up to that point, as an early period of liberalization in 1956-61- mainly a switch from a discretionary to a more neutral system, reduction in tariff dispersion and average tariff – was reversed in 1962, eight years before our data starts.
Colombia	Sachs and Warner report a liberalization date of 1991 for Colombia. ⁸ Garcia Garcia [1991] discusses an earlier liberalization between 1979 and 1982, but this was reversed in 1983-4 with a sharp increase in import restrictions. ⁹ He argues that macroeconomic interference such as capital market distortions and poor money management, combined with remaining pockets of protection reduced the impact of the liberalization on the economy. The <i>de facto</i> liberalization date is also 1991, with an increase of 6.1% in trade volume during that year.

³ Some of these previously open years (from 1977 on for ILO measures, 1970 for UNIDO-3 measures) are included in our sample, but the years studied are far enough from 1956 to be relatively unaffected by this earlier trade liberalization.

⁴ IDB [1997], Figure 17, p.43.

⁵ In PMC Vol. 3, pp. 1-142.

⁶ PMC, Vol. 7, p. 19, Figure 2.2.

⁷ In PMC, Vol. 1, p. 169-320.

⁸ The text of the article (see Table 2 and 15, for example) reports that trade liberalization occurred in 1991, though the Appendix reports that Colombia has been open since 1986. We follow the text, as total trade flows were relative constant in the late 1980s and did not begin to grow steadily until 1990. We test the effect of using the earlier year, 1986, in Appendix 3.

⁹ In PMC, Volume 4, pp. 142-270.

Appendix 1: Trade Liberalization Dates

Costa Rica	Sachs and Warner report liberalization date of 1986 for Costa Rica, focusing on the 1986 reduction in tariffs from an average of 53% to 26% and the reduction in the black market premium to 1%. They classify the country as closed from 1962-85. As a member of the Central American Common Market, its mean external tariff from 1966-86 was 53%. We find a <i>de facto</i> liberalization one year later in 1987, with an increase in trade volume of 13%. Trade volume remained roughly the same for 1988 (a decrease of 0.4%), and grew at an annualized rate of 4.9% for the remaining years in our sample (1989 to 1997).
Ecuador	Sachs and Warner report two liberalization periods for Ecuador: 1950-1982 and 1991-present. We use the latter, as we do not have the comparison data to study the earlier episode. Our data is far enough removed from the initiation of this open period in 1950 to not be affected by the earlier trade liberalization. There also is some evidence that efforts at trade liberalization started earlier: Fontaine [1992] reports that Ecuador reduced import quotas and improved export incentives, between 1980-88, at least in part to fulfill conditions for international development aid. The <i>de jure</i> trade reform actually began in 1990, but was not fully implemented until 1991.
El Salvador	Sachs and Warner report that El Salvador has been open since 1989. We find a <i>de facto</i> liberalization date of 1990, with an increase of 27% in trade volume relative to 1989. Trade volume remained nearly constant in 1991 – a decrease of 0.4% - and subsequently grew at an annualized rate of 6.9% until the end of our sample in 1997.
Ghana	Sachs and Warner report a liberalization date of 1985 for Ghana, focusing on changes in exchange rate policy that reduced the black market premium and eliminated control over foreign exchange allocation. We find a <i>de facto</i> liberalization in 1985 as well, with a 6.6% increase in trade volume over the previous year. Trade continued to grow quickly at 9% and 10% in 1986 and 87 before dropping 3% in 1988 and, with the exception of 11% growth in 1993, growing at a rate of less than 4% for the rest of the sample.
Guatemala	Sachs and Warner report that Guatemala has been open since 1988, with the only previous period of openness between 1950 and 1961 when Guatemala joined the Central American Common Market. We find a <i>de facto</i> liberalization in 1989, with a growth in trade of 5.16%.
Hungary	Sachs and Warner report a <i>de jure</i> liberalization in 1990 for Hungary. We find a <i>de facto</i> liberalization date in 1993, with a growth in trade volume of 5.04% in that year. Trade volume increased steadily from 1993 to the end of our sample in 1996 at an annualized rate of 5.7%.
India	Sachs and Warner report a liberalization date of 1994 for India. The country's high tariffs and many quantitative restrictions led most to consider it closed before 1991. The 1994 date comes from the start of a trade liberalization program and average tariff data. We also find a <i>de facto</i> opening in 1994.
Israel	Sachs and Warner report a <i>de jure</i> liberalization date of 1985 for Israel, based on the signing of a free trade agreement with the US and successful stabilization of inflation. Baruh and Halevi [1991] report a series of previous <i>de jure</i> liberalization episodes both before and potentially in our sample. ¹⁰ The first, in 1952-55, consisted mainly of the replacement of the single official exchange rate with 3 formal rates for different kinds of transactions. Imports decreased, however, in this period. The exchange rate was unified again in 1955. The second liberalization, in 1962-68 included a formal devaluation and reductions in import duties and export subsidies, and gradual reduction of protection of domestic industry. Policymakers began the very slow process of replacing quantitative restrictions with tariffs. The reduction of protection for domestic industry and rate of dispersion of protection accelerated in 1969, along with a decrease in the anti-export bias. The average level of tariff protection fell during the late 1960s and 1970s, from 96% in 1968 to 62% in 1972, and finally 25 % in 1977.
Kenya	Sachs and Warner report a liberalization date of 1993 for Kenya. They note that extensive trade liberalization occurred in the 1980s, but do not consider Kenya open during this period because the black market period was higher than 20% in 1989 and 1990. Fontaine [1992] argues that liberalization was attempted in 1974, 80, 84, and 85. The 1974 and 1980 attempts were reversed quickly when balance of payments deteriorated, but the objective of trade liberalization was affirmed in the 1986 Sessional Paper and Budget Speech. In light of past failures, however, it is unclear whether this announcement was credible enough to create an expectations problem by precipitating agents' adjustment. We find a <i>de facto</i> liberalization date of 1993 as well. Trade increased sharply by 32% in this year before leveling off to a much less dramatic growth rate of 4% in 1994 and close to 0 for subsequent years in our sample.

¹⁰ In PMC Vol. 3, P. 150-156.

Appendix 1: Trade Liberalization Dates

Mexico	Sachs and Warner place the <i>de jure</i> liberalization at 1986 in Mexico. Moderate tariffs and widespread import licensing restricted trade flows in the 1960s and 70s, while the black market premium reduced Mexico's openness in the 1980s. We find a <i>de facto</i> liberalization in 1987, with an increase in trade of 5.7%. Trade grew quickly and steadily from 1987 to the end of our sample in 1995 at an annualized rate of 9.1%.
Morocco	Sachs and Warner find a <i>de jure</i> liberalization date of 1984 for Morocco, based on information reported in Nsouli, et.al. [1995]. The true date may be slightly earlier: the authors report that quota coverage, mean tariff and black market premiums were below their thresholds for openness by the mid 1980s and the mean unweighted tariff, at 47%, was only 7 percentage points above their threshold in 1980. We find a <i>de facto</i> liberalization date of 1987, with an increase of 5.62 in trade volume in that year. Subsequent growth in trade volumes was uneven, but averages to annual rate of 6% until 1995.
New Zealand	Sachs and Warner report that New Zealand has been open since 1986. There is some evidence, though, that the process of liberalization began earlier in the 1980s. Lattimore and Rayner [1991] report two liberalizing initiatives between 1979-1984. ¹¹ The first identified the industries least able to face international competition and developed an industry plan to improve international competitiveness. The second component phased out import licensing for industries not on the Industry Plan. These policies were implemented even as export subsidies and major investment in import competing industries increased. Phase 3 began to cut out these costly interventions in 1984. There was a general shift toward the market in all aspects of the economy, manifested in tariff reduction and phasing out of licensing in trade policy. We find a <i>de facto</i> liberalization date of 1987, with a 7.5% increase in trade flows that year.
Paraguay	Sachs and Warner report a trade liberalization date of 1989 for Paraguay, focusing on the initiation of a trade liberalization program and elimination of the black market premium when the exchange rate was unified in this year. The tariff rate also dropped rapidly to 16.2% by December of 1989. We find a <i>de facto</i> date of 1990, with a massive trade flow increase of 86%. Subsequent growth in trade flows from 1991-1995 was a less spectacular 3% annually.
Philippines	The Philippines has been classified by Sachs and Warner as open since 1988. There is some evidence of earlier moves toward liberalization. Alburo and Shepherd [1991] mention a few earlier instances of trade liberalization: foreign exchange decontrol in 1960-5, promotion of nontraditional exports in 1970-3, and a Tariff Reduction Program that began 1980, was reversed in 1982, and resumed in 1986. ¹² We find a <i>de facto</i> liberalization date of 1988, with an increase in trade flows of 9.8% over the previous years. Our liberalization date appears to be in the middle of a trade surge: trade volume had increased by 10 and 11% in the two previous years and continued to grow at an annualized rate of 7.8% until 1995.
Poland	Poland's <i>de jure</i> and <i>de facto</i> liberalization date is 1990, with a 70% increase in trade volume in that year.
Spain	Sachs and Warner record that Spain has been open since 1960, but there is some evidence that the true impact of reforms did not come until later. An exchange rate crisis in 1959 forced policymakers to reduce some import controls, but de la Dehesa, Ruiz, and Torres [1991] note that "discretionality" took over in the mid and late 1960s and liberalization slowed as new "development plans" were implemented. They describe a generally liberalizing trend beginning in late 1950s, with significant steps taken after 1960 in 1960-6, 1970-74, and 1977-80. Each phase involved more tariff cuts, reductions in quantitative restrictions, and increased export promotion. The substantial changes in the structure of sectoral incentives for exports and imports, however, came with the Suarez government's efforts to overcome the economic imbalances that had accumulated during the political turmoil at the end of the Franco regime. We select 1977, the first year of the Suarez administration, as the <i>de jure</i> liberalization, thus selecting 1979, with an increase in trade volume of 8.32% as the <i>de facto</i> year of liberalization. The last phase was encouraged by Spain's signing of an agreement with the European Free Trade Agreement members in June 1979 and a July 1980 royal decree approving the tariff reductions agreed upon at the Tokyo Round. There was a steady growth in trade volume of 3-4% annually (zero growth in 1980) from 1980-1987. We are not as confident of this liberalization as others, as it is not coded in exactly the same way as the others - this was simply more

¹¹ In PMC Vol.6, Pp. 1-136.

¹² In PMC, Vol.2 pp. 130-308.

Appendix 1: Trade Liberalization Dates

	liberalization in an already "open" economy as defined by Sachs and Warner. From 1960 to 1975 Spain's trade openness index increased by 10 percentage points and there had been some changes in the sectoral origins of GDP as sector growth rates began to diverge. ¹³ Also, the economic situation deteriorated after the second oil price shock in 1979, so much of the liberalization may have been delayed until Spain joined the EEC in 1985.
Sri Lanka	Sri Lanka's <i>de facto</i> and <i>de jure</i> trade liberalization occurred in 1991, according to Sachs and Warner and the World Development Indicators data on trade volume. Trade volume increased by 6.4% in 1991 and continued to grow at roughly the same rate until 1995. Cuthbertson and Athokorala [1991] also report on an earlier episode of trade openness from 1977-83. ¹⁴ We do not use this episode, however, as arbitrarily selective export assistance dominated import liberalization during this period.
Trinidad and Tobago	Sachs and Warner record Trinidad and Tobago as "never open" based on their indicators and the discussion in World Bank [1994]. ¹⁵ We classify the country as open after 1994, however, based on the acceleration of the trade liberalization program that began in 1990. The liberalization was not as complete as others in our sample. Trinidad and Tobago continued to protect its agricultural sector via surcharges imposed on agriculture imports. Stamp duties, different valuation bases and differential consumption taxes continued to protect other sectors. Nevertheless, we code this as a liberalization episode that could have affected the domestic labor market, since the average tariff rate was reduced from 45% to 20% over 1993-1998, with much of the reduction occurring early in this period. Quantitative restrictions were also reduced on many import items : 92% of the quantitative restrictions on manufactured items, for example, were lifted in 1992-3. Imports more than doubled over this same period, while exports increased by about 50%. We code the <i>de jure</i> and the <i>de facto</i> date as 1994, with an increase of 29.04. Although there were some one-year increases in the imports plus exports over GDP earlier in the reform period (notably 1990), 1994 marks the beginning of a steady increase in trade flows for the country.
Turkey	Turkey has been open since 1989 according to Sachs and Warner. Baysan and Blitzer [1991], however, report that trade opening began in 1980. ¹⁶ This episode of liberalization included a 50% devaluation of the lira, increases in direct export incentives, limits on public spending and credit. Longer-run plans to dismantle the quantitative restriction system, open up foreign markets, and allocate resources to export sectors through market mechanisms were implemented in the 1980s, though nominal tariff rates did not fall below 40% until 1989. ¹⁷ We find a <i>de facto</i> date of 1990, with a trade increase of 7.8% over the previous year's trade volume.
Uruguay	Sachs and Warner report a liberalization date of 1990 for Uruguay. Favaro and Spiller [1991] describe earlier reforms in 1974-82. ¹⁸ Trade liberalization started under the military government was successful - high growth, move from traditional exports 75% to 40%, and reduction by ½ of imports of intermediate goods. But halted and reversed in 1982 during a deep recession. The high average tariff rates throughout the period, however, suggest that this earlier period was not a meaningful liberalization.

¹³ In de la Dehesa, Ruiz, and Torres, P. 155 and Table 2.1.

¹⁴ In PMC, Vol. 5, PP. 283-416.

¹⁵ See also Loser and Guerguil [1999] for further justification of this choice of dates.

¹⁶ In PMC Vol. 6, 263-405.

¹⁷ The potential impact of using the earlier date is discussed in Appendix 3.

¹⁸ In PMC, Vol 1. p. 321-407.

Appendix 1: Trade Liberalization Dates

TABLE A1: Trade Liberalization Years

Code	Country	Sachs & Warner ¹⁹	% Change in de jure lib. year	5% threshold	% Change in de facto lib. year	Available ILO	Available UNIDO3	Available UNIDO4
5	Argentina	1976	7.13	1976	7.13		X (1,2 yr)	
20	Bolivia	1985	-2.91	1986	40.32	X	X	
22	Brazil	1991	6.77	1991	6.77	X (5 yr)		
34	Chile	1976	10.97	1976	10.97		X(1,2 yr)	
36	Colombia	1991	6.14	1991	6.14	X	X	X
39	Costa Rica	1986	4.84	1987	13.74	X		
47	Ecuador	1991	7.83	1991	7.83		X	X
49	El Salvador	1989	-4.63	1990	27.47	X (1 yr)		
63	Ghana	1985	6.66	1985	6.66		X	X (1 yr)
70	Guatemala	1988	0.70	1989	5.16		X	X (1,2 yr)
77	Hungary	1990	-1.40	1993	5.04		X	
79	India	1994	5.07	1994	5.07		X	
85	Israel	1985	-0.38	1987	7.92	X	X	
91	Kenya	1993	32.05	1993	32.05		X	X
114	Mexico	1986	2.88	1987	5.69		X	
117	Morocco	1984	2.71	1987	5.62		X	
124	New Zealand	1986	1.26	1987	7.49		X	
133	Paraguay	1989	-6.42	1990	86.60	X		
135	Philippines	1988	9.76	1988	9.76	X	X	X(1,2 yr)
136	Poland	1990	72.04	1990	72.04	X	X	
156	Spain	..		1979	8.32	X	X	
157	Sri Lanka	1991	6.34	1991	6.34		X	
169	Trinidad & Tobago	1994	29.04	1994	29.04	X		
171	Turkey	1989	3.05	1990	7.84	X(1 yr)	X	X
178	Uruguay	1990	6.76	1990	6.76	X(1,2 yr)	X	X

¹⁹ Most recent liberalization when multiple attempts are reported.

Appendix 2: Simultaneous Reforms

Country	Liberalization Year	Classification	Simultaneous Reforms
Argentina	1976	Reformer	Martinez de Hoz's trade liberalization policy was accompanied by other reforms to promote market incentives: the exchange rate was unified by the end of 1976, and price and interest rate controls were lifted. There were still some interventions- tax refunds, production and export financing -to encourage non-traditional exports. Government expenditure increased in 1977, but fell afterwards. [Cavallo and Cottani , 1991]
Bolivia	1986	Neutral	The government did not appear to act significantly to offset trade liberalization. Public expenditure remained constant throughout the mid and late 1980s – there was no increase in non-financial public sector expenditure to signal the growth of elaborate compensation programs. Real wages, however, increased dramatically after 1986 (growing 19.6% in 1988), suggesting that trade liberalization was associated with productivity gains. [IADB, 1997]
Brazil	1991	Reformer	Trade liberalization was one part of the Collor administration's effort to overcome the economic crisis of 1990 and increase competition in Brazil's economy. Privatization began in late October 1991, and some attempts at deregulation were made (most importantly in steel and fuel pricing) [Abreu and Werneck, 1993]
Chile	1976	Reformer	Trade reform in the early years of the pro-market Pinochet administration was accompanied by privatization (or a self-financing rule for public enterprises), elimination of the fiscal deficit, and lifting of price and interest rate controls. The government did not offer subsidies to promote exports. The labor market was also liberalized: increasing the rights of the employer, decreasing pay and special benefits for workers. [de la Cuadra and Hachette, 1991; Stallings and Brock, 1993].
Colombia	1991	Reformer	President Gaviria and his team took advantage of the economic crisis in the early 1990s to pursue a variety of market-oriented reforms in addition to trade liberalization. Price controls were lifted, a financial sector reform passed by Congress, the exchange control system was liberalized, the regulatory framework was modernized, and investment in public services, telecommunications, and ports was opened up to the private sector. Several major banks were privatized, though important holdings in other sectors (mainly mining and energy) were kept under government control. [Hommes, 1996]
Costa Rica	1987	Reformer	Costa Rica's trade liberalization was accompanied by a general move toward market incentives. The country received a structural assistance loan in 1985 that carried conditions such as reduction of spending, increased privatization, etc. There were a series of financial reforms in the late 1980s: interest rates were liberalized, targeted credits were reduced. [IADB 1997]
Ecuador	1991	Reformer	De facto liberalization took place just before a major economic reform program. President Ballen (1992-1996) ran on a platform of modernizing the economy, promoting private sector expansion, among other reforms. Shortly after taking office, public expenditures were cut, and public employment was frozen (later cut by over 10%). Divestiture and deregulation of some public sector enterprises began later, as did contracting of public services to the private sector. The government sold full control in a number of medium public enterprises and granted private concessions for transport in 1994. In 1995 the government privatized the national airline, and congress passed legislation to enable the privatization of the state telephone company. Exchange rates began to reflect market pressures: the sucre was devalued over 30%. The Modernization of the State Law, provides the

Appendix 2: Simultaneous Reforms

Country	Liberalization Year	Classification	Simultaneous Reforms
			legal framework for further public sector reforms, including privatization. The Tax Reform Law and Customs Law simplified procedures and improved enforcement. [World Bank, 1996]
El Salvador	1990	Neutral	Trade liberalization in 1990 was not accompanied by other significant market-oriented reforms. The country was still in the throes of civil war. Efforts to rebuild the economy began with the signing of a peace agreement in 1992. As of 1998, public sector reforms, fiscal reforms, deregulation, and privatization were still in the planning stages. [World Bank 1998a]
Ghana	1985	Neutral	The 1985 trade liberalization in Ghana was part of the Rawlings administration's World Bank and IMF-supported Economic Recovery Program. Multiple exchange rates were at first shuffled to promote exports, then unified and subjected to a series of devaluations. Public sector employment (including in state-owned enterprises) was cut and distortions in wages reduced. Privatization, however, did not begin until the early 1990s. [Leith and Lofchie, 1993]
Guatemala	1989	Reformer	Guatemala made substantial reforms in the labor market in 1990. Financial sector reforms, including a lifting of controls on interest rates, a reduction of targeted credits and some modernization in banking legislation, came slightly later in 1991. The government did not appear to reduce its (already low) presence in production: the share of state-owned enterprises (SOEs) in economic activity grew from 1.6% in 1988 to 2.5% in 1990 and share of state-owned enterprises in gross domestic investment grew from 5.6% to 12.1% over the same time period. Net financial flows from government to SOEs shrank relative to GDP, however, from 0.3% in 1988 to -0.2% in 1990. [Lora, 1997; World Bank, 1995, Tables A.1-A.7])
Hungary	1993	Reformer	Trade liberalization came just before a move toward market incentives. Structural reforms, including currency devaluation, a new exchange-rate mechanism, a tight wage policy in the public sector, and fiscal measures to enhance revenues and cut expenditures accelerated in 1995 with the beginning of a structural reform program. Hungary accelerated privatization efforts, restructuring enterprises (including major commercial banks) and implementing financial sector and public finance reforms, in the mid 1990s. There have also been significant improvements in the legal and regulatory framework of the financial sector. As of 1999, the private sector's employed 2/3 of the labor force and produced over 80% of GDP. [World Bank, 1999a]
India	1994	Neutral	The 1994 trade liberalization was accompanied by a slow movement toward a more market-oriented economy. The government remained a large fiscal presence: central government deficit remained around 5-6 percent of GDP, and consolidated public sector deficit (including states) was still at 9 percent of GDP in 1997. Intervention in exchange markets continued, primarily to stabilize the rupee. The "new industrial policy" announced in July 1991 considerably relaxed the government's regulatory hold on investment and production decisions except for six "strategic" sectors, but there are still substantial restrictions and state involvement in some manufacturing sectors. Central and state governments continued to regulate the prices of most essential products, including many industrial inputs. [U.S. State Department, 1998; Srinivasan, 2000]
Israel	1987	Counteract	Israel's heterodox stabilization program may have offset the effects of trade liberalization. Social pacts based on broad coalitions of labor, government, and industry, set the patterns for prices, wages, and the exchange rate. [Bufman and Leiberman, 1995]

Appendix 2: Simultaneous Reforms

Country	Liberalization Year	Classification	Simultaneous Reforms
Kenya	1993	Neutral	As of 1997, limited progress had been made in other market-oriented reforms. The government did divest 22 enterprises in 1997 and reforms of large parastatals, including those in transport, telecommunication, petroleum, reinsurance, agricultural produce and hotels, are in various stages of implementation. Civil service reform has lagged. [World Bank, 1998b]
Mexico	1987	Reformer	Mexico's trade liberalization came in the middle of a general effort to reduce the state's presence in parts of the economy. The privatization program began before trade liberalization with the sale of medium and small businesses and continued after liberalization with more complicated transfers of larger, less naturally competitive enterprises such as the national telephone company and the national airline. [Sanchez and Corona, 1993]
Morocco	1987	Neutral	Trade liberalization appears to be a relatively isolated reform in Morocco. The share of state-owned enterprises in economic activity dropped from 20% in 1987 to 15% in 1990, but SOE investment as a percent of gross domestic investment and net financial flows from the government to SOEs as a percent of GDP remained steady at 18% and 0.1% respectively. [World Bank, 1995, Table A.1-A.7]
New Zealand	1987	Reformer	Trade liberalization in 1987 was part of an overall reform package that closely followed tenets of neoclassical theory. The fiscal deficit was reduced, in part by cutting expenditures on tariff compensation, also by reducing direct government involvement in investment projects, and corporatizing major investment departments. Monetary controls were lifted and the currency was floated. (Duncan and Bollard [1992], Lattimore and Rayner [1991])
Paraguay	1990	Reformer	Trade liberalization was part of a larger set of reforms to increase market incentives in Paraguay's economy. Multiple exchange rates were unified in 1989, and the exchange market and capital account were liberalized. The public sector deficit was reduced and financial management of the public enterprises tightened. Several public enterprise were slated for privatization, but the process has been slow, with only two out of six privatizations carried out by 1995. The tax system was simplified and modernized in 1991. Interest rates were liberalized in 1990; by 1991, they were completely freed. Selective credit controls were abolished almost completely. [World Bank, 1996]
Philippines	1988	Counteract	State-owned enterprises as a percentage of total economic activity rose from 1.9% in 1987 to 3.0% in 1989 and the share of SOE investment in gross domestic investment jumped from 6.8% in 1987 to 11.6 in 1990. Share of SOEs in total employment also increased from 0.7% in 1987 to 1.2% in 1990 (but then fell to 0.5% in 1991). Net financial flows from the government to SOEs rose from 1.6% of GDP in 1987 to 3.2% in 1989. [World Bank, 1995, Tab. A.1-A.7]
Poland	1990	Reformer	Trade liberalization in Poland was party of the "Balcerowicz Plan," a comprehensive, swiftly implemented, set of market-oriented reforms. The reforms included removal of price controls, reduction in government expenditure and investment, devaluation of the exchange rate, and removal of subsidies for energy. Privatization, however, was secondary to the main goal of stabilizing the economy. [Johnson and Kowalska, 1994]

Appendix 2: Simultaneous Reforms

Country	Liberalization Year	Classification	Simultaneous Reforms
Spain	1979	Reformer	Trade reform came during a period of recovery from the economic crisis of the mid 1970s. Government expenditures rose in the late 1970s, mainly due to welfare and unemployment payments as well as subsidies to public industries. Government intervention, however, decreased somewhat as price controls were lifted. [de la Dehesa, Ruiz, and Torres, 1991]
Sri Lanka	1991	Reformer	Privatization, including turning the management of most tea plantations over to the private sector, followed trade reform in Sri Lanka. Improvements in regulatory and incentive systems in the early 1990s also encouraged private participation in infrastructure, including power generation, telecommunications and ports. Since 1992, Significant regulatory, supervisory, and institutional reforms have been implemented in the financial sector since 1992, though the banking system was still dominated by two state commercial banks in 1998. [World Bank, 1998c]
Trinidad & Tobago	1994	Counteract	As of June 2000, trade liberalization was still far ahead of other market-oriented reforms. The government remained involved in gas-related products, agriculture, tourism, and several public utilities. The public sector employed about 25% of the labor force compared to the 20% involved in agriculture and industry. Infrastructure bottlenecks and inefficiencies in the delivery of public services still deter private sector activity. Policies to strengthen regulatory frameworks for the financial sector, water, power, and telecommunications were being planned but not yet implemented. [World Bank, 2000]
Turkey	1990	Counteract	The government remained involved in the economy and continued trade protection for state-owned enterprises. Uncompetitive SOEs were subsidized. [World Bank, 1995]
Uruguay	1990	Neutral	Neoliberal reform efforts other than trade liberalization were largely unsuccessful in the early 1990s. Law articles allowing for the privatization of the telecommunications monopoly were overturned in a referendum in 1992 and the public sector deficit accelerated sharply over 1992-1994. [World Bank, 1996]

Appendix 3 - Robustness

A. Multiple Liberalizations

In countries with multiple reported liberalization attempts, we chose episodes around which we can obtain a sufficient sample of pre-liberalization years and post-liberalization years in which the trade liberalization policies have not been reversed. If there are multiple such episodes, we choose the deeper reform. With the exception of Argentina, our reported results focus on the most recent liberalization.

An alternate method of examining the impact of trade liberalization on labor market shuffling would have been to consider the years around all liberalization episodes as "post liberalization" regimes. The earlier, less significant reforms could potentially have the same type of labor market effects as the more drastic reforms. Including the years just after these reforms as "pre-liberalization regime" years could bias our results toward not finding statistical evidence for shuffling. Also, these earlier reforms and reversals could provoke skepticism about the trade liberalization episodes we study. We would expect the private sector to react less (ie shuffle less) in response to non-credible reforms than to credible policy changes. Our results are robust to this alternate specification.

Multiple liberalization periods are only a minor problem for Bolivia and Ecuador. Bolivia was open earlier from 1956 to 1979. We include observations from 1977 on for ILO measures and 1970 for UNIDO-3 measures in our sample, as these years are far enough from the liberalization in 1956 to be considered unaffected. Sachs and Warner report two liberalization periods for Ecuador: 1950-1982 and 1991-present. We use the latter, as we do not have the comparison data to study the earlier episode. Our sample, beginning in 1970 is far enough removed from the initiation of this open period to not be affected by the earlier trade liberalization. We create LIB2 and LIB5 for all liberalizations in the countries that have more potential to bias our results. LIB (all years after liberalization) runs in to the later, more significant trade liberalization. We carry out an alternative analysis that includes a second liberalization episode for the following countries:

Colombia	Colombia had an earlier liberalization between 1967 and 1982. Export promotion came first from 1967-1975, but a reduction of the real effective exchange rate, and an accelerated reduction of import barriers in 1979 combined to increase import penetration over 1976-82. The reforms were reversed in 1983-4 with a sharp increase in import restrictions. We use the later liberalization in 1991, however, in our sample as the effects of this liberalization might be complicated by other macroeconomic interference such as capital market distortions. ¹ For the sake of completeness and a robustness check, however, we consider 1979, the year of accelerated reform, as a de facto liberalization year, and find a de jure year 1980. We do not have the data to include this liberalization in the ILO or UNIDO 4 sample (For ILO, we have data from 1975-1980 and 1985-1997, while for UNIDO4 we have data from 1981-1983,1985-1996), but we do code this as a liberalization (LIB2 and LIB5 only) for UNIDO3.
Israel	Israel also had a series of liberalizations before the 1987 one we use. A liberalizing trend in the 1970s falls in our sample. The reduction of protection for domestic industry and rate of dispersion of protection accelerated in 1969, along with a decrease in the anti-export bias. The average level of tariff protection fell during the late 1960s and 1970s, from 96% in 1968 to 62% in 1972, and finally 25 % in 1977. ² If we were to consider this last period as a liberalization episode, 1973 is the only candidate for a de facto liberalization year, with an increase in trade volume of 21%. Otherwise, 1987 is the first year that has an increase in trade volume of over 5%. We define 1973 as a liberalization year and code it accordingly for LIB2 and LIB5.

¹ Garcia Garcia [1991] suggests that this earlier reform may have been a "wasted opportunity."

² Baruh and Halevi [1991]

Appendix 3 - Robustness

Kenya	Kenya attempted liberalizations in 1974, 80, 84, and 85. The 1974 and 1980 attempts were reversed quickly when balance of payments deteriorated, but the objective of trade liberalization was affirmed in the 1986 Sessional Paper and Budget Speech. ³ If we take 1974 as a de jure date, 1974 would be a de facto date; and the 1980 or 1984 de jure liberalizations would have 1984 as the de facto date. The first liberalization is out of our data range and we code the latter as a second liberalization date for Kenya in UNIDO 3 digit and 4 digit analyses.
The Philippines	The Philippines began a Tariff Reduction Program in 1980, reversed it in 1982, and resumed in 1986. If we chose 1980 as the de jure liberalization year, then that would also be the de facto year, as there was a 22% increase in trade volume that year. This liberalization is unlikely to have had a large effect on the economy, as subsequent years have 0 or slightly negative change in trade volume. Nevertheless, we code 1980 as a second liberalization year for the Philippines in the alternative analysis.
New Zealand	New Zealand had two liberalizing initiatives between 1979-1984. The first identified the industries least able to face international competition and developed an industry plan to improve international competitiveness. The second component phased out import licensing for industries not on the Industry Plan. These policies were implemented even as export subsidies and major investment in import competing industries increased. Phase 3 began to cut out these costly interventions in 1984. There was general shift toward the market in all aspects of the economy, manifested in tariff reduction and phasing out of licensing in trade policy. There is also a 6% jump in 1979, so this year would count as a de facto liberalization if we took 1979 as the de jure liberalization. This liberalization may not have had a major effect, as it was so selective, but we consider it in the alternative analysis.
Sri Lanka	Sri Lanka shifted from quotas to tariffs, devalued and switched to a managed float, and imposed some new export assistance measures in 1977-1979. These reforms were reversed in 1983. This was a selective and incomplete liberalization, that included heavy and arbitrarily selective export promotion using revenues raised from a surcharge on dutiable imports. If we were to say that 1977 is a de jure liberalization then 1978 would be the de facto liberalization date according to our criteria. We consider this date for the UNIDO 3 digit sample.

B. The Expectations Problem

The second alternative set of regressions focuses on eliminating the expectations problem, at the expense of including some false or unfulfilled liberalizations. We use the earliest possible de jure liberalization dates to construct our indicators of liberalization regimes. In most countries, these are simply the Sachs and Warner de jure dates, but the following countries have slightly different dates:

Colombia	For Colombia, we use 1986, the date reported in the Appendix (rather than the text) of Sachs and Warner. Garay [1991] presents evidence that average tariff rates fell below 40% in 1986 and have remained low up to the present. Policy changes in this year appear to have had an immediate, though not sustained effect: trade flows increased by 6.9% in 1986, but changed by less than 1% per year for the rest of the 1980s.
Chile	For Chile, we use 1974. The reduction of tariffs and the reform program began in 1974, though it did not reach the level of openness required by Sachs and Warner until 1976. The later date seems to be appropriate, as trade volume increased by 13% in 1974, but then was -10% in 1975 and didn't start growing steadily until 1976, but the beginning of reform in 1974 ,might alter agents' expectations.

³ Fontaine [1992].

Appendix 3 - Robustness

Ecuador	We use 1990 for Ecuador. Trade liberalization actually began in 1990, but was not fully implemented until 1991 (the date Sachs and Warner recognize as the de jure liberalization). ⁴
India	India's earliest date of trade reform is 1991. Sachs and Warner report a liberalization date of 1994 for India, but the progression from closed to open economy actually took place over 1991-1994. The 1994 date appears to be the most appropriate indicator of a "real" liberalization, as it is based on the start of a trade liberalization program and average tariff data, but there is some evidence of adjustment in these earlier years. In 1992 and 1993 trade volume increased by 5 and 6% respectively, more than all other years except for 1995.
New Zealand	We take 1979 as the earliest liberalization date for New Zealand. Lattimore & Rayner [1991] report two liberalizing initiatives between 1979-1984. The first identified the industries least able to face international competition and developed an industry plan to improve international competitiveness. The second component phased out import licensing for industries not on the Industry Plan. These policies were implemented even as export subsidies and major investment in import competing industries increased. Phase 3 began to cut out these costly interventions in 1984. There was a general shift toward the market in all aspects of the economy, manifested in tariff reduction and phasing out of licensing in trade policy. There is also a 6% jump trade volume in 1979.
Spain	We exclude Spain from this sample altogether, as its earliest date of reform is 1950.
Turkey	For Turkey, we use 1983, the beginning of the Ozal administration, as the earliest possible liberalization date. Trade liberalization and a general move toward market-oriented policies formed part of this government's electoral platform, and Ozal did de-emphasize subsidies, liberalize imports by removing some quantitative restrictions and reforming tariffs, and steadily devalue the exchange rate (with the exception of a slight appreciation in 1985 as domestic inflation rose). ⁵ Liberalization during the 1980s was not extensive enough to be classified as a Sachs and Warner opening: average nominal tariff rates did not fall below 40% until 1989 and the trade volume did not increase significantly until 1990. ⁶ Nevertheless, this earlier liberalization may have affected agents' expectations. ⁷
Trinidad and Tobago	Trade liberalization and other economic reforms began in the early 1990s in Trinidad and Tobago. Asad and Rajapatirana [1993] identify 1990 as the beginning of an episode of trade liberalization. There is also a 39% increase in trade for this one year. We do not use this earlier date in our main sample for two reasons: first, Sachs and Warner consider the economy to be closed as of 1994, and second, the implementation of substantial tariff reductions required for membership in CARICOM (Caribbean Community regional trade agreement) did not begin until 1993-1994. The one-year increase in trade in 1990 was also followed by two years of 25% yearly decreases in trade. For the purposes of the test for whether expectations of trade reform affected adjustment to liberalization, we code 1990 as the earliest possible date of liberalization.

⁴ Fontaine [1992] also reports that Ecuador reduced some import quotas and improved export incentives, between 1980-88, at least in part to fulfill conditions for international development aid (p. 32). It is unclear whether these scattered efforts were seen as a precursor to a more general liberalization.

⁵ The most notable policy switch here was to move to a system where there were lists of permitted (importable) items, with everything else prohibited to a system with lists of prohibited goods and all else permitted.

⁶ Tariff rates were adjusted in December 1983 and again in January 1984. Some went up, some went down, but the overall effect was strongly liberalizing, according to Onis and Webb [1994].

⁷ The liberalization did affect the manufacturing sector's business practices: Fouran [1996] reports that the increased import penetration in the manufacturing sector in the early 1980s led to a small, but statistically significant, increase in total factor productivity growth and a small, significant decrease in price-cost margins.

Table 1: Summary Statistics for Measures of Sectoral Change

Variable	# Obs.	Mean	Std. Dev.	Min	Max
ILO					
CH2	1373	0.745	0.996	0	10.521
CH5	1166	1.346	1.592	0	11.872
SH2	173	2.631	3.105	0	18.739
SH5	147	4.453	5.040	0	24.398
EM2	173	4.593	8.124	-18.737	67.115
EM5	147	10.050	14.800	-24.541	82.443
UNIDO- 3 Digit					
CH2	11944	0.373	0.706	0	14.028
CH5	9500	0.630	1.098	0	23.192
SH2	453	3.695	4.478	0	33.622
SH5	362	4.765	5.372	0	32.832
EM2	453	3.661	12.830	-50.910	73.477
EM5	362	8.736	23.270	-72.607	78.990
UNIDO- 4 Digit					
CH2	5412	0.180	0.354	0	6.762
CH5	3046	0.265	0.455	0	4.823
SH2	78	6.251	3.823	0.674	17.360
SH5	44	8.542	5.303	1.429	23.947
EM2	78	2.681	10.935	-25.084	33.569
EM5	44	8.815	13.730	-39.480	35.852

Table 2: Conditional Means of Measures of Structural Change

ILO						
	Liberalization in the past 2 years (LIB2)		Liberalization in the past 5 years (LIB5)		Liberalization in the past (LIB)	
	NO	YES	NO	YES	NO	YES
CH2	0.760	0.672	0.787	0.646	0.812	0.666
CH5	1.420	1.051	1.487	1.083	1.535	1.186
SH2	2.701	2.271	2.873	2.035	2.980	2.196
SH5	4.954	2.416	5.498	2.425	5.480	3.520
EM2	4.816	3.437	4.558	4.679	4.572	4.618
EM5	10.231	9.310	9.891	10.356	9.678	10.387
UNIDO – 3 Digit						
CH2	0.369	0.406	0.367	0.398	0.376	0.368
CH5	0.620	0.695	0.606	0.709	0.608	0.682
SH2	3.641	4.071	3.629	3.934	3.851	3.344
SH5	4.643	5.512	4.447	5.786	4.550	5.259
EM2	3.873	2.191	4.574	0.353	4.670	1.383
EM5	9.245	5.636	10.437	3.277	11.062	3.409
UNIDO- 4 Digit						
CH2	0.177	0.185	0.184	0.169	0.175	0.186
CH5	0.261	0.268	0.264	0.267	0.261	0.268
SH2	6.760	4.422	6.795	5.468	6.649	5.809
SH5	8.764	8.113	8.193	8.832	8.193	8.832
EM2	1.228	7.896	3.110	2.065	3.092	2.227
EM5	6.127	14.012	9.270	8.435	9.270	8.435

Table 3 - Fixed Effects Regressions of Sectoral Change on Liberalization Status

Table 3a - ILO						
	CH2	CH5	SH2	SH5	EM2	EM5
LIB2	-0.075 (0.075)	-0.313** (0.108)	-0.255 (0.587)	-2.258** (0.814)	-2.189 (1.385)	-3.031 (2.082)
Adj. R ²	0.278	0.396	0.182	0.411	0.335	0.553
LIB5	-0.089 (0.065)	-0.245** (0.093)	-0.404 (0.483)	-2.228** (0.695)	-1.270 (1.145)	-3.440* (1.785)
Adj. R ²	0.279	0.394	0.184	0.422	0.330	0.558
LIB	-0.089 (0.070)	-0.187 (0.106)	-0.623 (0.453)	-2.001** (0.698)	-0.154 (1.081)	-0.292 (1.804)
Adj. R ²	0.279	0.392	0.190	0.414	0.325	0.546
# obs.	1373	1166	173	147	173	147
# countries	10	10	10	10	10	10

Table 3b - UNIDO 3-Digit						
	CH2	CH5	SH2	SH5	EM2	EM5
LIB2	0.033 (0.031)	0.065 (0.040)	0.519 (0.546)	0.849 (0.663)	-1.691 (1.745)	-4.074 (3.059)
Adj. R ²	0.351	0.459	0.271	0.336	0.093	0.247
LIB5	0.038 (0.024)	0.097** (0.032)	0.759* (0.445)	1.819** (0.540)	-4.020** (1.414)	-7.564** (2.498)
Adj. R ²	0.351	0.460	0.274	0.354	0.107	0.263
LIB	-0.002 (0.024)	0.057* (0.032)	-0.095 (0.430)	0.961* (0.536)	-3.281** (1.366)	-8.503** (2.439)
Adj. R ²	0.350	0.459	0.269	0.339	0.103	0.269
# obs.	11944	9500	453	362	453	362
# countries	20	18	20	18	20	18

Standard Errors in parentheses, clustered at the country-year level for CH regressions
 CH Regressions include country-sector effects; SH and EM regressions include country effects.
 *=statistically significant at the 10% level; **=statistically significant at the 5% level.

Table 3 (continued) - Fixed Effects Regressions of Sectoral Change on Liberalization Status

Table 3c - UNIDO - 4 Digit						
	CH2	CH5	SH2	SH5	EM2	EM5
LIB2	-0.006 (0.016)	-0.008 (0.016)	-2.467** (1.000)	-0.948 (1.641)	6.633** (2.933)	8.010* (4.319)
Adj. R ²	0.409	0.696	0.121	0.063	0.076	0.031
LIB5	0.012 (0.016)	0.011 (0.016)	-1.427* (0.862)	0.286 (1.628)	-0.871 (2.557)	-0.643 (4.456)
Adj. R ²	0.409	0.696	0.080	0.055	0.010	-0.056
LIB	-0.006 (0.014)	0.011 (0.016)	-1.381 (0.879)	0.286 (1.628)	-1.494 (2.599)	-0.643 (4.456)
Adj. R ²	0.409	0.696	0.077	0.055	0.013	-0.056
# obs.	5412	3046	78	44	78	44
# countries	7	5	7	5	7	5

Robust standard Errors in parentheses, clustered at the country-year level for CH regressions.

CH Regressions include country-sector effects; SH and EM regressions include country effects.

*=statistically significant at the 10% level; **=statistically significant at the 5% level.

Table 4 - Relative Impact of Trade Liberalization on Intersectoral Labor Movements

Independent Variable	Dependent Variable	Relative Effect (%)	Relative 95% confidence interval lower bound (%)	Relative 95% confidence interval upper bound (%)
ILO - Country*Sector Effects				
CH2	LIB2	-9.868	-29.605	9.868
CH5	LIB2	-22.042	-37.254	-6.831
CH2	LIB5	-11.309	-27.827	5.210
CH5	LIB5	-16.476	-28.985	-3.968
CH2	LIB	-10.961	-28.202	6.281
CH5	LIB	-12.182	-25.993	1.629
UNIDO 3 Digit - Country*Sector Effects				
CH2	LIB2	8.943	-7.859	25.745
CH5	LIB2	10.484	-2.419	23.387
CH2	LIB5	10.354	-2.725	23.433
CH5	LIB5	16.007	5.446	26.568
CH2	LIB	-0.532	-13.298	12.234
CH5	LIB	9.375	-1.151	19.901
UNIDO 4 Digit - Country*Sector Effects				
CH2	LIB2	-3.390	-21.469	14.689
CH5	LIB2	-3.065	-15.326	9.195
CH2	LIB5	6.522	-10.870	23.913
CH5	LIB5	4.167	-7.955	16.288
CH2	LIB	-3.429	-19.429	12.571
CH5	LIB	4.215	-8.046	16.475

NOTE: The relative effect is the percent increase in sectoral change (as measured by changes in sector shares) in a liberalized regime relative to the extent of shuffling during non-liberalization periods.

**Table 5 - Fixed Effects Regressions of Sectoral Change on Liberalization Status
Earliest Possible Liberalization Date**

Table 5a - ILO						
	CH2	CH5	SH2	SH5	EM2	EM5
LIB2	-0.069 (0.080)	-0.184 (0.116)	0.060 (0.596)	-1.754** (0.865)	-3.606** (1.388)	-8.349** (2.084)
Adj. R ²	0.278	0.391	0.181	0.396	0.352	0.594
LIB5	-0.158** (0.067)	-0.289** (0.105)	-0.764 (0.468)	-2.418** (0.684)	-0.856 (1.119)	-5.843** (1.722)
Adj. R ²	0.283	0.396	0.194	0.430	0.327	0.582
LIB	-0.164** (0.071)	-0.275** (0.114)	-0.890** (0.439)	-2.485** (0.676)	-0.210 (1.056)	-1.777 (1.771)
Adj. R ²	0.284	0.396	0.201	0.434	0.325	0.550
# obs.	1373	1166	173	147	173	147
# countries	10	10	10	10	10	10

Table 5b - UNIDO - 3 Digit						
	CH2	CH5	SH2	SH5	EM2	EM5
LIB2	-0.011 (0.030)	0.047 (0.037)	-0.259 (0.549)	0.615 (0.664)	-1.854 (1.754)	-2.556 (3.063)
Adj. R ²	0.350	0.459	0.270	0.334	0.093	0.245
LIB5	0.009 (0.023)	0.051 (0.031)	0.346 (0.435)	1.697** (0.521)	-3.217** (1.384)	-6.659** (2.415)
Adj. R ²	0.350	0.459	0.270	0.352	0.102	0.260
LIB	-0.017 (0.023)	0.030 (0.029)	-0.233 (0.408)	0.944* (0.512)	-3.623** (1.293)	-9.179** (2.319)
Adj. R ²	0.351	0.459	0.270	0.339	0.107	0.276
# obs.	11944	9500	453	362	453	362
# countries	20	18	20	18	20	18

Robust standard errors in parentheses, clustered at the country-year level for CH regressions.
CH Regressions include country-sector effects; SH and EM regressions include country effects.
*=statistically significant at the 10% level; **=statistically significant at the 5% level.

Table 5 (continued) - Fixed Effects Regressions of Sectoral Change on Liberalization Status - Earliest Possible Liberalization Date

Table 5c - UNIDO - 4 Digit						
	CH2	CH5	SH2	SH5	EM2	EM5
LIB2	-0.008 (0.019)	-0.027 (0.019)	-2.851** (0.974)	-0.191 (1.847)	7.877** (2.855)	2.821 (5.036)
# obs.	5412	3046	78	44	78	44
# countries	7	5	7	5	7	5
Adj. R ²	0.409	0.696	0.149	0.055	0.105	-0.048
<hr/>						
LIB5	0.012 (0.016)	-0.027 (0.016)	-1.691* (0.857)	-1.185 (1.677)	1.264 (2.562)	-6.135 (4.511)
# obs.	5412	3046	78	44	78	44
# countries	7	5	7	5	7	5
Adj. R ²	0.409	0.697	0.095	0.067	0.012	-0.008
<hr/>						
LIB	-0.006 (0.018)	-0.016 (0.033)	-2.283** (0.995)	-1.586 (2.331)	1.212 (3.003)	-5.595 (6.356)
# obs.	5412	3046	78	44	78	44
# countries	7	5	7	5	7	5
Adj. R ²	0.409	0.696	0.111	0.066	0.010	-0.035

Robust standard errors in parentheses, clustered at the country-year level for CH regressions.
 CH Regressions include country-sector effects; SH and EM regressions include country effects.
 *=statistically significant at the 10% level; **=statistically significant at the 5% level.

Table 6 - Regressions of Structural Change on Post-Liberalization Time Dummies

ILO			UNIDO 3 Digit			UNIDO 4 Digit		
	CH2	CH5		CH2	CH5		CH2	CH5
Year 1	-0.055 (0.124)	-0.307** (0.164)	Year 1	-0.001 (0.043)	0.082 (0.066)	Year 1	-0.005 (0.026)	-0.010 (0.023)
Year 2	-0.135* (0.079)	-0.233* (0.122)	Year 2	0.043 (0.050)	0.060 (0.060)	Year 2	0.019 (0.023)	0.024 (0.030)
Year 3	-0.234** (0.093)	-0.131 (0.093)	Year 3	0.071 (0.041)	0.156** (0.062)	Year 3	0.036 (0.024)	0.026** (0.013)
Year 4	0.051 (0.217)	-0.040 (0.162)	Year 4	0.021 (0.048)	0.108 (0.075)	Year 4	0.024 (0.033)	0.037 (0.029)
Year 5	-0.109 (0.101)	-0.027 (0.225)	Year 5	-0.004 (0.062)	0.069 (0.081)	Year 5	-0.009 (0.033)	0.015 (0.016)
Year 6	-0.038 (0.096)	0.099 (0.246)	Year 6	-0.026 (0.045)	-0.015 (0.090)	Year 6	-0.054 (0.043)	
Year 7	-0.028 (0.070)	-0.028 (0.177)	Year 7	-0.019 (0.074)	-0.235** (0.090)	Year 7	-0.079** (0.021)	
Year 8	0.063 (0.083)	-0.041 (0.163)	Year 8	-0.045 (0.061)	-0.157 (0.115)	Year 8	-0.134** (0.021)	
Adj R ²	0.277	0.389	Adj R ²	0.351	0.460	Adj R ²	0.412	0.696
# obs.	1373	1166	# obs.	11944	9500	# obs.	5412	3046

Robust standard errors in parentheses, clustered at the country-year level.

Regressions include country-sector effects (not reported).

*=statistically significant at the 10% level; **=statistically significant at the 5% level.

Table 7- Fixed Effect Regressions, Sample Split by Job Security

ILO 1 Digit: Low Job Security			ILO 1 Digit: High Job Security		
	CH2	CH5		CH2	CH5
LIB2	-0.308** (0.136)	-0.687** (0.228)	LIB2	0.144 (0.133)	-0.068 (0.142)
Adj. R ²	0.223	0.351	Adj. R ²	0.346	0.308
LIB5	-0.036 (0.169)	-0.422* (0.249)	LIB5	-0.028 (0.099)	-0.122 (0.108)
Adj. R ²	0.212	0.337	Adj. R ²	0.340	0.311
LIB	-0.007 (0.149)	-0.146 (0.267)	LIB	0.020 (0.093)	0.032 (0.123)
Adj. R ²	0.212	0.328	Adj. R ²	0.340	0.308
# obs.	406	364	# obs.	366	276
# countries	4	4	# countries	3	3

UNIDO 3 Digit: Low Job Security			UNIDO 3 Digit: High Job Security		
	CH2	CH5		CH2	CH5
LIB2	0.035 (0.047)	-0.033 (0.043)	LIB2	-0.005 (0.037)	-0.016 (0.061)
Adj. R ²	0.317	0.441	Adj. R ²	0.273	0.427
LIB5	0.056* (0.033)	0.074 (0.049)	LIB5	-0.050 (0.032)	-0.027 (0.046)
Adj. R ²	0.319	0.442	Adj. R ²	0.274	0.427
LIB	0.030 (0.031)	0.014 (0.045)	LIB	-0.101** (0.036)	-0.102** (0.047)
Adj. R ²	0.317	0.441	Adj. R ²	0.277	0.429
# obs.	3248	2915	# obs.	3878	3251
# countries	4	4	# countries	6	5

Robust standard errors in parentheses, clustered at the country-year level.

Regressions include country-sector effects (not reported).

*=statistically significant at the 10% level; **=statistically significant at the 5% level.

Table 8 - Fixed Effects Regressions of Changes in Sectoral Structure on Liberalization Regime

ILO: Non Reformer			ILO: Reformer		
	CH2	CH5		CH2	CH5
LIB2	-0.085 (0.116)	-0.269 (0.222)	LIB2	-0.066 (0.096)	-0.343** (0.095)
Adj. R ²	0.231	0.350	Adj. R ²	0.342	0.462
LIB5	-0.187** (0.088)	-0.301** (0.164)	LIB5	-0.003 (0.094)	-0.202* (0.106)
Adj. R ²	0.236	0.352	Adj. R ²	0.341	0.456
LIB	-0.191** (0.089)	-0.355** (0.159)	LIB	-0.001 (0.105)	-0.032 (0.139)
Adj. R ²	0.237	0.356	Adj. R ²	0.341	0.451
# obs.	674	538	# obs.	699	628
# countries	5	4	# countries	5	6

UNIDO 3: Non Reformer			UNIDO 3: Reformer		
	CH2	CH5		CH2	CH5
LIB2	-0.031 (0.038)	-0.126** (0.036)	LIB2	0.066 (0.046)	0.177** (0.061)
Adj. R ²	0.323	0.405	Adj. R ²	0.386	0.487
LIB5	-0.039 (0.035)	-0.088** (0.037)	LIB5	0.044 (0.031)	0.180** (0.041)
Adj. R ²	0.323	0.405	Adj. R ²	0.386	0.489
LIB	-0.097** (0.037)	-0.142** (0.038)	LIB	0.022 (0.028)	0.120** (0.037)
Adj. R ²	0.326	0.407	Adj. R ²	0.386	0.487
# obs.	5922	5251	# obs.	7940	6918
# countries	8	8	# countries	11	10

Robust standard errors in parentheses, clustered at the country-year level.

Regressions include country-sector effects (not reported).

*=statistically significant at the 10% level; **=statistically significant at the 5% level.

Table 9a. ILO Single Country Results for Structural Change (CH)

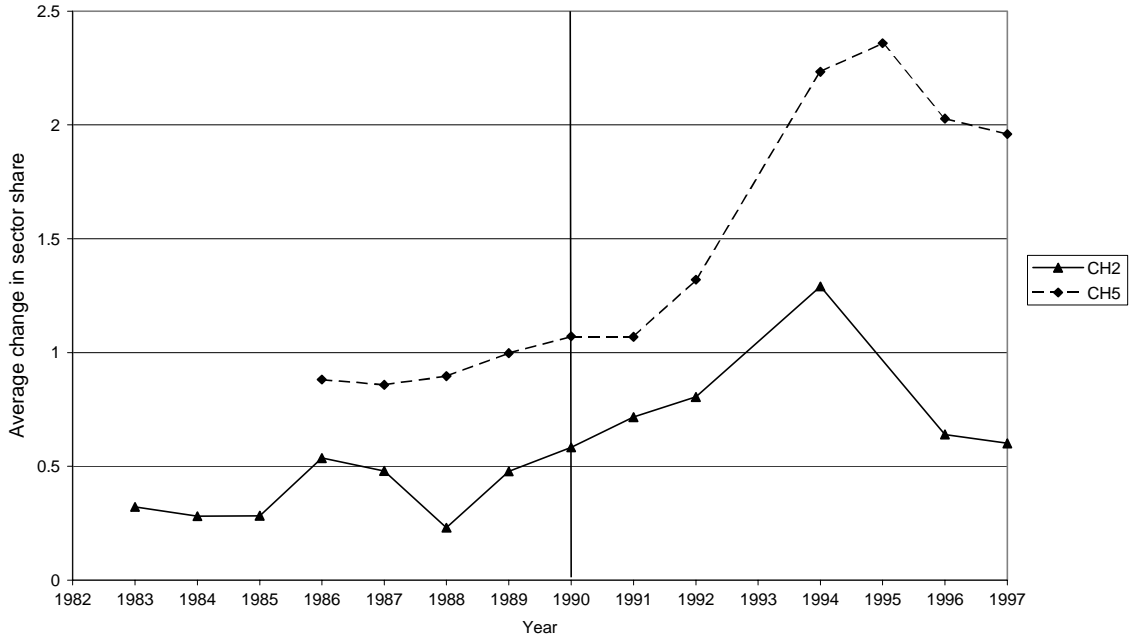
	Negative Effect	Negative Insignificant	Zero Effect	Positive Insignificant	Positive Effect
Reformer	Spain	Brazil Colombia	Costa Rica		Paraguay Poland
Neutral	Trinidad and Tobago	Uruguay	El Salvador	Bolivia	
Counteractive	Philippines Turkey			Israel	

Table 9b. UNIDO 3-Digit Single Country Results for Structural Change (CH)

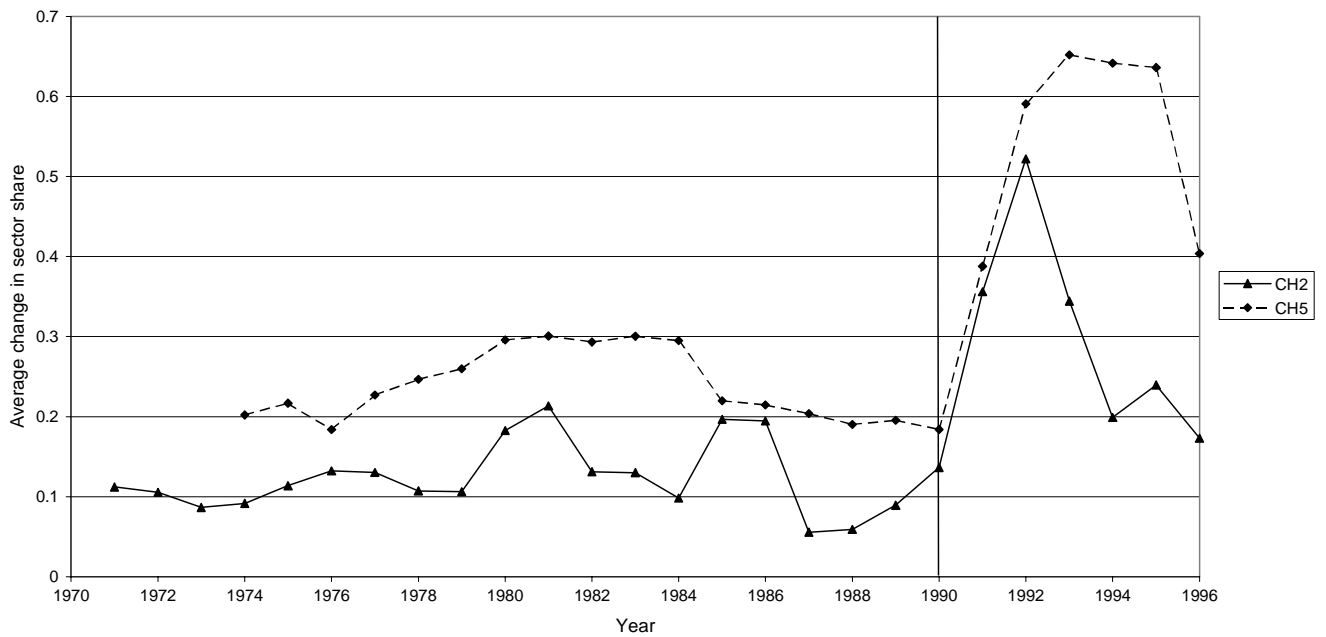
	Negative Effect	Negative Insignificant	Zero Effect	Positive Insignificant	Positive Effect
Reformer	Mexico	Guatemala	Colombia	Ecuador New Zealand Sri Lanka Argentina	Chile Hungary Poland Spain
Neutral	Kenya Uruguay	Bolivia India		Ghana	Morocco
Counteractive	Turkey	Israel	Philippines		

Figure 1 – Dynamic Evolution of Structural Change (CH) for Poland (Reformer)¹

Poland: ILO 1- Digit Data



Poland: UNIDO 3- Digit Data



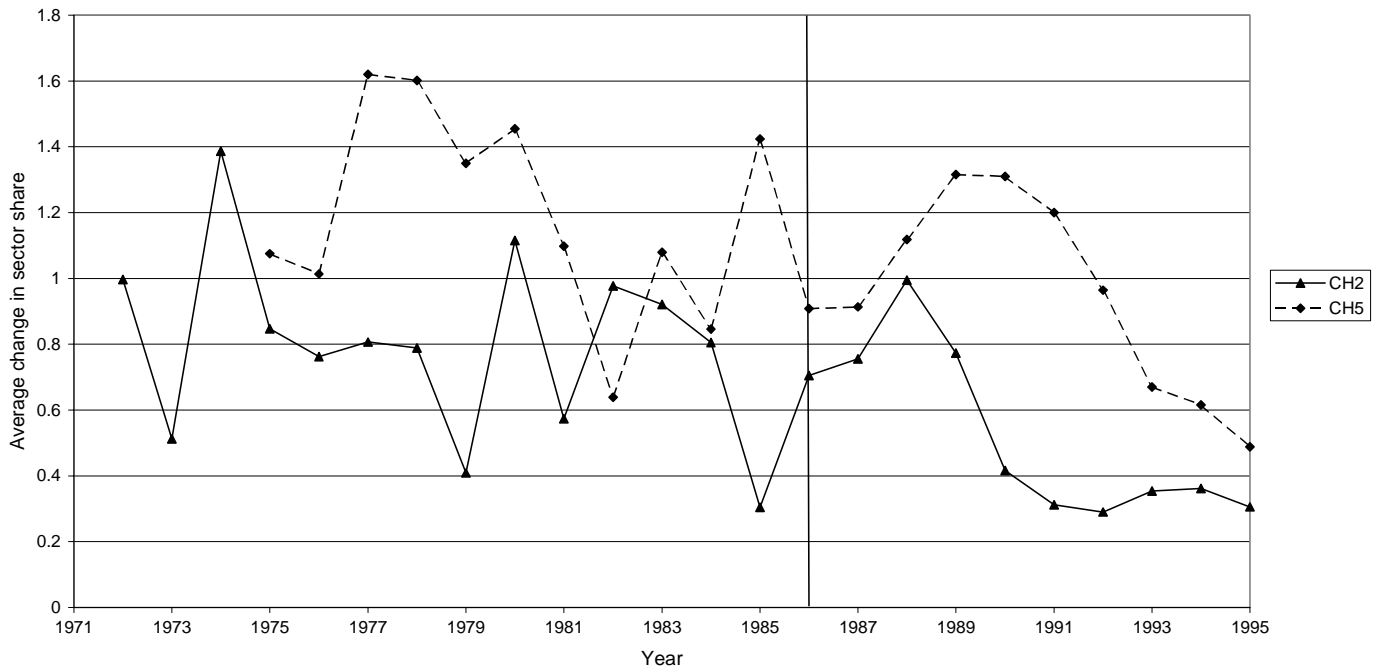
¹ Vertical lines indicate liberalization year

Figure 2 – Dynamic Evolution of Structural Change (CH) for Bolivia (Neutral)

Bolivia: ILO 1- Digit Data

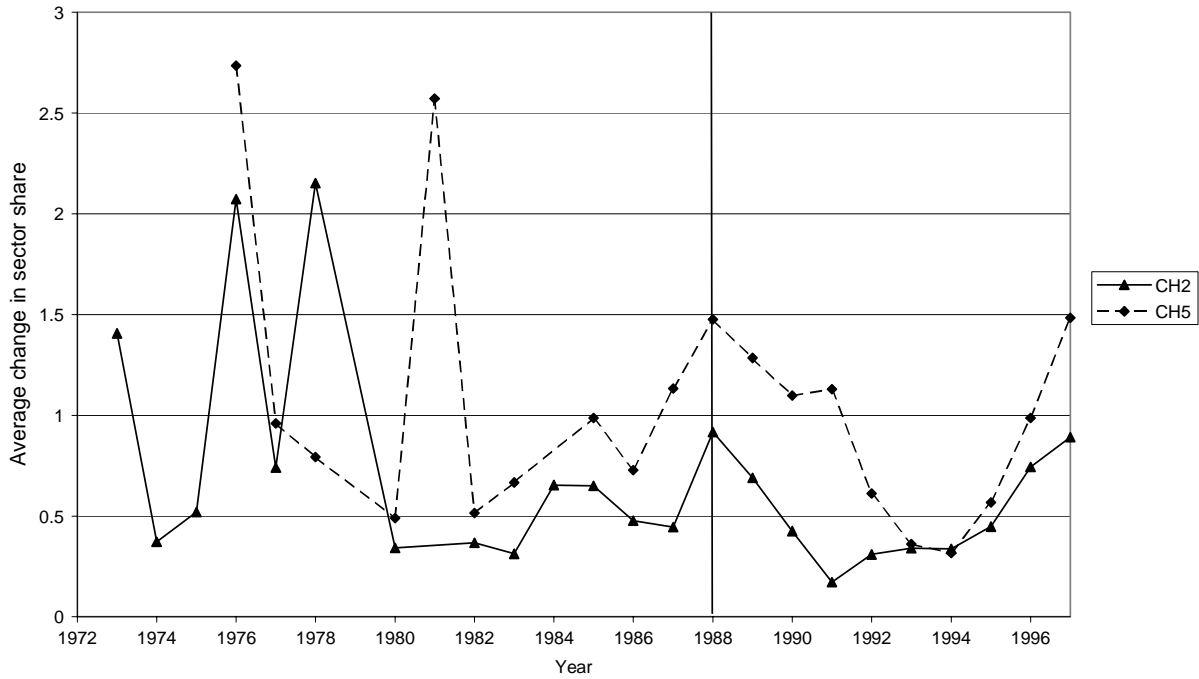


Bolivia: UNIDO 3- Digit Data



**Figure 3 – Dynamic Evolution of Structural Change (CH) for the Philippines
(counteractive)**

Philippines: ILO 1- Digit Data



Philippines: UNIDO 3- Digit Data

