LOOKING FOR LOCAL LABOR MARKET EFFECTS OF NAFTA

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Abstract—Using U.S. Census data for 1990 to 2000, we estimate effects of NAFTA on U.S. wages. We look for effects of the agreement by industry and by geography, measuring each industry's vulnerability to Mexican imports and each locality's dependence on vulnerable industries. We find evidence of both effects, dramatically lowering wage growth for blue-collar workers in the most affected industries and localities (even for service-sector workers in affected localities, whose jobs do not compete with imports). These distributional effects are much larger than aggregate welfare effects estimated by other authors.

I. Introduction

PERHAPS the most passionately debated issue in trade policy within the United States in a generation has been the implementation of the North American Free Trade Agreement (NAFTA), signed by the governments of the United States, Canada, and Mexico in 1993. Opponents believe that it has devastated some parts of the country by encouraging multinationals to shift operations to Mexico, while proponents argue that it has boosted U.S. exports and thus job growth. Despite the age of the agreement, as recently as 2008, it became the subject of intense political debate, with Democratic presidential candidates competing with each other in denunciations of the agreement in Ohio, a state in which many voters blame the agreement for local economic difficulties (Austen, 2008). Brown (2004) presents a passionate example of the liberal noneconomist's case against NAFTA, arguing that it has destroyed millions of U.S. jobs, as well as caused environmental problems.

One aspect of popular opposition to NAFTA has been the claim that it has had a disparate impact geographically, that it has impoverished particularly vulnerable towns even as others have prospered. Leonhardt (2008), for example, describes the anti-NAFTA sentiment in Youngstown, Ohio, which had suffered a long economic decline that many residents blamed partly on NAFTA. In particular, residents had recently seen the shuttering of the Youngstown Steel Door plant, which had been the leading supplier of steel doors for railway cars in North America for decades; the capital was purchased by a foreign firm and shipped to a plant in Mexico.[1]

Unfortunately, economists to date have not provided an answer to the question of whether NAFTA has indeed had the effects ascribed to it by its opponents. This paper is an attempt to do so. We ask whether we can identify subsets of U.S. workers whose incomes were seriously diminished by the agreement, and if so, whether they follow an identifiable geographic pattern.

Our approach is to do what seems like the simplest possible exercise to look for signs of the effects that NAFTA opponents claim. We try to identify local labor market effects of the tariff reductions brought about by NAFTA, using publicly available U.S. Census data from 1990 and 2000, taken from the IPUMS project at the Minnesota Population Center (www.ipums.org; see Ruggles et al., 2010). These data have enough richness to enable us to capture the features we need to capture.

Three features in particular should be highlighted. First, we need to be able to control for a worker's industry of employment in order to allow for the likelihood that workers in industries that compete with imports from Mexico will be affected differently from workers in other industries.[2] The Census data have a coarse but adequate division of workers into industries that allows us to do so.

Second, the issue that has been foremost in much of the political debate is a geographic one: the claim that workers in some vulnerable locations have been harmed relative to workers in other places. Thus, we need detailed geographic data and a measure of how vulnerable a given location is likely to be to the effects of NAFTA. The IPUMS data identify each worker as living in one of the Consistent Public-Use Microdata Areas, or *conspumas*, and this allows us to control for geography. In particular, in addition to controlling for the industry in which a worker is employed, we can control for how many of the other workers within a worker's conspuma are employed in industries that will compete directly with imports from Mexico. This will be interpreted as the local vulnerability of the labor market to the effects of NAFTA.

Finally, the agreement was framed as a gradual phase-in of tariff elimination between the three countries, starting in 1994 and continuing for ten years (with a few tariffs continuing to 15 years). The negotiated schedule of liberalization was different for each sector of the economy. As a result, for

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[1] Other examples abound. Brown (2004) argues that the agreement was a devastating blow to the towns of Nogales, Arizona, and El Paso, Texas. At the same time, the town of Laredo, Texas, enjoyed a dramatic economic boom based on traffic to and from Mexico following the agreement (Duggan, 1999). Kumar (2006) argues that the Texas economy as a whole has benefited from exports to Mexico as a result of the agreement.

[2] Note that we are not interested in imports from Canada, since tariffs between the United States and Canada had already been eliminated by the Canada–U.S. Free Trade Agreement.

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some industries, the period from 1990 to 2000 would represent the period of an announcement of tariff reductions, most of which occurred after 2000. For other industries, the same period would be one of rapid elimination of tariffs. Industries vary in both their initial tariffs and the rate of tariff drawdown, so we control for both separately.

Post-NAFTA, much work on the economic effects of the agreement has focused on trade creation and trade diversion. Romalis (2007) studies changes in trade flows following NAFTA and finds that the trade diversion effects of the agreement were substantial and swamped any benefits from trade creation, leaving a net aggregate welfare benefit for the United States of about 0. Caliendo and Parro (2015) calibrate and simulate an Eaton-Kortum type of model of North American trade to estimate the effects of NAFTA. Taking full account of enhanced trade in intermediate inputs and interindustry input-output linkages, they find very small increases in welfare for each NAFTA country as a result of the agreement. Neither of these papers addresses within-country income distribution, which is our focus.

A few papers have looked at aggregate effects on U.S. labor markets, summarized in Burfisher, Robinson, and Thierfelder (2001), and have found only small effects. Several authors have looked at labor market effects in Mexico. Hanson (2007) finds that in the most globalization-affected regions of Mexico over the introduction of NAFTA, both inequality and poverty fell relative to the rest of the country. Prina (2015, 2013) finds that Mexican small farmers tended to benefit from the agreement on balance and that there does not seem to have been much of an effect on rural landless workers. Robertson (2004) finds that the prices of unskilled-intensive goods fell in Mexico following NAFTA, reversing the prior trend and, with them, skilled-wage premiums. Chiquiar (2008) shows that skill premiums in Mexico following NAFTA fell in parts of the country more integrated with world markets relative to more isolated parts of the country. Trefler (2004) looks at the effect of the Canada-U.S. Free Trade Agreement on labor markets in Canada but did not look for local labor market effects.

We here borrow ideas from a variety of sources. A number of studies identify effects of a national trade shock on local labor markets, most notably the pioneering paper by Topalova (2007), who constructed an employment-weighted average tariff for each Indian district to identify the differential effects of local labor market shocks on different locations. Kovak (2013) uses a similar technique for Brazil, derived explicitly from a general equilibrium model. These studies indicate significant location-specific effects of trade shocks on wages, which of course implies mobility costs of some sort for workers that prevent them from arbitraging wage differences across locations. A rich literature examines the correlation of changes in industry tariffs or other industry-specific trade shocks with industry wages. Revenga (1992) finds effects of an industry's import price on that industry's wages in the United States. Pavcnik, Attanasio, and Goldberg (2004) find such effects for Colombia. Here, we allow for both local labor market effects and industry effects.

A number of studies have isolated effects of imports from a specific geographic origin on domestic labor markets. Bernard, Jensen, and Schott (2006) find that imports from low-wage countries have much more pronounced effects on the survival probabilities of U.S. plants in the same product category than imports from other locations. Ebenstein et al. (2014) show that offshoring to low-wage countries is associated with reductions in U.S. employment in the same industry, while offshoring to high-wage countries has the opposite effect. Autor, Dorn, and Hanson (2013) show that a rise in China's share of imports reduces wages in U.S. localities where employment is concentrated in the affected industries. Although Mexico is not a low-wage country by the definition used in these papers, we do isolate Mexico-specific effects of imports on U.S. workers in a similar manner.

In addition, Kennan and Walker (2011) and Artuç, Chaudhuri, and McLaren (2010) estimate structural models of labor mobility, the former focusing on geographic mobility and the latter on interindustry mobility. Both studies find large costs to moving, but not enough to keep a substantial number of workers from moving when economic shocks call for it. Our reduced-form regression can be interpreted as providing confirming evidence for such moving costs.

To anticipate results, we find that NAFTA-vulnerable locations that lost their protection quickly experienced significantly slower wage growth compared to locations that had no protection against Mexico in the first place, particularly for blue-collar workers. For the most heavily NAFTA-vulnerable locations, a high-school dropout would have up to 8 percentage points slower wage growth from 1990 to 2000 compared to the same worker in a location with no initial protection. There is, however, an even larger industry effect, with wage growth in the most protected industries that lose their protection quickly falling 17 percentage points relative to industries that were unprotected to begin with. We show that these results are not driven by preexisting trends, prevailing general globalization, or the coincident rise of imports from China.

To put it in concrete terms, the effect of NAFTA on most workers and on the average worker is likely modest, but for an important minority of workers, the effects are very negative. A high school dropout living in an apparel- and footwear-dependent small town in South Carolina, even if she is employed in the nontraded sector such as in a diner, where she would appear to be immune to trade shocks, would see substantially lower wage growth from 1990 to 2000 than if she were in, for example, College Park, Maryland, which has few NAFTA-vulnerable jobs. For the diner employee in the South Carolina town, as tariffs come down, the local workers in tradable sectors that do compete with Mexico start seeking jobs in the local nontraded sectors and so compete with the diner employee for employment. At the same time, if the same worker had been employed in those

vulnerable tradables sectors when the agreement was signed, she would be hurt twice, with a much lower wage growth than fellow workers who were already working in the diner. These effects, however, are much smaller, and statistically insignificant, for college-educated workers, whose incomes seem to be impervious to NAFTA effects.

II. Empirical Approach

The approach we have described requires a measure of protection by industry and also by geographic location. Note that for each industry j of the 89 Census traded-goods industries, we have an average tariff, τ_t^j, assessed on goods from industry j entering the United States from Mexico. To turn this into a measure of protection in geographic terms, we compute the initial average tariff in a given location, c, which we interpret as the vulnerability of the location to NAFTA. We define this similarly to the local average tariff in Topalova (2007) and Kovak (2013), but we take into account that Mexico is not good at producing everything; a high tariff on imports of good j from Mexico makes no difference if Mexico has no comparative advantage in j and will not export it regardless of the tariff. We thus form a local tariff, averaged across industries weighted by local employment in each industry and also by Mexico's revealed comparative advantage in each industry.

A location's weighted local average tariff (which we will sometimes call its vulnerability) is defined as

$$loc\tau_{1990}^c \equiv \frac{\sum_{j=1}^{N_{ind}} L_{1990}^{cj} RCA^j \tau_{1990}^j}{\sum_{j=1}^{N_{ind}} L_{1990}^{cj} RCA^j}, \tag{1}$$

where L_t^{cj} is the number of workers employed in industry j at conspuma c at date t, N_{ind} is the number of industries, and

$$RCA^j = \frac{\left(\frac{x_{j,1990}^{MEX}}{x_{j,1990}^{ROW}}\right)}{\left(\frac{\sum_i x_{i,1990}^{MEX}}{\sum_i x_{i,1990}^{ROW}}\right)}$$

is Mexico's revealed comparative advantage in j, a slight adaptation of Balassa's (1965) familiar formulation. Here, $x_{j,1990}^{MEX}$ is Mexico's exports of good j to the rest of the world excluding the United States (ROW) and $x_{j,1990}^{ROW}$ is total exports of good j from countries excluding the United States and Mexico to each other. Therefore, RCA^j is Mexico's share of ROW trade in good j, divided by Mexico's share in total ROW trade. The interpretation is that if $RCA^j > 1$, Mexico has more of a tendency to export j than the average product and thus has a revealed comparative advantage in good j.[3]

This gives rise to the realized local tariff change, $loc\triangle\tau^c \equiv \frac{\sum_{j=1}^{N_{ind}} L_{1990}^{cj} RCA^j \triangle\tau^j}{\sum_{j=1}^{N_{ind}} L_{1990}^{cj} RCA^j}$, where $\triangle\tau^j$ is the change in the tariff on good j imports from Mexico from 1990 to 2000.

Now, to show how we attempt to deal with variation in the timing of liberalization, for the moment set aside geography and focus on industry-level effects (which would be an appropriate approach if, for example, we were certain that geographic mobility costs were 0). Then we could estimate a regression as follows,

$$\log(w_i) = \alpha X_i + \sum_j \alpha_j^{ind} ind_{i,j} + \left\{\theta_1 yr2000_i \tau_{1990}^{j(i)} + \theta_2 yr2000_i \triangle\tau^{j(i)}\right\} + \epsilon_i, \tag{2}$$

where i indexes workers; X_i is a set of individual characteristics; $j(i)$ is the index of worker i's industry; $ind_{i,j}$ is a dummy variable that takes a value of 1 if individual i is employed in industry j; $yr2000_i$ is a dummy that takes a value of 1 if individual i is observed in the year 2000; $\triangle\tau^j = \tau_{2000}^j - \tau_{1990}^j$; ϵ_i is a random disturbance term; and the α's and θ's are parameters to be estimated.[4]

In this specification, two factors allow for wages to grow at different rates between 1990 and 2000 in different industries, both captured by the two terms in braces. The more obvious of these is that the tariff on industry i's products imported from Mexico may fall at different rates for different industries; this is captured by the change in tariff in the second term in braces. However, we also include the initial tariff separately from the change in tariff in the first of the two terms in braces. There are a number of reasons an industry whose tariff goes from 3% to 0% over the span 1990 to 2000 might show different effects from an industry whose tariff goes from 13% to 10%, for example. Among those reasons is the fact that the latter industry would be expected to undergo further liberalization in the subsequent years.[5] We allow for all such possible effects by controlling for the initial tariff separately from the change in tariff.

Equation (2) summarizes the essence of our approach to timing of liberalization, but in practice, we are interested in capturing more detail than it entails. In particular, we wish to allow the effects on wages to differ by educational class. We break the sample down into four classes—less than high school, high school graduate, some college, and college graduate—and allow both the initial wage and the wage growth to vary by these categories. This yields the richer regression equation:

[3] In all studies based on local average tariffs, how to treat nontraded industries is a tricky issue. Kovak (2013) studies a general equilibrium model with a nontraded sector, and suggests that since an index of nontraded prices will tend to move in equilibrium in the same direction as an index of traded prices, omitting nontraded employment in the calculation of the local average tariff will capture the effect of a given tariff change properly. This is also followed by Hasan, Mitra, and Ural (2007). In our approach, we effectively are doing the same thing, since for any nontraded industry j, we set RCA^j to 0.

[4] Note that our Census data, which we will describe in detail shortly, take the form of two cross sections rather than a panel. Each individual i in the sample is observed once; some are observed in 1990 and some in 2000.

[5] Artuç, Chaudhuri, and McLaren (2008) explore how anticipated future liberalization can affect the behavior of wages in a model of dynamic labor adjustment.

$$\begin{aligned}\log(w_i) = {} & \alpha X_i + \sum_j \alpha_j^{ind} ind_{i,j} + \sum_{k \neq col} \gamma_{1k} educ_{ik} \\ & + \sum_k \gamma_{2k} educ_{ik} yr2000_i + \sum_{k \neq col} \theta_{1k} educ_{ik} \tau_{1990}^{j(i)} \\ & + \sum_k \theta_{2k} educ_{ik} yr2000_i \tau_{1990}^{j(i)} + \sum_{k \neq col} \theta_{3k} educ_{ik} \triangle \tau^{j(i)} \\ & + \sum_k \theta_{4k} educ_{ik} yr2000_i \triangle \tau^{j(i)} + \epsilon_i, \end{aligned} \tag{3}$$

where $educ_{ik}$ is a dummy variable taking a value of 1 if worker i is in educational category k. The parameters of interest here, corresponding to the initial tariff effect and the impact effect discussed in the context of equation (2), are θ_{2k} and θ_{4k}.[6]

Equation (3) allows for a rich characterization of the response to NAFTA that varies by industry and education, but it does not yet allow for geography. To incorporate that, we include terms that treat local average tariffs as in equation (1) in a way that is parallel to the treatment of industry tariffs. In addition, to be consistent, in controlling for the level of protection by industry, we use the product of industry tariff with the revealed comparative advantage, $RCA^j \tau_{1990}^j$. We also allow for a different rate of wage growth for locations on the U.S.-Mexico border, producing our main estimating equation:

$$\begin{aligned}\log(w_i) = {} & \alpha X_i + \sum_j \alpha_j^{ind} ind_{i,j} + \sum_c \alpha_c^{conspuma} conspuma_{i,c} \\ & + \sum_{k \neq col} \gamma_{1k} educ_{ik} + \sum_k \gamma_{2k} educ_{ik} yr2000_i \\ & + \sum_{k \neq col} \delta_{1k} educ_{ik} loc \tau_{1990}^{c(i)} + \sum_k \delta_{2k} educ_{ik} yr2000_i loc \tau_{1990}^{c(i)} \\ & + \sum_{k \neq col} \delta_{3k} educ_{ik} loc \triangle \tau^{c(i)} + \sum_k \delta_{4k} educ_{ik} yr2000_i loc \triangle \tau^{c(i)} \\ & + \sum_{k \neq col} \theta_{1k} educ_{ik} RCA^j \tau_{1990}^{j(i)} + \sum_k \theta_{2k} educ_{ik} yr2000_i RCA^j \tau_{1990}^{j(i)} \\ & + \sum_{k \neq col} \theta_{3k} educ_{ik} RCA^j \triangle \tau^{j(i)} + \sum_k \theta_{4k} educ_{ik} yr2000_i RCA^j \triangle \tau^{j(i)} \\ & + \mu Border_{c(i)} yr2000_i + \epsilon_i, \end{aligned} \tag{4}$$

where $conspuma_{i,c}$ is a dummy variable that takes a value of 1 if worker i resides in conspuma c, $c(i)$ is the index of worker i's conspuma, and $loc \triangle \tau^{c(i)}$ is the change in tariff for location c, as defined at the beginning of this section.[7]

The parameters of primary interest here are $\delta_{2,k}$ and $\delta_{4,k}$, which measure the initial-tariff effect and the impact effect, respectively, for the local average tariff, and $\theta_{2,k}$ and $\theta_{4,k}$, which measure the initial-tariff effect and the impact effect, respectively, for the industry tariff. If it is easy for workers to move geographically, so that local wage premiums are arbitraged away but difficult for workers to switch industry, we will observe $\delta_{1,k}, \ldots, \delta_{4,k} = 0$ while $\theta_{1,k}, \ldots, \theta_{4,k} \neq 0$. In that case, industry matters but location does not. This, together with the assumption that $\theta_{2,k} = 0$, is how the model in a number of studies such as Pavcnik et al. (2004) is set up. On the other hand, if it is difficult for workers to move geographically but easy to switch industries within one location, we will see the opposite: $\delta_{1,k}, \ldots, \delta_{4,k} \neq 0$ while $\theta_{1,k}, \ldots, \theta_{4,k} = 0$. A pure Youngstown effect would be indicated by $\delta_{4,k} > 0$ while $\delta_{2,k} = \theta_{2,k} = \theta_{4,k} = 0$. This would imply that an export sector worker in Youngstown (with its industries that compete with Mexican imports) would suffer a wage reduction due to NAFTA, while an import-competing worker in Arlington, Virginia (with only very few workers employed in industries that compete with Mexican imports), would not. This is how the model in Kovak (2013) is set up.

Finally, for a location that loses all of its protection within the sample period, the effect on wages within the sample period is equal to $\delta_{2,k} - \delta_{4,k}$, while for an industry that loses all of its protection within the sample period, the effect on wages within the sample period is equal to $\theta_{2,k} - \theta_{4,k}$.

It should be noted that a change in wages brought about by trade policy will tend to overestimate the welfare change for the workers in question, because the welfare change depends on lifetime utility, which includes option value (Artuç et al., 2010). To assess those welfare changes, we would need a structural model, which is beyond the scope of this paper.

III. Data

We use a 5% sample from the U.S. Census for 1990 and 2000, collected from usa.ipums.org, selecting workers from ages 25 to 64 who report a positive income in the year before the census.[8] We include the personal characteristics of age, gender, marital status, whether the worker speaks English, race, and educational attainment (less than high school, high school graduate, some college, college graduate). In addition, we have the industry of employment and conspuma of residence for each worker, as well as the worker's pretax wage and salary income. Our sample size is 10,320,274 workers.

We use data on U.S. tariffs on imports from Mexico collected by John Romalis and described in Feenstra, Romalis, and Schott (2002). We constructed a concordance to map the eight-digit tariff data into the 89 traded goods industry categories of the Census in order to construct industry

[6] The term with θ_{3k} is included only for consistency; it does not seem to have much economic meaning and does not make much difference whether it is included in the regression.

[7] We make the identifying assumption that the initial tariffs and the changes in tariffs are uncorrelated with the shocks ϵ_i. In particular, this assumption would be violated if wages of blue-collar workers were affected by adverse preexisting long-run trends, which also affect initial tariffs through the political process. We study the viability of this identifying assumption, in particular the issue of preexisting trends in section IVC.

[8] The sample includes individuals who report being employed, unemployed, or not in labor force in the census year. We use the last industry of employment for the unemployed and those not in labor force.

TABLE 1.—SUMMARY STATISTICS

Variable	Mean	SD	Minimum	Maximum	Number
Individual level					
Age	41	10	25	64	10,320,274
Male	0.53	0.50	0	1	10,320,274
Married	0.67	0.47	0	1	10,320,274
English speaking	0.99	0.09	0	1	10,320,274
White	0.81	0.39	0	1	10,320,274
High school dropouts	0.11	0.31	0	1	10,320,274
High school graduates	0.33	0.47	0	1	10,320,274
Some college	0.30	0.46	0	1	10,320,274
College graduates	0.26	0.44	0	1	10,320,274
Border	0.04	0.21	0	1	10,320,274
Industry level					
Tariff in 1990, τ^j_{1990} (%)	2.1	3.9	0	17	89
Change in tariff, $\triangle\tau^j$ (%)	−1.7	3.4	−16.4	2.9	89
RCA_{1990}	0.8	2.5	0	22.1	89
$RCA\tau^j_{1990}$ (%)	1.0	2.0	0	8.8	89
$RCA\triangle\tau^j$ (%)	−0.9	1.6	−7.0	0.01	89
Change in Imports, $\triangle M^j$ (%)	2.9	6.5	−11.5	34.9	89
Conspuma level (excluding agriculture, RCA adjusted)					
Local tariff in 1990, $loc\tau^c_{1990}$(%)	1.03	0.67	0.09	4.74	543
Change in local tariff, $loc\triangle\tau^c$ (%)	−0.92	0.61	−4.30	−0.08	543
Change in imports, $\triangle M^c$(%)	0.75	0.56	−0.40	3.44	543

Industry-level tariff variables are computed from eight-digit HS tariff data weighted by imports from Mexico and are mapped into 89 tradable goods industries based on Census industry classification. RCA is Mexico's revealed comparative advantage in a particular industry as defined in the text. Conspuma-level variables are weighted by employment in industries of a given conspuma.

tariffs τ^j_t.[9] We computed time-invariant trade weights using imports from Mexico in 1990 to obtain a trade-weighted average tariff for each Census industry.[10] To construct Mexico's revealed comparative advantage in 1990, RCA^j, we used data on exports by reporting countries from the U.N. Comtrade.[11]

Our measures of location and industry are both coarse because of the nature of Census data. We would ideally prefer to have information on the county of residence for each worker, since a conspuma typically encompasses multiple counties.[12] By the same token, we have only 89 traded goods industries, and so cannot make use of the rich variation in tariff changes across tariff codes. Because of these issues, we are likely to underestimate the effects of trade on wages in both geographic and industry dimensions.

Table 1 shows descriptive statistics for the main control variables. The sample is 53% male and 81% white, with an average age of 41 years. High school dropouts are 11% of the total, with the remainder about evenly split between high school graduates, those with some college, and college graduates. The tariff in 1990 on Mexican goods ranged across traded goods industries from 0 to 17%, with a mean of 2%. These tariffs are generally below the U.S. Most Favored Nation (MFN) tariffs, which are charged on imports from World Trade Organization (WTO) members as a default. The difference is due to the Generalized System of Preferences (GSP), under which rich countries extend discretionary tariff preferences to lower-income countries (Hakobyan, 2015). After multiplying the tariff by RCA^j to correct for Mexico's pattern of comparative advantage, we obtain a product that ranges from 0 to 8.8% (for footwear). The initial average local tariff ranges across conspumas from approximately 0.09 to 4.74%, with a mean just above 1%.

Table 2 shows which industries received the most protection against Mexican imports, before adjusting for Mexico's comparative advantage (top half) and after (bottom half). Comparison of the top and bottom halves shows that the correction for Mexican comparative advantage makes a fair amount of difference.[13] The bottom half thus shows the industries that have the greatest potential to be vulnerable to NAFTA. The top two are footwear and oil and gas extraction, followed by carpets and rugs and plastics, all in the range of 7.7% to 8.8%. The relationship between the 1990 tariff levels and the decline in tariffs between 1990 and 2000 mostly stays close to the 45 degree line, but with plenty of deviations (figure 1). Industries whose tariffs fell much more slowly than average include Footwear (initial tariff is 17%; the 2000 tariff is 9.2%) and Structural Clay Products (initial tariff is 14.5%; the 2000 tariff is 7.1%). After adjusting

[9] Note that only 89 of the 238 Census industry categories produce tradable goods and can be mapped to trade data. Recall that nontraded industries are not included in the calculation of the local average tariff. See note 3.

[10] Trade data are obtained from the U.S. International Trade Commission Trade and Tariff DataWeb at http://dataweb.usitc.gov.

[11] The distribution of RCA is right-skewed (see the online appendix, figure A1), with two extreme values for industries Oil and Gas Extraction (22.1) and Newspaper Publishing and Printing (9.3).

[12] The Census does record county information, but the publicly available microsamples do not consistently report it because of rules to protect confidentiality.

[13] For example, of the 10 industries with the highest tariffs, only one (Carpets and Rugs) has a value of RCA above 1; at the same time, Grain Mill Products has the tenth highest tariff at 5.5%, but since its RCA is only 0.08, its corrected tariff is not even in the top 20. Clearly, ignoring Mexico's comparative advantage would give a very distorted picture of NAFTA's effects.

An earlier draft did not correct for Mexican comparative advantage at all. The results were qualitatively similar, but for the location variables, the impact and initial tariff effects were larger, and the net effect was much smaller. Those details are available in tables A1 and A2 the online appendix.

TABLE 2.—TOP TEN MOST PROTECTED INDUSTRIES IN 1990

Rank	Industry Name	τ^j_{1990} (%)	$\Delta\tau^j$
1	Footwear, except Rubber and Plastic	17.0	−9.2
2	Apparel and Accessories, except Knit	16.6	−16.4
3	Canned, Frozen, and Preserved Fruits and Vegetables	15.9	−15.3
4	Knitting Mills	15.7	−15.7
5	Structural Clay Products	14.5	−7.1
6	Yarn, Thread, and Fabric Mills	9.3	−9.2
7	Leather Products, except Footwear	7.4	−5.5
8	Dyeing and Finishing Textiles, except Wool and Knit Goods	7.4	−7.4
9	Carpets and Rugs	6.9	−5.0
10	Grain Mill Products	5.5	−3.8
	Adjusted for Mexico's Revealed Comparative Advantage		
Rank	Industry Name	$RCA\tau^j_{1990}$ (%)	$RCA\Delta\tau^j$
1	Footwear, except Rubber and Plastic	8.8	−4.8
2	Oil and Gas Extraction	8.3	−6.5
3	Carpets and Rugs	7.7	−5.6
4	Plastics, Synthetics, and Resins	7.0	−6.9
5	Canned, Frozen, and Preserved Fruits and Vegetables	6.6	−6.3
6	Dyeing and Finishing Textiles, except Wool and Knit Goods	6.5	−6.5
7	Structural Clay Products	3.9	−1.9
8	Agricultural Production, Crops	3.9	−3.8
9	Leather Products, except Footwear	3.3	−2.5
10	Blast Furnaces, Steelworks, Rolling and Finishing Mills	3.2	−2.5

The table reports 1990 tariff and change in tariff between 1990 and 2000 for 10 (out of 89) most protected industries. The top panel ranks the industries according to their imports-weighted average tariff; the bottom panel ranks the industries by their tariff adjusted for Mexico's revealed comparative advantage.

for Mexico's revealed comparative advantage, tariffs in these industries still fell the slowest. These are among the cloud of industries well off the 45 degree line. Two industries (Dairy Products; Printing, Publishing, and Allied Industries) actually experienced tariff increases between 1990 and 2000. Clearly, although initial tariffs and tariff changes are highly correlated, they have a lot of independent variation, and the results will show that it is important to control for both of these variables separately.

We have computed two versions of the local average tariff. In one, all industries are treated in the same way; in the second, we omit agriculture by setting its tariff equal to 0. The reason for doing this is that aggregation of industries is a particularly large problem for agriculture, as the Census makes no distinction between different crops. We know that corn, in particular, benefited greatly from NAFTA due to the elimination of Mexican corn quotas, while other crops, such as some vegetables, were likely hurt. However, with Census aggregation, we are forced to apply the same tariff to all agriculture. This resulted in various farming areas of the Great Plains, where corn is king, appearing, implausibly, in the top ten most vulnerable conspumas (figure 2). To eliminate this problem, we have performed parallel regressions with agriculture omitted by artificially setting the agriculture tariff equal to 0, and we report the two sets of regressions side by side. The results are close to identical, but we refer to the version without agriculture as our preferred specification.

Table 3 shows the conspumas with the highest and lowest 1990 local average tariffs on Mexican goods, and hence the most and least potential vulnerability to NAFTA (local average tariffs with agriculture omitted is used). The list is dominated by manufacturing areas of the Carolinas. The least vulnerable locations include Washington, D.C., and its suburbs in northern Virginia and Maryland. The relationship between the 1990 local tariff levels and the decline in local tariffs between 1990 and 2000 is mostly linear, but with plenty of deviations (figure 3). The largest differences between the initial local tariff and change in local tariff are observed in a conspuma in the state of Missouri (initial tariff is 2.98%; the change in tariff is −1.92%).

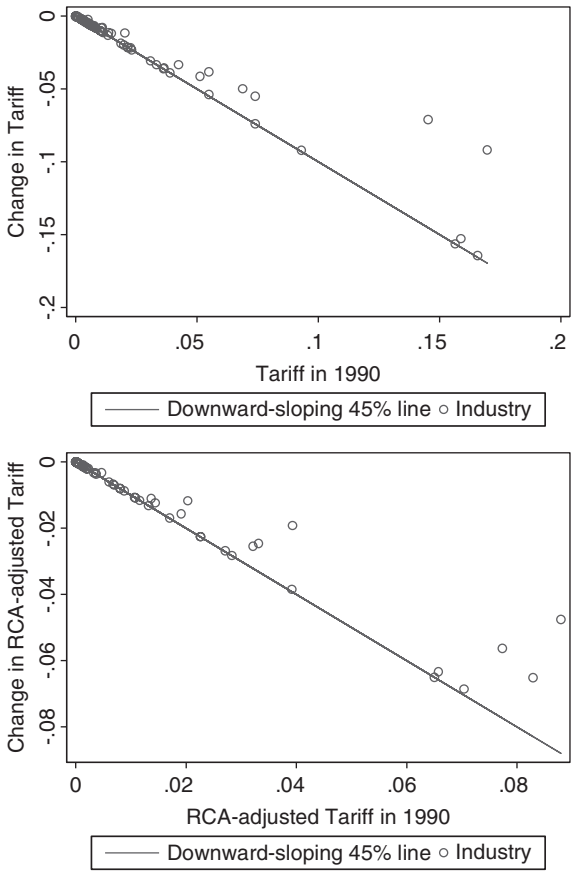
IV. Results

A. *Basic Estimates*

Table 4 shows the results for the main regression with all right-hand-side variables and industry and conspuma fixed effects. This is the estimation of equation (4), with clustering of standard errors by conspuma, industry, and year, following Cameron, Gelbach, and Miller (2006). To focus on the coefficients of interest, we do not show the worker controls, but they have unsurprising coefficients. Married white men enjoy a wage premium; there is a concave age curve; and workers with more education earn higher wages, ceteris paribus. The effect of the dummy for location on the Mexican border is both statistically insignificant and economically minuscule, implying half a percentage point of additional wage growth over a ten-year period. Evidently the experiences of towns like Laredo and towns like Nogales cancel each other out on average.

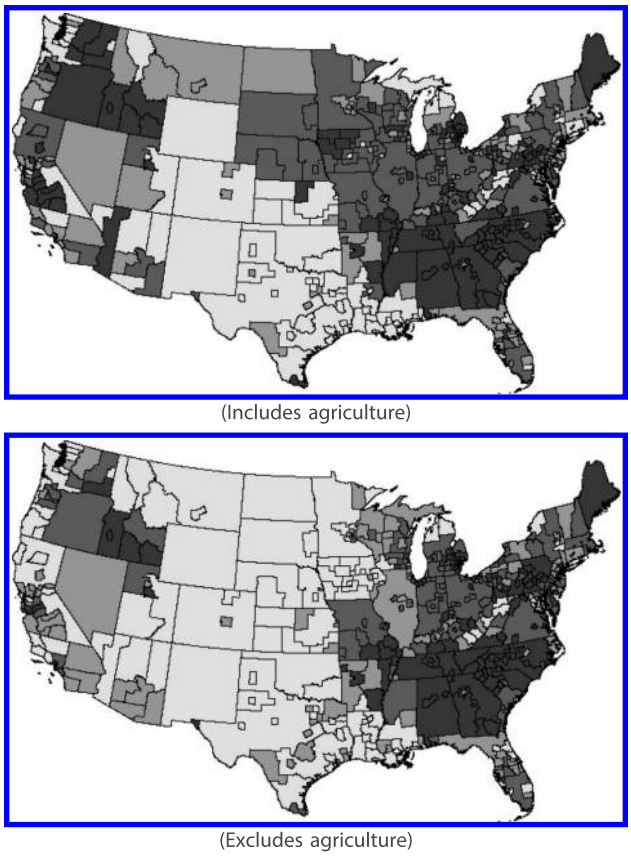
For each educational class k, the coefficients of interest are the equivalent of the key parameters in equation (4): $\delta_{2,k}$, which are listed in table 4 as the initial-tariff effect for the location-specific controls; $\delta_{4,k}$, listed as the impact effect for

FIGURE 1.—INDUSTRY TARIFF IN 1990 AND TARIFF DECLINE BETWEEN 1990 AND 2000



The figures omit two industries (Dairy Products; Printing, Publishing, and Allied Industries) that experienced tariff increase between 1990 and 2000, as well as those that faced 0 tariffs in 1990.

FIGURE 2.—VARIATION IN LOCAL AVERAGE TARIFF



the location-specific controls; $\theta_{2,k}$, listed as the initial-tariff effect for the industry-specific controls; and $\theta_{4,k}$, listed as the impact effect for the industry-specific controls. In addition, the values of $\delta_{2,k} - \delta_{4,k}$ and $\theta_{2,k} - \theta_{4,k}$ for the case with agriculture excluded are reported in table 5, together with the results of the test of the hypothesis that these differences are equal to 0. We present results with and without agriculture excluded for comparison; the results are very similar, and we will focus on our preferred specification with agriculture excluded throughout the paper.

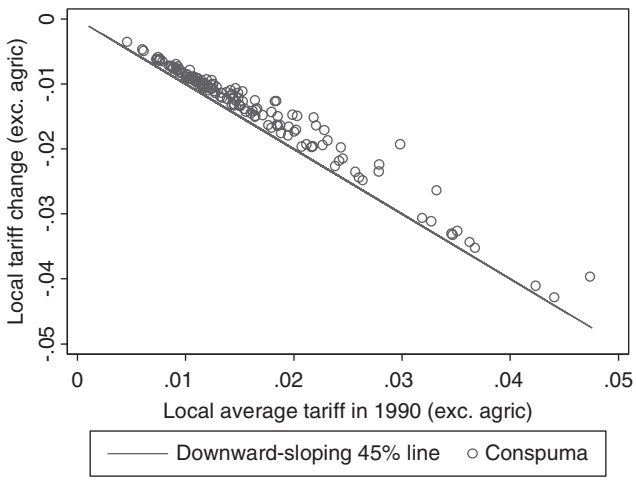
Looking first at the location variables, we find point estimates for high school dropouts of 12.68 for $\delta_{2,lhs}$ and 14.79 for $\delta_{4,lhs}$ (column 2 of table 4). Note first that the impact effect is larger than the initial tariff effect, and table 5 shows that $\delta_{2,lhs} - \delta_{4,lhs}$ takes a value of -2.12, with a high level of significance. In other words, among conspumas that lost their protection quickly under NAFTA, those that appeared to be very vulnerable had substantially lower wage growth for high school dropouts than those with low initial tariffs. Recalling that the most vulnerable conspumas had an initial local average tariff in the neighborhood of 4%, this implies a drop in wage growth over the 1990s of around 8 percentage points in such a conspuma, a substantial difference. However, for most high school dropouts, there is little effect; a 1 standard deviation increase in vulnerability (from the bottom panel of table 1) has an effect of $(0.67) \times (-2.11) = -1.41$ percentage points of wage growth over the decade for a conspuma that lost all of its protection by 2000. Similar comments apply for high school graduates and for workers with some college but with smaller magnitudes, while college graduates show much smaller, as well as statistically insignificant, coefficients.

Turning now to the coefficients on the industry effects, the first feature to point out is that, from table 4, the industry effects $\theta_{2,k}, \theta_{4,k}$ are less precisely estimated compared to the corresponding $\delta_{2,k}, \delta_{4,k}$ coefficients for the location effects. However, from table 5, the differences $\theta_{2,k} - \theta_{4,k}$ are precisely estimated (apart from college graduates, for whom the difference is not significantly different from 0). Recall that the most highly protected industries had an initial value of tariff times *RCA* in the neighborhood of 8%; high school dropouts in such an industry, if it lost its protection right away, would see wage growth of 17 percentage points lower than similar workers in an industry that had had no protection. Unlike the local tariffs, the effect of industry tariffs is significant for high school dropouts who are not at the extremes; a 1 standard deviation increase in the initial industry tariff (from the middle panel of table 1) has an effect of $3.9 \times (-2.143) = -8.36$ percentage points of wage

TABLE 3.—MOST AND LEAST VULNERABLE CONSPUMAS, EXCLUDING AGRICULTURE

Rank	State	Counties/Cities	$loc\tau^c_{1990}$ (%)	$loc\Delta\tau^c$
		A: Top Ten Most Vulnerable Conspumas		
1	Georgia	Catoosa, Dade, Walker	4.74	−3.96
2	North Carolina	Alamance, Randolph	4.41	−4.28
3	South Carolina	Oconee, Pickens	4.24	−4.10
4	South Carolina	Including Cherokee, Chester, Chesterfield, Clarendon	3.67	−3.53
5	South Carolina	Anderson	3.62	−3.43
6	North Carolina	Cabarrus, Rowan	3.54	−3.45
7	North Carolina	Alexander, Burke, Caldwell	3.51	−3.26
8	South Carolina	Including Abbeville, Edgefield, Fairfield	3.47	−3.32
9	North Carolina	Cleveland, McDowell, Polk, Rutherford	3.46	−3.30
10	Indiana	Gary	3.32	−2.64
		B: Top Ten Least Vulnerable Conspumas		
1	D.C.	Washington	0.09	−0.08
2	Washington	Kitsap	0.19	−0.18
3	Virginia	Arlington	0.21	−0.18
4	Maryland	Calvert, Charles, St. Mary's County	0.23	−0.19
5	Montana	including Flathead, Lincoln, Missoula, Ravalli	0.27	−0.24
6	Maryland	including College Park, Hyattsville, Prince George's	0.28	−0.24
7	Virginia	Alexandria	0.29	−0.25
8	Montana	Including Big Horn, Blaine, Carter, Chouteau	0.30	−0.24
9	South Dakota	Including Aurora, Beadle, Bennett, Brule, Buffalo	0.30	−0.28
10	Iowa	Calhoun, Hamilton, Humboldt, Pocahontas, Webster	0.30	−0.28

FIGURE 3.—LOCAL AVERAGE TARIFF IN 1990 AND LOCAL TARIFF DECLINE BETWEEN 1990 AND 2000



Only conspumas with $loc\tau^c_{1990} - |loc\Delta\tau^c| > 0.0011$ are plotted. Local tariffs are computed with tariffs on agriculture set to 0.

growth over the decade. Again, the effect is much smaller for high school graduates and those with some college, and negligible (as well as statistically insignificant) for college graduates.

The fact that both the location and the industry effects hit blue-collar workers, especially high school dropouts, but not college graduates suggests the possibility that the costs of moving or of switching industries are larger for less educated workers, so that more educated workers can adjust more easily and arbitrage wage differences away.[14]

To sum up, both locational and industry variables are highly statistically significant after controlling for a wide range of personal characteristics. This suggests that both costs of moving geographically and costs of switching industries are important. In addition, we find for blue-collar workers a significant "Youngstown" effect in the data: more vulnerable locations that lost their tariffs quickly had smaller wage growth compared with locations that had no NAFTA vulnerability at all, controlling for a broad range of personal characteristics. These effects are strongest for high school dropouts and disappear for college graduates. (We will show that this applies across industries, so that even workers in a nontraded industry—waiting on tables in a diner, for example—saw a sharp reduction in wages if they were in a vulnerable location that lost its protection quickly.)

In addition, the local labor market effects depend separately on the initial tariff and the change in tariff. Locations with high protection but that had not yet lost it saw wages rise relative to the rest of the country, possibly because of workers leaving the area and making labor more scarce.[15]

[14] It should be noted that Artuç et al. (2010) looked for differences in interindustry mobility costs and found no significant differences. However, they used only two skill categories (some college and no college), had a much smaller data set, and were not controlling for geographical mobility.

[15] The main story that emerges from table 5 is unaffected if one conducts a simpler regression that ignores initial tariff effects altogether—in other words, if one omits the δ_{2k} and θ_{2k} terms from equation (4) and regresses wage growth over the 1990s on tariff changes between 1990 and 2000. The results of this exercise tell a very similar story when compared with their analogues in table 5. However, a closer, look reveals a very important difference: the distributional story is nearly the opposite of what we find in the main regression, in the sense that overall, the strongest local labor market effects are borne by white-collar workers, whereas table 5 shows that overall, the strongest local labor market effects are borne by blue-collar workers. The difference is clearly important, but since these results result from an unwarranted parameter restriction ($\delta_{2k} = \theta_{2k} = 0$), we reject them in favor of our benchmark.

TABLE 4.—REGRESSION RESULTS

Dependent Variable: Log Wage	Including Agriculture (1)	Excluding Agriculture (2)
Location Effect		
Initial tariff effect		
Less than high school × $loc\tau^c_{1990}$ × (Year = 2000)	8.17***	12.68***
	(1.206)	(0.953)
High school × $loc\tau^c_{1990}$ × (Year = 2000)	0.53	3.77***
	(0.754)	(1.039)
Some college × $loc\tau^c_{1990}$ × (Year = 2000)	−4.47***	0.41
	(0.516)	(0.925)
College × $loc\tau^c_{1990}$ × (Year = 2000)	−6.32**	−5.71
	(2.912)	(3.724)
Impact effect		
Less than high school × $loc\triangle\tau^c$ × (Year = 2000)	8.66***	14.79***
	(1.239)	(1.064)
High school × $loc\triangle\tau^c$ × (Year = 2000)	0.12	4.69***
	(0.929)	(1.305)
Some college × $loc\triangle\tau^c$ × (Year = 2000)	−4.91***	1.95
	(0.808)	(1.364)
College × $loc\triangle\tau^c$ × (Year = 2000)	−5.69*	−4.78
	(3.010)	(3.762)
Industry Effect		
Initial-tariff effect		
Less than high school × $RCA\tau^j_{1990}$ × (Year = 2000)	−3.81	2.28**
	(2.867)	(1.109)
High school × $RCA\tau^j_{1990}$ × (Year = 2000)	−0.94	1.17
	(2.094)	(1.416)
Some college × $RCA\tau^j_{1990}$ × (Year = 2000)	−1.35	1.27
	(2.116)	(1.465)
College × $RCA\tau^j_{1990}$ × (Year = 2000)	−2.84	−0.90
	(1.947)	(2.171)
Impact effect		
Less than high school × $RCA\triangle\tau^j$ × (Year = 2000)	−4.58	4.42***
	(4.006)	(1.203)
High school × $RCA\triangle\tau^j$ × (Year = 2000)	−0.68	2.40
	(2.694)	(1.759)
Some college × $RCA\triangle\tau^j$ × (Year = 2000)	−0.99	2.71
	(2.733)	(1.837)
College × $RCA\triangle\tau^j$ × (Year = 2000)	−3.12	−0.60
	(2.449)	(2.856)

N = 10,320,274. R^2 = 0.31. The regressions include a constant, individual characteristics—age, age squared, dummies for male, married, white, speaking English, three of education groups (college graduate is the omitted category)—and a set of interaction terms specified in equation (4). Robust standard errors in parentheses are clustered by conspuma, industry and year. Significant at ***1%, **5%, *10%.

TABLE 5.—DIFFERENCES BETWEEN INITIAL-TARIFF AND IMPACT EFFECT, EXCLUDING AGRICULTURE

Parameter Difference	Point Estimate	F-Value
Location effect		
Less than high school, $\delta_{2,lhs} - \delta_{4,lhs}$	−2.110***	11.59
High school graduate, $\delta_{2,hs} - \delta_{4,hs}$	−0.915**	6.48
Some college, $\delta_{2,scol} - \delta_{4,scol}$	−1.538***	7.51
College graduate, $\delta_{2,col} - \delta_{4,col}$	−0.936	1.46
Industry effect		
Less than high school, $\theta_{2,lhs} - \theta_{4,lhs}$	−2.143***	16.33
High school graduate, $\theta_{2,hs} - \theta_{4,hs}$	−1.232***	6.76
Some college, $\theta_{2,scol} - \theta_{4,scol}$	−1.131***	6.90
College graduate, $\theta_{2,col} - \theta_{4,col}$	−0.302	0.11

The table reports the overall impact on wages (computed as a difference between initial-tariff and impact effect reported in column 2 of table 4) and its significance for each education group when a location or an industry loses all of its protection within the sample period. Significant at ***1%, **5%, *10%.

B. Robustness

Measuring comparative advantage. To be sure that our results are not driven by the way we measure our underlying variables, we further examine various measures of comparative advantage RCA^j. Although we have argued (see note 13) that correcting for Mexico's revealed comparative advantage is important in measuring the effect of NAFTA tariff changes, we have also run the main regression without this correction—in effect, we use equation (1) to compute the local average tariff, with $RCA^j \equiv 1$—and we report results in the online appendix, tables A1 and A2 column 2. The results are similar to the benchmark specification, except that both the initial tariff and impact effects are much larger, and their difference is smaller in magnitude and significant in fewer cases (and actually 0 for the case of locational effects for high school dropouts). Note that with the correction for RCA^j removed, now nontraded sectors have a positive weight in computation of the local tariffs (see note 3). If we follow Kovak's (2013) suggestion and omit nontraded workers from the computation of local tariffs, we obtain the results reported in the third column of tables A1 and A2. We find that these are similar to the benchmark specifications but with smaller magnitudes, and for industry effects, only the high school dropout coefficient is significant. Finally, we return to the benchmark specification and recalculate RCA^j using trade flows from 1980 rather than 1990 to eliminate any possibility of endogeneity, with the results reported in the last column of tables A1 and A2. The results are qualitatively as in the benchmark but with fewer significant results. We conclude that our main results are not driven by our handling of revealed comparative advantage.

Limiting the sample to service sector workers. In interpreting the main regression results, we have interpreted the coefficients on the location variables as telling us about what happens to a worker who is not in the tradable sector but employed in close proximity to workers who are. In table 6, we scrutinize that interpretation by limiting our sample only to workers in the service sector and running the main regression again. Of course, the industry-specific variables cannot be used in this exercise (apart from industry fixed effects), since those are all derived from tariffs, which do not apply to services. Again, standard errors are clustered by industry, conspuma, and year.

Comparing the last four lines of table 6 with table 5 shows almost identical point estimates. The table therefore confirms that local labor market effects do indeed apply to workers who are not employed in the tradable sector. Thus, a worker waiting on tables in a town heavily dependent on NAFTA-vulnerable jobs, although he or she is not employed in an industry producing tradable output, is nonetheless harmed indirectly by NAFTA, plausibly due to workers who are in a contracting tradables industry seeking employment in local nontraded industries, pushing those wages down, or due to

TABLE 6.—WORKERS IN SERVICES, EXCLUDING AGRICULTURE

Location effect	
Less than high school × $loc\tau^c_{1990}$ × (Year = 2000)	22.17***
	(1.277)
High school × $loc\tau^c_{1990}$ × (Year = 2000)	7.86***
	(1.579)
Some college × $loc\tau^c_{1990}$ × (Year = 2000)	1.83
	(1.442)
College × $loc\tau^c_{1990}$ × (Year = 2000)	−5.14
	(4.102)
Less than high school × $loc\triangle\tau^c$ × (Year = 2000)	24.71***
	(1.323)
High school × $loc\triangle\tau^c$ × (Year = 2000)	8.75***
	(1.978)
Some college × $loc\triangle\tau^c$ × (Year = 2000)	3.26*
	(1.882)
College × $loc\triangle\tau^c$ × (Year = 2000)	−4.19
	(4.202)

Differences between Initial Tariff and Impact Effect

	Point Estimate	F-Value
Less than high school, $\delta_{2,lhs} - \delta_{4,lhs}$	−2.54***	15.01
High school graduate, $\delta_{2,hs} - \delta_{4,hs}$	−0.93**	3.87
Some college, $\delta_{2,scol} - \delta_{4,scol}$	−1.43***	7.01
College graduate, $\delta_{2,col} - \delta_{4,col}$	−0.94	2.44

$N = 7{,}489{,}403$. $R^2 = 0.30$. The top panel replicates the main regression results (excluding agriculture) reported in column 2 of table 4 for a sample of service sector workers. The regression includes the full vector of control variables. Robust standard errors in parentheses are clustered by conspuma, industry, and year. The bottom panel reports the overall impact on wages and its significance when a location loses all of its protection within the sample period, analogous to table 5. Significant at ***1%, **5%, *10%.

diminished incomes in that locality, reducing demand for restaurant meals and other nontraded services.

C. *Are the Results an Artifact of Omitted Trends?*

A natural question arises from this exercise: Are the right-hand-side variables merely picking up trends in wages resulting from other forces? One might conjecture that protection had been concentrated in labor-intensive industries that are vulnerable to increased imports from low-wage economies, and the rise of imports from those economies since 1990 has depressed wages in those industries at the same time as their tariffs have come down. In that case, the movements in wages that we observe are caused by the coincident rise in imports from low-income economies and not by NAFTA. In addition, there is the possibility that the tariff changes we track are correlated with other aspects of globalization, and so the effects that they are picking up are not specific to trade with Mexico. For instance, U.S. MFN tariffs also declined over this period according to staged duty reductions under the Uruguay Round Agreements Act.

It turns out that this does not seem to be what drives our results. We address this question in several ways. First, we run a placebo regression, similar to an exercise in Autor et al. (2013), in which wage changes from the 1980s, before NAFTA, are regressed on our right-hand-side variables instead of wage changes in the 1990s. Second, if the driving force was increases in imports from low-wage economies, then Mexico's share of U.S. imports would not be correlated with wage changes, so we run a regression using such shares in place of the tariff data. Third, to verify that we are not merely picking up effects of coincidental rises in imports from China, we run our regression with imports from China included. We discuss each of these in turn.

TABLE 7.—PLACEBO REGRESSION WITH 1980 AND 1990 WAGES: DIFFERENCES BETWEEN INITIAL TARIFF AND IMPACT EFFECT

Parameter Difference	Point Estimate	F-Value
Location effect		
Less than high school, $\delta_{2,lhs} - \delta_{4,lhs}$	5.20***	64.61
High school graduate, $\delta_{2,hs} - \delta_{4,hs}$	3.37***	22.62
Some college, $\delta_{2,scol} - \delta_{4,scol}$	4.15***	24.36
College graduate, $\delta_{2,col} - \delta_{4,col}$	2.49**	3.96
Industry effect		
Less than high school, $\theta_{2,lhs} - \theta_{4,lhs}$	−0.12	0.05
High school graduate, $\theta_{2,hs} - \theta_{4,hs}$	−1.34**	5.12
Some college, $\theta_{2,scol} - \theta_{4,scol}$	−1.46***	8.51
College graduate, $\theta_{2,col} - \theta_{4,col}$	−0.71	0.36

The table reports the overall impact on wages (computed as a difference between initial tariff and impact effect) and its significance when a location or an industry loses all of its protection within the sample period, using 1980 and 1990 Census data and the same location and industry controls as in column 2 of table 4. We omit agriculture by setting its tariff equal to zero. Significant at ***1%, **5%, *10%.

A placebo regression. To investigate the possibility that we are merely picking up long-run trends that are correlated with initial tariffs, we repeat the basic regression described in equation (4) and reported in table 4, but with 1980 wages in place of 1990 wages and 1990 wages in place of 2000 wages. This is, of course, not an interesting regression to run in its own right, but it is useful as a test similar to the falsification exercise reported by Autor et al. (2013) in columns 4 to 6 of their table 2. If it produces coefficients similar to what we find in the benchmark regression, then we seem to be picking up preexisting trends rather than NAFTA effects. Rather than report the full details of the regression, we show the summary table 7, analogous to table 5. It is clear that the patterns picked up in the main regression disappear completely. The coefficients corresponding to the location effects change sign, and the coefficients corresponding to the industry effects do not exhibit any pattern. It is not clear what meaning, if any, to attach to the coefficients in table 7, but it is clear that the source of the findings in our main regression is not a preexisting trend.[16]

[16] In addition, it can be shown that conspumas that had high local average tariffs in 1990 and lost those tariffs by 2000 tended to have growing wages and employment in the 1980s relative to other locations. There is no statistically significant relationship at the industry level. Details are available on request. To check that table 7 does not imply that we are merely picking up reversion to the mean in our main regression, we run the main regression with 1980 wages and with wage growth in the 1980s as controls. The results of the main regression are unchanged; see online appendix table A7.

We have also experimented with using 2005 and 2010 Census data in the placebo in place of the 1980–1990 data, using the much smaller American Community Survey samples. These post-NAFTA samples produce coefficients with mostly the same sign as the benchmark regression, some larger and some smaller, some significant and some not. We argue that this is not a clean placebo, though, because tariffs were still being phased out over 2000 to 2010, which could explain the same sign of coefficients in the placebo regressions as in the benchmark, and because the effects of earlier tariff reductions could still be working themselves out over 2000 to 2010. Details are available on request.

TABLE 8.—CHANGE IN MEXICAN IMPORT SHARES, EXCLUDING AGRICULTURE

Dependent Variable: Log Wage	Change in Mexican Import Share (1)	Control for Change in Chinese Import Share (2)	IV: Change in Tariffs (3)	IV: Change in Share of Mexican Exports to ROW (4)
Location effect				
Less than high school × $\triangle M^c$ × (Year = 2000)	−0.45	−0.64	−1.76***	−15.05***
	(0.48)	(0.46)	(0.49)	(1.40)
High school × $\triangle M^c$ × (Year = 2000)	1.20***	0.71***	−0.54**	−4.07***
	(0.17)	(0.24)	(0.24)	(0.64)
Some college × $\triangle M^c$ × (Year = 2000)	0.23	−0.15	−2.36***	−3.14***
	(0.15)	(0.23)	(0.18)	(1.07)
College × $\triangle M^c$ × (Year = 2000)	−0.14	−0.95**	−2.26**	−1.25
	(0.42)	(0.45)	(0.92)	(1.79)
Industry effect				
Less than high school × $\triangle M^j$ × (Year = 2000)	−1.07***	−1.03***	−1.84***	−2.85***
	(0.09)	(0.09)	(0.29)	(0.25)
High school × $\triangle M^j$ × (Year = 2000)	−0.60***	−0.53***	−1.85***	−2.07***
	(0.08)	(0.09)	(0.29)	(0.24)
Some college × $\triangle M^j$ × (Year = 2000)	−0.52***	−0.44***	−1.46***	−1.62***
	(0.10)	(0.08)	(0.35)	(0.33)
College × $\triangle M^j$ × (Year = 2000)	−0.07	−0.08	−0.22	−0.28
	(0.16)	(0.13)	(0.49)	(0.48)

N = 10,320,274. The regressions include a constant, individual characteristics, and a full set of interaction terms between education group and change in the share of Mexican imports at the industry and location level. In all regressions, we omit agriculture by setting its share equal to 0. Robust standard errors in parentheses are clustered by conspuma, industry, and year. Significant at ***1%, **5%, *10%.

Mexican import shares. One concern with the results so far is that they could reflect ongoing trends in globalization more generally that just happen to be correlated with the decline in tariffs on Mexican goods. To deal with this concern, we perform a simple regression using changes in Mexican import shares instead of tariffs, since if the effects picked up in the main regression are really caused by a general increase in imports that happens to be correlated with the tariffs we use, then those effects will not be correlated with the share of Mexican imports in total imports. For industry j at date t, we compute Mexico's share, M_t^j, in U.S. imports of industry j goods. For each conspuma c, we find the local average value of M_t^j, with weights given by employment shares in 1990 within the conspuma and denote that local average as M_t^c. Figure A2 of the online appendix shows considerable variation in the industry Mexican import shares between 1990 and 2000, with Leather Tanning and Finishing and Railroad Locomotives and Equipment experiencing the largest increase (35 and 31 percentage points, respectively). The largest drop in Mexican import share is observed for industries Nonmetallic Mining and Quarrying, Except Fuels and Agricultural Production, Livestock, 11.5 and 10.3 percentage points, respectively. From table 1, the average change in Mexican import share across 89 traded goods industries is 2.9 percentage points, and the average change in local Mexican import share across all conspumas is 0.7 percentage points.

We run a wage regression with the following right-hand-side variables: the individual controls, industry, and conspuma fixed effects as in the main regression, plus the change in the industry Mexican import share, $\triangle M^j$, interacted with education class and year 2000 dummies; and the change in the local average Mexican import share, $\triangle M^c$, interacted with education class and year 2000 dummies. In effect, in a simplified form, the Mexican import shares take over the role of the U.S. tariffs on Mexican imports in the main regression. This approach is similar in spirit to Bernard et al. (2006) use of import penetration by low-wage countries (with parallels in Ebenstein et al., 2014, and Autor et al., 2013). Descriptive statistics are included in table 1, and the main results are shown in table 8 (we suppress all coefficient estimates except for the interactions with the change in import share and year 2000 dummy, since those are the coefficients of interest). Throughout table 8, we omit agriculture, in line with our preferred specification of the main regression.

Note that this import share regression is not a convincing way of evaluating NAFTA effects, because Mexico's import shares can change for many reasons not associated with NAFTA. Productivity shocks in Mexican industries can affect the import shares, and the Mexican macrocrisis of the mid-1990s may well have affected different industries differentially. In addition, if, say, China doubles its exports of a given product to the United States, that will push the Mexican import share down and may push U.S. wages in that industry down as well. For all of these reasons, this regression is a much noisier method of inferring effects of NAFTA than looking directly at policy, as in our main regression. However, it can still be useful because strong positive coefficients make it less plausible that the benchmark findings are driven by general globalization.

The first column of table 8 shows the simplest form of this regression. In this regression, the location effects essentially disappear. The location coefficients are mostly statistically insignificant, and the point estimates multiplied by even the largest change in location-average import share are economically negligible ($-0.45 \times 3.44\% = -1.55\%$ for high school dropouts, for example, meaning less than 2 percentage points

of reduced wage growth over 10 years for the most heavily affected worker). The industry results, however, come out more strongly than in the tariff regression. For each education class except for college graduates, a rise in the Mexican share of imports of the workers' industry implies a statistically significant drop in wages relative to workers in other industries. The effects are of significant magnitude as well. For an industry whose Mexican import share went from 10% to 20% (an increase of 10 percentage points, about 1 standard deviation above the mean increase; see table 1), they imply a drop in the cumulative growth of high school dropout wages of 11 percentage points over the decade. For the maximum rise in an industry's Mexican import share, 35 percentage points, the implied drop in cumulative wage growth for a high school dropout is 37.5 percentage points, an enormous deficit for a worker whose wages are already low.

The industry effects in column 1 of table 8 can put to bed the hypothesis that the only thing being picked up in the main regression is general liberalization. The fact that the location effects do not show up in this regression is not terribly important for our purposes, since this regression is not a reasonable way of measuring the effects of NAFTA. The lack of significant coefficients for the location terms could simply mean, for example, that the locations where wages grew the slowest were where local employers were getting hit by Chinese imports the hardest; here, the Mexican import share would be falling. Exploring this possibility, in the second column of table 8, we control for China's import share analogous to Mexico's import share; we see essentially the same coefficients on the industry effects, but the location effects now start to turn negative and significant, suggesting that part of the reason for the absence of location effects in the first column is indeed correlation between movements in the two countries' import shares. Finally, we have experimented with instrumenting for Mexico's import share, with both the change in the Mexico tariffs (column 3) and in the ratio of Mexico's exports to the ROW (excluding the United States) to worldwide exports to the ROW (excluding the United States) (column 4). In both of these IV regressions, the location effects show up as mostly large, negative, and significant.

We conclude that the data do indeed indicate very strong Mexico-specific effects, and we are not simply picking up a global liberalization trend.

China's import shares. Perhaps the prime candidate for a coincident trend that we could be picking up is the rise of imports from China, which Autor et al. (2013) have shown has had a strong effect on U.S. wages. As a final check to be sure that we are not merely picking up a coincident trend, we control explicitly for imports from China.

We add two variables to our basic specification in equation (4): the share of imports for each industry that comes from China and the employment-weighted local average of this share for each conspuma. We interact the first difference of both of these variables with the education class and year 2000 dummies. The summary results are reported in table 9 analogous to table 5.[17]

TABLE 9.—CONTROLLING FOR CHINESE IMPORT SHARE: DIFFERENCES BETWEEN INITIAL TARIFF AND IMPACT EFFECT

Parameter Difference	Point Estimate	F-Value
Location effect		
Less than high school, $\delta_{2,lhs} - \delta_{4,lhs}$	-1.92^{***}	11.74
High school graduate, $\delta_{2,hs} - \delta_{4,hs}$	-1.15^{***}	12.18
Some college, $\delta_{2,scol} - \delta_{4,scol}$	-1.84^{***}	16.88
College graduate, $\delta_{2,col} - \delta_{4,col}$	-1.37^{*}	3.63
Industry effect		
Less than high school, $\theta_{2,lhs} - \theta_{4,lhs}$	-2.38^{***}	30.98
High school graduate, $\theta_{2,hs} - \theta_{4,hs}$	-1.39^{***}	8.57
Some college, $\theta_{2,scol} - \theta_{4,scol}$	-1.38^{***}	7.37
College graduate, $\theta_{2,col} - \theta_{4,col}$	-0.29	0.11

The table reports the overall impact on wages when a location or an industry loses all of its protection within the sample period, using initial tariff and impact effect reported in table A3 of the online appendix (after controlling for changes in the share of Chinese imports). Significant at ***1%, **5%, *10%.

It is clear from table 9 that the results are barely affected in any substantive way by including trade with China. In particular, the more precisely estimated net effects are practically identical with the results in table 5. Trade with China and NAFTA appear to have had quite separate, distinguishable effects.[18]

Conclusion on coincident trends. We have examined the possibility that our regressions on the Mexico tariffs might actually be picking up preexisting trends, coincident multilateral trade liberalization, or the rise of imports from China rather than a NAFTA effect. Each of these possibilities is rejected by the data. The evidence suggests that there is a pure NAFTA effect in addition to other pressures on U.S. wages.

V. Nonwage Effects

To this point, we have focused on the effects of NAFTA on wages. We now ask whether we can see an effect on labor allocation across locations and on labor displacement as measured by claims for Trade Adjustment Assistance.

A. *Migration*

If local labor market effects are indeed strong, as they seem to be from the main regressions, then it is reasonable to ask if this results in some workers moving out of the locations that suffer the most adverse impacts, as Kovak (2013) found in the case of Brazil. We explore that possibility in table 10. In the regression reported there, the dependent variable is the change in the labor force of educational class k, either employed or unemployed, in conspuma c between 1990 and 2000. We regress this on $loc\tau^c_{1990}$ and $loc\triangle\tau^c$ to see

[17] The full results for the variables of interest are included in table A3 of the online appendix.

[18] The coefficients on Chinese import shares generally show that a higher rate of increase in that import share is associated with lower wage growth, whereas a higher rate of increase in the local average of this share is correlated with higher wage growth. Full results are available from the authors on request.

TABLE 10.—LABOR FORCE GROWTH REGRESSIONS, EXCLUDING AGRICULTURE

Dependent Variable: △ in Log Labor Force of	Less Than High School (1)	High School Graduates (2)	Some College Education (3)	College Graduates (4)
Initial tariff, $loc\tau^c_{1990}$	−35.16***	6.21	19.04***	0.90
	(7.873)	(4.664)	(6.099)	(7.601)
Change in tariff, $loc\triangle\tau^c$	−30.65***	5.28	17.66***	−0.88
	(8.472)	(5.046)	(6.439)	(8.182)
R^2	0.06	0.01	0.03	0.01

$N = 543$ conspumas. Robust standard errors in parentheses. Significant at ***1%, **5%, *10%.

if movements in workers are driven to a significant degree by the initial tariff or by realized tariff changes.

Focusing on high school dropouts, the main message is that a conspuma with a high level of protection that lost it by 2000 tended to lose high school dropouts over the 1990s relative to other conspumas (since $-35.16+30.65 = -4.51 < 0$; this difference is significant at the 1% level). This can also be seen in figure A3 of the online appendix, which plots the change in employment shares for each education class against initial local tariff. The figure shows that highly vulnerable conspumas tended to shed high school dropouts over the 1990s. For a highly vulnerable conspuma with an initial local average tariff of 4% that lost all of its protection by 2000, this implies a loss of 18% of high school dropouts over the decade.[19]

These results stand in contrast to findings in Autor et al. (2013, 2014), who find no effect of import competition from China on migration across commuting zones. Part of the reason for the difference may be aggregation, since the only group that we see as moving away from a NAFTA-vulnerable conspuma is the high school dropout group. In analyzing migration, Autor et al. (2013, table 4) separate college-educated workers from non-college-educated workers, and Autor et al. (table V) treat all workers together in one regression. In addition, it is of course possible that the China shock produced a different labor response than the NAFTA shock did.

B. *Trade Adjustment Assistance*

Under the Trade Adjustment Assistance (TAA) program administered by the U.S. Department of Labor, a worker can petition the federal government for assistance in retraining and temporary income assistance in the event that the worker has been laid off due to rising imports or offshoring (Kondo, 2013). Consequently, if NAFTA did displace workers in a given location, that should result in an increase in TAA petitions filed in that location. Although a thorough investigation of the TAA response to NAFTA would be beyond the scope of this paper, we can verify that there is indeed a strong correlation between TAA petitions and NAFTA-driven changes in local tariffs. We have obtained data on the total number of TAA petitions filed between 1994 and 2000 by geographic location and cause, as well as the estimated number of workers affected by these petitions.[20] From the descriptive statistics in table A5 of the online appendix, the average conspuma had filed 25.2 TAA petitions during this period, of which 3.1 specified Mexican competition and 16.6 were successful. The workers represented by these petitions amounted on average to 1.5%, 1.1%, and 0.24% of the conspuma's working-age population in 1990, respectively. The results of a simple regression reported in table A6 of the online appendix confirm that a drop in the local average tariff is associated with an increase in the number of petitions and affected workers covered by petitions. Thus, NAFTA-driven tariff changes are strongly correlated with TAA petitions, which Kondo (2013) has shown to be a useful measure of import shocks more generally.

VI. Conclusion

We have examined the distributional effect of NAFTA using U.S. Census data. Our focus is on the effects of reductions in U.S. tariffs on Mexican products under NAFTA on the wages of U.S. workers.

Limitations on the mobility of workers both geographically and across industries appear to be very important, because we find statistically and economically significant effects of both local employment-weighted average tariffs and industry tariffs on wages. We find that reductions in the local average tariff are associated with substantial reductions in the locality's blue-collar wages, even for workers in the service sector, while a reduction in the tariff of the industry of employment generates additional substantial wage losses. In other words, we have found both a Youngstown effect and "textile" effect or a "footwear" effect. The blue-collar diner worker in the footwear town is hurt by the agreement, as is the blue-collar footwear-factory worker in a town dominated with insurance companies. Worst hit of all is the blue-collar footwear worker in a footwear town, particularly

[19] These effects are not merely the result of high school dropouts leaving the labor force. Repeating the regression with the log change in working-age population for each educational class instead of the labor force provides similar effects (online appendix table A4). The effect on high school dropouts is −2.46, smaller than −4.5 but also significant at the 1% level.

[20] The data at the congressional district level are from the Department of Labor Trade Adjustment Assistance Consolidated Petitions Database prepared by Public Citizen (www.citizen.org). We used a concordance between congressional districts and Public-Use Microdata Areas (PUMAs) obtained from the Census Bureau to map TAA petitions into conspumas.

if that worker never finished high school. College-educated workers skate away mainly unharmed.

Perhaps the main finding is that the distributional effects of NAFTA are large for a highly affected minority of workers. Whether we define highly affected industries as industries that had been protected by a high tariff against Mexican imports or as industries whose Mexican share of imports rose quickly, the result is the same: Blue-collar workers in highly affected industries saw substantially lower wage growth than workers in other industries. Since studies of aggregate welfare effects of NAFTA such as Romalis (2007) and Caliendo and Parro (2015) find at most very small aggregate U.S. welfare gains from NAFTA (the most optimistic estimate is 0.2% in Caliendo & Parro, 2015), these distributional effects suggest strongly that blue-collar workers in vulnerable industries suffered large absolute declines in real wages as a result of the agreement.[21] This case study provides another example of the observation made by Rodrik (1994) that trade policy tends to be characterized by large redistributional effects and modest aggregate welfare effects, and hence emphasizes once again the importance of identifying the effects of trade on income distribution (see Harrison, McLaren, & McMillan, 2011, for a recent survey).

[21] However, it should be pointed out that measuring a change in real wages at a given date is not the same as measuring welfare changes; for that, one needs a structural model, for example, Artuç et al. (2010).

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