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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.

Keywords: Trade liberalization; Reform; Sectoral labor reallocation

JEL classification: F16; O24; J00

1. Introduction

To what extent is trade liberalization followed by intersectoral labor shifts? Interest groups, both for and against 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-

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induced structural change. Movements of labor and capital across sectors, however, are precisely what allow countries to reap the benefits of trade openness in classical trade models.

In these models, gains from trade are obtained by moving resources toward sectors in which a country has a comparative advantage. Such a 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 observable sectoral change. New trade theory also carries predictions about the effect of liberalization on sectoral structure. In models with increasing returns to scale, for instance, trade liberalization leads to the agglomeration of production in certain geographic locations, which can translate into observable intersectoral shifts at the country level[1]. In models where trade policy openness facilitates technological transmissions, labor reallocation will occur after a reduction in trading frictions if technological transmission affects sectors differently.

Other models point either to uniform effects of trade liberalization across sectors or to unobservable movements of labor. Many new trade models focus on intraindustry trade patterns, and as a result may not deliver clear predictions on labor shifts across observable sectoral categories. To the extent that intraindustry trade is prevalent, some forms of specialization might not be noticeable at available levels of data disaggregation. Specialization does occur yet may be unobservable in models with differentiated products and increasing returns to scale where increased trade induces shifts of resources within broadly defined sectors of activity[2].

Another class of theoretical models suggests that the effects of liberalization need not involve any labor movements. 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. The effects of economic integration result purely from the increased scale of economic activity. They 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. In 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.

Similarly, models based on procompetitive effects of trade policy carry the implication that the effects of trade need not involve specialization. Markusen (1981); Wacziarg (1997) show 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 increasing returns to scale models alluded to above may predict no sectoral labor shifts if technological transmissions

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

[2] A classic reference in this literature is Dixit and Norman (1980), p. 281–295.

affect productivity in all existing sectors identically. An important aspect of these theories is that gains from trade are possible in the absence of intersectoral factor movements[3].

Despite these 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. Our 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 at various levels of disaggregation and focusing mostly on developing or transition 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.

We attempt to measure shifts across closely related industries by using sectoral data at the highest available level of disaggregation for the countries in our sample (4-digit level of disaggregation within manufacturing). Internationally comparable plant level data would allow us to better capture intraindustry and interfirm specialization effects, but are not available for our sample. Levinsohn's (1999) case study of Chile uses 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 intrasectoral, or interfirm, consequences of trade liberalization, and should be viewed as a complement to them. We focus on a different set of predictions of existing trade theories, namely those relating to intersectoral movements.

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. There is no evidence of trade-induced structural change at the 4-digit level of disaggregation for manufacturing sectors. Overall, our results suggest trade liberalization has far smaller effects on intersectoral labor shifts than is often presumed.

This paper is organized as follows: Section 2 surveys past findings on the effects of trade liberalization on sectoral structure. Section 3 discusses our empirical strategy and data. Section 4 presents our empirical results. Section 5 considers robustness issues and extensions of our estimation framework. Finally, Section 6 concludes.

[3] In the working paper version of this article (Wacziarg and Wallack, 2001) we discussed the conceptual background underlying our empirical approach in more detail. We presented a simple model of trade-induced sectoral labor reallocation based on a Ricardian model with a continuum of goods. We showed that in such a model an exogenous reduction in trading frictions, for example induced by trade liberalization, will lead the country to abandon the production of a subset of varieties, and reallocate the corresponding labor to the varieties remaining in production. We extended this framework in several directions to illustrate more formally the points discussed above.

2. 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); 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 France, using firm-level job creation and destruction data.

Case studies of developing countries in Roberts and Tybout (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 it is consistent with the idea that sectoral structure displays a high degree of inertia with respect to trade reform[4].

Papageorgiou et al.'s (1991) 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 1965–1973 liberalization, except in the textile sector, where employment remained constant under import competition. An intersectoral correlation analysis of employment after the 1978–1979 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 1960–1965 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

[4] Tybout (1996) finds that more plants were exiting manufacturing than were entering in Chile during 1979–1982, 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 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.'s (1996) 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.

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[5]. 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 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.

3. Measurement and specification

This section describes the data and econometric methodology used in this paper. Our empirical approach draws on the measurement framework of the job reallocation literature, which we apply to sector level data[6].

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

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 levels of disaggregation, and a careful examination of apparent abnormalities in

[5] One 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.

[6] See Davis et al. (1996) for such methods applied to plant-level rather than sector-level data.

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[7].

To reflect the conceptual issues discussed in Section 2, we constructed several dependent variables for our empirical analysis. Our first set of dependent variables, labeled *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 et al. (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.

3.1.1. 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 2 years (CH2) and differences over 5 years (CH5)[8]:

$$CH_{st}(\tau) = |S_s^t - S_s^{t-\tau}| \tag{1}$$

where $\tau = 2, 5$. 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 (potentially resulting from population growth and uneven entry into the labor force). The other measures we use attempt to separate these sources of changes in CH.

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[7] We identified and examined 31 year to year changes of more than 100% in a sector's level of employment for the 1628 observations used in the ILO regressions. Twelve of these were in sectors with less than 10% of that country's average sectoral employment, where large changes may not be implausible. Hundred and fifteen of 238 large yearly changes in sector size in the UNIDO 3-digit data (which contain 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 6338 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.

[8] We 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.

3.1.2. 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 employment in sector s at time t by E_s^t,

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

The changes are computed over $\tau = 2$ and 5 years. In the numerator of Eq. (2), 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 gives the number of compensated changes in the structure of employment, or employment changes resulting from pure shifts of jobs across sectors[9]. It is interesting to note that SH_t will be zero whenever all sectors experience employment changes in the same direction.

3.1.3. 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[10]. This last set of measures can be computed as:

$$EM_t(\tau) = \frac{\sum_{s=1}^{S} E_s^t - \sum_{s=1}^{S} E_s^{t-\tau}}{\frac{1}{2}\sum_{s=1}^{S}(E_s^{t-\tau} + E_s^t)} \tag{3}$$

Table 1 presents summary statistics for the CH, SH and EM measures for our various data sets. To give a sense of the magnitude of the changes we are measuring, the average 2-year change in a sector's share of employment (CH2) is 0.74 percentage points for the ILO data. Summary statistics for SH2 suggest that over any given 2 year period, 2.63% of workers have shifted from one sector to another. Finally, the average growth in employment (EM2) over a 2 year period is 4.59%.

[9] We 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.

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

Table 1
Summary statistics and conditional means for measures of sectoral change

Variable	Number of observation	Mean	Standard deviation (S.D.)	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
ILO									
CH2	1373	0.745	0.996	0.760	0.672	0.787	0.646	0.812	0.666
CH5	1166	1.346	1.592	1.420	1.051	1.487	1.083	1.535	1.186
SH2	173	2.631	3.105	2.701	2.271	2.873	2.035	2.980	2.196
SH5	147	4.453	5.040	4.954	2.416	5.498	2.425	5.480	3.520
EM2	173	4.593	8.124	4.816	3.437	4.558	4.679	4.572	4.618
EM5	147	10.050	14.800	10.231	9.310	9.891	10.356	9.678	10.387
UNIDO 3-digit									
CH2	11,944	0.373	0.706	0.369	0.406	0.367	0.398	0.376	0.368
CH5	9500	0.630	1.098	0.620	0.695	0.606	0.709	0.608	0.682
SH2	453	3.695	4.478	3.641	4.071	3.629	3.934	3.851	3.344
SH5	362	4.765	5.372	4.643	5.512	4.447	5.786	4.550	5.259
EM2	453	3.661	12.830	3.873	2.191	4.574	0.353	4.670	1.383
EM5	362	8.736	23.270	9.245	5.636	10.437	3.277	11.062	3.409
UNIDO 4-digit									
CH2	5412	0.180	0.354	0.177	0.185	0.184	0.169	0.175	0.186
CH5	3046	0.265	0.455	0.261	0.268	0.264	0.267	0.261	0.268
SH2	78	6.251	3.823	6.760	4.422	6.795	5.468	6.649	5.809
SH5	44	8.542	5.303	8.764	8.113	8.193	8.832	8.193	8.832
EM2	78	2.681	10.935	1.228	7.896	3.110	2.065	3.092	2.227
EM5	44	8.815	13.730	6.127	14.012	9.270	8.435	9.270	8.435

3.2. 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 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 in a year following a de jure liberalization[11]. In our sample of 25 liberalization episodes, the gap between the de jure date and the de facto date exceeded

[11] The first 5% increase after the de jure liberalization pins down the de facto liberalization date, even though the increase may occur for more than 1 year following reform. For example, a country with a 20% trade to GDP ratio qualifies as having a de facto liberalization the first year t when its trade to GDP ratio rises to 21%, and t is a year that follows a de jure liberalization. This method is a modified version of that 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 Bank (1999).

2 years in only two cases (Hungary and Morocco), and the two dates coincided in 14 cases[12].

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[13]. 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. 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[14].

To address potential criticism that we relied too much on the SW dates, we checked each date of liberalization by examining relevant country case studies. With the exception of Spain and Trinidad and Tobago, we were able to corroborate all of the choices made by SW[15]. The sources and results of this systematic search are reported in more detail in a data appendix available separately, containing brief summaries of trade liberalizations in our sample and providing detailed background to support our use of the SW de jure liberalization years[16]. Table 2 reports our dates of de jure and de facto liberalization, along with the countries covered by each sectoral database. According to Sachs and Warner (1995), several countries underwent multiple liberal-

[12] In only one case (Honduras in 1991) was a country removed from our sample because its de jure reform was not followed by a de facto liberalization. There were isolated instances of de facto liberalization not preceded by de jure reforms, especially in the early 1970s when world trade was growing fast. As opposed to the sustained increase following de jure dates, however, these "tacit" or "silent" liberalizations were not characterized by sustained increases over several years.

[13] These results are available upon request.

[14] This criticism may be less serious when it comes to the dates of liberalization published by SW, 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 our data appendix. See also Sachs and Warner (1995), page 24, footnote 44 for an explanation of how they determined their dates of liberalization.

[15] The choice of Spain's liberalization date is explained in our data appendix. Dropping Spain from our sample did not modify our results. These results are available upon request.

[16] A companion paper containing the data appendix, along with all the results described in this paper as "available upon request" can be downloaded from: http://www.stanford.edu/ ~ wacziarg/papersum.html.

Table 2
Trade liberalization years

Code	Country	Sachs and Warner[a]	Percentage change in de jure LIB year	De facto year	Percentage change in de facto LIB year	Available ILO	Available UNIDO3	Available UNIDO4
5	Argentina	1976	7.13	1976	7.13		X	
20	Bolivia	1985	−2.91	1986	40.32	X	X	
22	Brazil	1991	6.77	1991	6.77	X		
34	Chile	1976	10.97	1976	10.97		X	
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		
63	Ghana	1985	6.66	1985	6.66		X	X
70	Guatemala	1988	0.70	1989	5.16		X	X
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
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 and Tobago	1994	29.04	1994	29.04	X		
171	Turkey	1989	3.05	1990	7.84	X	X	X
178	Uruguay	1990	6.76	1990	6.76	X	X	X

See the data appendix at http://www.stanford.edu/~wacziarg/papersum.html for further details on the dates of liberalization.

[a] Most recent liberalization when multiple attempts are reported.

izations during the years in our sample[17]. Our primary results are based on the most recent liberalization, as previous liberalizations tended to be shallower or quickly reversed[18].

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 2 years, zero otherwise. LIB5 does the same for the year of liberalization and 5 subsequent years, while LIB indicates the year of liberalization and all subsequent

[17] Multiple 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. Our data appendix 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.

[18] In 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.

years without a reversal as of 1995. We selected countries that had at least 3 years of sectoral employment data before and after their liberalization date. The sample of countries varied across data sets: 13 countries are included in the ILO dataset, 20 in the 3-digit UNIDO dataset and eight in the 4-digit level UNIDO dataset.

3.3. 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 binary liberalization measures by running fixed-effects regressions 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 country × sector dummy variables for structural change (CH) regressions[19]. Our estimated equations are as follows:

$$CH_{ist}(\tau) = \alpha_1 + \beta_1 \mathrm{LIB}_{it}(\tau') + v_{is} + \varepsilon_{ist} \tag{4}$$

$$SH_{it}(\tau) = \alpha_2 + \beta_2 \mathrm{LIB}_{it}(\tau') + \mu_i + \xi_{it} \tag{5}$$

$$EM_{it}(\tau) = \alpha_3 + \beta_3 \mathrm{LIB}_{it}(\tau') + \gamma_i + \zeta_{it} \tag{6}$$

In Eqs. (4)–(6), $CH(\tau)$ refers to our measure of structural change, $SH(\tau)$ to excess job reallocation, $EM(\tau)$ to the growth in aggregate employment (where $\tau = 1, 2, 5$ refers to the interval of time over which changes are computed), as defined in Section 3.1. LIB(τ') refers to our three measures of liberalization, defined in Section 3.2, where τ' indicates whether we are looking a 2, 5 years or all available years of data following the year of liberalization.

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 liberalized versus the non-liberalized regimes[20]. The ratio of each coefficient to the non-liberalization mean value of the corresponding dependent variable (in percentage terms) provides a measure of the economic significance of the effects of liberalization and facilitates comparisons across samples.

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 CH variable is characterized by one observation per country-sector-year, so that in any

[19] For 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.

[20] The 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, sector-countries) to account for time invariant country- and country × sector specific characteristics. Single-country regressions for the country-year-sector CH variables are presented and discussed in Section 5.

given country-year every sector is associated with the same liberalization status. Clustering at the country-year level allows us to correct standard errors in a way that acknowledges that observations may not be independent across sectors within a country-year. Clustering is not an issue for the SH and EM variables, which by construction only involve the country and year dimensions.

4. 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 sub-sectors 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 excess job reallocation.

4.1. 1-Digit, economy-wide results (ILO)

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 1 presents conditional means of our various outcome measures, conditioning on our liberalization indicators. For example, in a 2-year period with a liberalization in the past 5 years, a typical sector will experience a 0.65 percentage point absolute value change in its share of total employment. The average change in a 2-year period without a prior liberalization is 0.79 percentage points. Hence, the average 2-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 3. 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 2 years decreases the 5-year change in sector shares by 0.31 percentage points on average. Table 4 provides a notion of the economic significance of such an effect: a liberalization in the past 2 years (LIB2) brings about a 22.04% fall in structural change (as measured by the average 5-year change in sectoral employment shares). Similarly, 5-year sector changes after a liberalization (LIB) tend to be 12.18% lower than the average non-liberalization sector share change. Our results do not change when we control for country-specific effects only[21].

[21] These results are available upon request.

Table 3
Fixed-effects regressions of sectoral change on liberalization status

	CH2	CH5	SH2	SH5	EM2	EM5
ILO						
LIB2	−0.075	−0.313**	−0.255	−2.258**	−2.189	−3.031
	(0.075)	(0.108)	(0.587)	(0.814)	(1.385)	(2.082)
Adj. R^2	0.278	0.396	0.182	0.411	0.335	0.553
LIB5	−0.089	−0.245**	−0.404	−2.228**	−1.270	−3.440*
	(0.065)	(0.093)	(0.483)	(0.695)	(1.145)	(1.785)
Adj. R^2	0.279	0.394	0.184	0.422	0.330	0.558
LIB	−0.089	−0.187	−0.623	−2.001**	−0.154	−0.292
	(0.070)	(0.106)	(0.453)	(0.698)	(1.081)	(1.804)
Adj. R^2	0.279	0.392	0.190	0.414	0.325	0.546
Number of observations	1373	1166	173	147	173	147
Number of countries	10	10	10	10	10	10
UNIDO 3-digit						
LIB2	0.033	0.065	0.519	0.849	−1.691	−4.074
	(0.031)	(0.040)	(0.546)	(0.663)	(1.745)	(3.059)
Adj. R^2	0.351	0.459	0.271	0.336	0.093	0.247
LIB5	0.038	0.097**	0.759*	1.819**	−4.020**	−7.564**
	(0.024)	(0.032)	(0.445)	(0.540)	(1.414)	(2.498)
Adj. R^2	0.351	0.460	0.274	0.354	0.107	0.263
LIB	−0.002	0.057*	−0.095	0.961*	−3.281**	−8.503**
	(0.024)	(0.032)	(0.430)	(0.536)	(1.366)	(2.439)
Adj. R^2	0.350	0.459	0.269	0.339	0.103	0.269
Number of observations	11,944	9500	453	362	453	362
Number of countries	19	18	19	18	19	18
UNIDO 4-digit						
LIB2	−0.006	−0.008	−2.467**	−0.948	6.633**	8.010*
	(0.016)	(0.016)	(1.000)	(1.641)	(2.933)	(4.319)
Adj. R^2	0.409	0.696	0.121	0.063	0.076	0.031
LIB5	0.012	0.011	−1.427*	0.286	−0.871	−0.643
	(0.016)	(0.016)	(0.862)	(1.628)	(2.557)	(4.456)
Adj. R^2	0.409	0.696	0.080	0.055	0.010	−0.056
LIB	−0.006	0.011	−1.381	0.286	−1.494	−0.643
	(0.014)	(0.016)	(0.879)	(1.628)	(2.599)	(4.456)
Adj. R^2	0.409	0.696	0.077	0.055	0.013	−0.056
Number of observations	5412	3046	78	44	78	44
Number of countries	8	5	8	5	8	5

Standard errors in parentheses, clustered at the country-year level for CH regressions and robust to heteroskedasticity (Huber–White corrected). 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

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.

Turning to our measure of excess job reallocation (SH), our results suggest that it also decreases in a statistically significant way after liberalization at the 1-digit level. 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 (Table 3). The magnitudes are quite large: a liberalization in the past 2 years decreases SH5 by about 2.26 percentage points, or 41.07% of the average excess job reallocation for the years that have not had such a recent liberalization.

Finally, the overall growth of employment (EM) 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 3). Hence, we found no compelling evidence that trade liberalization affects aggregate employment growth.

4.2. 3-Digit manufacturing results (UNIDO)

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 Table 1 and the regression results presented in Table 3 suggest that the effect of liberalization on changes in sector shares in manufacturing employment (CH) is positive[22]. 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 2 years, for example, increases 5-year sector change by 0.07 percentage points, or 10.48% of the average change in sector shares in years without a recent liberalization.

Turning to our other measures of labor market responses at the three digit level, 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 to define the liberalized regime.

The effect of liberalization on employment growth is always negative. A liberalization in the past 5 years, for example, reduces the 2-year growth of manufacturing employment (EM2) by 4.02 percentage points. For comparison purposes, the average growth of manufacturing employment in non-liberalization regimes is 4.57%. 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).

4.3. 4-Digit manufacturing results (UNIDO)

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 5 years increases the 2-year absolute value change in employment shares by an average of 0.01 percentage points. This represents a 6.52% increase in the extent of structural change relative to non-liberalization years, and it is statistically insignificant. This compares to a 10.28% 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.

[22] With 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.

5. Robustness and extensions

This section presents several robustness checks and extensions of our basic findings. Among them, we examine the role of expectations, the possible endogeneity of liberalization, the role of barriers to labor mobility and possible counteractive policies as potential explanations for our findings.

5.1. 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). If their effects could start to be felt before they occur, our slope estimates would be biased 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 three 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 SW) 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. The expectations problem should be particularly acute in cases where the de facto liberalization date occurs several years after the initiation of trade reforms (Table 2). There were also cases where the SW date occurs several years after the trade reforms have been announced and started. 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[23]. In most cases, this is the SW 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 our data appendix.

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.

[23] These results are available upon request.

5.2. Endogeneity of liberalization

In the previous subsection, we pointed out that most big trade liberalizations are triggered by a political or an economic crisis. This has the potential to introduce a downward bias in the coefficient on the liberalization dummy for our measures of sectoral change and excess labor reallocation, and a possible upward bias in the case of aggregate employment growth[24]. A simple story that could account for our results is that if the experience of each country is characterized by a sharp drop in employment immediately preceding trade liberalization, and if this drop does not affect all sectors equally, our CH measure will tend to be higher before liberalization than after it, and our liberalization indicator will capture the end of a crisis rather than liberalization per se. This could explain why, when using the ILO data, many of our regression results suggest that labor reallocation is lower after liberalization than before[25].

To address this point, we carried out a very simple test: we excluded from our dataset the year of liberalization, and the 2 years that precede it. For the countries in the sample, this basically eliminates the pre-liberalization turmoil years. We reran our regressions on this modified sample[26]. These results show that our results change very little when the years preceding the year of liberalization are excluded from the sample. Comparing the corresponding parameter estimates with the baseline results of Table 3 shows that the signs, degree of statistical significance and orders of magnitude of the estimated coefficients for all specifications using the ILO and UNIDO 3-digit data are unchanged[27]. Hence, the possibility of pre-liberalization economic and political turmoil does not seem to greatly affect our results.

5.3. 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 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.

[24] We are grateful to an anonymous referee for pointing out this possibility.

[25] However, the effect of liberalization on the growth of aggregate employment (EM) tends to be negative in most specifications of Table 3, with the exception of one specification when using the UNIDO 4-digit data. This does not square very well with the idea that the liberalization dummy is simply a proxy for the end of an economic or political crisis.

[26] These results are available upon request.

[27] The results are slightly more unstable when using the UNIDO 4-digit data and the LIB5 and LIB measures of liberalization. However, this data covers a relatively small number of countries, with a correspondingly smaller number of datapoints. Therefore, it is not surprising that the results would be somewhat more unstable when further reducing sample size.

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 8 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. The results, available upon request, displayed 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[28]. Thus, the results described in the previous section do not seem attributable to a systematic dynamic pattern of structural change following liberalization[29].

5.4. Persistence of the labor response

Our baseline results implicitly assume that the disturbance term on the right hand-side of our regressions is uncorrelated through time. Clustering at the country-year level allows for a possible correlation of the residuals across sectors within a country-year but does not address possible persistence in the labor adjustment process. Usually, in a panel regression this is not a big issue, since the country-specific effect can in principle soak up a lot of persistence in unobservables. Here, this assumption might be wrong, as most models of the labor adjustment process imply persistence in response to shocks or policy changes[30].

One option to address the problem of persistence in the adjustment process would be to cluster standard errors at the country-sector level, therefore not requiring residuals within a country-sector observation to be independent across years. This would give us the correct fixed-effects standard errors in the presence of autocorrelation, at the cost of foregoing the standard clustering of standard errors at the country-year level. When we did this, our results were largely unchanged compared to clustering only at the country-year level, as is done in our baseline regressions. Specifically, the corrected standard errors tended to be slightly larger, but the pattern of coefficients that remain statistically significant at the 10% and 5% level was generally unchanged, except for the UNIDO 4-digit data, where few coefficients were significant to start with[31].

Our second approach tackles the problem of autocorrelation more directly by seeking a more efficient estimator. We examined the robustness of our baseline results by estimating the same equations using a fixed-effects feasible GLS estimator, assuming an AR(1) process for the error term. Doing this has the potential to improve the efficiency of our estimates, rather than simply correcting for possibly wrong standard errors. To our

[28] These graphs are available upon request.

[29] This 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 occurs in the post-liberalization period. Individual country estimates presented below address this issue.

[30] We are grateful to an anonymous referee for pointing this out.

[31] As a result, we opted to retain standard errors clustered at the country-year level as our benchmark estimates. Results with Huber–White robust standard errors clustered at the country-sector level are available upon request.

knowledge, the econometric theory for a more general form of autocorrelation in the fixed-effects model has not been developed[32]. Another limitation of this approach is that we cannot allow for clustering across country-years (again, we are not aware of an existing estimator that both allows for clustered standard errors and improves upon fixed-effects standard errors in the presence of autocorrelation). Finally, this estimator forces us to lose the first year of data. Despite these drawbacks, the basic message of our baseline results is preserved in the fixed-effects/GLS results[33]. In particular, the effect of liberalization on structural change (CH) remains negative for the ILO data, although the estimates are smaller in magnitude and fewer are significant statistically. For the UNIDO 3-digit data, the estimates are of the same sign and similar magnitude and their degree of statistical significance is improved when allowing for AR(1) residuals.

To summarize, when allowing for autocorrelation explicitly, the finding of a small positive effect of liberalization on structural change is slightly reinforced in the UNIDO 3-digit dataset, while the finding of a negative effect is slightly weakened in the ILO dataset.

5.5. Barriers to 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. Albuquerque 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[34].

To test the hypothesis that barriers to factor mobility, such as restrictions on hiring and firing workers, may impede post-liberalization labor reallocation 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[35]. We split our sample into two subsamples according to whether a country's level of job security is

[32] The econometric theory for fixed-effects/feasible generalized least squares in the presence of an AR(1) process is due to Bhargava et al. (1982). Baltagi and Wu (1999) present an extension to unequally spaced panels.

[33] These results are available upon request.

[34] The 1997 Economic and Social Progress Report of the IADB (IADB, 1997) notes 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.

[35] This 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 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.

above or below the sample mean and ran our regressions on the two groups separately. Results are presented in Table 5.

At the economy-wide level (ILO), 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 suggest 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. These UNIDO results suggest that there is less sectoral change after liberalization in countries where it is more costly to fire workers, although this effect is not statistically significant.

To summarize, the ILO and UNIDO results seem to point in opposite directions, and the low level of statistical significance of the UNIDO results makes it difficult to draw any inference. We can only conclude that these results show how little we can learn for sure from the available sectoral data on whether labor market rigidities limit trade-induced labor reallocation.

Table 5
Fixed-effect regressions, sample split by job security

	CH2	CH5	CH2	CH5
ILO 1-digit: low job security			*ILO 1-digit: high job security*	
LIB2	− 0.308**	− 0.687**	0.144	− 0.068
	(0.136)	(0.228)	(0.133)	(0.142)
Adj. R^2	0.223	0.351	0.346	0.308
LIB5	− 0.036	− 0.422*	− 0.028	− 0.122
	(0.169)	(0.249)	(0.099)	(0.108)
Adj. R^2	0.212	0.337	0.340	0.311
LIB	− 0.007	− 0.146	0.020	0.032
	(0.149)	(0.267)	(0.093)	(0.123)
Adj. R^2	0.212	0.328	0.340	0.308
Number of observations	406	364	366	276
Number of countries	4	4	3	3
UNIDO 3-digit: low job security			*UNIDO 3-digit: high job security*	
LIB2	0.035	− 0.033	− 0.005	− 0.016
	(0.047)	(0.043)	(0.037)	(0.061)
Adj. R^2	0.317	0.441	0.273	0.427
LIB5	0.056*	0.074	− 0.050	− 0.027
	(0.033)	(0.049)	(0.032)	(0.046)
Adj. R^2	0.319	0.442	0.274	0.427
LIB	0.030	0.014	− 0.101**	− 0.102**
	(0.031)	(0.045)	(0.036)	(0.047)
Adj. R^2	0.317	0.441	0.277	0.429
Number of observations	3248	2915	3878	3251
Number of countries	4	4	6	5

Standard errors in parentheses, clustered at the country-year level and robust to heteroskedasticity (Huber–White corrected). Regressions include country-sector effects (not reported).

* Statistically significant at the 10% level.

** Statistically significant at the 5% level.

5.6. 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.

It is possible 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 that 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. Since countries underwent a wide array of different policy changes in conjunction with trade reform, it would be difficult to categorize them using formal quantitative thresholds as in Sachs and Warner (1995). Instead, we carried out a thorough review of the case study literature in order to classify countries into three broad categories: reformer, neutral, and counteractive. Hence, our grouping is largely qualitative in nature, and reflects analysts' perceptions of the broad context of reform in each country in our sample. Our data appendix, available as a companion paper, provides details of each country's domestic reforms that accompanied trade liberalization.

A country was classified as a *reformer* if it was accompanied by a broad program of domestic market-oriented reforms. In Colombia, for example, 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 in 1991. Price controls were lifted, a financial sector reform was 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. A country was categorized as *neutral* if trade reform was enacted in isolation. For example, in El Salvador, 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. Finally, a country was classified as *counteractive* if trade reforms were actively counteracted by a program of domestic

[36] This 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.

interventionist policies. In the Philippines, for example, the share of government activity in the economy, measured by the importance of state-owned enterprises in gross domestic investment and employment, rose as trade reforms were implemented.

According to our classification, 14 out of our 25 episodes of trade liberalizations were in reformer countries, where they 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. Turkey, the Philippines, and Israel were *counteractive* countries where the government attempted to shield domestic industries and offset the potential effects of trade liberalization on the labor market. Fixed-effects regressions were 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 6 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

Table 6
Fixed-effects regressions of changes in sectoral structure on liberalization regime—sample split by reform status

	CH2	CH5	CH2	CH5
ILO: non-reformer			*ILO: reformer*	
LIB2	− 0.085	− 0.269	− 0.066	− 0.343**
	(0.116)	(0.222)	(0.096)	(0.095)
Adj. R^2	0.231	0.350	0.342	0.462
LIB5	− 0.187**	− 0.301**	− 0.003	− 0.202*
	(0.088)	(0.164)	(0.094)	(0.106)
Adj. R^2	0.236	0.352	0.341	0.456
LIB	− 0.191**	− 0.355**	− 0.001	− 0.032
	(0.089)	(0.159)	(0.105)	(0.139)
Adj. R^2	0.237	0.356	0.341	0.451
Number of observations	674	538	699	628
Number of countries	5	4	5	6
UNIDO 3-digit: non-reformer			*UNIDO 3-digit: reformer*	
LIB2	− 0.031	− 0.126**	0.066	0.177**
	(0.038)	(0.036)	(0.046)	(0.061)
Adj. R^2	0.323	0.405	0.386	0.487
LIB5	− 0.039	− 0.088**	0.044	0.180**
	(0.035)	(0.037)	(0.031)	(0.041)
Adj. R^2	0.323	0.405	0.386	0.489
LIB	− 0.097**	− 0.142**	0.022	0.120**
	(0.037)	(0.038)	(0.028)	(0.037)
Adj. R^2	0.326	0.407	0.386	0.487
Number of observations	5922	5251	7940	6918
Number of countries	8	8	11	10

Standard errors in parentheses, clustered at the country-year level and robust to heteroskedasticity (Huber–White corrected). Regressions include country-sector effects (not reported).

* Statistically significant at the 10% level.

** Statistically significant at the 5% level.

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.

5.7. 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 which might provide clues as to the causes of the findings in Section 4, we ran regressions of changes in sector share on liberalization dummies for individual countries. Since each year of data has 9 or 28 or 81

Table 7

	Negative effect	Negative insignificant	Zero effect	Positive insignificant	Positive effect
ILO					
Reformer	Spain	Brazil, Colombia	Costa Rica		Paraguay, Poland
Neutral	Trinidad and Tobago	Uruguay	El Salvador	Bolivia	
Counteractive	Philippines, Turkey			Israel	
UNIDO 3-digit					
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		
UNIDO 4-digit					
Reformer	Guatemala				Colombia, Ecuador
Neutral	Kenya	Uruguay		Ghana	
Counteractive			Turkey	Philippines	

Single country results for structural change (CH).

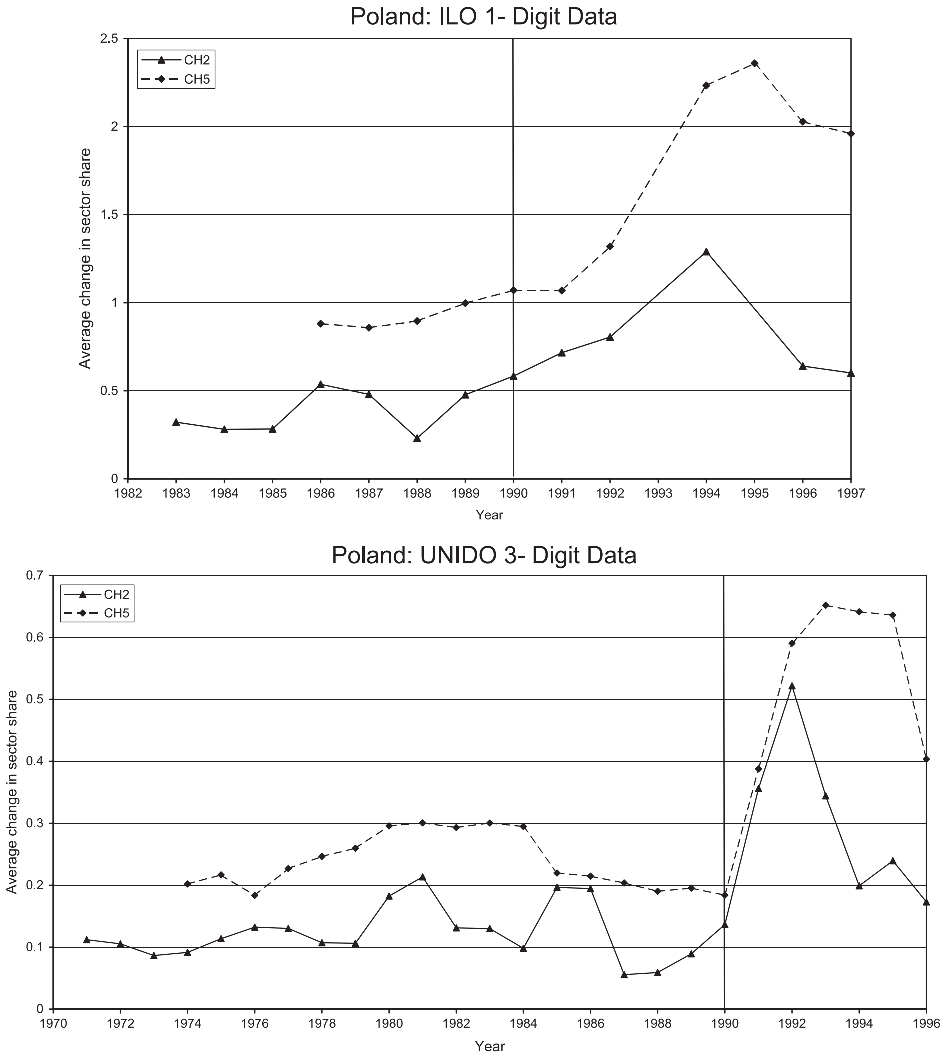

Fig. 1. Dynamic evolution of structural change (CH) for Poland (Reformer)[39].

sectors (for each level of disaggregation, respectively), this gives us enough data points for the regressions using CH1, CH2 and CH5 as dependent variables[37].

Table 7 displays a summary of the results for the ILO and UNIDO data sets[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

[37] Standard errors were clustered at the year level.

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

[39] Vertical lines indicate liberalization year.

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 liberaliza-

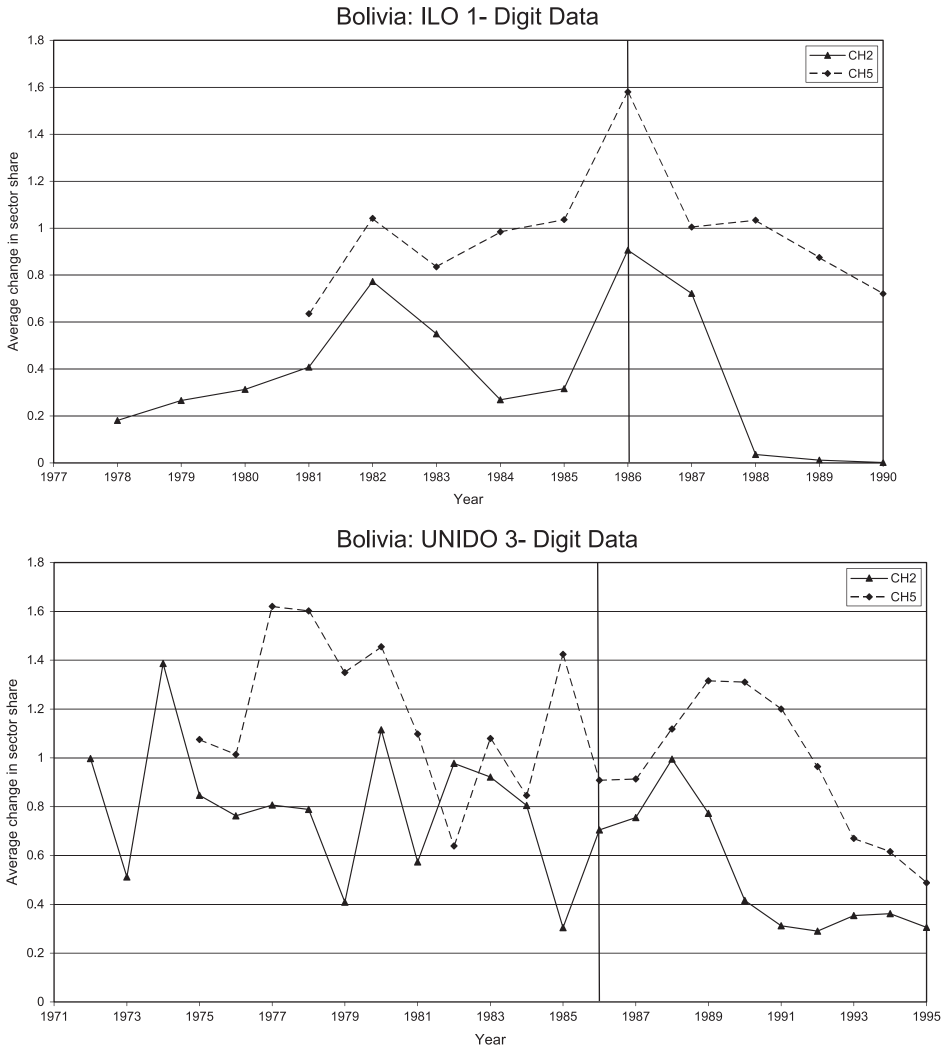

Fig. 2. Dynamic evolution of structural change (CH) for Bolivia (Neutral).

tion (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 7 focus on whether the mean changes before and after liberalization were different. They do not shed any light on the dynamics of the

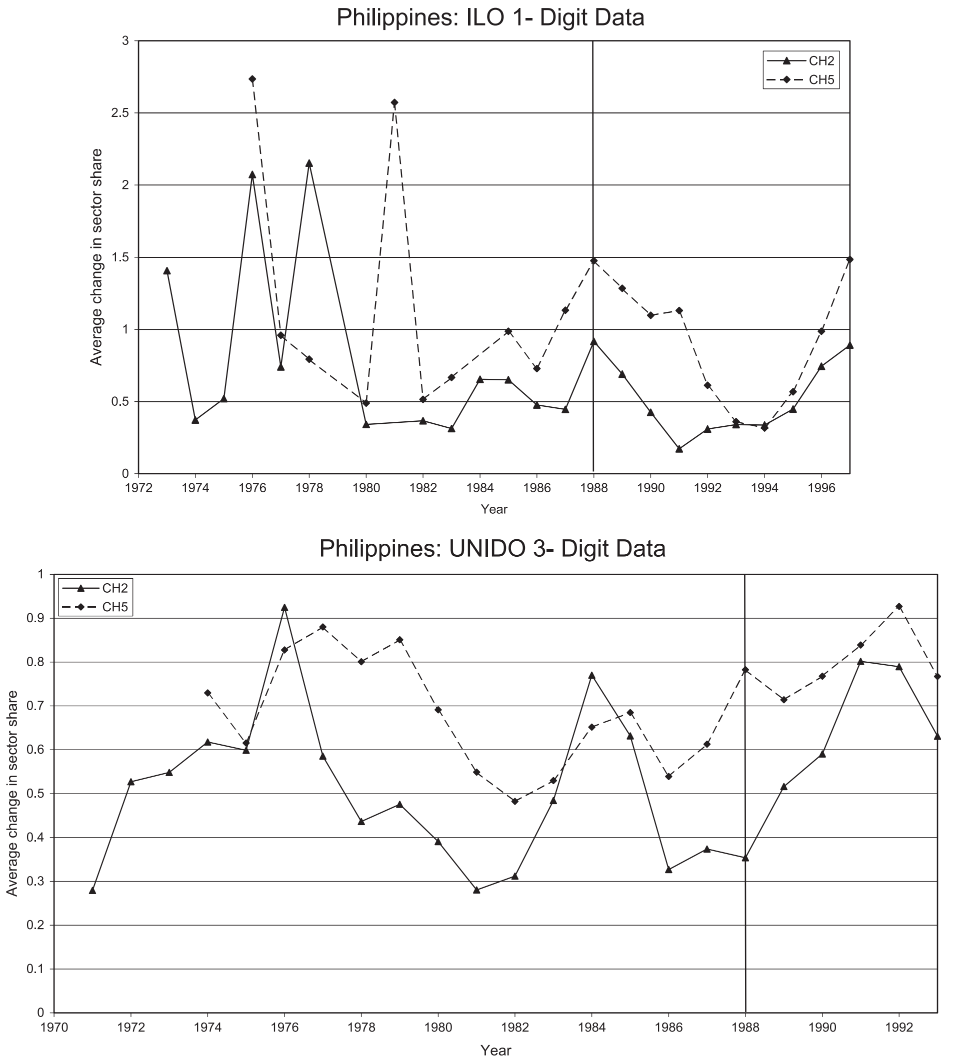

Fig. 3. Dynamic evolution of structural change (CH) for the Philippines (counteractive).

indicators of labor movement before and after liberalization, such as the timing of the changes in sector shares. Figs. 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 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 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 pooled results.

6. 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 shows that the presumption in favor of labor reallocation as a consequence of trade liberalization is, empirically, an unproven hypothesis. The data appear to lean more toward a zero or negative effect than a positive one. Liberalization episodes are followed, rather unexpectedly, by a reduction in the extent of intersectoral labor shifts at the economy-wide 1-digit level of disaggregation. Liberalization has 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. On the other hand, the extent of post-reform labor shifts appears to be only weakly related to the degree of labor market flexibility.

In summary, claims that trade liberalization generally leads to the absolute decline 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 subcategories 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 intrasectoral effects, such as those uncovered by some studies using firm-level data. The theoretical implication of this finding is 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[40].

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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[40] In fact, our results are consistent with findings in Bernard et al. (2003), using the US Census of Manufactures: while liberalization had a big impact on aggregate trade, the sectoral dimension was not the one at which the action was occurring.

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