

## Endogenous Skill Acquisition and Export Manufacturing in Mexico<sup>†</sup>

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*This paper presents empirical evidence that the growth of export manufacturing in Mexico during a period of major trade reforms (the years 1986 to 2000) altered the distribution of education. I use variation in the timing of factory openings across commuting zones to show that school drop-out increased with local expansions in export-manufacturing industries. The magnitudes I find suggest that for every 25 jobs created, one student dropped out of school at grade 9 rather than continuing through to grade 12. These effects are driven by less-skilled export-manufacturing jobs which raised the opportunity cost of schooling for students at the margin. (JEL F14, F16, J24, L60, O14, O19)*

Many developing countries have experienced rapid periods of industrialization driven by expansions in low-skill manufacturing exports. The existing trade literature has found that exporting firms pay higher wages (Bernard and Jensen 1995; see Bernard 1995 for Mexico) and that export expansions are often associated with rises in the returns to skill (surveyed in Goldberg and Pavcnik 2007; see Cragg and Epelbaum 1996, Hanson and Harrison 1999, and Verhoogen 2008 for Mexico). From these two stylized facts, it is tempting to conclude that schooling will rise with the arrival of new exporting opportunities. However, such an inference ignores the fact that new exports jobs have the potential to significantly raise the opportunity cost of schooling. If the rise in the opportunity cost of schooling outweighs any rise in the return to schooling, some youths will drop out of school at younger ages. This paper exploits variation in the timing of factory openings across commuting zones to show that this is indeed what occurred in Mexico between 1986 and 2000.

The finding that export expansions can reduce school attainment has important ramifications. From a macro perspective, many countries pursuing export-led

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growth strategies also want to upgrade the skill level of their workforce, believing that the positive externalities from education drive long-run growth rates (Lucas 1988). Therefore, understanding the particular job characteristics that raise or lower educational acquisition is vital for designing industrial and trade policies that can increase short-run growth rates without reducing education levels. In the last part of the paper, I address this question by exploring how heterogeneity in educational responses relates to industry and location characteristics of the new job arrivals.

New export employment opportunities have two offsetting effects. On the one hand, when a new firm opens, a student may drop out of school in order to take one of the abundant job openings at the time of the factory opening—the opportunity cost of schooling channel. On the other hand, if the student expects that vacancies will continue to be available and these jobs will sufficiently reward school acquisition, he or she may choose to stay in school longer—the return to schooling channel. Which effect dominates during periods of export-oriented industrialization is an empirical question.

Mexico provides a perfect setting to study the impacts of globalization on the labor force. Over the period spanned by the data (1986–2000), Mexico turned its back on an import substitution strategy and liberalized trade, joining the General Agreement on Tariffs and Trade (GATT) in 1986 and the North American Free Trade Agreement (NAFTA) in 1994. During these years, many new plants opened, often in the form of maquiladoras, to manufacture products for export. Total employment in export manufacturing sectors rose from under 900,000 formal sector jobs at the beginning of 1986 to over 2.7 million jobs in 2000. The majority of these jobs were low skill, with more than 80 percent of employees in the year 2000 possessing less than a high school degree.<sup>1</sup>

A unique dataset makes this analysis possible. I match cohort average education (my skill measure, calculated using 10 million schooling records from the 2000 census) to export-industry job growth in the cohort's commuting zone in the year the cohort turned age 16 (calculated using annual firm-level employment data from social security rolls covering the universe of formal sector firms).<sup>2</sup> At this “key exposure age,” compulsory education concludes and formal employment is first possible. I can then compare the school attainment of cohorts within a commuting zone who reached their key exposure age at the time of substantial factory openings to slightly younger or older cohorts who did not.

The primary empirical difficulty is reverse causation; that local skill levels may themselves determine firm employment decisions. In the context of my panel of 1,808 commuting zones and 14 cohorts, the exogeneity requirement is that, conditional on commuting-zone fixed effects, linear trends, and state-cohort fixed effects, firm employment decisions do not respond to deviations in the schooling of individual cohorts. I instrument employment changes with changes attributable solely to large single-firm openings, closings, expansions, and contractions. I argue that

<sup>1</sup>This period of Mexican reforms has been associated with an initial rise in the skill premium until the mid-1990s (Cragg and Epelbaum 1996; Hanson and Harrison 1999), followed by a skill premium decline thereafter (Robertson 2004; Airola and Juhn 2008). As I show in Section IVA, education decisions respond to the skill premium (the wage difference between employees of different skill levels) and the opportunity cost of school.

<sup>2</sup>I restrict attention to the nonmigrant population of Mexico since the location of migrants at age 16 is unknown. In Section III.E, I show that composition bias due to selective migration cannot explain my findings.

sizable expansions and contractions are associated with large fixed costs and not driven by changes in the labor supply of one or even several cohorts of youths.<sup>3</sup>

I find that the cohorts who reached their key exposure age during years of substantial expansions in export-industry employment in their commuting zone obtained relatively fewer years of school compared to less exposed cohorts. In terms of interpretation, this finding is not driven by new export manufacturing opportunities raising the education of all cohorts in the commuting zone but raising education least among cohorts at the key exposure age. (The change in school attendance of 16-year-olds between 1990 and 2000 was smallest in the commuting zones with the largest export-industry employment growth.) The magnitudes I find suggest that for every 25 new jobs that arrived, one student dropped out of school at grade 9 rather than continuing on through grade 12.

I present multiple pieces of additional evidence to support my claim that export-industry expansions reduced schooling by raising the opportunity cost of school: compared to other ages, the reduction in schooling is largest for jobs arriving at age 16 and dissipates entirely at older ages; I find similar patterns for grade-9 drop-out rates but not primary school drop-out where earlier exposure matters more; school attendance at the time of the 1990 census responds most to job shocks occurring in the previous year rather than earlier or later years, and the cohort aged 16 is most impacted; this drop in school attendance is matched by increases in the propensity to work in the export sector but not in other sectors; sex-specific school attainment responds more strongly to job shocks for that particular sex; the effects are not driven by parental work decisions or selective migration; and the returns to on-the-job training do not negate the reduction in formal schooling in wage terms.

The previous discussion focused on the schooling impacts of export manufacturing jobs. There are several reasons for this focus. First, the impact of trade on schooling decisions is of significant interest in its own right. Second, from a policy perspective, export manufacturing plays a special role in developing countries. While policymakers must often decide whether to encourage or restrict export manufacturing, especially foreign direct investment (FDI), such scenarios are rarer for services or nonexport manufacturing. This comes in part from the additional policy levers available for export manufacturing (for example, Mexico's maquiladora system that exempts exporting firms from tariffs on imported inputs). In contrast to their success in encouraging export manufacturing, Mexico and many other developing countries have struggled to generate employment growth in large-scale nonexport manufacturing. Meanwhile, services are generally nontradable and so location decisions are often tied to local demand.

In the last part of the analysis, I explore job creation across all sectors. While the arrival of formal jobs in nonexport sectors at age 16 is also associated with reduced schooling, the effects size is significantly smaller than for export sectors. The effect disappears altogether when I restrict attention to highly agglomerated industries where local demand shocks are less likely to bias estimates due to endogeneity. In order to understand the job characteristics generating this difference, I write down a conceptual framework that incorporates stochastic job opportunities

<sup>3</sup>This is especially true in Mexico, where a large quantity of migrant and informal labor ensures that changes in the drop-out decisions of individual cohorts have only a small impact on the set of potential hires.

and heterogeneous discount rates into an educational choice model. This framework highlights the particular job characteristics that determine whether job arrivals encourage or discourage educational acquisition. Drawing on schooling, wage, and industry of employment data from the 1990 census to characterize job arrivals, I find that drop-out is driven by job arrivals that require only a secondary school education, offer relatively high wage premia, and arrive in locations where there are many youths on the margin between secondary and high school. Once these job characteristics are accounted for, new export-sector jobs no longer generate statistically larger reductions in schooling than nonexport ones.

This paper provides evidence in support of models of trade with endogenous skill acquisition. Findlay and Kierzkowski (1983) endogenize human capital in a Heckscher-Ohlin model and show that trade exacerbates initial skill differences across countries by raising the return to the abundant skill—the Stolper-Samuelson effect. Trade can induce divergent growth paths if positive externalities to education are incorporated into such a model (Stokey 1991). Wood and Ridao-Cano (1999) test the hypothesis that trade reduces educational acquisition in unskilled-labor-abundant countries using a cross-country panel. However, it is difficult to infer causality in cross-country regressions, particularly when changes in education levels feed back into empirical measures of trade openness such as the ratio of exports to gross domestic product (GDP).

The results are also consistent with the findings of studies in history and development. Goldin and Katz (1997) show that industrialization slowed the growth of high school education in the early twentieth century United States, while Federman and Levine (2005) find industrialization increased enrollments in Indonesia. Closest to this paper, Le Brun, Helper, and Levine (2011) find industrialization had mixed effects in Mexico by looking at decadal changes in school attendance and manufacturing employment in the census. This paper improves on these studies by drawing on rich employment data at an annual frequency that both allows me to design an instrumental variables strategy that controls for potential reverse causality due to endogenous firm location choices and to explore heterogeneous effects by job type.<sup>4</sup>

Finally, a complementary literature looks at the educational impacts of the arrival of information technology (IT) service jobs in India. Munshi and Rosenzweig (2006), Shastry (2012), Jensen (2012), and Oster and Steinberg (2013) all find positive enrollment impacts from the arrival of relatively high-skilled service job opportunities in India.<sup>5</sup> All these studies explore new opportunities in a very specific sector in a small sample of locations. As these particular opportunities demanded relatively high skills compared to the local skill distribution, they substantially raised the return to schooling.<sup>6</sup> By drawing on disaggregated employment data across many industries and locations, this paper contributes to this literature by identifying the job characteristics that raise educational attainment and those that lower it.

<sup>4</sup>This analysis focuses on youths at school-leaving ages at the time of the export-industry job arrivals. For evidence on positive schooling effects of trade liberalization for younger children via the household income channel, see Edmonds and Pavcnik (2005) and Edmonds, Pavcnik, and Topalova (2010).

<sup>5</sup>Heath and Mobarak (2015) find similar outcomes for young Bangladeshi girls and the garment industry.

<sup>6</sup>India's experience may be regarded as the exception rather than the rule, as it is far more common for a developing country to have a revealed comparative advantage in low-skill manufacturing.

Section I introduces the rich dataset and the empirical methodology. Section II investigates the impact of export-industry job arrivals on educational attainment. Section III validates the methodology through a variety of additional exercises. Section IV explores why export-industry job creation leads to particularly pronounced reductions in schooling through the lens of a simple model. Finally, Section V discusses policy implications and concludes.

## I. Empirical Strategy

### A. Data

I combine two sources of data to explore the relationship between educational attainment and job opportunities in export manufacturing. Cohort education data come from a 10.6 percent subsample of the 2000 Mexican census collected by INEGI and available from IPUMSI (Minnesota Population Center 2007). The 10.1 million person records cover all 2,443 Mexican municipios (roughly equivalent to US counties). For reasons discussed in Section IC, I exclude Mexico City in my primary analysis.

The employment data originate from the Mexican Social Security Institute (IMSS), and cover the universe of formal private-sector establishments, including maquiladoras. IMSS provides health and pension coverage and all employees must enroll. I construct the main employment variable, net new jobs, from annual changes in employment by industry within each municipio.<sup>7</sup> The data cover 2.2 million firms between 1985 and 2000, with employment recorded on December 31 of each year. Table 1 reports sample means for both datasets.

For my primary analysis, I focus on the massive expansion of employment in export-oriented industries that dominated Mexico's manufacturing growth over the period of study. The IMSS data assign each firm to one of 276 industry categories, but do not indicate whether a firm exports. Thus, I define a firm as an exporter if it belongs to a three-digit International Standard Industrial Classification (ISIC) industry where more than 50 percent of output was exported for at least one-half of the sample years.<sup>8</sup> The resulting export industries are: Apparel; Footwear; Leather and Leather Products; Wood and Cork Products; Petrochemical Refinement; Metal Products; Electronic and Mechanical Machinery; Electrical Machinery; Transport Equipment; Scientific and Optical Equipment.<sup>9</sup>

Between 1986 and 1999, employment growth in these export-intensive industries accounted for 65 percent of the growth in IMSS-insured manufacturing employment. Figure 1 displays the annual employment growth in both export and nonexport manufacturing industries as well as in nonmanufacturing industries. While not all of the jobs in the industries that I classify as export manufacturing are in firms that

<sup>7</sup>The aggregations from the firm to municipio level were carried out at Instituto Tecnológico Autónomo de México (ITAM), where the data were held securely. Kaplan, González, and Robertson (2007) contains further details on the IMSS data.

<sup>8</sup>The 276 IMSS industry categories, the 119 used by the Mexican census, and the 72 used by ISIC (Rev. 2) were matched by hand. Export and output data come from the Trade, Production, and Protection 1976–2004 database (Nicita and Olarreaga 2007). Results are robust to raising or lowering the 50 percent cutoff.

<sup>9</sup>Online Appendix Figure C.1 provides further details regarding firm export orientation by industry grouping.

TABLE 1—SAMPLE MEANS

	Mean	Standard deviation	Observations
<i>Census sample (2000, age 16–28, nonmigrants, excluding Mexico City)</i>			
Age	21.54	0.0038	1,706,582
Years of school	8.51	0.0038	1,636,520
Employed (1 = yes, 0 = no)	0.52		1,706,582
Insured by IMSS (1 = yes, 0 = no)	0.42		1,706,582
Monthly log earned income (pesos)	7.47	0.0011	667,103
Sex (1 = male, 0 = female)	0.48		1,706,582
Commuting zone (CZ) size	8,540.26	816.7	1,808
Nonmigrants as proportion of full sample	0.81		2,060,457
<i>IMSS annual firm sample (1985–2000)</i>			
	Mean	Standard deviation	Observations
Firm size (employees)	12.08	0.044	11,365,321
Firm size (firms changing employment)	16.03	0.065	7,675,094
Firm size (firms hiring/firing $\geq 50$ in single year)	416.41	4.140	109,263
Worker sex (1 = male, 0 = female)	0.68		11,365,321
	Total in 1985	Total in 2000	Unique 1985–2000
Employees	4,472,491	12,509,298	
Firms	372,520	912,284	2,194,681
<i>Combined sample (1986–1999, weighted by cell population)</i>			
	Mean	Standard deviation	Observations
Jobs/working age population (all sectors)	0.188761	0.155967	25,289
Export jobs/working age population	0.034833	0.057492	25,289
Net new jobs/working age population (all sectors)	0.012288	0.020343	25,289
Net new export jobs/working age population	0.003069	0.007928	25,289
Net new jobs/worker (all sectors) (large $\Delta$ s)	0.006189	0.014048	25,289
Net new export jobs/worker (large $\Delta$ s)	0.002405	0.007072	25,289
Positive net new export jobs/worker (large $\Delta$ s)	0.006676	0.008846	1,446
Negative net new export jobs/worker (large $\Delta$ s)	–0.002354	0.006166	509
Nonzero net new export jobs/worker (large $\Delta$ s)	0.004448	0.009136	1,955
Net new export jobs/worker (large $\Delta$ s) (demeaned by CZ)	0.000000	0.005495	25,289

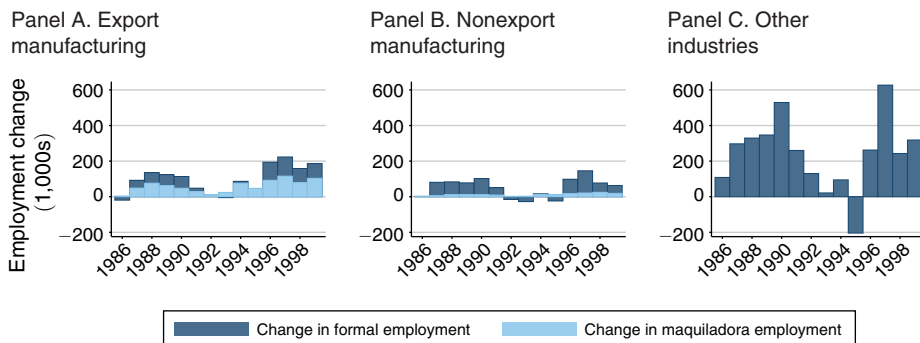


FIGURE 1. MANUFACTURING EMPLOYMENT CHANGES

Note: Formal employment changes calculated using IMSS employment data, maquiladora employment changes using INEGI maquiladora statistics.



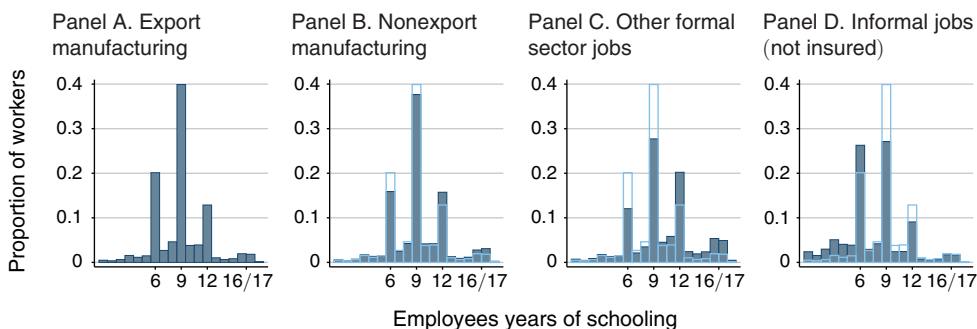


FIGURE 2. HISTOGRAM OF EDUCATION BY SECTOR (Age 16–28 in 2000, IMSS Insured)

Notes: Solid bars show the education distribution calculated using the 2000 census for formal sector workers ages 16 to 28 (my sample cohort). A formal worker is defined as a worker insured by IMSS or equivalent insurance scheme. Panels B–D overlay the export manufacturing education distribution from panel A as hollow bars for comparison purposes.

export, the majority are. In 2000, there were 2 million formal jobs in my export manufacturing grouping. One million of these jobs were in maquiladora firms according to INEGI maquiladora statistics. (Maquiladora job growth accounts for 60 percent of export-industry job growth as shown in Figure 1.) All of these maquiladoras are exporters since these export-assembly plants are legally required to export almost all their production.<sup>10</sup> A large number of the remaining 1 million export industry jobs are also in exporting firms. For example, the 2000 Encuesta Industrial Anual (EIA) surveys 5,801 large non-maquiladora firms. Of the 370,340 EIA jobs in my export industries, 51 percent are at firms that export more than 25 percent of their output.<sup>11</sup> Thus, in subsequent sections I refer to jobs in these export intensive sectors simply as “export jobs.” Section IIA uses these additional data sources to explore job creation at known exporters.

Figure 2 shows the education distribution of young workers—those aged 16–28, my sample cohorts—in each industry at the time of the 2000 census. Formal sector employees in export manufacturing industries are substantially less educated than formal sector workers in other industries; 81 percent of export-industry employees have less than a high school education compared to 75 percent of nonexport manufacturing employees and 62 percent of employees in other formal sectors. (Informal jobs are the least skilled with 85 percent of employees having less than high school.) Employees in export-manufacturing industries are also younger with 18 percent of employees age 18 or under in the year 2000 as opposed to 13 percent for nonexport manufacturing industries and 12 percent for other formal sector jobs (see online Appendix Figure C.2).

I combine the education and employment data using the 1985 municipio boundaries. In order for each location to represent a single labor market, I create commuting zones by combining municipios in the same Zona Metropolitana (as classified by INEGI) or where a significant number of commuters moved between them in the

<sup>10</sup>These firms were initially confined to border areas and employed mainly women, but by the year 2000 one-quarter of firms were in nonborder states and one-half of the employees were male.

<sup>11</sup>Only 20 percent of the 623,020 nonexport industry jobs are at firms exporting more than 25 percent.

2000 census.<sup>12</sup> The end result is a panel of 14 cohorts across 1,808 geographic units which I refer to as commuting zones.<sup>13</sup>

### *B. Schooling Decisions and Export Employment Shocks at Age 16*

Regressing school attainment on levels of export employment in the cross section is likely to provide biased estimates of the effect of export booms on schooling. If factories were drawn to the educated north of Mexico due to its proximity to the United States, there would be a positive correlation between schooling and export employment. If factories were drawn to poorer locations due to low wages or government incentives, there would be a negative one. Rather than relying on this cross-sectional variation, my identification strategy exploits differences in exposure to export employment shocks at age 16 across cohorts within the same commuting zone. In this section, I justify why shocks to job opportunities at age 16 are likely to have particularly pronounced effects on educational choices.

My argument proceeds in two steps. First, I will argue that, conditioning on education, the returns to entering the labor force will vary by year-of-entry and depend on the net new job creation in that year. Second, I will argue that this heterogeneity in returns will disproportionately affect the educational decisions of the cohort aged 16 in that year.

Forward-looking students trade off the foregone earnings from staying at school this period (the opportunity cost of schooling) with the future wage benefits from more education (the returns to schooling). There are a variety of models that generate the prediction that this trade-off depends, at least in part, on net new job creation that period (as opposed to the total stock of jobs in the location). In Section IVA, I present (and motivate) one such model where youths are heterogeneous in their discount rates, and formal firms pay noncompensating wage differentials and ration jobs. A youth is more likely to drop out of school in a year when many formal firms are hiring workers of their education level since they are more likely to obtain a job in a firm that pays persistently higher wages. Conversely, they are more likely to stay on at school if the new jobs are high skilled. In a pure matching model, youths will search for job opportunities each period and stay in school if there is no match. Hence, drop-out is more likely in periods of employment growth. Another possibility is that within-firm wage premia depend positively on labor demand conditions in the year of entry due to optimal lifetime contracts for risk-averse credit-constrained workers (Beaudry and DiNardo 1991). In each of these models, new job arrivals alter that year's schooling decisions by raising the opportunity cost of schooling as well as potentially changing the returns to schooling. The empirical specification is designed to uncover the net effect of these two forces.

At what age would we expect these shocks to the opportunity costs of schooling to be most pronounced? My main specification focuses on job arrivals in the year

<sup>12</sup>I classify commuting municipios as those where more than 10 percent of the working population reported commuting to a nearby municipio. In the few cases where a municipio sends workers to two municipios that do not send workers to each other, I create two synthetic municipios both containing the sending municipio (but with the weights of individuals from the sending municipio halved).

<sup>13</sup>Since the census was collected in February 2000, only firm data through 1999 are relevant. I lose one additional year of data when calculating employment changes, leaving 14 years of data.



the youth turned 16. I dub this the “key exposure age” for two reasons. First, formal sector factory jobs first become a direct alternative to school at this age as the legal minimum age for factory work is 16.<sup>14</sup> Younger cohorts cannot actually obtain these jobs and older cohorts would have been exposed to positive shocks in previous years. Therefore, there is a discrete jump at age 16 in the value of a factory job opportunity. Second, the density of youths on the margin between staying at school and dropping out is largest around this age. Compulsory schooling in Mexico ends with *Secundaria* (grade 9). Most children complete this grade at age 15 or 16. Although the compulsory schooling law only dates from 1992 and enforcement is rare, many youths drop out after this stage and a similar number enroll in high school but never complete tenth grade. Accordingly, age 16 is the most common age to leave school in Mexico and so shocks to the opportunity cost of schooling at this age will induce a particularly large number of youths to alter their education decisions.

Figures 2 and 3 provide empirical support for the claim that 16 is the key exposure age. Figure 2 shows that the modal level of completed schooling among young workers in the 2000 census is ninth grade (29 percent of workers compared to 21 and 13 percent for grades 6 and 12, respectively). Figure 3 draws on school attendance data from the 1990 census that lies in the middle of my sample period. The solid line shows that the largest change in the proportion of the cohort attending school occurs between ages 15 and 16 (and the dash-dot line shows the converse for the proportion of the cohort that is working). The primacy of age 16 is more pronounced looking at the dashed line that plots the change in the proportion of students in each cohort maintaining their correct grade for age.<sup>15</sup> There is a dramatic drop at age 16, with substantially fewer 16-year-olds having completed tenth grade than 15-year-olds having completed ninth grade. This drop dwarfs the changes at any other age. The dash-dot plot shows that grade completion rates conditional on attendance also plummet at age 16.<sup>16</sup>

In conclusion, I expect new export opportunities to have a particularly pronounced effect on the educational decisions of cohorts aged 16 at the time compared to younger or older cohorts. Section IIIA confirms this conjecture by repeating my analysis for other exposure ages.

### C. Empirical Specification

In order to determine the impact of new export job opportunities on cohort schooling, I regress cohort schooling on local expansions in export manufacturing employment,

$$(1) \quad S_{zc} = \beta l_{zc} + \delta_z + \delta_z c + \delta_{rc} + \varepsilon_{zc}.$$

<sup>14</sup>The minimum working age was 14 at the time. However, children under 16 years old require parental consent, medical documentation, cannot work overtime or late hours, and are forbidden from certain hazardous industries. These rules are enforced in formal manufacturing and the minimum working age is typically taken as 16.

<sup>15</sup>Since schooling starts at 6 years old, the correct grade is simply their age minus 6 years. Some youths would have only obtained grade  $x - 7$  if they progressed sequentially since age is recorded in February. However, this measurement error can not explain the discrete jump at age 16.

<sup>16</sup>I estimate grade completion rates by dividing the proportion of youths age  $x$  who have completed grade  $x - 6$  by the proportion of youths age  $x - 1$  who are both at school and have completed grade  $x - 7$ .

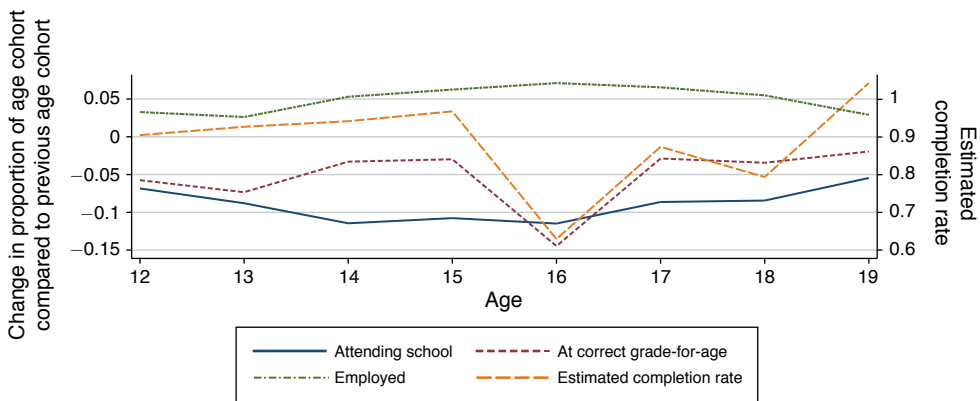


FIGURE 3. CHANGES IN SCHOOL ATTENDANCE, GRADE COMPLETION, AND EMPLOYMENT BY AGE (1990 Census)

Notes: Figure uses 1990 census to plot changes in the proportion of the cohort attending school (solid line), at the correct grade-for-age (dashed line), and in full-time employment (dash-dotted line), all compared to the age cohort one year younger. Correct grade-for-age measured as proportion of cohort at grade  $age - 6$ . Long-dashed line shows the estimated completion rate (the proportion of cohort at the correct grade-for-age divided by proportion of cohort one year younger both at the correct grade-for-age and attending school).

$S_{zc}$  is the average years of schooling obtained by February 2000 for the cohort born in year  $c$  in commuting-zone  $z$ ,<sup>17</sup> and  $l_{zc}$  is a measure of export employment shocks at age 16 that I describe below. I also include commuting-zone fixed effects,  $\delta_z$ , commuting-zone-specific time trends,  $\delta_{zc}$ , and state-time dummies,  $\delta_{rc}$ , where  $r$  indexes the state. (Time and cohort trends are equivalent since the schooling of each cohort is observed only once in the year 2000.)

My export employment shock measure is the year-on-year employment growth at formal manufacturing firms in export-oriented industries located in commuting zone  $z$  in the year the cohort turned age 16. Since a new factory hiring 100 workers will have a much more muted effect on local labor market conditions in a large city compared to a small rural municipio, I divide this employment change by the population aged 15–49 to generate net new export-industry jobs per working-age person, henceforth abbreviated to “net new export jobs per worker”.<sup>18</sup>

$$(2) \quad l_{zc} = \frac{\text{export employment}_{z,c+16} - \text{export employment}_{z,c+15}}{\text{working-age population}_{z,1990}}$$

As the working-age population may be endogenous to new factory openings, I use the commuting-zone population aged 15–49 from the 1990 census close to the beginning of the sample period. In order to get a sense of magnitudes,  $l_{zc}$  ranges between  $-0.17$  and  $0.19$ . A large expansion (the ninetieth percentile among the 7,800 expansions or contractions) created 0.008 jobs per working-age person or

<sup>17</sup>I do not use data from 1990 census in calculating  $S_{zc}$  as these data only cover four cohorts. Additionally, the sampling methodology for selecting who received the long-form surveys changed between 1990 and 2000.

<sup>18</sup>If the job shocks were not scaled by population in this manner, we would expect heterogeneous treatment effects across locations that would not be captured by the additive commuting-zone fixed effects.

0.16 jobs per member of the cohort aged 16 that year. Table 1 reports a range of additional summary statistics for these shocks.

The state-time dummies control in a flexible manner for the fact that education was trending upward, but at different rates across Mexico.<sup>19</sup> The commuting-zone-specific fixed effects and time trends control for the fact that educational outcomes vary across locations within a state, and low-education commuting zones may be catching up with high-education ones.

I restrict the sample to nonmigrants, defined as someone who reports being born in the same state they are currently living in and who also lived in their current commuting zone in 1995. Including in-migrants confounds the impact of local job opportunities on education since the census does not ask where they lived when they were at school. Therefore, my estimates are only representative of the nonmigrants who comprise 80 percent of the full census sample. In Section III E, I provide evidence that potential selection biases related to migration cannot explain my finding that export job arrivals reduced schooling.

I exclude Valle de México (the commuting zone that includes Mexico City) since it constitutes two entire states so is swept out by the state-time dummies.<sup>20</sup> I weight each cohort-commuting-zone observation by the number of individuals the cell represents. Hence, my results are representative of the Mexican nonmigrant population excluding Valle de México.

My empirical strategy compares the average schooling of a cohort who was heavily exposed to local factory openings in export-oriented industries at their key exposure age to older and younger cohorts in the same commuting zone who did not receive such a shock to their employment opportunities at this age. I flexibly control for time trends using cohorts of the same age living in nearby commuting zones where factories did not open at the key exposure age. I now turn to discussing the potential threats to identification and present a novel instrumentation strategy.

#### *D. Threats to Identification and Instrumentation Strategy*

I address three econometric concerns: omitted variables, reverse causality, and measurement error. Omitted variables will bias coefficients if a third factor affects both a commuting zone's education level and its attractiveness as a location for a firm. The commuting-zone fixed effects sweep out time-invariant features of the commuting zone. The state-time dummies control for omitted variables that change over time within the 32 states of Mexico. Finally, commuting-zone-level time trends control for omitted variables that change over time within a commuting zone in an approximately linear fashion.

There are two obvious omitted variables that may affect schooling and correlate with detrended local employment changes. First, a factory may agree to make complementary investments when it opens, for example, building a school. Unfortunately, there are no annual data at the municipio level from which to construct controls. Therefore, I rely on the fact that such investments affect all cohorts,

<sup>19</sup>The state-time dummies also remove trends that arise because younger cohorts have had less time to complete their education, and the degree of measurement error for younger cohorts may vary by state.

<sup>20</sup>As a robustness check, I replace state-time dummies with region-time dummies and include Mexico City.

with younger cohorts exposed for more years and likely to see larger effects (the opposite to what I find). Additionally, Helper, Levine, and Woodruff (2006) report that school building decisions in Mexico were made nationally prior to 1992 and at the state level afterward, with little municipio say in either period. Second, there may be local demand shocks that both affect schooling decisions and alter the demand for local manufacturing output. My focus on export industries mitigates this concern as demand comes from foreign rather than local consumers.<sup>21</sup>

The second econometric concern is reverse causality. The local education distribution determines the relative wages of different skill groups, and relative wages affect firm employment and location decisions.<sup>22</sup> If new factories lower education, and low schooling levels also attract factories,  $\hat{\beta}$  will be biased in an ambiguous direction. In my panel setting, bias occurs if deviations in detrended cohort schooling (i.e., after accounting for commuting-zone fixed effects and linear trends, and state-time fixed effects) affect past, present, or future firm employment decisions. Therefore, while a firm may wish to locate in a low-skill location, or in a location where skills are declining over time, one or several cohorts with an unusually strong aversion to schooling must not influence a firm's decision to open in a location.

To deal with reverse causality, I require an instrument for  $l_{zc}$ , the net new export jobs per worker defined in equation (2) above. My instrument is the net new export jobs per worker generated by large single-firm expansions/openings and contractions/closings—positive or negative changes of 50 or more employees in a single year at a single firm. As these large single firm changes comprise 79 percent of the total change in employment over the period, the instrument correlates strongly with  $l_{zc}$ . For the instrument to be exogenous to the error term in equation (1), firms can only respond to deviations in cohort schooling—those not accounted for by the commuting-zone fixed effects and linear trends, and the state-time fixed effects—through the small expansions and contractions that are excluded from my instrument.

I argue that this exclusion restriction is plausibly satisfied. The large (and hence costly) expansions and contractions in the export industries I focus on are typically driven by external demand shocks interacted with stable commuting-zone characteristics (distance to US border, existing input suppliers, etc.),<sup>23</sup> not by changes in local labor supply. Even in cases where changes in labor supply do drive firm location decisions, deviations in the school attainment of 16-year-olds will play a negligible role for two reasons. First, a cohort of 16-year-olds is a very small component of the local skill distribution in Mexico—where a large number of both informal and migrant workers compete for formal sector jobs<sup>24</sup>—and so total labor supply will be little affected by small deviations in local drop-out rates. Second, in order to base location decisions on these deviations, entrepreneurs must obtain cohort-varying information about education levels in a commuting zone, which is not readily available.

<sup>21</sup> In online Appendix D, I further focus only on industries where production is geographically agglomerated and hence job creation is driven by national rather than local demand factors.

<sup>22</sup> Bernard, Robertson, and Schott (2010) show that factor prices are not equalized across Mexico, resulting in an inverse relationship between relative wages and relative skill levels. In the extreme, if there is no informal sector, unemployment or migration, one additional drop-out results in one new formal employee.

<sup>23</sup> Very large firms may expand or contract by more than 50 people even absent an external demand shock. For robustness, I restrict attention to expansions and contractions that are unusually large for a given firm.

<sup>24</sup> Only one-third of Mexican working-age adults are in formal private sector employment.

For the reasons above, large single-firm expansions and contractions are unlikely to be influenced by deviations in the schooling of the cohort aged 16 at the time. However, multiple years of serially correlated schooling shocks, for example due to a school closure, could both have nonnegligible effects on total labor supply and be observable to entrepreneurs. Three factors limit the size of the bias in this case. First, any correlation between past schooling deviations (the deviations that are plausibly observable to the entrepreneur) and current location decisions will be divided by the number of cohorts in my panel (14). Second, older cohorts have progressively smaller impacts on the pool of local labor a firm can hire as many will no longer be seeking employment. Third, any persistent trends in schooling will be absorbed by the commuting zone linear time trend.

In Section IIIC, I provide explicit evidence in support of the instrumental variable strategy. I show that large expansions and contractions in the late 1980s correlate with education decisions recorded in 1990, but large expansions and contractions in the early 1990s do not.

An additional specification addresses reverse causation head on. I explicitly control for the schooling levels of previous cohorts by including four lags of  $S_{zc}$ . These lags soak up the component of the error term correlated with  $l_{zc}$  through the serial correlation in schooling.<sup>25,26</sup>

The third econometric concern is measurement error in  $l_{zc}$ . IMSS registration defines firm formality. However, some firms existed informally prior to registering with IMSS, thus formalization appears as new job creation. Such measurement error attenuates  $\hat{\beta}$  and could also bias my results if an omitted variable both encouraged firms to register and affected education choices. The IV strategy above mitigates this concern since large firm expansions and contractions occur in larger firms that would find it difficult to evade IMSS registration.

Finally, I cluster all standard errors at the commuting zone level to prevent misleading inference due to serial correlation in the error term across years within a commuting zone (Bertrand, Duflo, and Mullainathan 2004). The large number of groups (1,808 commuting zones) mitigates concerns regarding spurious correlation.

## II. Basic Results

Table 2 presents the results from running the regression specification in equation (1). Column 1 contains the ordinary least squares (OLS) results. Column 2 contains the instrumental variable (IV) results, in which I instrument net new export jobs per worker with net new export jobs per worker attributable to changes of 50 or more employees in a single firm in a single year. As expected, the first stage of the IV is highly significant. Column 3 contains the reduced-form (RF) results from regressing cohort schooling directly on the instrument. Column 4 repeats

<sup>25</sup>To be more precise, imagine the true data-generating process is  $S_{zc} = \beta l_{zc} + u_{zc}$ , where shocks to average cohort schooling may be positively serially correlated:  $u_{zc} = \rho u_{zc-1} + v_{zc}$  with  $0 < \rho < 1$ . Firms locate where there is a high proportion of drop-outs in the previous cohort of 16-year-olds:  $l_{zc} = \pi S_{zc-1} + \epsilon_{zc}$ ,  $\pi < 0$ . If I run the regression  $S_{zc} = \beta l_{zc} + \epsilon_{zc}$ ,  $\hat{\beta}$  will be negatively biased. Running  $S_{zc} = \beta l_{zc} + \gamma S_{zc-1} + \epsilon_{zc}$  results in unbiased estimates of  $\beta$ . If factory openings are also serially correlated,  $\hat{\beta}$  will be attenuated toward zero.

<sup>26</sup>As lagged dependent variables are necessarily correlated with the error term in a panel regression, I exclude the commuting-zone fixed effects and trend.

TABLE 2—THE EFFECT OF NEW EXPORT-MANUFACTURING JOBS ON EDUCATIONAL ATTAINMENT

	Cohort average completed years of schooling				
	OLS (1)	IV (large $\Delta$ s) (2)	RF (large $\Delta$ s) (3)	RF (large $\Delta$ s) (4)	RF and LDV (5)
Net new export manufacturing jobs/worker at age 16	-3.155 (0.638)	-3.192 (0.614)	-3.306 (0.690)	-3.246 (0.684)	-3.380 (0.551)
Net new jobs/worker at age 16 (all other sectors)				-1.169 (0.514)	
L.Cohort schooling					0.309 (0.0164)
L2.Cohort schooling					0.274 (0.0132)
L3.Cohort schooling					0.171 (0.00946)
L4.Cohort schooling					0.132 (0.0105)
Observations	25,289	25,289	25,289	25,289	25,192
$R^2$	0.942	0.942	0.942	0.942	0.915
Commuting zones	1,808	1,808	1,808	1,808	1,808
Kleibergen-Paap $F$ -stat (first stage)		11,121			

*Notes:* Dependent variable is cohort average years of schooling in the year 2000. Independent variable is net new export-manufacturing jobs per worker arriving in cohort's commuting zone at age 16. The IV column instruments net new jobs per worker by the net new jobs per worker attributable to firms that expand or contract their employment by 50 or more employees in a single year. The RF columns replaces net new jobs per worker with the instrument. State-time dummies, commuting zone dummies, and commuting zone linear trends not shown. The RF and LDV (lagged dependent variable) column replaces fixed effects and commuting zone trends with four lags of cohort schooling. Regressions weighted by cell population, exclude Mexico City and migrants. Commuting zone clustered standard errors in parentheses.

the RF specification but also includes a control for general employment trends at the commuting zone level (the net new jobs per worker at age 16 created through large expansions and contractions in nonexport industries). Column 5 repeats the RF specification but includes four lags of cohort schooling instead of the fixed effects. (Online Appendix Figure C.3 presents a visual plot of the RF strategy for the 30 commuting zones that experienced the largest change in export employment over the sample period.)

In all five specifications, the arrival of new export-manufacturing jobs at age 16 significantly reduces cohort schooling ( $\hat{\beta} < 0$ ) with coefficients between  $-3.155$  and  $-3.380$ . The effect size for formal nonexport job arrivals is significantly different and one-third as large (a finding I return to in Section IV). The differences between the OLS and IV results are small, suggesting that any bias due to reverse causation or measurement error is not severe.

The interpretation of the coefficients from the IV and RF specifications are subtly different. The RF coefficient estimates the effect of the subset of new export jobs that were created through large openings/expansions and closings/contractions. The IV coefficient scales the RF coefficient by the first stage in order to provide an estimate of the impact of all export job arrivals. However, a single large factory opening or expansion will be highly salient, and hence may have different educational impacts compared to an equivalent number of small expansions. In this scenario, where



treatment effects are heterogeneous, there are well-known difficulties in interpreting the IV coefficient. In contrast, the RF coefficient is straightforward to interpret, unbiased if the instrument is exogenous, and potentially the coefficient of interest for policymakers hoping to encourage new factory openings or substantial expansions. Accordingly, for the remainder of the paper I report only the RF coefficients.

The magnitude of the coefficient in Table 2 implies substantial educational impacts. As a concrete example, the ninetieth percentile of the distribution of large firm expansions corresponds to 0.017 net new export jobs per worker. Using the reduced-form coefficient, such a shock results in the exposed cohort obtaining 0.06 years less school on average. Alternatively, I would find the effect size I do if for every 25 new export jobs that arrived, one student in the cohort dropped out at grade 9 rather than continuing on to grade 12.<sup>27</sup>

As with any difference-in-differences regression, my results only imply that the education of cohorts heavily exposed to new factory openings at age 16 fell relative to other cohorts in the commuting zone who were less exposed at these ages. It is possible that, due to a new factory opening, education actually rose across every cohort in the commuting zone but relatively less for the cohorts aged 16. This interpretation would imply that the commuting zones which experienced the largest growth in export employment also saw the largest increases in the school attendance of 16-year-olds between the 1990 and 2000 censuses. To tease apart these two interpretations, I run the following regression:

$$(3) \quad \text{Attend}16_{z,2000} - \text{Attend}16_{z,1990} = \gamma \sum_{t=1990}^{1999} l_{zt} + \varepsilon_z,$$

where  $\text{Attend}16_{z,t}$  is the proportion of the commuting zone  $z$  cohort aged 16 at the time of the year  $t$  census who are currently attending school. The independent variable,  $\sum_{t=1990}^{1999} l_{zt}$ , is the total change in the number of export jobs per worker between January 1, 1990 and December 31, 1999. Once more, I present OLS results as well as IV and RF regressions using the large expansions/contractions instrument. I also report results that include the initial level of school attendance as an independent variable to control for low education commuting zones catching up (either the initial value for the cohort aged 16 or the initial value for the cohort one year older for which there are less obvious endogeneity concerns).

Table 3 presents the results of the regression in equation (3). The estimate of the coefficient on the change in export employment,  $\hat{\gamma}$ , is significantly negative in all specifications. School attendance rose least in the commuting zones that saw the most export job growth over the decade. Therefore, I conclude that export expansions

<sup>27</sup> If 1 out of 25 new jobs induced a member of the cohort of nonmigrants aged 16 to forgo 3 years of education, cohort schooling would fall by  $3/25 = 0.12$  years with the arrival of as many new jobs as members of the cohort. If one job arrived for every person of working-age, cohort schooling would fall by approximately 25 times as much,  $0.12 \times 25 = 3$ , since 16-year-old nonmigrants comprise 3.87 percent of the Mexican population aged 15–49 in the 2000 census. This is the approximate effect size I find—i.e., a three-year reduction in cohort schooling from 1 new export job per worker,  $l_{zc} = 1$ . Of course, the cohort can obtain a higher or lower proportion of the new jobs if some students would have dropped out anyway or drop out and don't find a job. These proportions seem reasonable: in the 2000 census, 11.5 percent of formal export manufacturing workers are aged 18 or younger, 74 percent of whom were nonmigrants.

TABLE 3—CHANGES IN SCHOOL ATTENDANCE 1990–2000 AND NEW EXPORT-MANUFACTURING JOBS

	$\Delta$ Proportion of 16-year-olds attending school at census, 1990–2000					
	OLS			IV (large $\Delta$ s)		
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta$ Net new export manufacturing jobs/worker (1990–1999)	–0.222 (0.0420)	–0.208 (0.0400)	–0.227 (0.0415)	–0.198 (0.0424)	–0.186 (0.0409)	–0.202 (0.0419)
Proportion of 16-year-olds attending school, 1990		–0.156 (0.0115)			–0.101 (0.0124)	
Proportion of 17-year-olds attending school, 1990			–0.102 (0.0128)			–0.101 (0.0128)
Constant	0.0772 (0.00225)	0.154 (0.00602)	0.119 (0.00567)	0.0766 (0.00226)	0.126 (0.00649)	0.118 (0.00567)
Observations	1,777	1,777	1,743	1,777	1,743	1,743
$R^2$	0.015	0.109	0.050	0.015	0.096	0.050
	RF (large $\Delta$ s)					
	(7)	(8)	(9)			
$\Delta$ Net new export manufacturing jobs/worker (1990–1999)	–0.213 (0.0458)	–0.195 (0.0437)	–0.218 (0.0453)			
Proportion of 16-year-olds attending school, 1990		–0.156 (0.0115)				
Proportion of 17-year-olds attending school, 1990			–0.101 (0.0128)			
Constant	0.0760 (0.00220)	0.153 (0.00602)	0.117 (0.00565)			
Observations	1,777	1,777	1,743			
$R^2$	0.012	0.105	0.046			

*Notes:* Dependent variable is the change in proportion of 16-year-olds attending school at time of census, 1990–2000. Independent variable is change in net new manufacturing jobs per worker arriving in commuting zone between January 1, 1990 and December 31, 1999. Columns 2, 5, and 8 include initial proportion of 16-year-olds attending school as a control (17-year-olds in columns 3, 6, and 9). IV and RF columns use the net new jobs per worker attributable to firms that expand or contract employment by 50 or more employees in a single year either as an IV or as main regressor. Regression weighted by commuting zone population of 16-year-olds in 1990 census, excludes Mexico City and migrants.

in a locality led not just to relative but also absolute declines in schooling for the cohorts aged 16 at the time.<sup>28</sup>

#### A. Alternative Methodologies for Classifying Exporting Firms

The nature of the IMSS data means that I am not able to identify the actual export status of each employer. In this section, I explore a range of alternative codings for export status.

I first explore two alternate classifications of the IMSS industry codes. My main specification classifies the 58 three-digit manufacturing industries as “export” if

<sup>28</sup>It seems implausible that the new export jobs led to educational attainment of 16-year-olds rising across Mexico but less in the particular locations where the jobs were actually arriving.

TABLE 4—ALTERNATIVE EXPORTER MEASURES

	Cohort average completed years of schooling							
	RF (large Δs)				(OLS)	(RF/OLS)		(OLS)
	Exports/ worker (1)	Ann. coded (2)	Maquiladora agg. imp. (3)	Maquiladora micro imp. (4)	EIA panel (5)	Maq. agg. + EIA (6)	Maq. micro + EIA (7)	EIA panel (8)
Net new export manufacturing jobs/worker at age 16	-4.241 (0.885)	-2.746 (0.651)						
Net new maquiladora jobs/worker at age 16			-2.106 (0.881)	-1.856 (0.898)		-2.117 (0.878)	-1.869 (0.897)	
Net new EIA export jobs/worker at age 16					4.435 (3.232)	4.487 (3.250)	4.477 (3.251)	
Net new EIA blue collar export jobs/worker age 16								-17.66 (13.35)
Net new EIA white collar export jobs/worker age 16								90.41 (52.51)
Observations	25,289	25,289	25,289	25,289	25,275	25,289	25,289	25,275
R <sup>2</sup>	0.942	0.942	0.942	0.942	0.942	0.942	0.942	0.942
Commuting zones	1,808	1,808	1,808	1,808	1,807	1,808	1,808	1,807

*Notes:* Dependent variable is cohort average years of schooling in the year 2000. Independent variables are alternate measures of net new jobs per worker created by exporters in cohort's commuting zone at age 16. Columns 1 and 2 use alternate classifications described in main text. Columns 3 and 4 restrict attention to maquiladoras identified either from matching the IMSS sample to industry-by-municipio maquiladora aggregates or to the census of maquiladoras. Columns 5 and 8 focus on employment growth in a panel of large non-maquiladoras which export more than 25 percent of output. Columns 6 and 7 combine the panel of large non-maquiladoras with the maquiladoras. State-time dummies, commuting zone dummies, and commuting zone linear trends not shown. Regressions weighted by cell population, exclude Mexico City and migrants. Commuting zone clustered standard errors in parentheses.

more than one-half of output was exported for at least one-half of the sample years. Column 1 of Table 4 reruns the basic specification, equation (1), but codes an industry as export if real exports per worker during the sample period were in the top quartile of all manufacturing industries. Column 2 allows industries to change status over time by coding export industries as those exporting more than one-half of their output that year. Results are similar under both alternative classifications.

It is possible to approximately identify the IMSS firms that are maquiladoras by matching the firm-level employment data to INEGI statistics on annual maquiladora employment by industry, state-industry, and municipio.<sup>29</sup> Since maquiladoras are legally required to export almost all their output, these firms are exporters and accounted for 985,232 net new jobs between 1986 and 1999 (and 816,708 of 1,370,950 net new jobs in industries I classify as export). Column 3 of Table 4 reruns the basic specification but focuses only on job growth at these “known” exporters. Column 4 uses an alternative concordance provided by IMSS where maquiladoras are identified by matching firm-level IMSS data with records from the maquiladora census.<sup>30</sup> In both cases I find significant, if slightly smaller, declines in schooling.

<sup>29</sup>These data come from the INEGI website. I classify firms as maquiladoras when the number of employees in a given cell of the INEGI statistics (e.g., year-state-industry) is equal or greater than the employees in that cell in the IMSS data. As firms appear in several overlapping aggregates, I can iterate this process until convergence. I am able to classify all the potential maquiladoras in four iterations.

<sup>30</sup>About 80 percent of total maquiladora employment can be matched using this concordance.

Finally, I draw on the Encuesta Industrial Anual (EIA) and Encuesta Industrial Mensual (EIM). These surveys cover 3,200 to 6,800 large non-maquiladora firms depending on the year (out of 87,000 to 142,000 non-maquiladora manufacturing firms in IMSS). I code a firm as an exporter if more than 25 percent of output is exported. Unfortunately, the INEGI sampling methodology is not designed to capture new firm openings since the firm list was refreshed only once over the sample period and there are no clear criteria for inclusion. Additionally, data prior to 1993 do not contain municipio identifiers. Therefore, I follow the methodology of Verhoogen (2008) exactly and create a consistent EIA panel of 1,114 firms that are present in every period and calculate the annual change in employment among exporters in each commuting zone (creating 59,753 net new jobs between 1986 and 1999). The resulting variable contains intensive margin changes for the set of (not necessarily representative) firms that were both large and in business throughout the sample.<sup>31</sup>

In contrast to maquiladoras, column 5 of Table 4 shows an insignificant positive schooling impact of job growth in the EIA panel. Columns 6 and 7 combine both maquiladora and EIA firms and show that the coefficients differ significantly (with *p*-values of 5.2 and 6.2 percent, respectively). These results are not particularly surprising. The skill level of the jobs is likely to be a major determinant of schooling impacts. Maquiladoras are export assembly operations known for demanding low levels of skill. In contrast, Verhoogen (2008) shows that, for this panel of large non-maquiladoras, demand for skill is higher among exporters than nonexporters. Column 8 provides support for this explanation by breaking job growth in EIA firms into blue- and white-collar jobs. As expected, the positive effect is driven by more-skilled white-collar job growth, with a negative coefficient on less-skilled blue-collar job growth. Section IV explores heterogeneity by skill level in more detail using skill measures from the 1990 census.

### B. Alternative Samples and Specifications

In online Appendix A, I demonstrate the robustness of my main finding to many additional specifications: removing the various time fixed effects and trends, considering alternative samples (excluding 781 small commuting zones with no formal sector, excluding metropolitan areas or big cities, including Mexico City, not weighting by cohort size, breaking results up by region, controlling for the roll out of Progresa) and exploring alternate specifications (using different schooling measures, extending the 50 employee threshold of my instrument, restricting attention to unusually large expansions/contractions at the firm level, separately exploring expansions and contractions, and allowing for state-level spillovers). Summarizing, there is a negative impact of new export job arrivals on the educational attainment of cohorts aged 16 at the time that is robust to alternative samples and specifications.

<sup>31</sup>These data are confidential and only year-municipio-industry level aggregates were available precluding the use of my RF specification here. As a firm may become an exporter during the sample period, some export job creation comes from nonexport jobs becoming export jobs.

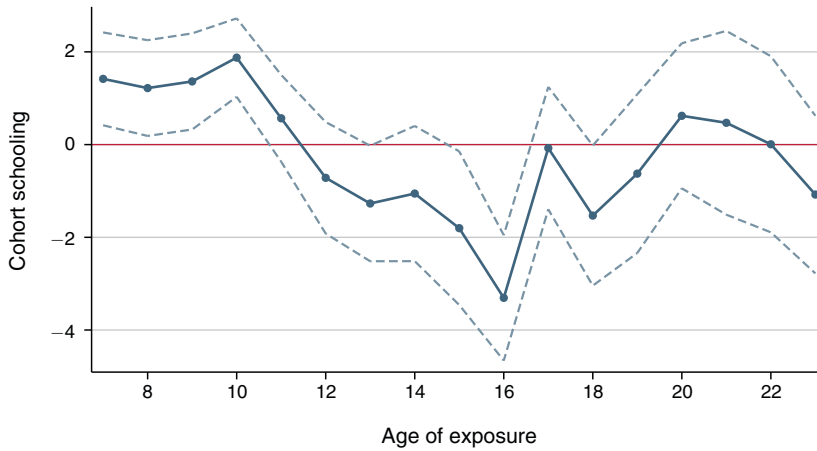


FIGURE 4. EXPORT JOB SHOCKS AT DIFFERENT AGES OF EXPOSURE

Notes: Figure plots the coefficients from 17 regressions that regress cohort average schooling on net new export manufacturing shocks using the specification in equation (1) but replacing export job shocks at age 16 with export job shocks at one of 17 different ages between 7 and 23. Dashed line shows 95 percent confidence intervals.

### III. Validating the Methodology

In the previous sections, I argued that new export opportunities alter the returns to and opportunity cost of schooling; and that I identified the net effect on school attainment by comparing heavily exposed cohorts to less exposed cohorts within and across commuting zones. In this section, I present a range of additional evidence in support of this claim.

#### A. The Age-16 Exposure Window and Effects at Other Ages

My identification strategy is built on the assumption that youths are disproportionately affected by new employment opportunities at age 16. In Section IB, I justified this assumption by appealing to the fact that this age is both the legal factory employment age and the time when students are deciding whether to attend high school or not.

This assumption can be tested. To do so, I repeat my basic specification, equation (1), but replace net new export jobs per worker at age 16 with job shocks at other ages. I run 17 regressions, one for each age of exposure between 7 and 23. As previously, my job shocks include only large expansions and contractions. Figure 4 plots these 17 coefficients and the associated 95 percent confidence intervals. Reassuringly, the largest negative impacts on cohort schooling occur from export employment shocks at age 16. There are also significant negative effects from export employment shocks at three other ages around which students transition between school stages (ages 13, 15, and 18) but these are substantially smaller in magnitude. Shocks at the youngest ages of 7–11 and the older ages of 20–21 raise cohort schooling, although only the first of these is significant at the 5 percent level.

As export employment shocks are likely to be serially correlated, running separate regressions for each age of exposure may be misleading. Instead, I can include multiple ages of exposure in same regression at the cost of further shrinking the sample size (for each additional age of exposure, I lose one year of my short 14-year panel). Simultaneously including five ages of exposure centered around age 16, I still find that exposure at age 16 leads to the largest reduction in school attainment (the full results are reported in online Appendix Table C.1).<sup>32</sup> In summary, the more pronounced effects at age 16 are consistent with the maintained assumption that age 16 is the key exposure age.

### B. *Are Students Really Dropping Out of School at Age 16?*

Up to this point, my dependent variable has been school attainment—as measured by the highest completed grade at the time of the 2000 census. In the absence of true panel data which track school attendance in every year, completed schooling is a sensible metric since not all students are at the same grade at age 16, the age factory employment is first legal. In this section, I explore an alternative metric, the grade-9 drop-out rate.

As emphasized in Section IB, if new job arrivals at age 16 raised the opportunity cost of schooling, we would expect the bulk of the reduction in school attainment to come from students dropping out during the transition between secondary and high school (grades 9 and 10). Hence, I now turn to analyzing grade-9 drop-out rates directly. I can also perform a placebo-like test. If the opportunity cost of school channel is at play, drop-out rates for youths transitioning between secondary and high school should respond most to shocks at age 16 compared to other ages. In contrast, whether students ever reach this stage (i.e., complete secondary school) should respond most to shocks at younger ages when this preceding schooling decision is typically made.

I calculate grade-9 drop-out rates for each cohort and commuting zone by dividing the number of students obtaining exactly 9 years of school by the number of students obtaining 9 or more years of school.<sup>33</sup> With this measure in hand, I reproduce the multiple-exposure-age exercise in Section IIIA but replace the dependent variable, cohort average schooling, with the grade-9 drop-out rate. The dark line in Figure 5 plots the coefficients from separate regressions of grade-9 drop-out rates on net new export jobs per worker at each age of exposure between 7 and 23. As hypothesized, the largest grade-9 drop-out rates occur for export employment shocks at age 16. The magnitude of the coefficient on exposure at age 16 is a sizable 0.374 (significant at the 5 percent level). This coefficient implies that a shock at the ninetieth percentile of large firm expansions would increase grade-9 drop-out rates by 0.64 percentage points.

<sup>32</sup>The magnitude of the coefficient on shocks at age 16,  $-4.09$ , is even larger than in the baseline specification. The second largest coefficient is on shocks at 18,  $-1.60$ , and borderline significant at the 10 percent level.

<sup>33</sup>I restrict attention to cohorts aged 16 and above at the time of the 2000 census and include in the denominator 16-year-olds who are both currently attending school and have completed grade 9. Of course, these are not true drop-out rates since a youth who repeats a grade may appear to have dropped out while a student who did drop out but later returned to school would not appear as a drop-out.



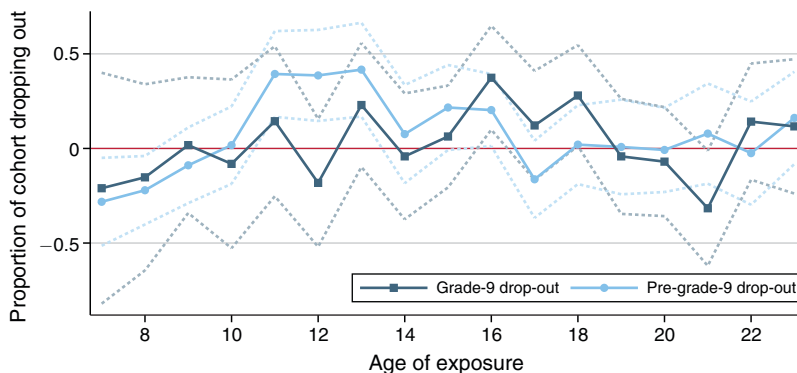


FIGURE 5. GRADE-9 AND PRE-GRADE-9 DROP-OUT AT DIFFERENT AGES OF EXPOSURE

Notes: Each solid line in the figure plots the coefficients from 17 regressions that regress cohort average drop-out rates on net new export job shocks. The darker line shows grade-9 drop-out rates (i.e., the proportion of the set of students with 9 or more years of schooling who obtain no additional years of schooling beyond 9). The lighter line shows pre-grade-9 drop-out rates (i.e., the proportion of students that obtain fewer than 9 years of schooling). Both lines use the specification in equation (1) but replace schooling with the relevant drop-out rates and replacing the export job shocks at age 16 with export job shocks at one of 17 different ages between 7 and 23. Dashed lines show 95 percent confidence intervals.

I perform a similar exercise but using the proportion of each cohort that have not graduated secondary school by the time of the 2000 census (i.e., they dropped out pre-grade-9) as my dependent variable. In contrast to the results with the grade-9 drop-out rate, the light line in Figure 5 shows that the largest increase in this pre-grade-9 drop-out rate occurs with export employment shocks at ages 11 to 13, consistent with the fact that these are the ages at which the decision to enter secondary school is made.

As before, I can combine multiple ages of exposure in the same regression. Online Appendix Table C.1 reports these regressions of both grade-9 and pre-grade-9 drop-out rates on the five ages of exposure centered around age 16 and age 13. Complementing Figure 5, the largest coefficients across the four regressions are those for shocks at age 16 on grade-9 drop-out rates and shocks at age 13 on pre-grade-9 drop-out rates.

### C. Cross-Sectional Evidence from the 1990 Census

I now turn to another data source that has been used sparingly up to this point: the census conducted in March 1990. As this census falls in the middle of my sample period, I can use contemporaneous school attendance and employment measures to: (i) provide further evidence that age 16 is the key exposure age; (ii) show that youths did drop out to work in export manufacturing; and (iii) provide support for my identification assumption that large employment shocks are not driven by education decisions made in prior years.

To provide further support for my key exposure age, I run a linear probability model and regress a dummy for individual  $i$ 's school attendance at the time of the March 1990 census,  $A_{i90}$ , on export employment shocks in three preceding and three

subsequent years. In order to trace the impact on different age groups, I include all cohorts  $c$  aged between 12 and 19 at the time of the census alongside cohort fixed effects  $\delta_c$ , and allow the coefficients  $\beta_{ct}$  on the net new export jobs per worker in year  $t$  to differ for each cohort,

$$(4) \quad A_{izc} = \sum_{t=1987}^{1992} \beta_{ct} l_{zt} + \delta_c + \varepsilon_{izc}.$$

This specification compares the school attendance of youths in commuting zone  $z$  shocked in 1989, the year preceding the March census, to youths of the same age in similar commuting zones that did not receive shocks that year. (And provides estimates based on similar comparisons for shocks in other years and at other ages.)

As in my panel specification, I restrict attention to large expansions and contractions that are plausibly exogenous. However, here I do not include commuting-zone fixed effects. Such fixed effects sweep out the shocks for one age group and cannot capture the highly nonlinear differences in school attendance at different ages between high and low schooling locations.<sup>34</sup> However, since locations do differ in their schooling dynamics, I restrict attention to a comparable set of locations; the 178 commuting zones that experienced an expansion in export manufacturing employment in at least one year between 1987 and 1992. Online Appendix C reports qualitatively similar results using all commuting zones and commuting-zone fixed effects (with each set of coefficients relative to the omitted category, shocks at age 16), as well as results for a similar exercise that also incorporates the 2000 census and shocks between 1997 and 2000.

Panel A of Figure 6 plots the 48 coefficients (6 annual shocks plotted separately for each of the 8 age cohorts) from estimating equation (4). Export employment shocks in 1989 (the year immediately preceding the survey) significantly reduce school attendance at age 16 with a  $\hat{\beta}_{16, 1989}$  of  $-1.90$  and a standard error of  $0.88$ . (The raw coefficients, both here and in the remainder of this section, are relegated to online Appendix C.) Consistent with 16 being my key exposure age, the negative effect on school attendance is most pronounced for 1989 employment shocks on the cohort aged 16 in comparison both to shocks in other years for this same cohort or to shocks in any year for older and younger cohorts.

The second purpose of this section is to show that these 16-year-olds really are dropping out of school to take up export jobs. I repeat the specification above but now replace the school attendance dummy with a dummy for whether the youth is working full time; or alternatively, working full time in an export manufacturing industry. The coefficients from these regressions are plotted in the second and third panels of Figure 6. For the cohort aged 16, both the proportion of the cohort working and the proportion working in exports respond most to export employment shocks in 1989. Thus, the previously documented decline in school attendance is mirrored by a rise in export manufacturing employment. Reassuringly, both the attendance and employment coefficients are of similar magnitudes ( $-2$  and  $+2$ , respectively). The coefficient on export employment is slightly lower at  $1.4$ , suggesting that some

<sup>34</sup>To see this nonlinearity note that attendance gaps between better and worse locations only appear between ages 14 and 21 (before which almost everyone is in school and after which almost no one is).

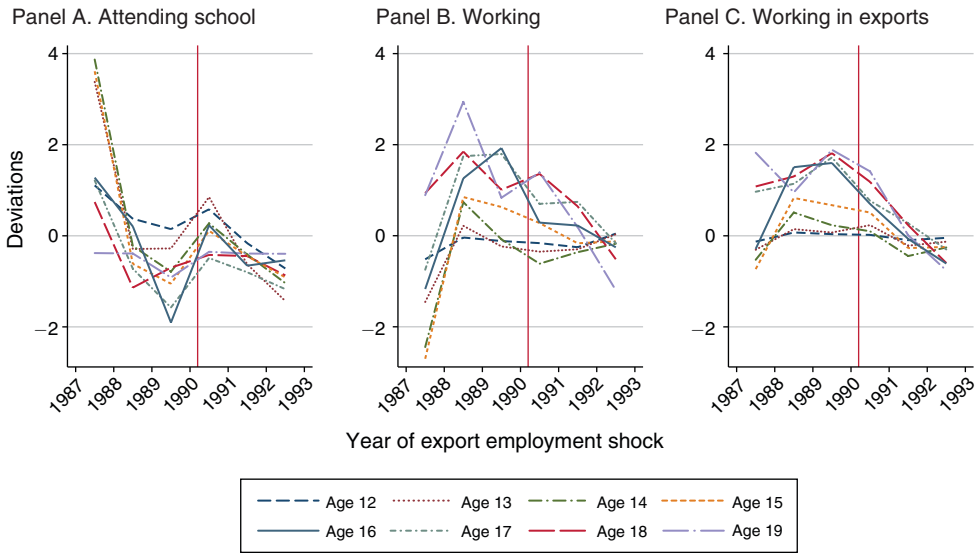


FIGURE 6. CROSS-SECTIONAL EVIDENCE FROM THE 1990 CENSUS

*Notes:* Each panel plots 48 coefficients from regressions of a binary variable from the 1990 census (denoted by panel title) on export employment shocks in each year between 1987 and 1992. I include cohorts aged 12 to 19 and age fixed effects, and allow the coefficients on the shocks to differ by age. Full specification shown in equation (4) attendance of main text. Sample restricted to commuting zones with at least one year of employment growth in export manufacturing between 1987 and 1992. Lines connect coefficients of same age cohort and x-axis denotes the year of the shock. Vertical lines drawn at the time of the March 1990 census.

of those dropping out due to export shocks either end up in other sectors or are misclassified by census enumerators.

I can further explore these sectoral employment choices by replacing school attendance in equation (4) with dummies for employment in the other industries reported in the 1990 census (services, primary industries, nonexport manufacturing, insufficiently specified manufacturing, and unclassified). In contrast to the large response of export manufacturing employment noted above, employment propensities in these other industries do not appear to respond to export employment shocks (see online Appendix Figure C.6). For example, for 1989 shocks on the cohort aged 16, the magnitude of the change in employment in export manufacturing is more than four times greater than the change in the sector with the second largest effects (insufficiently specified manufacturing—the sector where misclassified export jobs would show up).

Although employment impacts seem concentrated in export manufacturing, I cannot rule out that some of these new jobs may be at informal export manufacturing firms.<sup>35</sup> For example, if a new factory generates demand for informal subcontracting, my estimated schooling impacts combine the effects of both these types of job creation.

<sup>35</sup> Unfortunately, the 1990 census doesn't distinguish between formal and informal jobs.

The final purpose of this section is to provide support for the IV strategy outlined in Section ID. I argued that large firm expansions and contractions (which I use to calculate my employment shocks) are plausibly exogenous to education decisions made in prior periods: conditional on commuting-zone-specific fixed effects and trends, and state-time fixed effects. To test this claim, I explore whether employment expansions and contractions in the years immediately following 1989 are related to educational choices made by youths in the March 1990 census. As can be seen from any of the panels in Figure 6 (or C.6), there is no evidence that the shocks after the census of March 1990 are related to either school attendance or employment outcomes. For all age groups and all three dependent variables, the coefficients are bunched around zero for shocks in the years following 1989. More formally, focusing on the school attendance regression in equation (4) above and the coefficients for cohorts aged 16, I cannot reject the null hypothesis that the coefficients on export shocks in the three years following the census are equal to zero (an  $F$ -statistic of 0.0083 distributed  $F(3,163)$ ). In contrast, shocks in prior years do alter school attendance propensities: I can reject the null that the coefficients on shocks in preceding years are zero at the 1 percent level (the  $F$ -statistic is 4.05).

#### D. Gender Differences

The main analysis groups males and females together. In this section, I explore heterogeneity across genders. As a first pass, columns 1 and 2 of Table 5 rerun my main specification by gender. I replace cohort average schooling in equation (1) with either  $S_{zcm}$ , the average schooling of males in the cohort, or  $S_{zcf}$ , the average schooling of females in the cohort. As I run the two regressions separately, I allow for gender-specific trends. Although both male and female schooling significantly decline with new export job arrivals, male schooling declines more than female with coefficient estimates of  $-4.4$  and  $-2.5$ , respectively.

The independent variable, net new export jobs per worker at age 16, is the same in both specifications. Thus, these gender differences can come about either because boys and girls respond differently to the same job opportunities, or because some job opportunities are gender-specific. In the latter case, new export jobs that are particularity suited to women will have a more pronounced effect on female schooling than male and vice versa. Verifying this prediction serves as a simple test of my assumed mechanism, that schooling responds to new export opportunities through changes in the opportunity cost and returns to school.

In order to carry out this test, I utilize the fact that the IMSS employment data provide firm-level employment by gender. These data cannot be used directly, since the sex ratio in a factory may be endogenous to boys' and girls' schooling choices. Accordingly, I use the sex ratio among new jobs created during the first five years of my sample (1985 to 1989) to create a commuting-zone and three-digit-industry specific sex ratio and assume subsequent employment changes in that location-industry pair maintained this ratio.<sup>36</sup> I then multiply these sex ratios by the total employment

<sup>36</sup>For commuting zones where there is no job creation in that industry between 1985 and 1989, I use the sex ratio for local job creation at the two-digit industry level, then at the one-digit level, then at the three-digit state level, and finally the three-digit national level. I obtain similar results using different orderings of this algorithm.

TABLE 5—SEX-SPECIFIC NET NEW EXPORT JOBS AND EDUCATIONAL ATTAINMENT

	Cohort schooling							
	Male (1)	Female (2)						
All RF (large Δs)								
Net new export manufacturing jobs/worker at age 16 (NNEM)	-4.364 (1.105)	-2.511 (1.099)						
Observations	25,113	25,160						
R <sup>2</sup>	0.885	0.913						
Commuting zones	1,808	1,808						
	NNEM jobs/worker at 16				Cohort schooling			
	Male (3)	Female (4)	Male (5)	Female (6)	Male (7)	Female (8)	Male (9)	Female (10)
All RF (large Δs)								
(Male prop. NNEM jobs 1985–1989) × (NNEM jobs/worker at age 16)	1.589 (0.148)	0.459 (0.127)			-5.541 (2.400)	0.517 (2.271)		
(Female prop. of NNEM jobs 1985–1989) × (NNEM jobs/worker at age 16)	0.724 (0.126)	1.230 (0.116)			-0.407 (1.712)	-2.238 (3.352)		
(Male prop. export manuf. jobs 1985) × (NNEM jobs/worker at age 16)			1.516 (0.121)	0.506 (0.104)			-6.223 (1.693)	-2.477 (1.623)
(Female prop. export manuf. jobs 1985) × (NNEM jobs/worker at age 16)			0.560 (0.101)	1.413 (0.106)			-1.869 (2.047)	-2.559 (2.478)
Observations	16,258	16,258	25,113	25,113	16,144	16,204	25,113	25,160
R <sup>2</sup>	0.960	0.936	0.953	0.931	0.903	0.926	0.885	0.913
Commuting zones	1,808	1,808	1,808	1,808	1,806	1,808	1,808	1,808

Notes: Dependent variable in columns 1–2, and 7–10 is the (gender-specific) cohort average years of schooling in the year 2000. Dependent variable in columns 3–6 is the (gender-specific) net new export jobs per worker arriving in cohort’s commuting zone at age 16 attributable to firms that expand or contract their employment by 50 or more employees in a single year. Independent variables in columns 1–2 are same export jobs per worker measure but not gender-specific. Independent variables in other columns are predicted gender-specific export job shocks obtained by multiplying actual non-gender-specific export job shocks by sex ratios of prior job shocks between 1985 and 1989 (columns 3–4 and 7–8, where sample excludes cohorts who turned 16 prior to 1990) or sex ratios of factories open in 1985, the first year of the dataset. State-time dummies, commuting zone dummies, and commuting zone linear trends not shown. Regressions weighted by cell population, exclude Mexico City and migrants. Commuting zone clustered standard errors in parentheses.

shocks for that location-industry pair to provide annual measures of gendered local labor demand that are plausibly exogenous to contemporaneous education decisions. I also present a second variant that multiplies the total employment shocks by the sex ratio at plants in that location-industry pair in 1985, the first year of my IMSS data.<sup>37</sup>

With these two sets of sex-specific export employment shocks  $\hat{l}_{zcm}$  and  $\hat{l}_{zcf}$  in hand, I run

$$(5) \quad S_{zcf} = \beta_m \hat{l}_{zcm} + \beta_f \hat{l}_{zcf} + \delta_z + \delta_z c + \delta_{rc} + \varepsilon_{zc}$$

for female schooling, or similarly for male schooling by replacing  $S_{zcf}$  with  $S_{zcm}$ . I hypothesize that  $\beta_f < \beta_m$  if female schooling is the dependent variable and  $\beta_m < \beta_f$  if male schooling is.

<sup>37</sup>The first measure is preferred since few locations had factories in 1985 and the sex ratio of jobs created in the export booms of the 1980s and 1990s differed substantially from the prior sex ratio.

Prior to presenting the results of this test, I confirm that these predicted sex-specific shocks are correlated with the realized sex-specific employment shocks  $l_{zcm}$  and  $l_{zcf}$ . I replace the schooling dependent variable with either  $l_{zcm}$  or  $l_{zcf}$  in the specification above. These results are presented in columns 3–6 of Table 5. Reassuringly, for both variants of shock—using either  $\Delta$  1985–1989 sex ratios or 1985 sex ratios—I find that male jobs are stronger predictors of male job creation than female ones and vice versa for female job creation. Columns 7–10 of Table 5 present the test itself. As hypothesized, and for both variants, male schooling declines significantly with male export employment shocks but there is a substantially smaller and insignificant decline in response to female shocks.<sup>38</sup> Similarly, female school attainment declines more with female export employment shocks than with male shocks (although the coefficient on female shocks in female columns is smaller than for male shocks in male columns, and it is not significantly different from zero). Youths respond more to employment shocks specific to their gender, and the response is larger for males.

### E. Alternative Mechanisms

The previous sections provide a range of evidence that new export opportunities lower school attainment by raising the opportunity costs of schooling and hence inducing youths on the margin to drop out of school. Here I directly dismiss three alternative mechanisms.

*Parental Work Channels.*—New export jobs may also attract adult family members, not just youths. As education is a normal good, new jobs should increase household income and hence raise schooling. To explain the reduction in schooling I find, adults who previously looked after the young children in the household would have to enter the workforce and make a youth stay home instead.

The cross-sectional methodology introduced in Section IIIC allows me to dismiss this parental employment explanation. I rerun the specification in equation (4) on the cohort aged 16, but allow the coefficients on export employment shocks to differ depending on whether one of the youth's parents is employed in export manufacturing at the time of the census.<sup>39</sup> If my results were driven by parental work channels, I would expect the reduction in school attendance due to shocks in 1989 to be most pronounced in households where a parent is working in export manufacturing. In fact, the opposite is true, with reductions in attendance driven by youths in households where parents are not employed in export manufacturing. I reach a similar conclusion looking at households where any household member (e.g., a sibling) works in exporting. These results are shown in online Appendix Figure C.7 where I also show that export employment shocks do not change the probability that parents work but do increase the probability they work in export manufacturing.

*Selective Migration.*—As discussed in Section IC, the census does not record where migrants were living at age 16. As I cannot match migrants to local job

<sup>38</sup> I find marginally insignificant  $p$ -values of 0.15 and 0.16 (columns 7 and 9) for the test of the  $\beta_m = \beta_f$  null.

<sup>39</sup> Note that I am not able to append family backgrounds in the main analysis since older cohorts in my sample have left their parental homes by the time of the 2000 census.



opportunities at their key exposure age, I excluded them from my sample and my results only pertain to the nonmigrant population.<sup>40</sup>

One-fifth of export manufacturing workers are internal migrants. My results understate the true educational decline if potential future migrants reduce their schooling in response to new opportunities at exporters in other commuting zones. I cannot evaluate this claim using my identification strategy. However, suggestive of this hypothesis, McKenzie and Rapoport (2011) find that the option to migrate to the United States lowers educational attainment in Mexico.

Migration could bias my results if local labor market conditions alter the composition of out-migrants. For example, a new factory opening may deter a low-skill worker from migrating, but have no impact on the migration decision of a high-skill worker. The average education of nonmigrants would then fall due to reduced out-migration of low-skill workers. I address this concern in online Appendix B. First, I show that new export-manufacturing jobs do not increase the size of the sample cohort or the sample cohort's share of the working population. Second, I use data from the 2000 census on the municipio of residence in 1995 to show that new export jobs arrivals deter relatively more educated youths, rather than less educated youths, from migrating internally. This parallels Chiquiar and Hanson (2005), who find that emigrants to the United States are more skilled than nonmigrants.<sup>41</sup> Therefore, the negative schooling impacts I find are potentially even larger in magnitude since compositional effects due to selective out-migration will likely bias the coefficient toward positive values.

Another concern is that exogenous inflows of migrants may alter both education and factory location decisions. An inflow of low-skill migrant labor would lower local unskilled wages, attracting factories and encouraging local students to acquire more education. As with selective out-migration, such migrant inflows bias my negative point estimates toward zero.

Of course, in-migration may reduce the responsiveness of nonmigrant education to new factory openings (since locals are less likely to obtain these factory jobs and in-migrants would put downward pressure on the wages the jobs paid). In online Appendix B, I show that this hypothesis is correct. A large number of migrants working in the export sector of a particular commuting zone attenuates the educational impacts of new export job arrivals. In fact, consistent with the opportunity cost of schooling channel, the regression estimates imply that if all local export workers are migrants there would be no negative effects at all.

*Income Effects and High Returns to On-the-Job Training.*—One explanation for my findings is that the return to on-the-job training exceeds the return to schooling for students who choose to drop out as a result of new export job arrivals. In this section, I show that there is no support for such a conjecture. By the year 2000, the youths induced to drop out by the arrival of export jobs are earning, if anything, lower wages than they would have earned had these jobs never arrived in their localities.

<sup>40</sup>Rural youths will be underrepresented in this sample if they are more likely than urban youths to migrate in search of jobs. As urban areas contain three-quarters of Mexico's population and most of its formal sector jobs, this underrepresentation will only have a small impact on my population-weighted estimates.

<sup>41</sup>Of course, these emigration patterns to the United States may not have been driven by local labor demand shocks so the international migration evidence is only suggestive.

TABLE 6—NET NEW EXPORT JOBS AND LATER-LIFE INCOME

	Total income (including 0s) (1)	Earned income (including 0s) (2)	log hourly wage (3)	log earned income (full-time workers) (4)	Mincerian returns (full-time workers) (5)
RF (large $\Delta$ s)					
Net new export manufacturing jobs/worker at age 16	-613.4 (2,647)	-236.4 (2,650)	-0.0712 (0.190)	-0.0494 (0.202)	-0.101 (0.138)
Observations	25,287	25,288	23,934	23,724	20,611
$R^2$	0.675	0.696	0.886	0.903	0.292
Commuting zones	1,808	1,808	1,808	1,807	1,759

*Notes:* Dependent variable is the cohort average income, earned income, log hourly wage, log earned income, or Mincerian returns in the year 2000. Independent variables are net new export jobs per worker arriving in cohort's commuting zone at age 16 and attributable to firms that expand or contract their employment by 50 or more employees in a single year. Log earned income and Mincerian returns restricts attention to workers working at least 20 hours per week. Mincerian returns calculated by regressing log earned income on schooling and a sex dummy separately for workers in every single cohort and commuting zone and taking the coefficient on schooling. State-time dummies, commuting zone dummies, and commuting zone linear trends not shown. Regressions weighted by cell population, exclude Mexico City and migrants. Commuting zone clustered standard errors in parentheses.

The 2000 census records the total income, the earned income, and the hours worked in the previous month. I replace the dependent variable in equation (1) with several cohort-level income measures. The identification arguments are the same as those discussed in Section ID. However, reverse causality is less worrisome here as cohort income deviations in the year 2000 are unlikely to influence factory location decisions in previous years.

Table 6 presents these results. To capture both extensive and intensive margins, columns 1 and 2 present results with the average total or earned income of the cohort as the dependent variable (including the zeros in the averages). Columns 3 and 4 focus on the intensive margin and replace the dependent variable with the average log wage or log income among full time workers in the cohort. Finally, column 5 replaces the dependent variable by a simple proxy for the Mincerian rate of return; the schooling coefficient from a commuting-zone and cohort-specific regression of log earned income on both years of schooling and a sex dummy.<sup>42</sup>

If the return to on-the-job training in the export sector exceeds the return to school we would expect positive effects of export jobs on incomes. Instead, all five coefficients are negative although not significantly so (the coefficients are of reasonable magnitudes but the standard errors are too large to detect such effect sizes given the noise in the census income data). Of course, income losses do not imply welfare losses. Impatient or credit-constrained students will rationally forgo schooling for immediate income gains, knowing that in a few years their salaries will be lower than if they had stayed at school (particularly if year-of-entry wage premia fade). Policymakers could still have paternalistic concerns for their citizens if they believe adolescents discount the future particularly heavily when faced with delayed gains. Similarly, peer effects at this stage of life are particularly strong, and may cause excessive drop-out rates.

<sup>42</sup> Given that the effects found in this paper operate on schooling margins between primary and high school, I restrict attention to the rate of return within this schooling range.

The magnitudes of the coefficients imply low returns to schooling for these cohorts. I find a coefficient of  $-0.05$  for log income and  $-0.07$  for log wages. Combining these with the schooling coefficients in Table 2 implies Mincerian “rates of return to schooling” of around 2 percent.<sup>43</sup> This return to an additional year of school is lower than returns of around 7.5 percent found by Psacharopoulos et al. (1996) for Mexico. A low estimate of the return to schooling is not surprising. These estimates are identical to local average treatment effects (LATEs) from regressing log income on schooling where schooling is instrumented by net new export jobs. The LATE is likely to be lower than the average returns to school since new export jobs directly raise wages and the LATE group (youths whose decisions are altered by factory openings) are likely to have lower returns to education.

#### IV. What Is Different about Export Manufacturing?

The previous sections have focused on the schooling impacts of export manufacturing jobs. The introduction outlined several motivations for this focus. However, in order to better understand the mechanisms at play, I now explore whether the schooling impacts of export jobs differ from job creation in other sectors? And if so, why?

Recall that in column 5 of Table 2, I modified equation (1) to include net new jobs in nonexport industries as a control (again focusing on large expansions and contractions). Figure 7 plots the coefficients on both export and nonexport jobs from running this specification for every age of exposure between 7 and 23. Across the age distribution, the schooling impacts of new export job opportunities closely track the impacts of new job opportunities in other sectors. The striking exception is at age 16 where new export jobs lead to substantially greater reductions in school attainment (recall the coefficient on export jobs was  $-3.25$  compared to  $-1.17$  for all other jobs, with the difference significant at the 1.2 percent level).

Endogeneity issues are a greater concern for new nonexport jobs, even using large expansions and contractions. Unlike exports, demand in these industries may be driven by local consumption patterns that are correlated with local unobservables. In order to mitigate these concerns, online Appendix D reports similar patterns using only job growth in industries that are highly geographically concentrated and so shocks are likely to be driven by national or international, rather than local, demand.<sup>44</sup> In fact, using only shocks in these highly agglomerated industries, new export job arrivals at age 16 continue to significantly reduce schooling while nonexport job arrivals lead to small (and insignificant) increases in schooling.

Why are the effects of new export jobs so pronounced at age 16 yet this is not the case for jobs in other sectors? This is the age I dubbed the key exposure age, where the opportunity cost of schooling channel is strongest. Hence, it is reasonable to think that the opportunity cost channel is particularly strong for new export jobs, either because of the skills these jobs demand, the wages they pay, or the

<sup>43</sup>The return to schooling is simply the wage or income coefficient divided by the coefficient on schooling.

<sup>44</sup>I restrict attention to 95 of the 276 four-digit IMSS industries that had spatial Herfindahl indexes of more than 0.1 in 2000. This strategy also mitigates concerns that export shocks are partially driven by local demand shocks for nonexport production in industries I categorize as export.

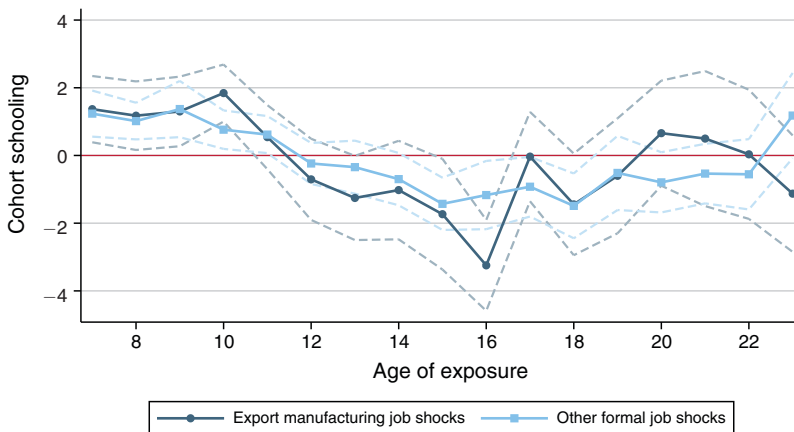


FIGURE 7. DIFFERENTIAL EFFECTS OF EXPORT AND NONEXPORT JOB SHOCKS

Notes: Figure plots the coefficients from 17 regressions that regress cohort average schooling on both net new export jobs and net new job arrivals in all other sectors using the specification in equation (1) but replacing shocks at age 16 with shocks at one of 17 different ages between 7 and 23. Dashed lines show 95 percent confidence intervals. Coefficients on export job shocks are significantly different from nonexport job shocks at age 16 (an *F*-stat of 6.29 with a *p*-value of 0.012).

characteristics of youths in the locations they open. In order to explore these explanations, I lay out a conceptual framework that clarifies how these characteristics affect schooling choices.

A. A Conceptual Framework for Understanding Educational Choices

Forward-looking youths in a particular commuting zone choose among three discrete education levels,  $s = (1, 2, 3)$ , corresponding to primary school, secondary school, and high school, respectively (the three most common educational levels in Mexico). Youths make an irreversible decision in each period  $t$  to either stay on at school and obtain an additional level of education or enter the labor force with their current level of schooling.

A student at school receives  $u(\bar{y})$  utility that includes any family support and the direct utility from schooling. A worker with schooling  $s$  who entered the labor force in period  $i$  earns a wage of  $y_{s,i,t}$  in period  $t$ . I assume log utility and a Mincer-like wage function:

$$(6) \quad u(y_{s,i,t}) = \ln y_{s,i,t} = a_0 + \gamma s + b[t - i] + \varepsilon_{s,i}$$

where  $a_0$  is the base salary,  $\gamma$  is the return to an additional level of school,  $b$  is the return to experience, and  $\varepsilon_{s,i}$  is a stochastic and persistent year-of-entry wage premium specific to skill  $s$ .

These year-of-entry wage premia exist as only certain firms will offer a worker a job in any given year. In a year when more formal firms are hiring, a student is more

likely to obtain a job at a firm paying persistently higher wages.<sup>45</sup> Accordingly, these wage premia are a weakly increasing function of new job opportunities,  $l_i$ , in the year of entry into the labor force,

$$(7) \quad \varepsilon_{s,i} = \omega_{s,i} \phi_{s,i} l_i,$$

where  $\phi_{s,i}$  is the proportion of the new jobs available to workers with school  $s$  and  $\omega_{s,i} \geq 0$  captures the premia the new jobs pay compared to job opportunities in a normal year.

A student lives forever, cannot borrow or save, and discounts at the rate  $\rho$ . The drop-out decision of an enrolled student in period  $t$  depends on whether her discount rate is above the discount-rate cutoff,  $\bar{\rho}_{s,t}$ , obtained by equating the net present value of dropping out with school  $s$  with the net present value of obtaining exactly one more school stage,  $s + 1$ :

$$(8) \quad \bar{\rho}_{s,t} = \frac{\gamma + E_t \varepsilon_{s+1,t+1} - b - \varepsilon_{s,t}}{a_0 + \gamma s + \varepsilon_{s,t} - \ln \bar{y}} \equiv \frac{RS_{s+1,t}}{OC_{s,t}}.$$

This expression is intuitive. The numerator corresponds to the perceived per-period utility gain from possessing an additional level of schooling (which I define as the “return to schooling”  $RS_{s+1,t}$ ). The denominator corresponds to the utility difference between working this period and being at school (which I define as the “opportunity cost of schooling”  $OC_{s,t}$ ). An impatient student with a high discount rate,  $\rho \geq \bar{\rho}_{1,t}$ , will choose  $s = 1$  in period  $t$ ; in period  $t + 1$  a student with an intermediate discount rate,  $\bar{\rho}_{1,t} > \rho \geq \bar{\rho}_{2,t+1}$ , will choose  $s = 2$  and a patient student with a low discount rate,  $\rho < \bar{\rho}_{2,t+1}$ , will choose  $s = 3$ .<sup>46</sup>

*Aggregate Schooling of the Cohort Aged 16 and New Plant Openings.*—I first derive an expression for the average schooling of the cohort at my key exposure age, age 16. In order to generate within-cohort variation in schooling, I assume that youths are heterogeneous in their discount rates but otherwise identical.<sup>47</sup> Since many Mexicans start school late or repeat grades<sup>48</sup>—and the effect of new job arrivals will depend on a youth’s preexisting level of education—I divide the cohort into two groups: a proportion  $\theta$  of youths who are not lagging behind (i.e., if they chose to stay at school they have  $s = 2$  at age 16) and a proportion  $1 - \theta$  who are lagging behind one period (i.e., they have only just completed  $s = 1$  at age 16). I allow the two groups to draw from different distributions of discount rates:  $\rho$  is distributed with a PDF  $f(x)$  and CDF  $F(x)$  across the continuum of nonlagging youths, and with

<sup>45</sup> Firm-specific premia may derive from efficiency wage, fair wage, insider bargaining, or search models. Frías, Kaplan, and Verhoogen (2009) document firm-specific wage differentials in Mexico. Duval Hernandez (2006) presents evidence of formal sector job rationing in Mexico. Oreopoulos, von Wachter, and Heisz (2012) and Beaudry and DiNardo (1991) present evidence for year-of-entry wage premia across and within firms, respectively.

<sup>46</sup> I restrict attention to the simple case where both the return to and opportunity cost of schooling are positive and the  $\varepsilon$  shocks are small enough that the ranking  $\bar{\rho}_{1,t} > \bar{\rho}_{2,t+1}$  is always preserved.

<sup>47</sup> Youths may also differ in their ability which would affect wages and  $\bar{y}$ . This additional heterogeneity will not alter the sign predictions as long as ability is observable and  $\varepsilon_{s,i}$  is weakly increasing in  $l_i$  for all abilities.

<sup>48</sup> For example, 33.6 percent of 16-year-olds attending school in the 1990 census had not completed grade 9.

a PDF  $g(x)$  and CDF  $G(x)$  across lagging youths. These CDFs and the discount-rate cutoffs pin down aggregate schooling,  $S$ , of the age-16 cohort in period  $T$ :

$$(9) \quad S = 1 + \theta [F(\bar{\rho}_{1,T-1}) + F(\bar{\rho}_{2,T})] + (1 - \theta) [G(\bar{\rho}_{1,T}) + G(\bar{\rho}_{2,T+1})].$$

I now examine an unanticipated and one-off plant opening in period  $T$ . The opening generates a large number of vacancies,  $l_T > 0$ , as the whole plant must be staffed at the time of opening. In subsequent periods there are smaller (known) vacancy shocks,  $l_{T+t} = \delta^t l_T$  with  $\delta \in [0, 1)$ , demanding the same skills and paying the same premium. Taking a first-order Taylor expansion of equation (9) around  $l_T = 0$ —the counterfactual of no plant opening—I obtain the resulting change in cohort schooling:

$$(10) \quad S|_{l_T > 0} - S|_{l_T = 0} = \underbrace{\frac{d(\bar{\rho}_{1,T})}{d\varepsilon_{1,T}}}_{<0} \Big|_{l_T=0} d_{1,T} \omega_{1,T} \phi_{1,T} l_T + \underbrace{\frac{d(\bar{\rho}_{1,T})}{d\varepsilon_{2,T+1}}}_{>0} \Big|_{l_T=0} \delta d_{1,T} \omega_{2,T} \phi_{2,T} l_T \\ + \underbrace{\frac{d(\bar{\rho}_{2,T})}{d\varepsilon_{2,T}}}_{<0} \Big|_{l_T=0} d_{2,T} \omega_{2,T} \phi_{2,T} l_T + \underbrace{\frac{d(\bar{\rho}_{2,T})}{d\varepsilon_{3,T+1}}}_{>0} \Big|_{l_T=0} \delta d_{2,T} \omega_{3,T} \phi_{3,T} l_T,$$

where  $d_{1,T} \equiv g(\bar{\rho}_{1,T})(1 - \theta)$  is the density of youth on the primary/secondary school margin and  $d_{2,T} \equiv g(\bar{\rho}_{2,T})(1 - \theta)\delta + f(\bar{\rho}_{2,T})\theta$  is the density of youth on the secondary/high school margin (with the lagging-youth density adjusted by  $\delta$  as they receive the shock one period later).

The first term captures the impact of any job arrivals that require only primary school. These jobs induce students on the margin between primary and secondary school to drop out by raising the opportunity cost of school ( $OC_{1,T}$ ), lowering the return to school ( $RS_{2,T}$ ) and hence lowering the discount-rate cutoff  $\bar{\rho}_{1,T}$ . Since nonlagging youths already have  $s = 2$  (or chose to drop out), only the lagging proportion  $1 - \theta$  respond to these lowest skill jobs.

The next two terms capture the impacts of job arrivals requiring secondary school. For lagging students on the margin between primary and secondary, these jobs induce school acquisition by raising the return to school ( $RS_{1,T}$ ) and hence the discount-rate cutoff  $\bar{\rho}_{1,T}$  (term two). These same jobs induce school drop-out for students on the margin between secondary and high school in both groups by raising the opportunity cost of school ( $OC_{2,t}$ ), lowering the return to school ( $RS_{3,t}$ ), and hence lowering the  $\bar{\rho}_{2,T}$  and  $\bar{\rho}_{2,T+1}$  cutoffs (term three).

Finally, any new jobs requiring high school raise the returns to school for both groups and encourage youths on the margin between secondary and high school to acquire more education (term four). Online Appendix Figure C.8 illustrates these shifts in discount-rate cutoffs.

The return to schooling in Mexico rose pre-NAFTA before falling in the late 1990s (see footnote 1). Against this backdrop, I note that a plant opening can lead to lower cohort schooling despite the return to schooling rising if the opportunity cost of schooling also rises (for example, if new export jobs pay high wages but relatively more so for high-skill positions).



### B. Exploring Heterogeneity in the Impact of New Job Arrivals

The framework above suggests that the relationship between employment expansions at age 16 and schooling is ambiguous and mediated by three factors: the skill level demanded by the new jobs (the  $\phi_{s,TS}$ ), the wage premia these jobs pay over existing job opportunities (the  $\omega_{s,TS}$ ), and the density of youths on the drop-out margin at age 16 (the  $d_{s,TS}$ ). The importance of the skill demanded was apparent from Table 4 which showed that new maquiladora and blue-collar EIA jobs reduced schooling, but white-collar EIA jobs did the opposite. In this section, I expand on the analysis using census data on schooling and wages matched to an individual's industry of employment (recall IMSS does not record schooling). To minimize endogeneity concerns, I use data on younger workers (those aged 16 to 28) in the 1990 census to characterize the post-1990 job arrivals, and then explore how these characteristics relate to heterogeneity in the schooling impacts of these job shocks.

In terms of the first factor, the skill level, I categorize jobs in each state-industry cell (for the 86 industries I can match across the census and IMSS) by the proportion of workers in each of three schooling bins in the 1990 census: workers without completed secondary schooling (grade 8 or less); with secondary school but without completed high school (between grades 9 and 11); and high school and above (grade 12 or more). I then impute the skill distribution of new jobs by multiplying IMSS job arrivals at the commuting-zone-industry level by these three skill proportions from the relevant state-industry pair in the census.

The top-left panel of Figure 8 plots, by sector, the imputed skill distribution of net new job arrivals per worker in the years 1991 to 1999 attributable to large firm expansions and contractions (the variation used in my preferred regression specifications).<sup>49</sup> Export industries have a much larger share of employees falling into the first two bins—i.e., workers without high school—compared to nonexport sectors (89 percent compared to 72 percent). As all my IMSS shocks are formal, this skill discrepancy may be due to the fact that export sectors have more informal jobs and formality is not recorded in the 1990 census. The top-right panel of Figure 8 shows this is not the case, with an even larger discrepancy when I repeat the exercise using skill proportions estimated from the 2000 census where formality is recorded.

I use wage income from the census to calculate proxies for wage premia (the ratio of wage income for full time employees in an industry relative to the average wage in that locality for that skill level), again at the state-industry level. The middle panels of Figure 8 (the left panel using the 1990 census, the right using the 2000 census) plot the average value of these premia by sector and skill, with the average imputed by weighting the state-industry wage premia by 1991–1999 IMSS job arrivals attributable to large expansions and contractions. New formal jobs arrived in industries that paid above average wages for their localities, although slightly more so for jobs in nonexport sectors than in export sectors.

Finally, I require proxies for the density of youths on the secondary ( $d_1$ ) and high school ( $d_2$ ) drop-out margins. It is straightforward to show that the densities are simple functions of three objects: (i) the proportion of the entire cohort dropping

<sup>49</sup> Online Appendix Figure C.9 repeats this exercise using job shocks across all years (1986–1999) and all firms.

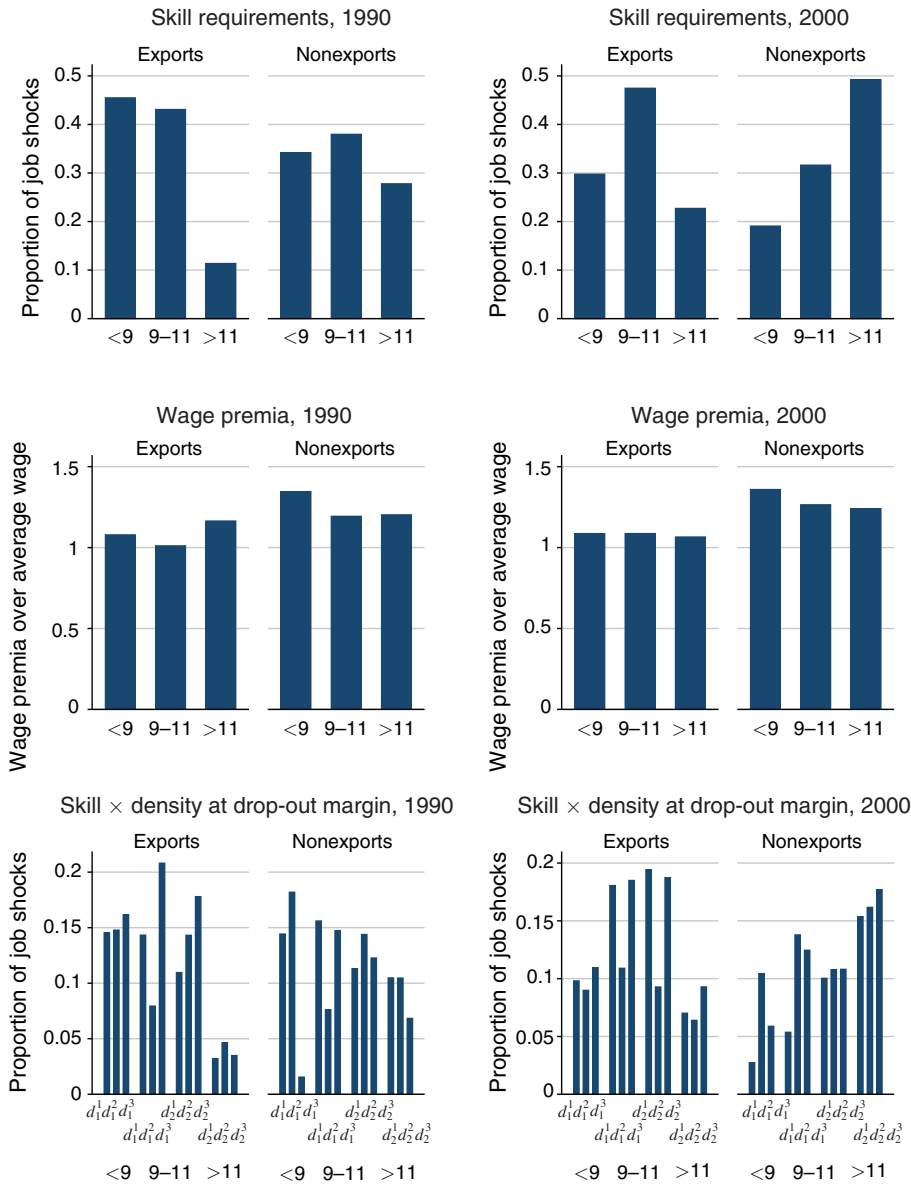


FIGURE 8. SKILL DIFFERENCES BY SECTOR

Notes: Top row plots skill distribution of net new jobs per worker attributable to large expansions and contractions occurring between 1991 and 1999. Job arrivals categorized into three skill bins based on highest completed educational stage of workers in each state-industry at time of census; primary school (grades < 9), secondary school (grades 9–11), and high school or above (grades > 11). Second row plots wage premia (over the average commuting zone wage for that skill bin) paid by these same net new job arrivals. Bottom row subdivides the skill bins in top row into terciles (denoted by superscripts) of the relevant density of marginal youths for each skill level detailed in equation (10). Left panels use 1990 census information, right panels use 2000 census (where I focus on formal sector employees only).

out at that educational stage; (ii) the proportion of the cohort that is both nonlagging and drops out at that stage; and (iii) the persistence of job shocks across years,  $\delta$ .<sup>50</sup> The first two objects I can estimate separately for each commuting zone from the school attainment and attendance data from recent cohorts of 16-year-olds in the 1990 census.<sup>51</sup> The third, the perceived persistence rate, is unobservable, and so I assume  $\delta = 0.5$ . I explore alternate proxies for these objects later in this section.

The bottom-left panel of Figure 8 summarizes by sector these density measures from the 1990 census. I allocate the skill proportions from the top row of Figure 8 across national terciles  $d_s^1$ ,  $d_s^2$ , and  $d_s^3$  of the relevant density term  $d_s$  for that skill level from equation (10) (where there are two relevant densities  $d_1$  and  $d_2$  for jobs that demand secondary school, and so two blocks of three bars). The sum of the three bars in each block equals the proportion of jobs in the relevant skill bin in the top row. The higher the third bar in each block relative to the first two, the larger the share of jobs arriving in locations with many youths on the schooling margin relevant to jobs of that skill level. Compared to nonexport jobs; export jobs demanding primary school arrive disproportionately in locations with many youths on the primary/secondary margin (compare the “<9” block of bars across sectors), and export jobs demanding secondary school arrive disproportionately in locations with many youths on the secondary/high school margin (compare the second “9–11” block of bars across sectors).

In the right panel of Figure 8, I use data from the 2000 census that exclude informal jobs. Patterns are similar except that there is a substantial increase in the share of nonexport jobs demanding primary school that arrive in locations with many youths on the primary/secondary margin. This suggests that inference using measures from the 1990 census, where I cannot exclude informal jobs, may be misleading for the least-skilled IMSS jobs.

I now turn to supplementing my baseline regression specification, equation (1), with the four job-characteristic controls derived in equation (10):

$$(11) \quad S_{zc} = \beta_0 l_{zc} + \beta_1 d_{1z} \omega_{1z} \phi_{1z} l_{zc} + \beta_2 \delta d_{1z} \omega_{2z} \phi_{2z} l_{zc} + \beta_3 d_{2z} \omega_{2z} \phi_{2z} l_{zc} \\ + \beta_4 \delta d_{2z} \omega_{3z} \phi_{3z} l_{zc} + \delta_z + \delta_z c + \delta_{rc} + \varepsilon_{zc}.$$

I calculate  $\omega_{sz} \phi_{sz} l_{zc}$ —for cohort  $c$  and for each of the three skill bins  $s$ —by multiplying net new jobs  $l_{zsj}$  in industry  $j$  in the year the cohort turned 16 by the state  $r$ -industry  $j$  specific wage premia and skill proportions described above, and then

<sup>50</sup>The density in the first term of equation 10,  $g(\bar{p}_{1,T})(1 - \theta)$ , can be rewritten as  $h(\bar{p}_{1,T}) - f(\bar{p}_{1,T})\theta$  where  $h(\cdot)$  is the p.d.f. of the entire cohort. Similarly,  $g(\bar{p}_{2,T})(1 - \theta)\delta + f(\bar{p}_{2,T})\theta$  equals  $\delta h(\bar{p}_{2,T}) + (1 - \delta)f(\bar{p}_{2,T})\theta$ .

<sup>51</sup>In terms 1 and 3 of equation (10), job arrivals induce drop-out for youths with  $\rho_s$  just below the (preshock) secondary and high school drop-out cutoffs, respectively. Thus, for object (i), I use the proportion of the cohort with exactly 9 (the schooling of youths with  $\rho_s$  just below  $\bar{p}_{1,i}$ ) or 12 (the analog for  $\bar{p}_{2,i}$ ) years of school. I take a weighted average of the 5 cohorts aged 20–24 that have had time to complete high school. For object (ii), I use the change in the proportion of the cohort at the correct age-for-grade and at school in the final year of secondary school, grade 9, and the proportion for the cohort one year older in grade 10 (and similarly for the high school to college transition). In terms 2 and 4 of equation (10), job arrivals induce school acquisition for youths with  $\rho_s$  just above the cutoffs. Thus, I calculate similar measures for object (i) but use the proportions with exactly 7–8 years of school (the school attainment of youths who start but do not complete secondary school) or 10–11 years of school (the analog for high school). For object (ii), I use the change in attendance of students at the correct age-for-grade between 7 and 8, or 10 and 11, years of school.

summing across all 86 three-digit industries:  $\sum_j \omega_{srj} \phi_{srj} l_{zsj}$ . The  $d_{sz}$  density terms, also described above, premultiply this summation. As explained above, I focus on job shocks post-1990, the year my proxies are measured, and again restrict attention to large single-firm expansions and contractions.

Columns 1–4 of Table 7 present results for all IMSS job arrivals (columns 5–8 show results just for the export industries that were my focus up to now). I build the analysis in steps. Column 1 presents the specification without characteristic controls. Using post-1990 shocks only, there is no negative effect of all-industry job shocks (column 5 shows that export shocks post-1990 had significant negative impacts on schooling, as they did in the full sample). Columns 2–3 add the skill and skill-wage interactions, respectively.<sup>52</sup> The coefficients on the key secondary school job shocks—recall that secondary school was the modal level of education for export sector employees in my 2000 census sample—are negative but only significantly so when the wage interactions are also included. However, as is clear from equation (10), a weak negative coefficient on these intermediate-skill job shocks is not surprising since it conflates two effects: lagging youths who acquire more education due to these jobs, and non-lagging youths whose drop-out rates increase.

Column 4 includes the full set of four interaction terms in equation (11) that allow me to disentangle these two effects. The coefficient on the third interaction is negative, large, and highly significant. School drop-out is driven by jobs demanding secondary school arriving in locations with many youths on the secondary/high school drop-out margin. I find support for the sign predictions on two of the three other interactions. As predicted, both secondary and high school jobs induce skill acquisition if a sufficient number of youths are on the relevant margin (although only the former is significant). Less intuitively, jobs demanding primary school induce school acquisition for youths on the relevant margin (although, as noted earlier, proxies drawn from the 1990 census may be particularly misleading for this category as formality is not recorded).

Once the job characteristics captured by the interaction terms are accounted for, export jobs no longer have a differentially negative effect on school drop-out. Column 9 adds export job shocks to the all jobs specification in column 1, and column 10 further includes the four interaction terms. The coefficient on new export jobs—i.e., the differential impact of exports—shrinks from a significant  $-1.920$  to an insignificant  $-0.862$ . The interaction terms have substantial explanatory power. Running the nonexport shocks over the sample period through the coefficients on the interaction terms in column 10 and rescaling by the average shock size, I find that nonexport job arrivals reduced average schooling by 0.046 years for a shock size of 1 (i.e., one job arrival per worker). Running export job shocks through the same coefficients, I find export jobs reduced average schooling by 0.547 years, or a differential impact of 0.501. This explained component is two-thirds of the unexplained component.<sup>53</sup>

Columns 11–16 find qualitatively similar results using a range of alternative proxies. Column 11 calculates wage premia using hourly rather than total wages. Column 12 calculates density measures using cohorts aged 17–21 rather than 20–24 at the time of the 1990 census. Column 13 uses estimates of  $\delta$ , the vacancy persistence

<sup>52</sup>One skill interaction drops out since the sum of job shocks in all skill bins equals the total job shock.

<sup>53</sup>The unexplained component is the effect of new export jobs in row 2 of column 10.

TABLE 7—EXPLORING HETEROGENEITY DUE TO JOB AND LOCATION CHARACTERISTICS

LHS: Cohort average years of schooling, RF (large Δs)	Interactions of all job shocks baseline specification				Interactions of export job shocks baseline specification			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Net new jobs/worker at age 16	-0.226 (0.422)	1.130 (1.196)	2.747 (2.267)	-0.607 (0.828)				
Net new export manufacturing jobs/worker at age 16					-1.783 (0.755)	2.767 (4.247)	-4.607 (9.321)	0.324 (1.842)
2nd schl. × jobs/worker age 16		-2.556 (3.527)				-10.06 (10.19)		
> 2nd schl. × jobs/worker age 16		-2.568 (2.171)				-2.114 (6.204)		
< 2nd schl. × wage × jobs/worker age 16			-1.462 (2.146)				8.805 (10.84)	
2nd schl. × wage × jobs/worker age 16			-3.724 (2.256)				-6.215 (8.461)	
> 2nd schl. × wage × jobs/worker wage 16			-2.484 (2.485)				9.575 (10.68)	
< 2nd schl. × wage × density <sub>1</sub> × jobs/worker age 16				12.02 (5.177)				7.773 (16.35)
2nd schl. × wage × δ density <sub>1</sub> × jobs/worker age 16				37.97 (20.31)				-22.68 (33.20)
2nd schl. × wage × density <sub>2</sub> × jobs/worker age 16				-65.79 (22.03)				-127.1 (48.58)
> 2nd schl. × wage × δ density <sub>2</sub> × jobs/worker age 16				49.50 (41.24)				173.7 (124.9)
Observations	16,258	16,258	16,258	16,199	16,258	16,217	16,217	16,163
R <sup>2</sup>	0.952	0.952	0.952	0.952	0.952	0.952	0.952	0.952
Commuting zones	1,808	1,808	1,808	1,801	1,808	1,803	1,803	1,797

LHS: Cohort average years of schooling, RF (large Δs)	Export main effect and interactions of all job shocks							
	Baseline spec.	Alt wage	Alt avg	Alt delta	Alt dens	High Herfindahl		
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Net new jobs/worker at age 16	0.133 (0.499)	-0.410 (0.857)	-0.553 (0.852)	-0.538 (0.954)	-0.693 (0.655)	-0.402 (0.815)	1.355 (0.786)	-0.00974 (1.350)
Net new export manufacturing jobs/worker at age 16	-1.920 (0.946)	-0.862 (0.939)	-0.864 (0.945)	-0.835 (0.959)	-1.275 (0.926)	-1.228 (1.003)	-3.151 (1.531)	-2.217 (1.696)
< 2nd schl. × wage × density <sub>1</sub> × jobs/worker age 16		11.25 (5.212)	11.62 (5.019)	10.58 (5.349)	10.09 (4.865)	8.443 (5.688)		12.77 (7.436)
2nd schl. × wage × δ density <sub>1</sub> × jobs/worker age 16		35.25 (20.45)	35.28 (19.80)	31.64 (25.82)	27.30 (8.716)	29.18 (19.51)		35.59 (32.31)
2nd schl. × wage × density <sub>2</sub> × jobs/worker age 16		-60.25 (23.34)	-55.71 (22.44)	-66.06 (25.66)	-17.19 (7.043)	-74.23 (34.66)		-82.51 (38.23)
> 2nd schl. × wage × δ density <sub>2</sub> × jobs/worker age 16		42.60 (41.37)	44.09 (41.38)	28.96 (32.10)	0.215 (0.489)	30.52 (61.56)		216.6 (76.41)
Observations	16,258	16,199	16,199	16,226	16,199	16,199	16,258	16,199
R <sup>2</sup>	0.952	0.952	0.952	0.952	0.953	0.952	0.952	0.953
Commuting zones	1,808	1,801	1,801	1,804	1,801	1,801	1,808	1,801

Notes: Dependent variable is cohort average schooling in the year 2000. Independent variables are net new jobs per worker arriving in cohort's commuting zone at age 16 attributable to firms that expand or contract employment by 50 or more employees in a single year, as well as interactions for the skill level of the job, the wage premia on offer, and the density of youth on the relevant schooling margin. The job arrivals are categorized into primary school (< 2nd schl.), secondary school (2nd schl.), and high school and above (>2nd schl.) based on the education level of sample-age employees in the relevant state-industry pair in the 1990 census. Wage premia (relative to average wage in that location for workers in that skill bin) are also from the 1990 census at the state-industry level. Density measures for the primary/secondary margin (Density<sub>1</sub>) and secondary high school margin (Density<sub>2</sub>) are calculated using school attendance and highest completed education in the 1990 census. See Section IVB for additional details. Columns 5–8 focus on export-sector job arrivals between 1991 and 1999, while the remaining columns focus on all job arrivals between 1991 and 1999 (with columns 9–16 also including the main effect of export job arrivals). State-time dummies, commuting zone dummies, and commuting zone linear trends not shown. Regressions weighted by cell population exclude Mexico City and migrants. Commuting zone clustered standard errors in parentheses.

rate, from an AR(1) regression of net new job arrivals at the industry-commuting zone level. Column 14 replaces the density proxies in terms two and four of equation (10) with those used in terms one and three, respectively (see footnote 51 for definition of these proxies). Finally, columns 15–16 redo the analysis using only job shocks in the highly-agglomerated industries described in online Appendix D. The bulk of high-skill job creation occurs in nonexport sectors where local demand shocks may drive labor demand. In column 16, where these endogeneity concerns are mitigated, the high-skill interaction term is positive, significant, and large in magnitude—implying that more skilled jobs encourage school acquisition.

In summary, the negative schooling impacts induced by the large influx of export manufacturing jobs in Mexico seem to be driven by the fact that many of these jobs demanded workers with secondary educations, paid reasonably high wages and arrived in locations where many youths were on the margin between secondary and high school. These export jobs were not intrinsically different to other jobs, they just possessed a set of characteristics that disproportionately induced school drop-out.

## V. Conclusions

This paper finds that for Mexico during the period 1986 to 2000, the massive expansion of export manufacturing altered the distribution of education. In particular, the influx of new export-manufacturing jobs reduced the schooling of cohorts at their key exposure ages at the time. The magnitudes I find suggest that for every 25 new jobs created, one student dropped out at grade 9 rather than continuing on through grade 12.

The specific characteristics of export manufacturing in Mexico can explain these negative schooling impacts. The export manufacturing boom generated an abundance of new low-skill formal job opportunities which substantially raised the opportunity cost of schooling for youths on the drop-out margin at age 16. My findings suggest that there would not have been the same negative schooling impacts had these jobs demanded more educated workers and had they arrived in parts of the country where fewer youths were on the drop-out margin at the legal factory employment age.

These findings are relevant for designing industrial and trade policies. Many developing countries, including Mexico, have prioritized raising the education level of the workforce at the same time as pursuing an export-oriented industrialization strategy. Given the trade-off between these goals in the Mexican context, it is vital for policymakers designing industry- or location-specific policies to know which types of new manufacturing opportunities pull students out of school and in what context.

There are also several policy remedies that can mitigate the negative educational impact of export jobs by lowering the opportunity cost of schooling rather than altering the type or location of job arrivals. For example, payments that condition on school attendance reduce the net benefit of entering the labor force.<sup>54</sup> Alternatively, raising the age of earliest employment in export manufacturing ensures that most youths would have already chosen their final education level before being

<sup>54</sup>The much-studied Progreso program in Mexico does just that, providing cash transfers to parents who keep their children in school up to grade 9. The roll out was too late to have an impact on my sample.



eligible to work in these plants. Finally, reducing the psychic cost of returning to school in later life would allow adults to obtain the foregone education should the export-manufacturing jobs dry up or should the adult come to regret their decision.

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