NBER WORKING PAPER SERIES

ENDOGENOUS SKILL ACQUISITION AND EXPORT MANUFACTURING IN MEXICO

David G. Atkin

Working Paper 18266
http://www.nber.org/papers/w18266

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2012

Special thanks to David Kaplan and ITAM for computing and making available the IMSS Municipality level employment data. Thanks to Angus Deaton, Penny Goldberg and Gene Grossman for guidance and encouragement throughout. Further thanks to Joe Altonji, Treb Allen, Chris Blattman, Richard Chiburis, Dave Donaldson, Marco Gonzalez-Navarro, Leonardo Iacovone, Amit Khandelwal, Fabian Lange, Adriana Lleras-Muney, Marc Melitz, Mushfiq Mobarak, Marc-Andreas Muendler, Hannah Pitt, Guido Porto, Nancy Qian, Jesse Rothstein, Sam Schulhofer-Wohl, Eric Verhoogen and numerous seminar participants for their useful comments. Financial aid from the Fellowship of Woodrow Wilson Scholars at Princeton University is gratefully acknowledged. Any errors contained in the paper are my own. The views expressed herein are those of the author and do not necessarily reflect the views of the National Bureau of Economic Research.

Endogenous Skill Acquisition and Export Manufacturing in Mexico
David G. Atkin
NBER Working Paper No. 18266
August 2012
JEL No. F16,J24,O12,O14,O19

ABSTRACT

This paper presents empirical evidence that the growth of export manufacturing in Mexico during a period of major trade reforms, the years 1986-2000, altered the distribution of education. I use variation in the timing of factory openings across municipalities to show that school dropout increased with local expansions in export manufacturing. The magnitudes I find suggest that for every twenty jobs created, one student dropped out of school at grade 9 rather than continuing through to grade 12. These effects are driven by the least-skilled export-manufacturing jobs which raised the opportunity cost of schooling for students at the margin.

David G. Atkin
Economic Growth Center
Yale University
P.O. Box 208269
New Haven, CT 06520
and NBER
davidgatkin@gmail.com

1 Introduction

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 and Zhou 2003 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 both schooling and incomes will rise with the arrival of new exporting opportunities. However, such an inference ignores the fact that new high-wage job arrivals 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 and can even end up with lower incomes in the future, commensurate with their lower skill levels. This paper exploits variation in the timing of factory openings across municipalities 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 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 while maintaining education levels. In the second part of the paper, I use the heterogeneity in experiences across Mexico to understand the features of the new export opportunities that induced school dropout, and what types of job would have encouraged school acquisition.

A simple conceptual framework guides my empirical analysis. I incorporate stochastic job opportunities and heterogeneous discount rates into an educational choice model. This framework illustrates that new 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. A new factory opening is most likely to lower the aggregate schooling of a cohort if the factory hires many unskilled workers at attractive wages, and many members of the cohort are of legal employment age and still attending school at the time of the factory arrival.

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 GATT in 1986 and 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.[1]

A unique data set makes this analysis possible. I match cohort average education (my skill measure calculated using 10 million schooling records from the 2000 Census) to the export-industry job growth in the cohort's municipality in the years the cohort turned age 15 and 16 (calculated using annual firm-level employment data for the universe of formal sector firms).[2] At these key school-leaving ages, compulsory education concludes and formal employment is first possible. I can then compare the school attainment of cohorts within a municipality who reached their key school-leaving 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 formal firm employment decisions. In the context of my panel of 1,808 municipalities and 13 cohorts, the exogeneity requirement is that firm employment decisions do not respond to deviations in the schooling of an individual cohort. I instrument employment changes with changes attributable solely to large single-firm openings, closings, expansions and contractions. I argue that such sizable expansions or contractions are associated with large fixed costs and not driven by changes in the labor supply of a single cohort of youths.[3]

I find that the cohorts who reached their key schooling leaving ages during years of substantial expansions in export-industry employment in their municipality obtained relatively fewer years of school compared to less exposed cohorts. This finding is not the result of new export manufacturing opportunities raising the education of all cohorts in the municipality, but raising education the least among the cohorts at key school-leaving ages. (The change in school attendance of 16 year olds between the 1990 and 2000 censuses was smallest in the municipalities with the largest export-industry employment growth). The magnitudes I find suggest that for every twenty new jobs that arrived, one student dropped out of school at grade 9 rather than continuing on through grade 12.

In the second part of the empirical analysis, I use the full distribution of manufacturing employment to verify the predictions of the conceptual framework. I confirm that high-skill manufacturing job arrivals at key school-leaving ages induce school acquisition, while low-skill arrivals increase school dropout. The dropout effects of low-skill job arrivals are accentuated when

[1]Trade liberalization in Mexico has been associated with an initial rise in the skill premium until the mid 1990's (Cragg and Epelbaum 1996, Hanson and Harrison 1999), followed by a skill premium decline (Robertson 2004, Airola and Juhn 2005, Lopez-Acevedo 2006).

[2]I restrict attention to the non-migrant population of Mexico since the location of migrants at ages 15 and 16 is unknown. In section 6.2, I show that composition bias due to selective migration cannot explain my findings.

[3]This is especially true in Mexico, where a large quantity of migrant and informal labor ensures that changes in the dropout decisions of a single cohort comprise a very small part of the potential labor that a firm can hire.

high wage premia are on offer to unskilled workers and there are many youths on the dropout margin at ages 15 and 16. Counterfactuals suggest that had the new export jobs possessed the skill distribution of the Mexican formal service sector, or arrived in less educated areas in the South of the country, then the schooling reductions I document would not have occurred.

In the final part of the empirical analysis, I present three key robustness checks in support of my finding that export expansions reduced schooling: sex-specific educational attainment only responds to new export job opportunities for that particular sex; dropout from low-skill export jobs is largest at the key school-leaving ages and dissipates at older ages; and reductions in cohort schooling derive from sensible changes in the distribution of education within cohorts.

One potential explanation for my findings is that the return to on-the-job training exceeds the return to schooling for the students who choose to drop out. However, I show that such a conjecture is false. By the end of the sample period, the youths induced to drop out by the arrival of low-skill export jobs are earning lower wages than they would have earned had these new jobs never arrived in their localities. Of course, despite finding long-run wage reductions, I cannot conclude that new export jobs adversely affected the welfare of these youths since I do not observe their discount rates. But a back of the envelope calculation suggests that students would have to possess very high discount rates of between 14 and 23 percent in order to rationalize the decision to forgo schooling for the immediate income gains that come with a new export job.

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.[4] Trade can induce divergent growth paths if positive externalities to education are incorporated into such a model (Stokey 1991). Wood and Ridao-Cano (1999) tests the hypothesis that trade reduces educational acquisition in unskilled labor abundant countries using a cross-country panel approach. However, it is difficult to infer causality in cross-country regressions, particularly when there may be feedback from changes in education levels to measures of trade openness such as the ratio of exports to 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 20th century United States, while Federman and Levine (2005) and Le Brun, Helper, and Levine (2009) find industrialization increased enrollments in Indonesia and had mixed effects in Mexico. This paper improves on these studies by drawing on a richer data set 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.[5]

[4] Ambiguous results obtain when credit constraints are incorporated(Chesnokova and Krishna 2009).

[5] This analysis focuses on the schooling of youths who were at school-leaving ages at the time of new export

Finally, a complementary literature looks at the educational impacts of the arrival of IT service jobs in India. Munshi and Rosenzweig (2006), Shastry (2008), Jensen (2009) and Oster (2010) all find positive enrollment impacts from the arrival of relatively high-skilled service job opportunities in India.[6] 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 employment distribution, they substantially raised the return to education.[7] Similarly, I observe that new high-skill manufacturing jobs increased educational acquisition for Mexican youth. Thus, by drawing on very disaggregated employment data across many industries and a large diverse country, this paper is able to improve on this literature by identifying the characteristics of the jobs that raise educational attainment and those that lower it.

The next section lays out the conceptual framework. Section 3 introduces the rich data set and discusses the empirical methodology. Section 4 investigates the effects of new export-manufacturing job arrivals on educational attainment. Section 5 explores the heterogeneity in the impacts of manufacturing job creation and performs several counterfactuals. Section 6 presents important robustness checks and discusses alternative interpretations related to migration or on the job training. Finally, section 7 discusses policy implications and concludes.

2 A Conceptual Framework for Understanding Educational Choices

In this section, I lay out a conceptual framework that clarifies the channels through which a new factory opening affects educational choices. The framework guides the empirical strategy and provides testable implications regarding the job and location characteristics that drive heterogeneity in these educational responses.

2.1 A stylized educational choice model

Forward-looking youths in a particular municipality and cohort choose among three discrete education levels, $s = (0, 1, 2)$. Members of this cohort c make an irreversible decision in period c to either stay on at school or enter the labor force with $s = 0$. In period $c+1$, students who chose to remain in school make a second irreversible decision to either enter the labor force with $s = 1$ or stay at school and obtain $s = 2$.

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

$$u(y_{s,c+s,t}) = \ln y_{s,c+s,t} \quad = \quad a_0 + \gamma s + b[t-c-s] + \varepsilon_{s,c+s}, \tag{1}$$

job arrivals. For evidence on positive schooling effects of trade liberalization on younger children through the household income channel, see Edmonds and Pavcnik (2005) and Edmonds, Pavcnik, and Topalova (2008).

[6] Heath and Mobarak (2012) finds similar outcomes for young Bangladeshi girls in garment producing villages.

[7] 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.

where a_0 is the base salary for an uneducated worker, γ is the return to an additional level of school and b is the return to an additional period of experience. The only unusual term is the stochastic $\varepsilon_{s,c+s}$, a persistent wage premium that is specific to workers with skill s entering the labor force in year $c+s$. This non-neoclassical feature generates the finding that schooling decisions respond to the number of job vacancies at key school-leaving ages.

These year-of-entry wage premia exist as only certain firms will offer a worker a job in any given year. Formal sector employment in Mexico is characterized by firm-specific non-compensating wage differentials and job rationing.[8] Therefore, the wage a worker receives will depend on which firm hires him or her. In a year when more formal firms are hiring, a student is more likely to be able to obtain (and retain) a job at a firm that pays persistently higher wages.[9] Accordingly, the year-of-entry wage premia are a weakly increasing function of net new formal factory jobs per working-age population, l_{c+s}, in the year of entry into the labor force, $c+s$;

$$\varepsilon_{s,c+s} = \varepsilon_s(l_{c+s}) \text{ with } \frac{\partial \varepsilon_{s,c+s}}{\partial l_{c+s}} \equiv \omega_{s,c+s}\phi_{s,c+s} \geq 0,$$

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

A student lives forever, cannot borrow or save and discounts at the rate ρ. An enrolled student in period $c+s$ is indifferent between dropping out with school s or obtaining exactly one more school stage, $s+1$, if the net present value is equal across the two options:

$$\sum_{\tau=c+s}^{\infty} \frac{u(y_{s,c+s,\tau})}{[1+\rho]^{\tau-c-s}} \quad = \quad u(\bar{y}) + \sum_{\tau=c+s+1}^{\infty} \frac{\mathbb{E}_{c+s} u(y_{s+1,c+s+1,\tau})}{[1+\rho]^{\tau-c-s}}. \tag{2}$$

Equations 1 and 2 determine discount rate cutoffs, $\bar{\rho}_{s,c+s}$, at which a student in period $c+s$ is indifferent between dropping out with school s or obtaining exactly $s+1$:

$$\bar{\rho}_{s,c+s} = \frac{\gamma + \mathbb{E}_{c+s}\varepsilon_{s+1,c+s+1} - b - \varepsilon_{s,c+s}}{a_0 + \gamma s + \varepsilon_{s,c+s} - \ln \bar{y}} \equiv \frac{RS_{s+1,c+s}}{OC_{s,c+s}}. \tag{3}$$

This expression is intuitive. The numerator corresponds to the perceived per-period utility gain from possessing an additional year of schooling (which I define as the "return to schooling" $RS_{s+1,c+s}$). The denominator corresponds to the utility difference between working this period and being at school (which I define as the "opportunity cost of schooling" $OC_{s,c+s}$).

I restrict attention to the simple case where both the return to schooling and opportunity

[8] These firm-specific premia may derive from efficiency wage, fair wage, insider bargaining or search models. Frias, Kaplan, and Verhoogen (2009) document firm-specific wage differentials in Mexico. Duval Hernandez (2006) presents evidence of formal sector job rationing in Mexico.

[9] Oreopoulos, Von Wachter, and Heisz (2006) present evidence for year-of-entry wage premia. Even within a firm, there may be wage premia that depend on the labor demand conditions during the year of entry due to optimal lifetime contracts for risk-averse credit-constrained workers (Beaudry and DiNardo 1991).

cost of schooling are positive and where the shocks ε are sufficiently small such that the ranking $\bar{\rho}_{0,c} > \bar{\rho}_{1,c+1}$ is always preserved.[10] Hence, a student with a high discount rate, $\rho \geq \bar{\rho}_{0,c}$, will choose to drop out of school with $s = 0$, a student with $\bar{\rho}_{0,c} > \rho \geq \bar{\rho}_{1,c+1}$ will choose to drop out of school with $s = 1$ and a student with a low discount rate $\rho < \bar{\rho}_{1,c+1}$ will obtain $s = 2$.

2.2 Aggregate schooling and new factory openings

In order to understand how factory openings affect cohort schooling, I introduce heterogeneity in discount rates. (The variable that controls a student's trade off between the return to schooling and the opportunity cost of schooling.) I assume ρ is distributed with probability density function $f(x)$ and cumulative density function $F(x)$ across a continuum of students within a cohort.

Year-of-entry wage premia $\varepsilon_{s,c+s}$ and at-school utility $u(\bar{y})$ may also vary across individuals. For example, a new factory may employ only high ability types and so only this subgroup enjoys a rise in their wage premia. This second layer of heterogeneity will not alter the key sign predictions on aggregate schooling as long as ability is independent of ρ. Therefore, I focus on the simpler case where discount rate cutoffs $\bar{\rho}_{s,c+s}$, are common across the cohort and aggregate schooling, S_c, of the cohort is pinned down by the two discount rate cutoffs:

$$S_c = F(\bar{\rho}_{0,c}) + F(\bar{\rho}_{1,c+1}). \tag{4}$$

I now explore the impact of an unanticipated and one-off factory opening in period c. The new factory opening generates a large number of vacancies, $l_c > 0$, in year c as the whole factory must be staffed at one time. In subsequent periods there are (known) smaller vacancy shocks, $l_{c+n} = \delta l_c$ with $\delta \in [0, 1)$. I assume that the factory's skill demands $\phi_{s,c}$ and wage premia $\omega_{s,c}$ are known and fixed over time (e.g. $\phi_{s,c+t} = \phi_{s,c}$ and $\omega_{s,c+t} = \omega_{s,c}$ for all t) .

The derivative of cohort schooling with respect to a factory opening is ambiguous and depends on the $\phi_{s,c}$s, the proportions of new jobs available to workers with various education levels:

$$\frac{dS_c}{dl_c} = f(\bar{\rho}_{0,c})\frac{d(\frac{RS_{1,c}}{OC_{0,c}})}{d\varepsilon_{0,c}}\omega_{0,c}\phi_{0,c} + \sum_{s=0}^{1} f(\bar{\rho}_{s,c+s})\frac{d(\frac{RS_{s+1,c+s}}{OC_{s,c+s}})}{d\varepsilon_{1,c+1}}\omega_{1,c}\delta\phi_{1,c} + f(\bar{\rho}_{1,c+1})\frac{d(\frac{RS_{2,c+1}}{OC_{1,c+1}})}{d\varepsilon_{2,c+2}}\omega_{2,c}\delta\phi_{2,c}. \tag{5}$$

The first term captures the impact of the low-skill job arrivals ($\phi_{0,c}l_c$) and is weakly negative. When these jobs arrive, the opportunity cost of schooling ($OC_{0,c}$) rises, the return to schooling ($RS_{1,c}$) falls and hence the discount rate cutoff $\bar{\rho}_{0,c}$ declines. Therefore, youths on the margin between $s = 0$ and $s = 1$ drop out of school. In contrast, the third term captures the impact of the increase in high-skill jobs available in the future ($\phi_{2,c}l_c$) and is weakly positive. With

[10] In the absence of any ε shocks, $\bar{\rho}_{0,c}$ is always larger than $\bar{\rho}_{1,c+1}$. If shocks to wage premia are very large, the student faces an optimal stopping problem. Staying at school now brings the option value associated with a large number of high paid jobs suddenly becoming available next period.

these job arrivals, the return to schooling ($RS_{2,c+1}$) rises for youth choosing between $s = 1$ and $s = 2$ and hence the discount rate cutoff $\bar{\rho}_{1,c+1}$ declines. Mid-skill jobs ($\phi_{1,c}l_c$) have ambiguous effects since they raise the $\bar{\rho}_{0,c}$ cutoff but lower the $\bar{\rho}_{1,c+1}$ cutoff.

Implication 1: *A factory opening has ambiguous effects on cohort average schooling,* $\frac{dS_c}{dl_c} \lesseqgtr 0$.

Implication 2: *Low-skill job arrivals lower cohort average schooling,* $\frac{\partial S_c}{\partial \phi_{0,c} l_c} \leq 0$*, high-skill job arrivals raise cohort average schooling,* $\frac{\partial S_c}{\partial \phi_{2,c} l_c} \geq 0$.

Equation 5 highlights heterogeneity in impacts conditional on the skill level of the jobs arriving. First, the schooling impacts of either low- or high-skill job arrivals will be accentuated if the location has many youths around the relevant discount rate cutoffs. Such density differences arise naturally across locations if youths in richer locations obtain more parental income support while at school.[11] Second, the impact of a factory shock depends on the degree to which year-of-entry wage premia shift the cutoffs (location specific characteristics $|\frac{d\bar{\rho}_{0,c}}{d\varepsilon_{0,c}}|$ and $|\frac{d\bar{\rho}_{1,c+1}}{d\varepsilon_{2,c+2}}|$), and the degree to which this particular factory raises year-of-entry wage premia (job specific characteristics $\omega_{0,c}$ and $\omega_{2,c}$).

Implication 3: *The dropout effects of the lowest-skill job arrivals and the acquisition effects of highest-skill job arrivals are accentuated by the density of youths at the appropriate discount rate cutoff,* $f(\bar{\rho}_{0,c})$ *or* $f(\bar{\rho}_{1,c+1})$*, and the magnitude of the shift in the cutoff,* $|\frac{d\bar{\rho}_{0,c}}{d\varepsilon_{0,c}}\omega_{0,c}|$ *or* $|\frac{d\bar{\rho}_{1,c+1}}{d\varepsilon_{2,c+2}}\omega_{2,c}|$.

The return to schooling in Mexico rose pre-NAFTA before falling in the late 1990's (see references in footnote 1). Against this backdrop, I note that a factory opening can simultaneously raise the return to schooling and lower cohort schooling. If new export jobs raise all manufacturing wages but relatively more so for higher-skill positions, both the return to schooling and the opportunity cost of schooling will rise. For small rises in the return to schooling, school dropout increases since the rise in the opportunity cost is necessarily larger.[12] Rises in manufacturing wages for high-skill workers will have particularly muted impacts on school acquisition if there are few high skill jobs in the industry (low $\phi_{2,T}$) or little turnover (low δ).

In conclusion, a new factory opening has ambiguous effects on cohort schooling. On the one hand, an abundance of new job opportunities that require relatively low levels of schooling raises the opportunity cost of schooling and reduces educational attainment. On the other hand, a factory opening that brings many skilled jobs raises the return to schooling, and increases educational attainment. The effects of both low- and high-skill job arrivals are accentuated if factories locate in areas where there are many youths on the relevant margins and if the factories offer unusually high wage premia for the skill level of job they demand.

[11] The opportunity cost of schooling ($a_0 + \varepsilon_{0,c} - \ln \bar{y}$) will be lower and the discount rate cutoff $\bar{\rho}_{0,c}$ will be higher in these locations, and hence densities $f(\bar{\rho}_{0,c})$ will differ if $f'(x) \neq 0$.

[12] For example, in the case of only two schooling levels ($f(\bar{\rho}_{1,c+1}) = 0$), equation 5 and equation 3 imply that $\frac{dRS_{1,c}}{dl_c} > 0$ and $\frac{dS_c}{dl_c} < 0$ if $1 + \bar{\rho}_{0,c} > \frac{d\varepsilon_{1,c+1}}{dl_c} / \frac{d\varepsilon_{0,c}}{dl_c} > 1$.

3 Empirical Implementation

3.1 Data

I combine two sources of data to explore how educational attainment responds to job arrivals 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). I cannot append family backgrounds as the older cohorts in my sample have left their parental homes by the time of the Census. The 10.1 million person records cover all 2,443 Mexican municipalities. For reasons discussed in section 3.2, 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 insurance 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 municipality.[13] The data cover 2.2 million firms between 1985 and 2000, with annual employment recorded on December 31st of each year. Sample means for both data sources are shown in table 1.

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 an industry category, but do not indicate whether a firm exports. Therefore, I define a firm as an exporter if it belongs to a 3-digit ISIC classification (Rev. 2) industry where more than 50 percent of output was exported for at least half the years in the sample.[14] The resulting export industries are: Textiles; Apparel; Shoes; Leather; Wood and Furniture; Electrical, Electronic, Transport and Scientific Equipment; Toys, Clocks and Ceramics.[15]

Between 1986 and 1999, employment growth in these export-intensive industries accounted for 73 percent of the growth in IMSS-insured manufacturing employment. Figure 1 displays the annual employment growth in both export and non-export manufacturing industries. While not all of the jobs in the industries that I classify as export manufacturing are in firms that export, the majority are. In 2000, there were 2.3 million formal jobs in my export manufacturing grouping. 1.2 million of these jobs were in Maquiladora firms according to INEGI aggregate Maquiladora statistics. (Maquiladora job growth accounts for 59 percent of export-industry job growth and is plotted in figure 1.) All of these Maquiladoras are exporters since these export-assembly plants are legally required to export almost all their production.[16] A large

[13]The aggregations from the firm to municipality level were carried out at ITAM, where the data were held securely. Kaplan, Gonzalez, and Robertson (2007) contains further details on the IMSS data.

[14]The industry categories used by IMSS, the 2000 Census and the 3-digit ISIC classification (Rev. 2) were matched by hand. The 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.

[15]Online Appendix C provides further details regarding the export orientation of firms by industry.

[16]These firms were initially confined to border areas and employed mainly women, but by the year 2000 one quarter of firms were in non-border states and half the employees were male.

number of the remaining 1.1 million export industry jobs are also in exporting firms. For example, the 2000 Encuesta Industrial Annual (EIA) surveys 7,000 large non-Maquiladora firms. Of the 290,000 EIA jobs in export industries, 43 percent are at firms that export more than 25 percent of their output and 76 percent are at firms exporting more than 10 percent.[17] In section 4.1, I use these two additional data sources (the Maquiladora statistics and the EIA) to perform robustness checks using job creation at known exporters.

The education distributions among young workers in the 2000 Census are broken down by industry in figure 2. Formal sector employees in export manufacturing industries are substantially less skilled than those in non-export manufacturing industries; 81 percent of export-industry employees have less than a high school education compared to 74 percent of non-export employees. In comparison to manufacturing, formal service sector jobs are highly skilled and informal jobs are the least skilled. Additionally, export manufacturing employees are younger with 11 percent of employees age 18 or under in the year 2000 as opposed to only 6 percent for non-export manufacturing.

I combine the education and employment data using the 1985 municipal boundaries. In order for each location to represent a single labor market, I merge together any municipalities in either the same Zona Metropolitan (as classified by INEGI) or the same commuting zone.[18] These adjustments result in a panel of 13 cohorts across 1,808 municipalities.[19]

Finally, I restrict the sample to non-migrants. I define a non-migrant as someone who reports being born in the same state they are currently living in and who also lived in their current municipality 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 non-migrants who comprise 80 percent of the full Census sample. In section 6.2, I provide econometric evidence that selection biases related to migration cannot explain my finding that export job arrivals reduced schooling.

3.2 Empirical Specification

The impact of new export job opportunities on cohort schooling is ambiguous (conceptual framework Implication 1). To determine the sign of the relationship in the Mexican context,

[17]In contrast, 14 percent of EIA jobs in non-export industries are at firms exporting more than 25 percent.

[18]I make this correction as if workers commute to nearby municipalities, the error terms will be spatially correlated. I classify commuting municipalities as those where more than 10 percent of the working population reported commuting to a nearby municipality in the 2000 Census. In the few cases where a municipality sends workers to two municipalities that do not send workers to each other, I create two synthetic municipalities both containing the sending municipality (but with the weights of individuals from the sending municipality halved).

[19]I lose one year of data when calculating employment changes. Since the Census was collected in February 2000, only firm data through 1999 is relevant. I am left with 14 years of data, but the two-year exposure window described in the next section reduces the panel to 13 cohorts.

I regress cohort schooling on local expansions in export manufacturing employment:

$$S_{mc} = \beta l_{mc} + \delta_m + \delta_m c + \delta_{rc} + \varepsilon_{mc}. \tag{6}$$

S_{mc} is the average years of schooling obtained by February 2000 for the cohort born in year c in municipality m.[20] The new export job opportunities measure, l_{mc}, is the formal employment growth in export-oriented manufacturing industries in municipality m divided by the working-age population. I sum this measure over the two years that the cohort turned age 15 and 16:[21]

$$l_{mc} = \frac{\Delta export\, employment_{m,c+16}}{working\text{-}age\, population_{m,c+16}} + \frac{\Delta export\, employment_{m,c+15}}{working - age\, population_{m,c+15}}.$$

I include municipality fixed effects, δ_m, municipality-specific time trends, $\delta_m c$, and a full set of state-time dummies, δ_{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.) The state-time dummies control in a flexible manner for the fact that education trended upwards during the period, but at different rates across Mexico.[22] The municipality-specific time trends control for the fact that within states, low-education municipalities may be catching up with higher-education municipalities.

I exclude Valle de México (the metropolitan zone that includes Mexico City) from my sample since it constitutes two entire states and hence is swept out by the state-time dummies.[23] I weight each cohort-municipality observation by the number of individuals the cell represents. Hence, my results are representative of the Mexican non-migrant population excluding Valle de México.

The main specification focuses on new jobs arriving in the two year period in which the youth turned 15 and 16. I dub this period the "key exposure age" for two reasons. First, compulsory schooling in Mexico ends with Secundaria (grade 9). Most children complete this grade at either age 15 or 16 depending on their birth date (and the Census only reports current age not the date of birth). Although the compulsory schooling law only dates from 1992 and enforcement is rare, ages 15 and 16 are still the two most common school-leaving ages and the age at which the decision to attend high school is made. Second, formal sector factory jobs first become a direct alternative to school at this age, as the legal minimum age for factory work is 16.[24] Therefore, Implication 3 of the conceptual framework suggests that an abundance of new export-manufacturing vacancies will disproportionately affect the school decisions of the cohorts

[20] I do not use data from 1990 Census in calculating S_{mc} as these data only cover 3 cohorts. Additionally, it is not clear how to weight these observations as the sampling methodology changed between 1990 and 2000.

[21] I obtain the working-age population data by linearly interpolating the municipality population aged 15-49 in 1990, 1995 and 2000 (available from INEGI).

[22] 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.

[23] As a robustness check, I replace state-time dummies with region-time dummies and include Mexico City.

[24] The minimum working age is 14. However, children under 16 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.

aged 15 and 16 at the time. Many youths are on the dropout margin at this age, and the jump in the opportunity cost of schooling is particularly pronounced since direct employment is possible. Section 6.1 examines many other exposure ages and provides empirical support for the conjecture that ages 15 and 16 are the key exposure ages.

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 ages to older and younger cohorts in the same municipality who did not have such a shock to their employment opportunities at these ages. I flexibly control for time effects using the cohorts with same year of birth living in nearby municipalities where factories did not open at key school-leaving ages. I now turn to discussing the potential threats to identification and present a novel instrumentation strategy.

3.3 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 municipality's education level and its attractiveness as a location for a firm. Using the municipality fixed effects, I am able to sweep out time-invariant features of the municipality. The state-time dummies control for omitted variables that change over time within the 32 states of Mexico. Finally, municipality-level time trends control for any omitted variable that varies over time within municipality in an approximately linear fashion.

There are two omitted variables that may affect schooling and be correlated with detrended employment changes over time within municipalities. First, a factory may agree to make complementary investments when it opens, for example building a school. Unfortunately, there are no suitable annual data available at the municipality level to serve as time-varying controls. Therefore, I rely on the fact that such complementary investments would affect all cohorts, with younger cohorts exposed for more years and likely to see larger effects (the opposite to my findings). Additionally, Helper, Levine, and Woodruff (2006) report that school building decisions in Mexico were made at the national level prior to 1992 and at the state level afterward, with little municipality say in either time period. Second, there may be local demand shocks that both affect schooling decisions and alter the demand for local manufacturing output. In order to address this concern, I primarily focus on export industries whose demand derives from foreign rather than local consumers.[25]

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.[26] If new factories do lower education and low schooling levels attract

[25] In Online Appendix D, I incorporate non-export job creation and focus only on industries where production is geographically agglomerated and hence job creation is driven by national rather than local demand factors.

[26] Bernard, Robertson, and Schott (2004) show that factor prices are not equalized across Mexico, resulting

factories, $\hat{\beta}$ will be biased in an ambiguous direction. In a panel setting, reverse causality will bias the coefficients if deviations in de-trended cohort schooling affect firm employment decisions in the past, present or future. Therefore, while a firm may wish to locate in a less skilled location, or in a location where skills are declining over time, the firm's decision to open in a particular locality must not be influenced by a single cohort with an unusually strong aversion to schooling.

In order to deal with reverse causality, I instrument for net new export jobs per worker, l_{mc}, with 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). The instrument is highly correlated with net new jobs per worker as these large single firm changes comprise 69 percent of the total change in employment over the period. For the instrument to be exogenous to the error term in equation 6, I require that firms respond to cohort schooling deviations only through the small expansions and contractions that are excluded from my instrument.

An unusually high dropout rate for the current cohort of youths is unlikely to drive large (and costly) expansions and contractions. First, a single cohort is a very small component of the local skill distribution and so total labor supply is unresponsive to small annual deviations in local dropout rates. This assumption is especially plausible for Mexico, which has a large number of both informal and migrant workers competing for formal sector jobs.[27] Second, I focus on exporting industries. Large expansions in these industries are generally driven by external demand factors interacted with stable municipality characteristics (distance to US border, existing input suppliers etc.). This is the same logic behind the Bartik (1991) style instruments used in the trade literature by Topalova (2010) and others to explore the impact of employment contractions induced by import competition. I note that the Bartik (1991) approach is not feasible in the context of export expansions or a high-frequency panel.[28] Third, entrepreneurs must obtain cohort-varying information about education levels in a municipality, which is not readily available.

For the three reasons above, large single-firm expansions and contractions are unlikely to be influenced by current cohort schooling deviations. However, I also require that future and past schooling deviations do not cause large expansions and contractions. Future deviations in cohort education are unknown at the time of the firm's decision and so presumably will not affect location decisions. In contrast, several years of serially-correlated shocks to schooling in the recent past, for example a school closure, could have a non-negligible effect on the current labor supply and hence location decisions. Three factors limit the size of the bias in this case. First,

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 dropout results in one new formal employee.

[27]Only one third of Mexican working-age adults are in formal private sector employment.

[28]The initial (pre 1986) local industrial structure interacted with national employment growth by industry is not a good predictor of annual variation in new export factory openings within a municipality.

any correlation between past schooling deviations and current location decisions will be divided by the number of cohorts in my panel (thirteen). 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 municipality linear time trend.

An additional specification addresses the issue of reverse causation head on. I explicitly control for the the schooling levels of the four previous cohorts of youths by including four lags of S_{mc}. These lags soak up the component of the error term that is correlated with l_{mc} through the serial correlation in schooling.[29] As lagged dependent variables are necessarily be correlated with the error term in a panel regression, I exclude the municipality fixed effects and trend. In conclusion, the IV strategy and the lagged dependent variable specification plausibly deals with the issue of reverse causation.

The third and final econometric concern is measurement error in employment changes. IMSS registration defines firm formality. However, some firms existed informally prior to registering with IMSS, thus formalization will appear in my data set as genuine new job creation. Such measurement error will attenuate the coefficients and could also bias my results if an omitted variable both encouraged firms to register and affected education choices. The instrumental variable strategy above also mitigates this concern. Large firm expansions and contractions can only occur in larger firms that would find it very difficult to evade IMSS registration.

Finally, I cluster all standard errors at the municipality level to prevent misleading inference due to serial correlation in the error term across years within a municipality (Bertrand, Duflo, and Mullainathan 2004). The large number of groups (1808 municipalities) mitigates concerns regarding spurious correlation (Baltagi and Kao 2000).

4 Basic Results

Figure 4 shows the basic econometric strategy for the 30 municipalities that experienced the largest change in export-industry employment per worker over the sample period. The figure plots the residuals from the regression of both cohort schooling and net new export manufacturing factory jobs per worker at ages 15 and 16 on the remaining terms in equation 6. In many of these municipalities, cohort schooling fell in years when there was an unusually large amount of export job creation and vice versa.

The basic specification, regression 6, aggregates these effects over all 1808 municipalities. Column 1 of table 2 contains the ordinary least squares (OLS) results. Column 2 contains the

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

instrumental variable (IV) results, in which I instrument net new export-manufacturing jobs per worker with the net new export-manufacturing 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 extremely significant. Column 3 contains the reduced form (RF) results from regressing cohort schooling directly on the instrument. Column 4 repeats the RF specification but also includes an additional variable to control for general employment trends at the municipality level (the net new jobs per worker at ages 15 and 16 created in the remainder of formal firms not included in the RF net new export-manufacturing job variable). Column 5 repeats the RF specification but explicitly includes four lags of cohort schooling instead of the fixed effects.

In all five specifications, the arrival of new export-manufacturing jobs at ages 15 and 16 significantly reduces cohort schooling ($\beta < 0$) with coefficients between -1.845 and -2.624. The differences between the OLS and IV specifications are small, suggesting that reverse causation is not sufficiently severe that it generates a spurious negative coefficient.

The interpretation of the coefficients from the IV and RF specifications are subtly different. The IV coefficient estimates the impact of export job arrivals at key school-leaving ages on cohort schooling. The RF coefficient estimates the effect of the subset of new export jobs that were created through large openings/expansions and closings/contractions. 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 reduced form coefficients.

The magnitude of the coefficient in table 2 implies substantial educational impacts. As a concrete example, the 90th percentile of the distribution of large firm expansions and contractions corresponds to 0.034 net new export jobs per worker over the two year exposure period. Using the reduced form coefficient, such a shock results in the exposed cohort obtaining 0.09 years less school on average. Alternatively, the coefficient I find would result from one student in the cohort dropping out at grade 9 rather than continuing on to grade 12 for every twenty new manufacturing jobs that arrived.[30]

[30] To calculate this number, I assume that a single cohort comprises 5 percent of the Mexican population aged 15-49. If every student who took a factory job obtained 3 years less education, and enough new jobs arrived to employ the entire cohort (0.05 jobs per worker), cohort average schooling would fall by 3 years. If only one in twenty of the new factory jobs induced a student in that cohort to drop out, cohort schooling would fall by 3/20=0.15 years with the arrival of 0.05 jobs per worker, and 3 years with the arrival of one job per worker. This is the approximate effect size I find. Of course, the cohort can obtain a higher proportion of the new jobs if some students would have dropped out anyway. These proportions seem reasonable: in the Census sample, 11 percent of export manufacturing workers are age 18 or younger.

As with any difference-in-difference regression, my results only imply that the education of cohorts heavily exposed to new factory openings at ages 15 and 16 fell relative to other cohorts in the municipality 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 municipality but relatively less for the cohorts aged 15 and 16. This interpretation would imply that the municipalities which experienced the largest growth in export employment saw the most dramatic increases in the school attendance of 16 years olds between the 1990 and 2000 censuses. Hence, I use cross-sectional variation to run the following regression:

$$Attend16_{m,2000} - Attend16_{m,1990} = \gamma \sum_{t=1990}^{1999} l_{mt} + \varepsilon_{mc}, \tag{7}$$

where $Attend16_{m,t}$ is the proportion of the cohort aged 16 at the time of the Census in year t in municipality m who are attending school. Similar results are reported using school attendance at age 15 as the dependent variable. The independent variable, $\sum_{t=1990}^{1999} l_{mt}$, is the total change in the number of export jobs per worker between January 1st 1990 and December 31st 1999. Once more, I present reduced form regressions using job changes attributable to large single firm expansions or contractions. I also report results that include the initial level of school attendance as an independent variable to control for low education municipalities catching up.

The regression results are shown in table 3. The coefficient on the export employment variable is significant and negative in all the specifications. School attendance rose the least in the municipalities that saw the most export job growth over the decade. Therefore, I conclude that expansions in export-industry employment in a locality led not just to relative but also absolute declines in schooling for the cohorts in the locality aged 15 and 16 at the time.[31]

In section 6, I present three placebo-type tests that suggest that these results do originate from students weighing up the opportunity cost of, and the return to schooling. Alternative explanations due to migration or on the job training are also dismissed in this section. In Online Appendix B, I demonstrate the robustness of the basic results to many additional specifications: removing the various time fixed effects and trends, considering alternative samples (excluding 781 small municipalities with no formal sector, excluding metropolitan areas or big cities, including Mexico City, breaking results up by region and sex) and exploring alternative specifications (using different schooling measures, extending the threshold of my instrument, breaking up job expansions and contractions, and allowing for state-level spillovers). In summary, there is a robust negative impact of new job arrivals in export manufacturing on the educational attainment of cohorts aged 15 and 16 at the time.

[31]It also seems implausible that the new export jobs led to educational attainment of 15 and 16 year olds rising across Mexico but less in the particular locations where the jobs were actually arriving.

4.1 Alternative Measures of 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 corroborate my findings using two additional data sources that allow me to identify a subset of known exporters.

The Encuesta Industrial Anual (EIA) and Encuesta Industrial Mensual (EIM), survey around 7,000 large non-Maquiladora firms. Crucially, the EIA data contain the proportion of production exported and the EIM contain the number employees in each firm. I code a firm as an exporter if more than a certain percentage of its output is exported. I present results for two thresholds, either 10 percent or 25 percent of output exported. There are two problematic features of these data. First, the INEGI sampling methodology is not designed to capture new firm openings since the panel was only refreshed once over the period and there are no clear criteria for inclusion. Second, the data prior to 1993 do not contain municipality identifiers that can be matched to the Census municipalities. Accordingly, I follow the methodology of Verhoogen (2008) exactly and create a consistent EIA panel of 1,114 firms that are present in every period of the sample and calculate the annual change in employment among exporting firms in each municipality. As I am restricted to the consistent panel, all of these changes are purely on the intensive margin.[32]

It is also possible to approximately identify the firms that are Maquiladoras in my full IMSS sample by matching firm level employment data to INEGI aggregate statistics on annual Maquiladora employment by industry, state-industry and municipality.[33] Since Maquiladoras are legally required to export almost all their output, all these firms must be exporters.

Between 1986 and 1999, the EIA exporter panel (25 percent cutoff) and the Maquiladoras accounted for 67,670 and 985,232 net new jobs respectively (both excluding Mexico city). In contrast, the job creation attributed to export-industry firms in my main analysis was only 1,529,926.

Columns 1 and 2 of table 4 reruns the basic specification, equation 6, but using the new job creation attributable to this sub-sample of known exporters in lieu of the new export-manufacturing job measure used previously. With all these export jobs grouped together, there is a significant negative impact of new export job creation on schooling. The shocks attributable to Maquiladoras and EIA firms are included separately in columns 3 and 4. Both the coefficient on Maquiladora job creation and EIA exporter sample job creation (10 percent

[32] These data are confidential and only aggregate extracts were available (at the year-municipality-industry level). In the absence of firm level data, I use the total change in employment (the OLS regression) rather than RF specification. As a firm may become an exporter during the sample period, some of the export job creation in this exercise derives from non-export jobs become export jobs.

[33] These data come from the INEGI website. I classify firms as Maquiladoras when the number of employees in a given cell of the Maquiladora statistics (e.g. year-state-industry) is equal or greater than the number of employees in that cell in the IMSS dataset. The fact that each firm appear in several overlapping aggregates allows me to iterate this process until convergence. Due to the highly clustered nature of Maquiladora production in Mexico, I am able to classify all the potential Maquiladoras in 4 iterations.

threshold) are significantly negative and around the same magnitude as the coefficients in my main specification. In contrast, the coefficient on export job growth in the EIA exporter sample (25 percent threshold) is insignificant and slightly positive.

These results are not particularly surprising. The theoretical framework suggested that the skill levels of the new jobs arrivals was a critical determinant of the educational impacts. Maquiladoras are export assembly operations well known for demanding very low levels of skill. In contrast, the work of Verhoogen (2008) suggests that the demand for skill among Mexican non-Maquiladora firms increased with the proportion of sales a firm exports.

5 Exploring heterogeneity in the impact of new factory openings

The results of section 4 show that the massive policy-induced expansion of export manufacturing in Mexico reduced aggregate schooling for the cohorts who were at their key school-leaving ages at the time. However, the very different impacts of Maquiladora's and large non-Maquiladora exporters suggest that the basic results may be masking substantial heterogeneity. In this section, I use the conceptual framework to explore how the effects of new factory openings depend on the characteristics of both the factories and the municipalities in which the factories locate. In order to obtain the full range of job attributes, I draw on the entire universe of formal manufacturing employment shocks that Mexico experienced between 1986 and 1999.[34]

5.1 Job-specific skill requirements

Recall Implication 2 of the model: the opening of a factory employing relatively unskilled workers will raise the opportunity cost of schooling and discourage school acquisition, while a new factory employing relatively skilled workers will raise the return to schooling and encourage school acquisition. The regressions in section 4 bundle together both these types of jobs. Therefore, in order to test Implication 2, I require a skill-specific net new job shock.

Unfortunately, the IMSS employment data do not contain worker schooling levels. However, the 2000 Census records both the industry in which individuals work and their level of education. As Census and IMSS industry codes differ, I draw on the NAICS 3-digit classification to create a cross-walk containing 18 exhaustive and mutually exclusive manufacturing subindustries indexed by i (e.g. Food, Clothing etc.). I then combine worker education data from the Census with IMSS employment data to estimate the skill composition of each l_{mc} job shock.

I generate the skill-specific job shock as follows: For every municipality, I calculate the 33rd and 66th percentile of the distribution of education amongst all workers who belong to my 13 sample cohorts. I then compute for every municipality-subindustry pair the proportions $\phi_{s,mi}$ of

[34] As noted in section 3.3, the demand for domestic goods may be driven by local demand shocks which could be correlated with time-location-varying variables that affect schooling. Online Appendix D repeats the analysis of section 5 but for the subset of industries which have a high level of industrial concentration and so demand is likely to be driven by national rather than local demand shifters.

sample-age formal-sector workers that fell into the following three relative skill categories indexed by s:[35] (1) *Low skill*–The bottom third of the distribution of education across all sample-age workers in the municipality, (2) *Mid skill*–The middle third of this municipality education distribution, and (3) *High skill*–The top third of this municipality education distribution. I assume these proportions remained fixed within municpality-subindustry cells. Therefore, the proportion of each job shock l_{mc} that falls into each of the three skill categories is equal to $\sum_i \phi_{s,mi} l_{mci}$, where l_{mci} are net new job arrivals at ages 15 and 16 in subindustry i.

Figure 3 shows that the new manufacturing job arrivals are evenly distributed across the three relative skill categories (36 percent are categorized as low skill, and 33 percent high skill).

With these measures in hand, I rerun specification 6, but replace net new jobs at ages 15 and 16 with net new low-skill job creation per worker $\sum_i \phi_{LS,mi} l_{mci}$, mid-skill job creation per worker $\sum_i \phi_{MS,mi} l_{mci}$ and high-skill job creation per worker $\sum_i \phi_{HS,mi} l_{mci}$ at ages 15 and 16:

$$S_{mc} = \beta_1 \sum_i \phi_{LS,mi} l_{mci} + \beta_2 \sum_i \phi_{MS,mi} l_{mci} + \beta_3 \sum_i \phi_{HS,mi} l_{mci} + \delta_m + \delta_m c + \delta_{rc} + \varepsilon_{mc}. \quad (8)$$

Table 5 shows the results of this regression for net new job creation across all manufacturing industries. I report very similar results in section 6 for the subsample of export-manufacturing industries (alongside various placebo-type tests).

The ordering and signs of the coefficients are as predicted by Implication 2. Column 1 repeats the basic specification, specification 6, for the full manufacturing sample. As with the subsample of export-manufacturing jobs, I find that net new manufacturing job arrivals at ages 15 and 16 significantly reduce cohort average schooling (a coefficient of -1.69 on net new manufacturing jobs per worker in column 1). Column 2 shows that the negative aggregate schooling effects found in column 1 are driven by the arrival of relatively low-skill jobs, with a significantly negative coefficient of -9.02 on net new low-skill manufacturing jobs per worker. Conversely, when comparatively high-skill jobs arrive, educational attainment significantly rises, with a coefficient of 3.63 on net new high-skill manufacturing jobs per worker. Mid-skill jobs have an effect in between these two extremes, with a small and insignificant negative coefficient.

Table 5 also shows the results of a variety of alternative skill categorizations. In columns 3 and 4, I once more combine the subindustry-municpality job flows from the IMSS data with the education distributions of sample-age formal-sector workers from the 2000 Census. Column 3 divides net new job arrivals into low and high skill based on the proportion of employees who have

[35] I choose a relative skill measure as the educational requirements of any particular job are unknown (I only know the distribution of education among the workers who hold the jobs). In more educated municipalities, a job demanding the same level of skill will be carried out by higher education workers. Therefore, I use a measure of skill that is relative to the other jobs in the municipality (including informal and service employment). If a worker falls on the boundary of the educational terciles, the job is split between the two adjacent groupings. If no formal workers are observed in a cell, I sequentially include older workers, informal workers and workers in related industries.

less than high school (12 years of schooling), or high school and above. Column 4 divides net new job arrivals into low and high skill based on the proportion of employees categorized as production workers (low skill) or non-production workers (high skill) based on the 2-digit occupation codes in the Census. In column 5, I draw on a direct measure of job-specific skill levels that is available for the small subset of firms in the EIA panel described in section 4.1. The EIM survey records the number of production and non-production workers in each firm in each month. Therefore, column 5 divides net new job arrivals in the EIA panel into production and or non-production workers. All three specification lend support to Implication 2. The arrival of relatively low-skill manufacturing job opportunities induces significant reductions in schooling, while no such effects are found for the arrival of relatively high-skill manufacturing job opportunities.

5.2 Characteristics of factories and municipalities

Implication 3 makes further predictions about how the effects of a new factory opening should vary with the characteristics of both the factory and the municipality in which it opens. The effects of both low- and high-skill job arrivals are accentuated by the density of youths at the appropriate discount rate cutoff ($f(\bar{\rho}_{0,c})$ and $f(\bar{\rho}_{1,c+1})$) and the magnitude of the shift in the cutoff ($|\frac{d\bar{\rho}_{0,c}}{d\varepsilon_{0,c}}\omega_{0,c}|$ and $|\frac{d\bar{\rho}_{1,c+1}}{d\varepsilon_{2,c+2}}\omega_{2,c}|$). I test these two conjectures by interacting skill-specific net new job arrivals with proxies for the density of youths and the size of the cutoff shift.

First, I require proxies for $f(\bar{\rho}_{0,c})$ and $f(\bar{\rho}_{1,c+1})$, the density of youths around the dropout cutoffs. I use the proportion of youths in the cohort that obtain some high school (10 to 12 years of schooling) as their final level of schooling. Youths with some high school encompass the two groups likely to be most affected by new job arrivals at 15 and 16; those just above the secondary school dropout margin and those just below the cutoff for continuing beyond high school. Any cohort-level density measure will be correlated with cohort schooling and, hence, may be endogenous. Therefore, I also report results using the proportion of youths that obtain some high-school among the cohorts one and two years older than the oldest cohort in my sample.

Second, I require proxies for $|\frac{d\bar{\rho}_{0,c}}{d\varepsilon_{0,c}}\omega_{0,c}|$ and $|\frac{d\bar{\rho}_{1,c+1}}{d\varepsilon_{2,c+2}}\omega_{2,c}|$, the degree to which a particular skill-specific job shock shifts the discount-rate cutoffs. Broadly speaking, the magnitude of the shift is a function of municipality characteristics captured by $\frac{d\bar{\rho}_{s,c}}{d\varepsilon_{s,c}}$ (which depends, inter alia, on the gap between the base wage for a low-skill worker and the income equivalent utility from schooling, $a_0 - \ln \bar{y}$) and job specific features captured by $\omega_{s,c}$ (the wage premia the new jobs pay over existing job opportunities for that skill level). As good measures for the relevant municipality features are unavailable, I focus on the time-series variation in the wages on offer due to year-to-year changes in the subindustry composition of new job arrivals in the municipality.

Once more, I use the education data in the Census for formal workers in my sample cohorts. I calculate average log earned income, $\log y_{s,mi}$, for each skill-municipality-subindustry cell for recent entrants into the labor force.[36] I obtain a skill-municipality-cohort specific wage premium by computing a weighted average of these earned incomes using $\phi_{s,mi}l_{mci}$ (net new job arrivals at the skill-municipality-cohort-subindustry level) as weights. I assume that the municipality-specific components of $\frac{d\bar{\rho}_{s,c}}{d\varepsilon_{s,c}}$ and $\omega_{s,c}$ remain approximately constant over the sample period, and de-mean the weighted averages by municipality. The resulting variable is my proxy for the skill-specific "wage premium". The measure contrasts years when most of the new skill-s manufacturing jobs that arrived in the municipality were in subindustries paying low wages to skill-s workers with years when most new skill-s job arrivals were in subindustries paying high wages to skill-s workers.

Denoting the proxy for the density of youths as $\widehat{f_m(\bar{\rho}_{mc})}$, the proxies for the job-specific wage shifters as $\widehat{\omega_{s,mc}}$, and skill-specific job shocks $\sum_i \phi_{s,mi}l_{mci}$ as $l_{s,mc}$, I run the following specification:

$$\begin{aligned} S_{mc} = {} & \beta_1 l_{LS,mc} + \beta_2 l_{MS,mc} + \beta_3 l_{HS,mc} + \beta_4 \widehat{f_m(\bar{\rho}_{mc})} l_{LS,mc} + \beta_5 \widehat{f_m(\bar{\rho}_{mc})} l_{MS,mc} \\ & + \beta_6 \widehat{f_m(\bar{\rho}_{mc})} l_{HS,mc} + \beta_7 \widehat{\omega_{LS,mc}} l_{LS,mc} + \beta_8 \widehat{\omega_{MS,mc}} l_{MS,mc} + \beta_9 \widehat{\omega_{HS,mc}} l_{HS,mc} \\ & + \beta_{10} \widehat{f_m(\bar{\rho}_{mc})} + \beta_{11} \widehat{\omega_{LS,mc}} + \beta_{12} \widehat{\omega_{MS,mc}} + \beta_{13} \widehat{\omega_{HS,mc}} + \delta_m + \delta_m c + \delta_{rc} + \varepsilon_{mc}. \end{aligned} \tag{9}$$

Implication 3 predicts that the proxy interactions will be positive for high-skill shocks, negative for low-skill shocks and ambiguous for mid-skill shocks. Table 6 displays the results of these regressions. Columns 1 and 2 shows the interactions of skill-specific shocks with the two different proxies for the density of marginal youths. Column 3 shows the interactions with the wage premium proxy. Columns 4 and 5 contain both the density and wage premium interactions. The sign predictions are supported for all the interactions. The interactions with low-skill job arrivals are significantly negative in all columns. However, the high-skill job interactions are only significantly positive for the density measure in columns 1 and 4. The school dropout effects of new manufacturing jobs are driven by low-skill jobs offering high wage premia to low-skill workers arriving in municipalities with many youths on the dropout margin.

Finally, I return to the finding of section 4 that the arrival of new export jobs at ages 15 and 16 reduced cohort schooling. Columns 6 to 11 include the net new job arrivals in export manufacturing industries in addition to the various interactions. Once the job characteristics are controlled for, new export jobs no longer have significantly negative impacts on schooling. The coefficient on net new export jobs falls from a significant -2.618 (table 2) to insignificant coefficients in the range between -1.285 and -0.939. Most of this attenuation is due to the inclusion of the skill-specific job arrival measures. There is nothing extraordinary about new export-manufacturing jobs. These jobs simply possessed certain characteristics (many low-skill jobs offering high wage premiums) and located in a certain set of municipalities (those with

[36] I define new entrants as employees whose age minus years of schooling is between 6 and 11. If no formal sector wage is observed for a particular municipality-subindustry-skill cell in my data set, I substitute the wages of older workers, then the wages of informal workers and finally the wages of workers in similar subindustries.

large numbers of marginal youths) conducive towards a large net dropout effect.

5.3 Mexico's Counterfactual Education Distribution Without Export Jobs

The results of the previous sections can be used to explore how schooling would have responded if the massive influx of new export jobs had possessed different characteristics or had located in different parts of the country.

Prior to exploring this counterfactual, I estimate the aggregate impacts of the actual inflows and verify that these estimates are plausible. To do this, I obtain predicted values by multiplying the coefficient on net new export job arrivals from column 3 of table 2 by the actual export industry employment shocks, l_{mc}, that arrived between 1986 and 1999. I then calculate the weighted average of these predicted values across all municipalities and cohorts. Net new job arrivals in export industries reduced schooling by an average of 0.012 years for the cohorts aged 16-28 in the year 2000 (row 1 of table 7). This reduction in schooling would be the result of 58,169 students forgoing exactly three year education—secondary or high school for example—in order to take a new export job. In the 2000 Census, 698,473 members of my sample aged 16-28 were employed in the export industries. The estimates seem reasonable as many students may choose to work in the export industries without altering their education decision, may forgo less than three years of school or may not be directly employed at the factory.

I also obtain a mean decline of 0.012 years of school if I multiply the skill-specific coefficients from column 2 of table 5 by the export employment shocks broken down by relative skill level in the manner described in section 5.1 (row 2 of table 7).[37] This number can be contrasted with the results of a counterfactual that explores what would have happened had the export jobs demanded a different distribution of skills. As seen in figure 2, formal service sector jobs are relatively more skilled than formal manufacturing jobs. In terms of my relative skill measure, only 18 percent of the new formal service sector jobs fall within the bottom third of the local education distribution, while 57 percent fall in the top third. New service sector employment opportunities are unlikely to be exogenous to local demand shocks and so are not analyzed using my empirical methodology. However, I can ask the question what would have happened if the manufacturing jobs that arrived in Mexico 1986-1999 had been high-skill manufacturing jobs that demand a distribution of skills similar to formal sector service jobs? If every new export manufacturing shock had the same skill proportions as the service sector proportions referenced above, there would have been essentially no decline in cohort average education (row 3 of table 7).

Finally, I obtain a mean decline of 0.016 years of school if I multiply the coefficients from

[37] Results are almost identical if I use the coefficients on skill-specific job arrivals from the regression reported in table 8 that considers only the export industry subsample (0.014 years or 70,737 students forgoing 3 years of education). As I show in section 6.1, there are also (smaller) effects at other ages of exposure and the total effect rises to 0.019 years if these are accounted for.

column 5 of table 6 by the skill-specific export employment shocks and the appropriate density and wage premia proxies. This number can be contrasted with the results of counterfactuals that explore what would have happened had the export jobs arrived in different locations and paid different levels of wage premia. In both cases, I replace the actual values of my proxies for density and wage premia with the 25th percentile of the (population weighted) distribution of these proxies. Had the new export jobs been concentrated in less educated municipalities where there were fewer youths on the dropout margin at ages 15 and 16, there would have actually been a small increase in schooling of 0.001 years (row 6 of table 5.2). In contrast, the negative schooling impacts would not have been altered if new export job growth had been concentrated in subindustries with relatively low wage premia.

6 Robustness Checks and Alternative Interpretations

Before concluding, I perform three placebo-like robustness checks and explore several alternative interpretations of my main result: that the arrival of new export-manufacturing jobs at ages 15 and 16 significantly reduced cohort schooling.

6.1 Evidence that students weigh up opportunity cost and return to schooling

I present three tests to convince the reader that my findings are not spurious but derive from students weighing up the opportunity cost of schooling and the return to schooling: (1) sex-specific educational attainment only responds to new export job opportunities for that particular sex, (2) dropout from low-skill export jobs is largest at ages 15 and 16 when the opportunity cost is largest and dissipates at older ages of exposure when educational decisions are complete, and (3) reductions in schooling come from an increase in the number of students not progressing beyond the schooling stage they were at when the new export jobs arrived.

If students weigh up the opportunity cost of schooling and the return to schooling, female export job expansions should primarily affect women and male export job expansions should primarily affect men. The IMSS data break down job growth by sex and allow me to replace the three skill-specific job arrivals measures in equation 8 with six sex- and skill-specific job arrivals measures.[38] Column 1 of table 8 regresses the average schooling of males in each cohort on these six measures for net new export jobs per worker at ages 15 and 16. Column 2 runs the equivalent regression for female schooling. Column 3 reports the standard (non sex-specific) specification in equation 8 but for net new jobs per worker in export manufacturing rather than all manufacturing.

As predicted, male schooling declines with male employment growth in low-skill export jobs and rises with male employment growth in high-skill export jobs (although only the low-skill result is significant). In contrast, the signs on the female employment growth variables

[38] Skill categories are calculated as previously except now using only male (female) job arrivals and male (female) individuals from the Census to calculate male (female) skill categories.

are inverted and insignificant. For female schooling the pattern is reversed, with the largest dropout effects coming from female employment growth in low-skill export jobs and the largest acquisition effects coming from female employment growth in high-skill export jobs (again only the low-skill result is significant). Interestingly, the magnitudes of the coefficients on new low-skill export job arrivals are similar across both sexes.

If students weigh up the opportunity cost of schooling and the return to schooling, the dropout effects of new low-skill export jobs should be most pronounced if those jobs arrive at ages 15 and 16 and should dissipate at older ages. At ages 15 and 16 the opportunity cost of schooling channel is magnified for the two reasons documented in section 3.2. First, direct employment in the factory becomes legal (hence the factory wage premia are available to dropouts and the shift in the discount rate cutoff is large). Second, this age is both the legal and modal school-leaving age (hence a large density of youths are close to the dropout margin). Meanwhile, new export job arrivals at older ages should have smaller schooling impacts since the jobs arrive after most of the cohort has already left school for good. In figure 5, I plot the coefficients from running both specifications 6 and 8 but replacing export job shocks at ages 15 and 16 with export job shocks at every age of exposure between 11-12 and 20-21.[39]

As predicted, the dropout effects of both new export job opportunities and new low-skill export job opportunities peak at ages 15-16, justifying my choice of this age as the key exposure age. Results are also shown for regressions of sex-specific export jobs on sex-specific schooling. The peak at age 15-16 for low-skill export job arrivals is even more stark in the sex-specific specifications, although these estimates are substantially noisier. For high-skill export jobs, where education decisions are altered by changes in the return to schooling not opportunity costs, the largest school acquisition effects are from job arrivals at ages 13-14. Reassuringly, the effects of all three type of shock dissipate as students grow older, when more students have finalized their educational decisions. A counterfactual exercise similar to section 5.3 generates an average cohort schooling reduction of 0.019 years due to export job creation between 1986 and 1999.[40]

If students weigh up the opportunity cost of schooling and the return to schooling, new low-skill export jobs should increase the proportion of students obtaining no additional schooling

[39] The job shocks are likely to be serially correlated over time. Unfortunately, the shortness of my 13 cohort panel precludes the simultaneous inclusion of many exposure ages. Therefore, although the movements in the coefficients across different exposure ages are informative, this approach cannot identify large jumps in the effect size at adjacent ages. In results available on request, I find evidence of complementarities between job arrivals at different ages. I include job shocks at age 13/14, job shocks at age 15/16, and the interaction of the two. The two main effects are significantly negative and the interaction term significantly positive, suggestive of the fact that many of the youths who would have dropped out at 15/16 with a new factory opening drop out at 13/14 instead.

[40] I multiply the coefficients from running specification 8 for every exposure age between 11 and 21 by the skill-specific export shocks that each cohort experienced. The average school reduction is calculated for cohorts aged 16-28 in 2000. This exercise should be treated with caution since every regression includes municipality and state-time fixed effects, and the municipality and state-time means differ across regressions.

beyond the level they were at when the new export jobs arrived. In contrast, new high-skill export jobs should increase the proportion of students obtaining higher levels of education. I rerun the specification in equation 8 but replace cohort average schooling by four new dependent variables; the proportion of the cohort whose highest completed schooling stage was primary school, secondary school, high school and university respectively.[41] Table 9 displays the results from the four regressions. As predicted, low-skill export jobs significantly increase the proportion of the cohort who do not graduate beyond secondary school and reduce the proportion of the cohort obtaining higher schooling stages. High-skill export jobs increase the proportion of students obtaining university educations.

Taken together, these three pieces of evidence provide strong support that my findings do derive from students trading off the opportunity cost of, and return to, schooling. The arrival of low-skill export jobs at the key ages of 15 and 16 substantially raised the opportunity cost of schooling and tipped a segment of the population towards dropping out of school. When relatively high-skill export jobs arrived, school attainment rose along with the return to schooling.

6.2 Selective migration

My results only pertain to the population of non-migrants. As the Census does not record where migrants were living at ages 15 and 16, I cannot match these individuals to local job opportunities at these ages. Therefore, I exclude migrants from my sample.[42]

Many export manufacturing workers are internal migrants. My results would understate the true educational decline if potential migrants reduce their schooling in response to new opportunities at export factories in other municipalities. I cannot evaluate this claim using my identification strategy. However, suggestive of this hypothesis is the McKenzie and Rapoport (2006) finding 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 non-migrants would then fall due to reduced out-migration of low-skill workers. I address this concern in Online Appendix A. First, I show that new export-manufacturing jobs do not increase the size of the sample cohort. Second, I use Census data on the municipality of residence in 1995 to show that when new export jobs arrive, it is the relatively more educated youths

[41]These four bins correspond to 0-8, 9-11, 12-15 and 16 or more years of school. Mexican children start school at age 6 and the modal student should complete 9 years of school in the summer of the year they turn 15. Therefore, if students stop attending school at the time of job arrivals in the year they turn 15-16, the proportion of students obtaining 9-11 years of schooling should rise.

[42]Rural youths will be underrepresented in this sample if they are more likely than urban youth 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.

who are deterred from migrating, not the least educated. This result only applies to internal migrants, but Chiquiar and Hanson (2005) find similar effects for emigrants to the United States. Therefore, the negative schooling impacts that I find are likely even larger in magnitude since compositional effects due to selective out-migration bias the coefficient upwards.

Alternatively, large exogenous inflows of migrants may alter both education and factory location decisions. An increase in low-skill migrant labor should lower the local unskilled wage, simultaneously attracting factories and encouraging local students to acquire more education. Hence, such migrant inflows would likely exert an upward bias on my estimates.

A large number of in-migrants should reduce the responsiveness of non-migrant education to new factory openings (since locals are less likely to obtain these factory jobs and a large supply of migrants reduces factory wages). Online Appendix A shows that this hypothesis is correct. A large number of migrants working in a particular municipality significantly attenuates the educational impacts of new export job arrivals. The negative schooling effects I document for the local population would likely be larger in the absence of internal migration.

6.3 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 such a conjecture is false. By the year 2000, the youths induced to drop out by the arrival of low-skill export jobs are earning lower wages than they would have earned had these new jobs never arrived in their localities.

The 2000 Census records the earned monthly income and hours worked in the previous month. I replace the dependent variable in specifications 6 and 8 with cohort means of either log earned income or log hourly wages.[43] The identification arguments are identical to those discussed in section 3.3. 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.

The income and wage results mirror the schooling results and are shown in table 10. The arrival of low-skill export jobs at ages 15 and 16 significantly reduce both log earned income and wages (columns 2 and 4). In contrast, there are no significant income or wage effects from the arrival of mid- and high-skill export jobs. If on-the-job training fully compensated for the reduction in schooling, the cohorts that experienced many low-skill export job arrivals at ages 15 and 16 would not have relatively lower incomes by the year 2000.

The magnitude of the coefficients conform with the estimates of the return to schooling in Mexico. I find a negative coefficient of -1.123 on log income and -1.511 on log wages for low-skill arrivals. Combining these estimates with the schooling coefficients in table 8 implies

[43] These measures exclude part-time workers who worked less than 20 hours a week. Therefore, I am evaluating the wage margin not the participation margin. I winsorize log incomes and wages at the 1 percent tails.

Mincerian “rates of return to schooling” of between 9.4 and 12.0 percent per year.[44] This return to an additional year of schooling is in the range of between 7.5 percent and 16.1 percent suggested by Patrinos (1995) and Psacharopoulos, Velez, Panagides, and Yang (1996) for Mexico. An estimate of the return to schooling in the lower end of this range is not surprising. These estimates are equivalent to IV estimates of log income on schooling where schooling is instrumented by net new export job arrivals. The average returns to schooling are likely to be higher since new export job arrivals directly raise wages and the LATE group (youths whose decisions are altered by factory openings) are likely to have low returns to education.

I can also calculate a back-of-the-envelope discount rate cutoff using the conceptual framework. I assume a constant income throughout the workers lifetime and an at-school utility equivalent to the utility derived from half of work income. These assumptions imply a discount rate of 13.6 percent for the marginal youth induced to drop out by new low-skill export job arrivals.[45] The implied discount rate rises to 23.2 percent if at-school income is equal to two thirds of work income.

It is important to note that incomes 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. Policymakers could still have paternalistic concerns for their citizens if they believe that adolescents are particularly predisposed to discount the future heavily when faced with delayed gains.[46] Similarly, peer effects at this stage of life are particularly strong, and may cause excessive dropout rates.

Credit constraints can also lead to youths dropping out of school with new export opportunities in order to fund high-return investment opportunities that pay off many years into the future. In related work, Atkin (2008) shows that the cohorts of women exposed to new factory openings at key school-leaving ages make larger health investments in their children.

6.4 Other mechanisms through which new export jobs may affect schooling

New formal export-manufacturing jobs may create additional jobs in the informal sector. For example, a new factory may generate demand for informal subcontracting or food carts outside the factory. My methodology cannot attribute the schooling effects I find solely to the job creation at the new factory, since controls for annual changes in informal employment are not available. In contrast, I can dismiss indirect employment effects that work through formal-sector job creation (results were similar when I controlled for other formal sector job creation in table 2).

---

[44] The return to schooling is simply the wage or income coefficient divided by the coefficient on schooling.

[45] From equation 3, the discount rate of the marginal dropout is $\bar{\rho}_{s,c+s} = \frac{\ln y_{s+1} - \ln y_s}{\ln y_s - \ln(y_s/2)}$, or simply the return to one year of school (9.4 percent for the log income estimates) divided by $\ln 2$.

[46] Adolescents may be particularly predisposed to such behavior as the frontal lobes associated with planning and decision making only fully develop in adulthood (see discussion in Oreopoulos 2007). In support of such hypotheses, 74 percent of American school dropouts surveyed by Bridgeland, DiIulio, and Morison (2006) would want to stay in school if they could relive that decision.

I have focused on the employment decisions of the youths themselves, but adult family members may also obtain one of the new export-manufacturing jobs. Household incomes will rise in this case. As education is a normal good, the job arrivals should raise, not lower, schooling. Schooling may decline if the household member in charge of looking after young children enters the workforce and the youth must stay home instead. As this will typically be the mother or sister, such a hypothesis is not consistent with new male job opportunities driving male schooling decisions (table 8). The results for female schooling may be driven in part by female job opportunities for a mother or sister raising the demand for childcare services. However, the fact that low-skill export job arrivals had similar effects for males and females make this explanation less likely.

7 Conclusions

This paper finds that for Mexico during the period 1986 to 2000, the new manufacturing opportunities generated by trade liberalization altered the distribution of education. In particular, the influx of new export-manufacturing jobs reduced the schooling of cohorts at their key school-leaving ages at the time. The magnitudes I find suggest that for every twenty 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. Export manufacturing generated an abundance of new low-skill jobs which substantially raised the opportunity cost of schooling for youths on the dropout margin at ages 15 and 16. Counterfactuals suggest that there would have been no negative schooling impacts had these jobs demanded a more educated set of workers, or had they arrived in less-educated parts of the country where fewer youths were on the dropout 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 to know which types of new manufacturing opportunities pull students out of school and in what context.

There are several potential policy remedies that do not require altering the type or location of jobs that arrive. A system of payments conditional upon school attendance would neutralize the negative educational impact of export-manufacturing jobs.[47] Alternatively, the age of earliest employment in export manufacturing could be raised to ensure that most Mexican workers will have already chosen their final education levels before being allowed 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.

[47] The much-studied Progresa 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.

References

Airola, C., and J. Juhn (2005): "Wage Inequality in Post-Reform Mexico," Working Papers 2005-01, Department of Economics, University of Houston.

Atkin, D. (2008): "Working for the Future: Female Factory Work and Child Health in Mexico," Discussion paper, Yale University.

Baltagi, B., and C. Kao (2000): "Nonstationary Panels, Cointegration in Panels and Dynamic Panels: A Survey," *Advances in Econometrics*, 15, 7–52.

Bartik, T. (1991): *Who Benefits from State and Local Economic Development Policies?* WE Upjohn Institute for Employment Research Kalamazoo, MI.

Beaudry, P., and J. DiNardo (1991): "The Effect of Implicit Contracts on the Movement of Wages Over the Business Cycle: Evidence from Micro Data," *The Journal of Political Economy*, 99(4), 665–688.

Bernard, A. (1995): "Exporters and Trade Liberalization in Mexico: Production Structure and Performance," Discussion paper, MIT.

Bernard, A., and J. Jensen (1995): "Exporters, Jobs, and Wages in US Manufacturing: 1976-1987," *Brookings Papers on Economic Activity: Microeconomics*, 1995, 67–112.

Bernard, A. B., R. Robertson, and P. K. Schott (2004): "Is Mexico A Lumpy Country?," NBER Working Papers 10898, National Bureau of Economic Research.

Bertrand, M., E. Duflo, and S. Mullainathan (2004): "How Much Should We Trust Differences-in-Differences Estimates?," *Quarterly Journal of Economics*, 119(1), 249–275.

Bridgeland, J., J. DiIulio, and K. Morison (2006): "The Silent Epidemic: Perspectives of High School Dropouts," *Report by Civic Enterprises.*

Chesnokova, T., and K. Krishna (2009): "Skill Acquisition, Credit Constraints, and Trade," *International Review of Economics and Finance*, 18(2), 227–238.

Chiquiar, D., and G. Hanson (2005): "International Migration, Self-Selection, and the Distribution of Wages: Evidence from Mexico and the United States," *Journal of Political Economy*, 113(2), 239–281.

Cragg, M., and M. Epelbaum (1996): "Why has Wage Dispersion Grown in Mexico? Is it the Incidence of Reforms or the Growing Demand for Skills?," *Journal of Development Economics*, 51, 99–116.

Duval Hernandez, R. (2006): "Informality, Segmentation and Earnings in Urban Mexico," Discussion paper, Center for U.S.-Mexican Studies.

Edmonds, E., and N. Pavcnik (2005): "The Effect of Trade Liberalization on Child Labor," *Journal of International Economics*, 65(2), 401–419.

Edmonds, E., N. Pavcnik, and P. Topalova (2008): "Trade Adjustment and Human Capital Investments: Evidence from Indian Tariff Reform," Discussion paper, CEPR Discussion Papers.

Federman, M., and D. Levine (2005): "The Effects of Industrialization on Education and Youth Labor in Indonesia," *The BE Journal of Macroeconomics*, 5(1), 1.

Findlay, R., and H. Kierzkowski (1983): "International Trade and Human Capital: A Simple General Equilibrium Model," *The Journal of Political Economy*, 91(6), 957–978.

Frias, J. A., D. Kaplan, and E. Verhoogen (2009): "Exports and Wage Premia: Evidence from Mexican Employer-Employee Data," Discussion paper, Columbia University.

Goldberg, P., and N. Pavcnik (2007): "Distributional Effects of Globalization in Developing Countries," *Journal of Economic Literature*, 65, 39–82.

Goldin, C., and L. Katz (1997): "Why the United States Led in Education: Lessons from Secondary School Expansion, 1910 to 1940," NBER Working Papers 6144, National Bureau of Economic Research.

Hanson, G., and A. Harrison (1999): "Trade Liberalization and Wage Inequality in Mexico," *Industrial and Labor Relations Review*, pp. 271–288.

Heath, R., and M. Mobarak (2012): "Supply and Demand Constraints on Educational Investment: Evidence from Garment Sector Jobs and the Female Stipend Program in Bangladesh," Discussion paper.

Helper, S., D. Levine, and C. Woodruff (2006): "How Does Economic Liberalization affect Investment in Education?: Evidence from Mexico," Discussion paper, UCSD.

Ibarraran, P. (2004): "Essays on Labor Markets and Economic Integration in Mexico," Ph.D. thesis, University of California, Berkeley.

Jensen, R. T. (2009): "Economic Opportunities and Gender Differences in Human Capital: Experimental Evidence for India," NBER Working Papers 16021, National Bureau of Economic Research.

Kaplan, D. S., G. M. Gonzalez, and R. Robertson (2007): "Mexican Employment Dynamics: Evidence from Matched Firm-Worker Data," Discussion paper, ITAM.

Le Brun, A., S. Helper, and D. Levine (2009): "The Effect of Industrialization on Children's Education–The Experience of Mexico," *UC Berkeley: Institute for Research on Labor and Employment*.

Lopez-Acevedo, G. (2006): "Mexico: Two Decades of the Evolution of Education and Inequality," Policy Research Working Paper Series 3919, The World Bank.

Lucas, R. (1988): "On the Mechanics of Economic Development," *Journal of Monetary Economics*, 22(1), 3–42.

McKenzie, D., and H. Rapoport (2006): "Can Migration Reduce Educational Attainment? Evidence from Mexico," Policy Research Working Paper Series 3952, The World Bank.

Minnesota Population Center (2007): "Integrated Public Use Microdata Series-International: Version 3.0," Available online at http://international. ipums. org/international.

Munshi, K., and M. Rosenzweig (2006): "Traditional Institutions Meet the Modern World: Caste, Gender, and Schooling Choice in a Globalizing Economy," *American Economic Review*, 96(4), 1225–1252.

Nicita, A., and M. Olarreaga (2007): "Trade, Production, and Protection Database, 1976-2004," *The World Bank Economic Review*, 21(1), 165.

Oreopoulos, P. (2007): "Do Dropouts Drop Out Too Soon? Wealth, Health and Happiness from Compulsory Schooling," *Journal of Public Economics*, 91(11-12), 2213–2229.

Oreopoulos, P., T. Von Wachter, and A. Heisz (2006): "The Short- and Long-Term Career Effects of Graduating in a Recession: Hysteresis and Heterogeneity in the Market for College Graduates," NBER Working Papers 12159, National Bureau of Economic Research.

Oster, E. (2010): "Do Call Centers Promote School Enrollment? Evidence from India," Discussion paper, University of Chicago Booth School of Business.

Patrinos, H. (1995): "Education and Earnings Differentials," Discussion paper, World Bank.

Psacharopoulos, G., E. Velez, A. Panagides, and H. Yang (1996): "Returns to Education During Economic Boom and Recession: Mexico 1984, 1989 and 1992," *Education Economics*, 4(3), 219–230.

Robertson, R. (2004): "Relative Prices and Wage Inequality: Evidence from Mexico," *Journal of International Economics*, 64(2), 387–409.

Shastry, G. K. (2008): "Human Capital Response to Globalization: Education and Information Technology in India," Discussion paper, University of Virginia.

Stokey, N. L. (1991): "Human Capital, Product Quality, and Growth," *The Quarterly Journal of Economics*, 106(2), 587–616.

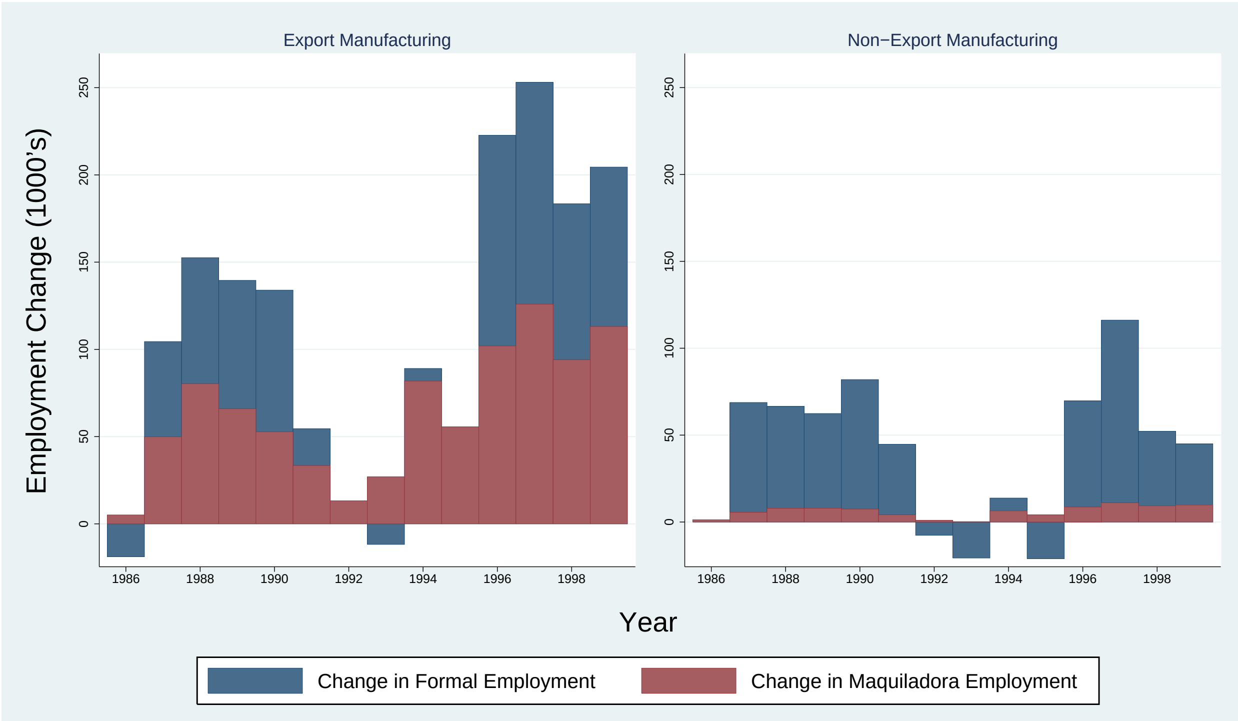
Topalova, P. (2010): "Factor Immobility and Regional Impacts of Trade Liberalization: Evidence on Poverty from India," *American Economic Journal: Applied Economics*, 2(4), 1–41.

Verhoogen, E. (2008): "Trade, Quality Upgrading and Wage Inequality in the Mexican Manufacturing Sector," *Quarterly Journal of Economics*, 123(2).

Wood, A., and C. Ridao-Cano (1999): "Skill, trade, and international inequality," *Oxford Economic Papers*, 51(1), 89.

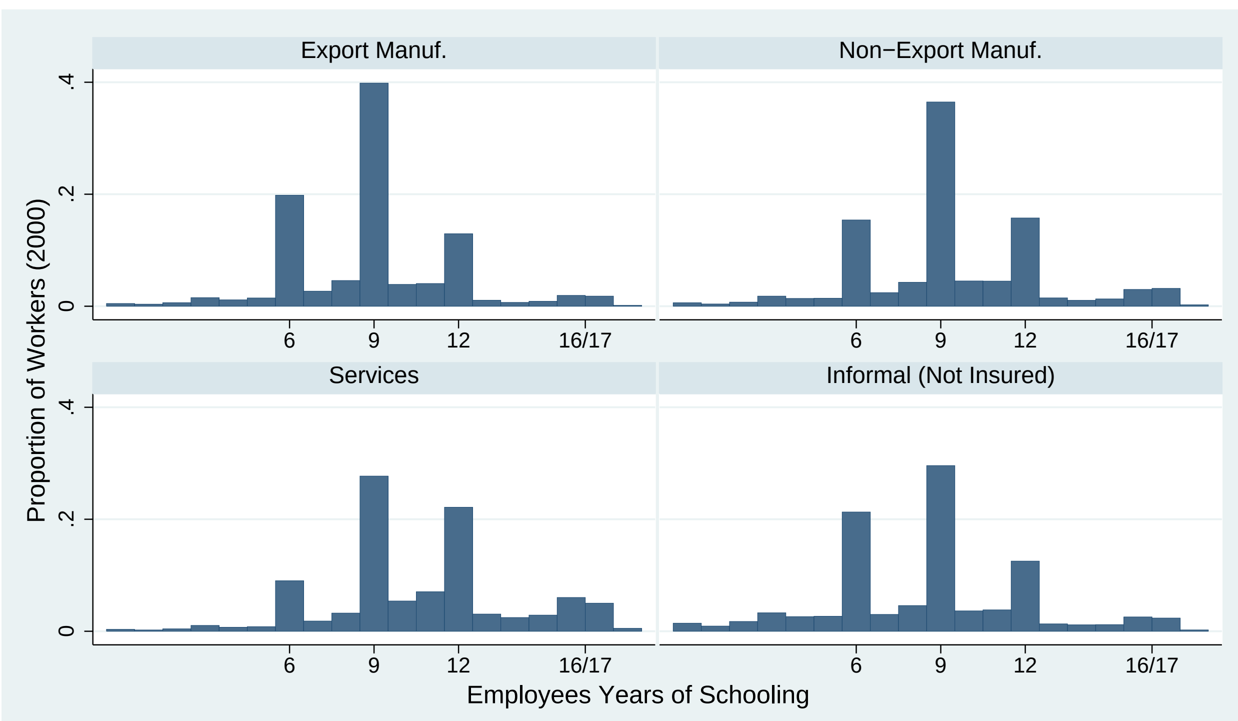
Zhou, L. (2003): "Why do Exporting Firms Pay Higher Wages?," Discussion paper, Emory University, Atlanta.

Figure 1: Manufacturing Employment Changes



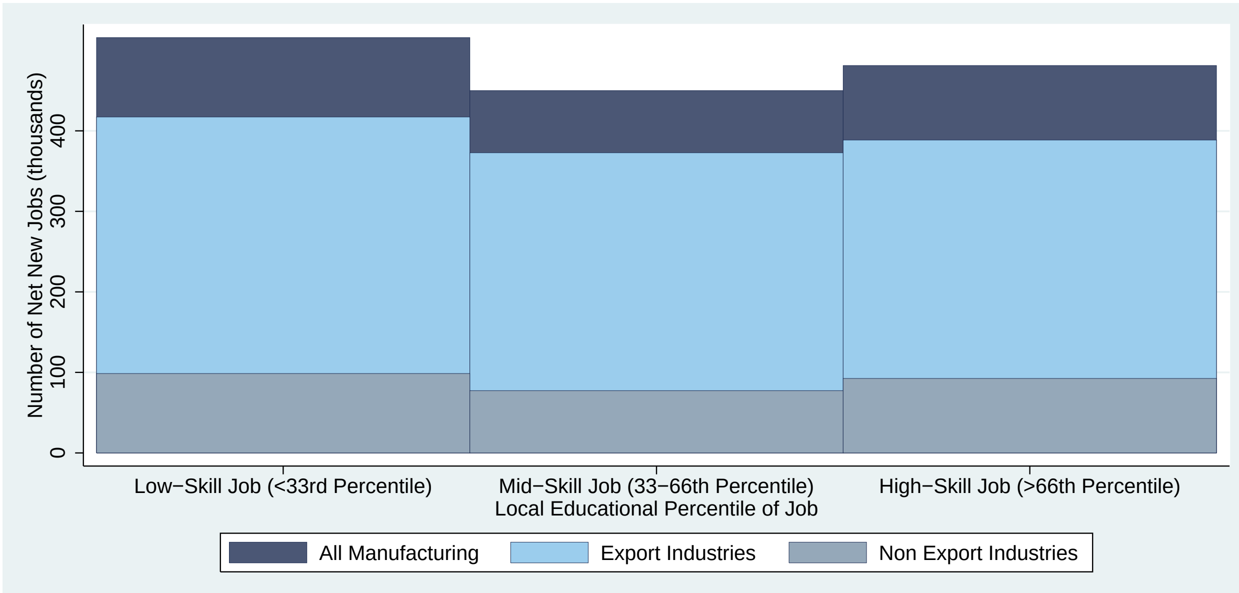
Note: Formal employment changes calculated using IMSS employment data. Maquiladora employment changes calculated using INEGI Maquiladora statistics.

Figure 2: Histogram of Education by Industry Grouping (Age 16-28 in 2000, Insured by IMSS)



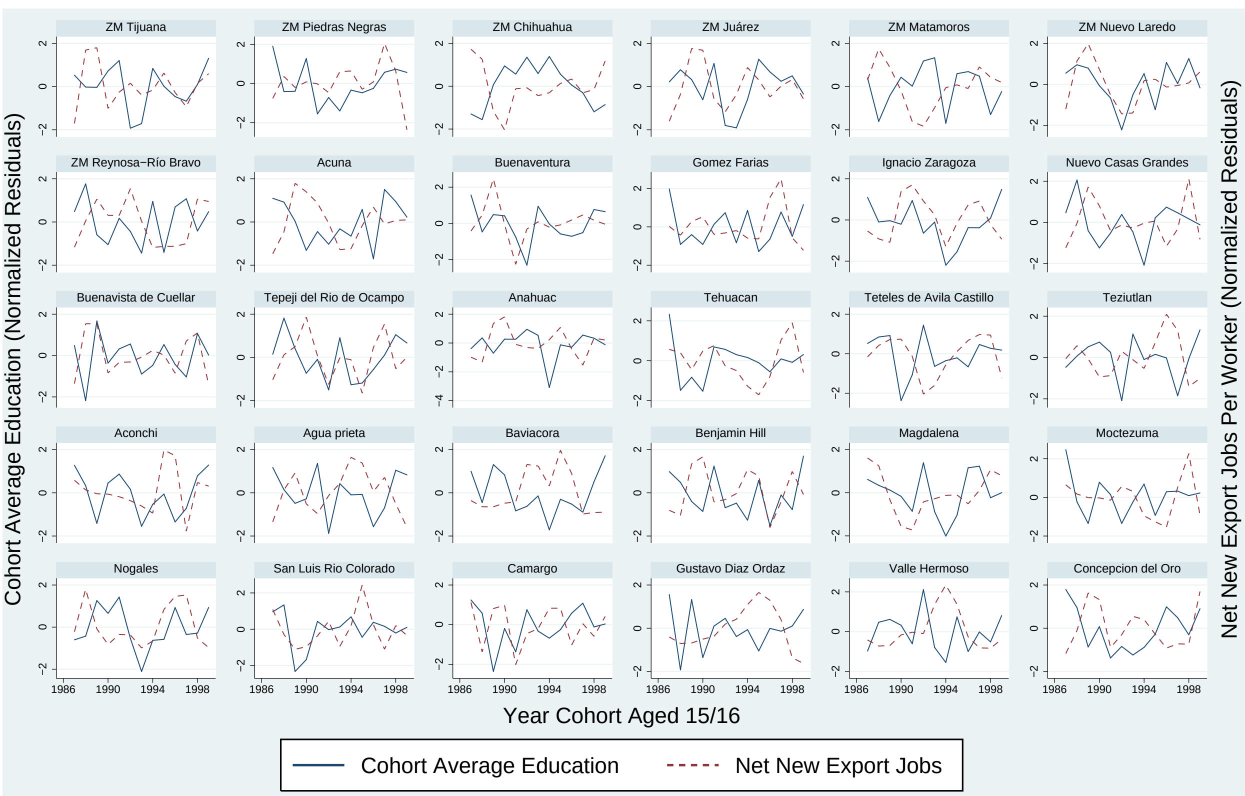
Note: The education distribution is calculated using the year 2000 Census for formal sector workers age 16 to 28 (my sample cohort). A formal worker is defined as a worker insured by IMSS or equivalent insurance scheme.

Figure 3: New Job Arrivals by Relative Skill Groupings



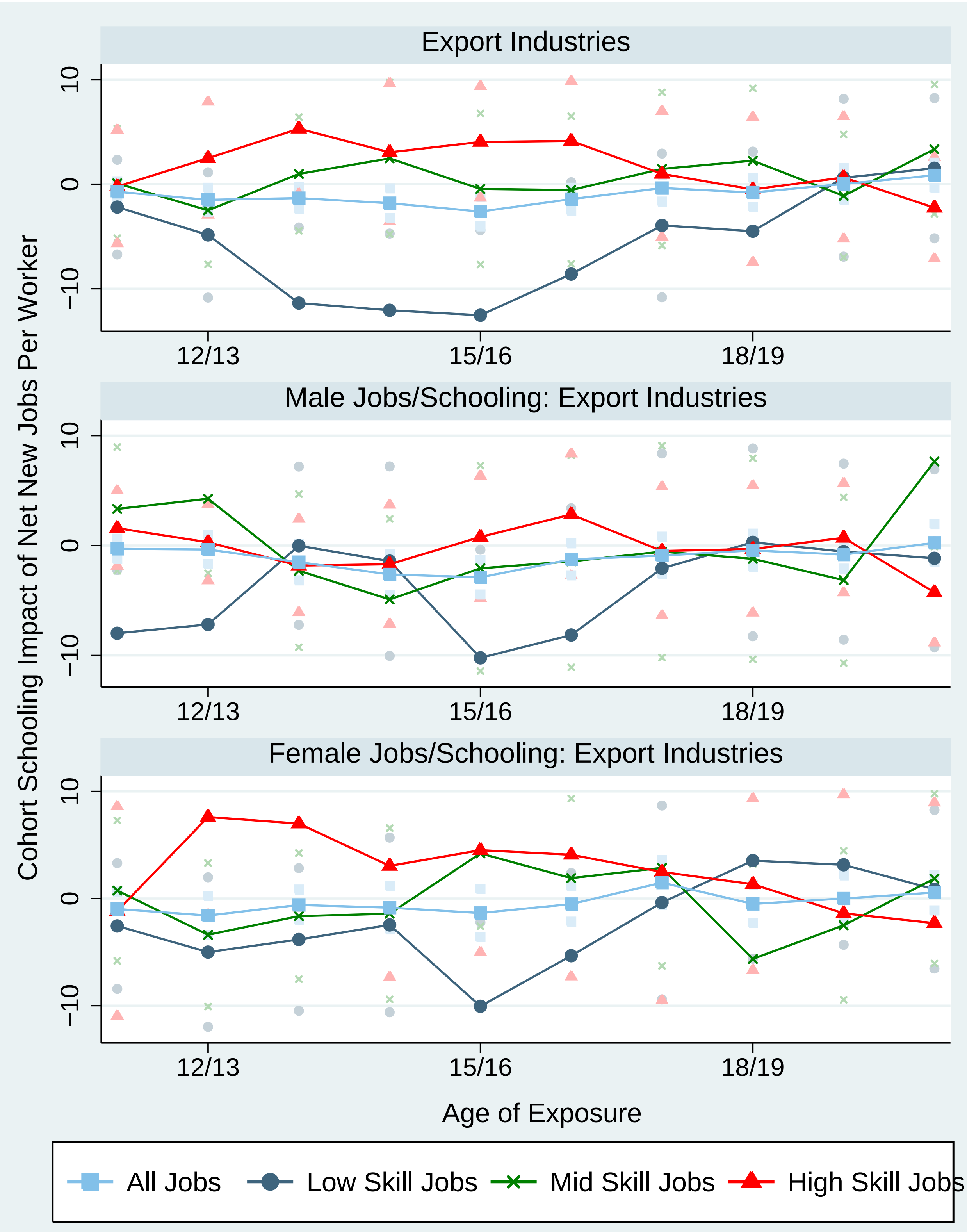
Note: Net new job arrivals 1986-1999 categorized into low, mid and high skill based on the relative education level of employees in each municipality-subindustry pair in the 2000 Census.

Figure 4: Visual Identification for 30 Municipalities with Largest Export Job Shocks



Note: Residuals from regressions of cohort schooling and net new export jobs at ages 15 and 16 on state-time dummies, municipality dummies and municipality linear trends. Residuals are plotted against the year the cohort turned 16 for the 30 municipalities with largest cumulative export job arrivals per worker.

Figure 5: Exposure at Different Ages (Export Industry Sample)



Note: Plot of coefficients from regressing (sex-specific) cohort schooling on (sex-specific) net new jobs per worker. Net new jobs broken into high, mid and low skill as described in section 5.1. Regressions run separately at every ages of exposure from 11/12 to 20/21. Unconnected markers display 95 percent confidence intervals.

Table 1: Sample Means

Census Schooling Sample (2000, Age 16-28, Non Migrants, Excludes Mexico City)			
	mean	standard deviation	observations
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	0.0005	1,706,582
Insured by IMSS (1=yes, 0=no)	0.42	0.0011	1,706,582
Monthly Log Earned Income (Pesos)	7.47	0.0011	667,103
Sex (1=male, 0=female)	0.48	0.0005	1,706,582
Municipality Size	8540.26	816.7	1808
IMSS Annual Firm Sample (1985-2000)			
	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
Proportion Male Workers	0.68		11,365,321
Unique Firms			2,194,681
Employees (1985)			4,472,491
Firms (1985)			372,520
Employees (2000)			12,509,298
Firms (2000)			912,284

Table 2: The Effect of New Export-Manufacturing Jobs on Educational Attainment

	(1)	(2)	(3)	(4)	(5)
	Cohort Average Completed Years of Schooling				
	OLS	IV (Large Δs)	RF (Large Δs)	RF (Large Δs)	RF & LDV
Net New Export Manufacturing Jobs/Worker at Age 15-16	-2.624***	-2.466***	-2.618***	-2.318***	-1.845***
	(0.655)	(0.624)	(0.724)	(0.716)	(0.415)
Net New Jobs/Worker at Age 15-16 (All Other Sectors)				-2.112***	
				(0.383)	
L.Cohort Schooling					0.307***
					(0.0173)
L2.Cohort Schooling					0.285***
					(0.0145)
L3.Cohort Schooling					0.165***
					(0.00998)
L4.Cohort Schooling					0.128***
					(0.0109)
Observations	23,484	23,484	23,484	23,484	23,399
R^2	0.944	0.557	0.944	0.944	0.915
Municipalities	1808	1808	1808	1808	1808
Kleibergen-Paap F-stat (1^{st} Stage)		6797			

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 municipality at ages 15 and 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, municipality dummies and municipality linear trends not shown. The RF & LDV column replaces fixed effects and municipality trends with four lags of cohort schooling. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Table 3: Changes in School Attendance 1990-2000 and New Export-Manufacturing Jobs

	(1)	(2)	(3)	(4)	(5)	(6)
	Change in Proportion of 16 Year Olds Attending School at Census, 1990-2000					
	OLS		RF (Large Δs)		Prop. 15 Year Old (RF)	
ΔNet New Export Manuf. Jobs/Worker (1990-1999)	-0.134**	-0.188***	-0.193***	-0.179***	-0.380***	-0.226***
	(0.0669)	(0.0572)	(0.0655)	(0.0561)	(0.0756)	(0.0596)
Proportion of 16 Year Olds Attending School, 1990		-0.271***		-0.269***		-0.355***
		(0.0106)		(0.0106)		(0.0107)
Constant	0.059***	0.195***	0.060***	0.194***	0.066***	0.280***
	(0.00221)	(0.00563)	(0.00224)	(0.00561)	(0.00251)	(0.00674)
Observations	1,781	1,781	1,781	1,781	1,774	1,774
R^2	0.002	0.271	0.005	0.270	0.014	0.391

Notes: Dependent variable is the change in proportion of 16 year olds attending school at time of Census, 1990-2000 (except columns 5 and 6 where dependent variable is change in proportion of 15 year olds). Independents variables are the change in net new manufacturing jobs per worker arriving in the municipality between January 1st 1990 and December 31st 1999, and the initial proportion of 16 year olds attending school (15 year olds in columns 5 and 6). The RF (LargeΔs) 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 the independent variable. Regression weighted by municipality population of 16 year olds in 1990 Census (15 year olds in columns 5 and 6), excludes Mexico City and migrants. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Table 4: Alternative Exporter Measures

	(1)	(2)	(3)	(4)
	Cohort Average Completed Years of Schooling (OLS)			
	10 percent Exporter Threshold in EIA Panel		25 percent Exporter Threshold in EIA Panel	
Net New Export Manuf. Jobs/Worker at Age 15-16	-1.920***		-1.452*	
	(0.718)		(0.783)	
Net New EIA Panel Export Jobs/Worker at Age 15-16		-1.962*		0.703
		(1.107)		(1.845)
Net New Maquiladora Jobs/Worker at Age 15-16		-1.902**		-1.908**
		(0.896)		(0.897)
Observations	23,484	23,484	23,484	23,484
R^2	0.944	0.944	0.944	0.944
Municipalities	1808	1808	1808	1808

Notes: Dependent variable is cohort average years of schooling in the year 2000. Independent variables are net new jobs per worker created by known exporters in cohort's municipality at ages 15 and 16. Known exporters identified using the EIA panel and INEGI Maquiladora statistics. Columns 1 and 2 define EIA exporters as firms exporting more than 10 percent of output, columns 3 and 4 use a cuttoff of 25 percent of output. State-time dummies, municipality dummies and municipality linear trends not shown. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Table 5: Exploring Heterogeniety Due to Job-specific Skill Requirements

	(1)	(2)	(3)	(4)	(5)
	Cohort Average Completed Years of Schooling				
		RF (Large Δs)			OLS
Net New Manufacturing Jobs/Worker at Age 15-16	-1.687*** (0.582)				
Low Skill Net New Manufacturing Jobs/Worker at Age 15-16		-9.026*** (3.394)			
Mid Skill Net New Manufacturing Jobs/Worker at Age 15-16		-1.974 (2.615)			
High Skill Net New Manufacturing Jobs/Worker at Age 15-16		3.628*** (1.398)			
Less than High School Net New Manuf. Jobs/Worker at Age 15-16			-2.110*** (0.755)		
High School or More Net New Manuf. Jobs/Worker at Age 15-16			-0.424 (2.131)		
Production Workers (Census) Net New Manuf. Jobs/Worker at Age 15-16				-2.254*** (0.781)	
Non-Production Workers (Census) Net New Manuf. Jobs/Worker at Age 15-16				0.329 (2.231)	
Production Workers (EIA Panel) Net New Manuf. Jobs/Worker at Age 15-16					-5.256** (2.453)
Non-Production Workers (EIA Panel) Net New Manuf. Jobs/Worker at Age 15-16					0.440 (4.662)
Observations	23,484	23,484	23,484	23,484	23,484
R^2	0.944	0.944	0.944	0.944	0.942
Municipalities	1808	1808	1808	1808	1808

Notes: Dependent variable is the cohort average years of schooling in the year 2000. Independent variables are net new manufacturing jobs per worker arriving in cohort's municipality at ages 15 and 16. In columns 2 to 5, net new job arrivals are broken into various skill categories as described in section 5.1. The RF (LargeΔs) columns are reduced form regressions, and regresses schooling on net new jobs per worker attributable to firms that expand or contract employment by 50 or more employees in a single year. State-time dummies, municipality dummies and municipality linear trends not shown. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Table 6: Exploring Heterogeniety Due to Job and Location Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Cohort Average Completed Years of Schooling, RF (Large Δs)										
	Density$_c$	Density$_0$		Density$_c$	Density$_0$		Density$_c$	Density$_0$		Density$_c$	Density$_0$
Low Skill Net New Manuf. Jobs/Worker at Age 15-16	19.75***	15.41**	1.736	39.36***	23.30***	-7.646**	21.30***	16.68**	2.991	40.82***	24.54***
	(6.995)	(6.899)	(5.251)	(7.241)	(7.444)	(3.762)	(7.336)	(7.168)	(5.503)	(7.531)	(7.718)
Mid Skill Net New Manuf. Jobs/Worker at Age 15-16	-2.905	-3.288	21.36	19.14	15.41	-1.122	-1.760	-2.691	23.86	22.25	17.55
	(4.674)	(5.094)	(15.64)	(15.39)	(16.32)	(2.662)	(4.779)	(5.447)	(15.86)	(15.75)	(16.56)
High Skill Net New Manuf. Jobs/Worker at Age 15-16	-1.287	1.405	12.66**	-5.642	-4.146	3.812***	-1.260	1.374	12.49**	-6.074	-4.262
	(2.549)	(3.227)	(5.443)	(7.479)	(9.823)	(1.438)	(2.545)	(3.250)	(5.772)	(7.661)	(10.25)
Low Skill Net New Jobs× Density of Marginal Youth	-115.0***	-117.3***		-106.5***	-110.0***		-115.2***	-118.1***		-106.7***	-110.9***
	(31.29)	(40.28)		(30.32)	(39.98)		(31.28)	(40.71)		(30.30)	(40.38)
Mid Skill Net New Jobs× Density of Marginal Youth	-5.630	-4.028		-10.40	-10.11		-6.570	-3.308		-11.47	-9.367
	(23.69)	(33.43)		(23.46)	(35.04)		(23.88)	(34.30)		(23.58)	(35.77)
High Skill Net New Jobs× Density of Marginal Youth	26.52*	12.58		27.71**	13.92		27.50*	13.65		28.74**	15.08
	(14.09)	(21.99)		(13.91)	(22.17)		(14.39)	(22.11)		(14.19)	(22.27)
Low Skill Net New Jobs× Wage Premia for Low Skill			-10.02***	-20.49***	-8.551**				-9.921***	-20.37***	-8.467**
			(3.595)	(3.839)	(3.620)				(3.568)	(3.814)	(3.598)
Mid Skill Net New Jobs× Wage Premia for Mid Skill			-23.68	-21.94	-18.33				-25.36	-23.89	-19.86
			(15.83)	(14.39)	(16.26)				(16.01)	(14.63)	(16.38)
High Skill Net New Jobs× Wage Premia for High Skill			-9.093	4.218	5.405				-8.728	4.685	5.488
			(5.543)	(7.003)	(9.239)				(5.927)	(7.197)	(9.685)
Density of Marginal Youth	3.069***			3.103***			3.069***			3.102***	
	(0.149)			(0.149)			(0.149)			(0.149)	
Wage Premia for Low Skill			-0.006**	-0.008***	-0.006**				-0.006**	-0.008***	-0.006**
			(0.00230)	(0.00205)	(0.00229)				(0.00231)	(0.00205)	(0.00229)
Wage Premia for Mid Skill			-0.016	-0.012	-0.018				-0.016	-0.012	-0.018
			(0.0261)	(0.0289)	(0.0260)				(0.0262)	(0.0290)	(0.0261)
Wage Premia for High Skill			-0.006	0.006	-0.005				-0.004	0.008	-0.004
			(0.0278)	(0.0329)	(0.0285)				(0.0280)	(0.0331)	(0.0286)
Net New Export Manuf. Jobs/Worker at Age 15-16						-1.129	-1.264	-0.939	-1.114	-1.285	-0.968
						(1.176)	(1.287)	(1.198)	(1.191)	(1.295)	(1.215)
Observations	23,484	23,471	23,484	23,484	23,471	23,484	23,484	23,471	23,484	23,484	23,471
R^2	0.950	0.944	0.944	0.950	0.944	0.944	0.950	0.944	0.944	0.950	0.944
Municipalities	1808	1807	1808	1808	1807	1808	1808	1807	1808	1808	1807

Notes: Dependent variable is cohort average schooling in the year 2000. Independent variables are net new jobs per worker arriving in cohort's municipality at ages 15 and 16 attributable to firms that expand or contract employment by 50 or more employees in a single year, as well as interactions for the density of marginal youth and the wage premia on offer. The job arrivals are categorized into low, mid and high skill based on the relative education level of employees in those municipality-subindustry pairs in the 2000 Census. Density$_c$ columns use the cohort-varying density proxy while Density$_0$ columns use the value of the density proxy for the two cohorts prior to the oldest cohort in the sample. State-time dummies, municipality dummies and municipality linear trends not shown. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Table 7: Total and Counterfactual Cohort Schooling Outcomes(cohorts aged 16-28 in 2000)

Counterfactual Description	Mean Change in Cohort Schooling	S.D.
Actual Net New Export Manufacturing Jobs 1986-1999	-0.0117	(0.0002)
Actual Skill-Specific Net New Export Jobs 1986-1999	-0.0115	(0.0002)
New Export Jobs with Service Sector Skill Distribution	-0.0003	(0.0000)
Actual Skill-Spec. Net New Export Jobs & Interactions	-0.0159	(0.0003)
Density of Marginal Youth Proxy at 25th Percentile	0.0014	(0.0001)
Wage Premia Proxy at 25th Percentile	-0.0158	(0.0003)

Notes: Mean cohort schooling impacts for cohorts aged 16-28 in the year 2000 calculated using the history of net new export job arrivals and coefficient estimates from tables 2, 5 and 6. Counterfactual described in column 1.

Table 8: Sex-Specific Net New Export Jobs and Educational Attainment

	(1)	(2)	(3)
	Cohort Average Completed Years of Schooling, All RF (Large Δs)		
	Male Schooling	Female Schooling	Schooling
Low Skill Net New Export Manuf Jobs/Worker at Age 15-16			-12.54***
			(4.157)
Mid Skill Net New Export Manuf Jobs/Worker at Age 15-16			-0.451
			(3.692)
High Skill Net New Export Manuf Jobs/Worker at Age 15-16			4.053
			(2.724)
Low Skill Net New Male Export Manuf Jobs/Worker at Age 15-16	-10.08**	-3.061	
	(4.910)	(4.112)	
Mid Skill Net New Male Export Manuf Jobs/Worker at Age 15-16	-1.832	-1.579	
	(4.950)	(4.198)	
High Skill Net New Male Export Manuf Jobs/Worker at Age 15-16	0.786	-0.628	
	(2.944)	(1.979)	
Low Skill Net New Female Export Manuf Jobs/Worker at Age 15-16	3.509	-8.165*	
	(4.672)	(4.425)	
Mid Skill Net New Female Export Manuf Jobs/Worker at Age 15-16	-2.942	3.952	
	(4.241)	(3.417)	
High Skill Net New Female Export Manuf Jobs/Worker at Age 15-16	-2.821	6.609	
	(5.928)	(5.227)	
Observations	23,484	23,484	23,484
R^2	0.944	0.944	0.944
Municipalities	1808	1808	1808

Notes: Dependent variable is the (gender-specific) cohort average years of schooling in the year 2000. Independent variables are the (gender-specific) net new export jobs per worker arriving in cohort's municipality at ages 15 and 16 attributable to firms that expand or contract their employment by 50 or more employees in a single year. Job arrivals categorized into low, mid and high skill based on relative education level of employees in those municipality-subindustry pairs in the 2000 Census (by gender for columns 1 and 2). State-time dummies, municipality dummies and municipality linear trends not shown. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Table 9: Net New Export Jobs and the Proportion of Students at Different School Levels

All RF (Large Δs)	(1)	(2)	(3)	(4)
	Proportion of Cohort with Various Years of Completed Schooling			
	0-8 Yrs of Schl	9-11 Yrs of Schl	12-15 Yrs of Schl	16-18 Yrs of Schl
Low Skill Net New Export Manuf.	0.693	1.101*	-1.143*	-0.652**
Jobs/Worker at Age 15-16	(0.503)	(0.655)	(0.615)	(0.299)
Mid Skill Net New Export Manuf.	-0.00113	-0.0162	-0.202	0.219
Jobs/Worker at Age 15-16	(0.474)	(0.595)	(0.544)	(0.212)
High Skill Net New Export Manuf.	-0.112	-0.202	-0.0646	0.378*
Jobs/Worker at Age 15-16	(0.399)	(0.491)	(0.451)	(0.202)
Observations	23,484	23,484	23,484	23,484
R^2	0.936	0.882	0.869	0.914
Municipalities	1808	1808	1808	1808

Notes: Dependent variable is the proportion of the cohort whose highest obtained level of schooling by the year 2000 falls within various schooling bins. Independent variables are net new export jobs per worker arriving in cohort's municipality at ages 15 and 16 attributable to firms that expand or contract their employment by 50 or more employees in a single year. The net new export job arrivals are categorized into low, mid and high skill based on the relative education level of employees in those municipality-subindustry pairs in the 2000 Census. State-time dummies, municipality dummies and municipality linear trends not shown. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Table 10: Net New Export Jobs, Skill Levels and Later Life Income

	(1)	(2)	(3)	(4)
	Log earned income (RF (Large Δs))		Log hourly wage (RF (Large Δs))	
Net New Export Manufacturing	-0.120		-0.166	
Jobs/Worker at Age 15-16	(0.116)		(0.126)	
Low Skill Net New Export Manuf.		-1.123*		-1.511**
Jobs/Worker at Age 15-16		(0.635)		(0.703)
Mid Skill Net New Export Manuf.		0.801		1.106
Jobs/Worker at Age 15-16		(0.584)		(0.765)
High Skill Net New Export Manuf.		-0.142		-0.237
Jobs/Worker at Age 15-16		(0.658)		(0.783)
Observations	22,041	22,041	22,232	22,232
R^2	0.950	0.950	0.935	0.935
Municipalities	1807	1807	1808	1808

Notes: Dependent variable is the cohort average log earned income or log hourly wage in the year 2000. Independent variables are net new export jobs per worker arriving in cohort's municipality at ages 15 and 16 attributable to firms that expand or contract their employment by 50 or more employees in a single year. The net new export job arrivals are categorized into low, mid and high skill based on the relative education level of employees in those municipality-subindustry pairs in the 2000 Census. State-time dummies, municipality dummies and municipality linear trends not shown. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Appendices

Appendix A Migration

As discussed in section 6.2, migration could bias my results if local labor market conditions alter the composition of out-migrants. If new export jobs prevent low-education individuals from migrating, the average education of the cohort declines with export job arrivals despite no student altering their schooling decision. I perform two empirical tests in order to dismiss this possible explanation for my findings.

The first test explores the size of different cohorts of non-migrants. If these composition effects are important, and if the less educated are deciding not to migrate, the size of the sample cohort should rise with new jobs in export manufacturing. To test this hypothesis, I replace cohort years of schooling with log cohort size, $\ln N_{mc}$, in both specification 6 and specification 8:[1]

$$\ln N_{mc} = \beta l_{mc} + \delta_m + \delta_m c + \delta_{rc} + \varepsilon_{mc}, \quad (10)$$

$$\ln N_{mc} = \beta_1 \sum_i \phi_{LS,mi} l_{mci} + \beta_2 \sum_i \phi_{MS,mi} l_{mci} + \beta_3 \sum_i \phi_{HS,mi} l_{mci} + \delta_m + \delta_m c + \delta_{rc} + \varepsilon_{mc}.$$

Columns 1 and 2 of table A.1 show the results from these two regressions. There is no evidence in either specification that cohort size responds positively to net new export-manufacturing job arrivals at ages 15 and 16. In fact, the basic specification finds a small decline in cohort size.

The second test directly examines the hypothesis that relatively uneducated youths were disproportionately deterred from migrating due to a new factory opening. If this hypothesis is correct, I should find that the education of out-migrants rises relative to the education of non-migrants in a municipality when new export-manufacturing jobs arrive.

The 2000 Census records where each individual was living in 1995. Therefore, for every municipality-cohort pair, I calculate the average education of individuals who lived in the municipality in 1995 but not in 2000, $S_{leave,mc}$, divided by the average education of individuals who lived in the municipality in both 1995 and 2000, $S_{stay,mc}$. My dependent variable is the mean value of this ratio for the five cohorts who turned 15 or 16 between 1995 and 1999, $\frac{1}{5}\sum_{t=95}^{99} \frac{S_{leave,mt}}{S_{stay,mt}}$. This variable is then regressed on the sum of the changes in export-manufacturing employment per worker between January 1995 and December 1999 (potentially broken down

[1] I use log cohort size as municipality populations vary greatly. Therefore, I am considering proportional changes in cohort size. Net new jobs are already scaled, as they are divided by the number of workers in the municipality.

by skill). I also include a full set of state dummy variables:

$$\frac{1}{5}\sum_{t=95}^{99}\frac{S_{leave,mt}}{S_{stay,mt}} = \beta\sum_{t=1995}^{1999} l_{mt} + \delta_r + \varepsilon_m, \tag{11}$$

$$\frac{1}{5}\sum_{t=95}^{99}\frac{S_{leave,mt}}{S_{stay,mt}} = \beta_1\sum_{t=1995}^{1999}\sum_i \phi_{LS,mi}l_{mti} + \beta_2\sum_{t=1995}^{1999}\sum_i \phi_{MS,mi}l_{mti} + \beta_3\sum_{t=1995}^{1999}\sum_i \phi_{HS,mi}l_{mti} + \delta_r + \varepsilon_m.$$

If my finding that new export jobs reduce schooling is driven by the less educated remaining in the municipality, the ratio of leavers to stayers education will increase with net new export job arrivals between 1995 and 1999 ($\beta > 0$ and $\beta_1 > 0$).

Results are reported in columns 3 and 4 of table A.1. Both β and β_1 are significantly negative, not positive. New export-manufacturing jobs keep the more educated youth in the municipality. This is strong evidence, at least for the later years in the sample, that when new export jobs arrive, out-migration effects tend to raise cohort education through composition effects. This result only applies to internal migrants, but Chiquiar and Hanson (2005) find that emigrants to the United States are also drawn from relatively educated portions of the population. Therefore, the magnitude of my finding that new export-manufacturing jobs reduce schooling is likely to be attenuated by out-migration.

In-migration may generate heterogeneity in the impact of new export-manufacturing jobs. In the extreme, if only migrants are employed in export manufacturing in a particular municipality and labor markets are segmented, then new job arrivals should have no impact on the education decisions of local youth. In this scenario, these export jobs do not enter into a local youth's choice set. In less extreme cases, large numbers of migrants are likely to reduce the local manufacturing wage and hence make new export-manufacturing jobs less attractive alternatives to schooling.

I test this hypothesis using the in-migrants I identify in the 2000 Census. I interact new export job arrivals at ages 15 and 16 (potentially broken down by skill) by ϑ_m, the proportion of manufacturing jobs held by migrants in the year 2000 in each municipality:

$$S_{mc} = \beta l_{mc} + \gamma\vartheta_m l_{mc} + \delta_m + \delta_m c + \delta_{rc} + \varepsilon_{mc}, \tag{12}$$

$$\begin{aligned} S_{mc} &= \beta_1\sum_i \phi_{LS,mi}l_{mci} + \beta_2\sum_i \phi_{MS,mi}l_{mci} + \beta_3\sum_i \phi_{HS,mi}l_{mci} \\ &\quad + \gamma_1\vartheta_m\sum_i \phi_{LS,mi}l_{mci} + \gamma_2\vartheta_m\sum_i \phi_{MS,mi}l_{mci} + \gamma_3\vartheta_m\sum_i \phi_{HS,mi}l_{mci} + \delta_m + \delta_m c + \delta_{rc} + \varepsilon_{mc}. \end{aligned}$$

If the presence of a large number of migrants reduces the impact of new job arrivals on the local population, I expect β (and β_1) to be negative, and γ (and γ_1) to be positive.

Results are reported in columns 5 and 6 of table A.1. I find the expected sign pattern, although only the sign on the low-skill migrant interaction, γ_1, is significant. The implication of this finding is that, in the absence of internal migration in Mexico, local education would decline even more with the arrival of new low-skill export-manufacturing opportunities at ages 15 and 16.

Table A.1: Net New Export Jobs, Skill Levels and Migration

	(1)	(2)	(3)	(4)	(5)	(6)
	Migration		*Skill-Biased Migration*		*Migrant Interaction*	
			All RF (Large Δs)			
	Log Cohort Size		Ratio of Leavers School to Stayers School		Cohort Schooling	
Net New Export Manuf. Jobs/Worker at Age 15-16	-0.0192***		-1.071***		-2.890*	
	(0.00609)		(-3.350)		(1.513)	
Low Skill Net New Export Manuf Jobs/Worker at Age 15-16		-0.0513		-3.499***		-24.87***
		(0.0397)		(-3.508)		(8.625)
Mid Skill Net New Export Manuf Jobs/Worker at Age 15-16		0.0252		-0.0154		5.065
		(0.0321)		(-0.0153)		(6.451)
High Skill Net New Export Manuf Jobs/Worker at Age 15-16		-0.0349		0.0921		8.244*
		(0.0277)		(0.126)		(4.436)
Net New Export Manuf Jobs					0.915	
×Migrant Prop. in Manuf					(4.198)	
Low Skill Net New Export Jobs						45.86**
×Migrant Prop. in Manuf						(22.96)
Mid Skill Net New Export Jobs						-13.82
×Migrant Prop. in Manuf						(17.90)
High Skill Net New Export Jobs						-23.36
×Migrant Prop. in Manuf						(17.98)
Observations	23,484	23,484	1,663	1,663	23,484	23,484
R^2	0.793	0.793	0.049	0.052	0.944	0.944
Municipalities	1808	1808	1808	1808	1808	1808

Notes: Dependent variable in columns 1 and 2 is log cohort size for non-migrants in the year 2000. Dependent variable in columns 3 and 4 is the schooling of individuals who lived in the municipality in 1995 but not in 2000 divided by the schooling of individuals who lived in the municipality in both 1995 and 2000 (for cohorts who turned 15 or 16 between 1995 and 1999). Dependent variable in columns 5 and 6 is cohort average schooling. Independent variables in Columns 1, 2, 5 and 6 are net new export jobs per worker arriving in cohort's municipality at ages 15 and 16 attributable to firms that expand or contract their employment by 50 or more employees in a single year. Columns 3 and 4 use the total net new export jobs per worker attributable to firms that expand or contract their employment by 50 or more employees in a single year that arrived between January 1995 and December 1999. The net new export job arrivals are categorized into low, mid and high skill based on the relative education level of employees in those municipality-subindustry pairs in the 2000 Census. Columns 1, 2, 5 and 6 include State-time dummies, municipality dummies and municipality linear trends. Columns 3 and 4 include State dummies Regression weighted by cell population and excludes Mexico City. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Appendix B Robustness Results

I present a variety of robustness checks on my key finding, that cohort schooling declines with net new export-manufacturing jobs at ages 15 and 16. Tables B.2 to B.5 rerun the preferred reduced form specification, repeated in column 1 of each table, with several modifications.

Table B.2 explores how sensitive results are to the removal of the various fixed effects and trends. Columns 2 to 4 sequentially remove the municipality linear trends, the state-specific time dummies and finally the basic time-dummies. Column 5 removes both the municipality fixed effects and linear trends. The coefficient remains negative and becomes about twice as large in magnitude with the removal of the municipality linear trends. One potential explanation is that, with only 13 years of data, the inclusion of a linear trend terms risks over-fitting and causing attenuation due to the inclusion of an excessive number of controls in the regression. Removing the other time fixed effects in columns 3 and 4 results in even larger coefficients. Meanwhile, the removal of the municipality fixed effects and linear trends flips the sign of the relationship. Therefore, the effects I find are primarily coming from variation within municipalities across time as opposed to variation across municipalities within time.

Tables B.3 and B.4 investigate how results vary across different geographic samples. In column 2 of table B.3, I exclude the 781 municipalities that had no formal sector employment during the period. As these municipalities are generally very small, they have little impact on my weighted regression results. Column 3 controls for the fact that the Progresa conditional cash transfer program was rolled out at the end of the sample period. Progresa could potentially be a cause of omitted variable bias as the program encouraged children to stay in school at the tail end of my sample period by offering cash incentives. Therefore, I include a Progresa dummy takes the value 1 in the 1998 and 1999 if more than 10 percent of the population reported receiving Progresa or Procampo payments in the 2000 Census (no specific Progresa indicator is available in the Census). Column 4 excludes the two large cities in the sample, Monterrey and Guadalajara, which may have been driving my population weighted results. In both columns 3 and 4, results are unchanged.

In table B.4, I replace the state-time fixed effects with region-time fixed effects using three regions of Mexico (North, Center and South). The region-time fixed effects allow me to include Mexico City (which is its own State). Additionally, region-time fixed effects allow me to separately run regressions on various sub-samples for which there is limited variation in the presence of 31×13 state-time dummies. Column 1 reproduces the standard specification but with region-time fixed effects. The coefficient is similar but slightly smaller than the state-time fixed effects specification. The coefficient changes little in column 2 where I includes the Valle de México metropolitan zone that contains Mexico City. Column 3 restricts the sample to the 54 metropolitan zones in Mexico. Column 3 restricts the sample to the 1754 non-metropolitan municipalities in my data set. The results suggest that the dropout effects I find are coming primarily from non-metropolitan municipalities. Columns 5, 6 and 7 focus on the Northern region, the Central region and the Southern region of Mexico respectively. Results are similar in the North and Center of the country. In the South of the country I find an insignificant and large negative coefficient. Since there was very little export job creation in the South, it is unsurprising that this coefficient is imprecisely estimated.

Finally, table B.5 examines various alternative specifications. In column 2, I cap education at 12 years and recalculate cohort schooling. By capping education at 12 years, most of the sample will have reached their final level of schooling by the year 2000, mitigating concerns that the amount of misreporting varies with the skill level of the municipality. In column 3, I further restrict attention only to individuals not at school at the time of the Census. Results are very similar in both cases. Therefore, I can be confident that my results are driven by students making school dropout decisions before the end of high school.

Columns 4 and 5 of table B.5 look at the impact of sex-specific schooling on sex-specific net new export job arrivals. I find similar smaller effects for women than men. The main text shows that this result is due to the fact that the school acquisition effects from high-skilled export job arrivals was larger for women than for men (as opposed to smaller dropout effects from low-skill job arrivals for women compared to men). Column 6 shows that my results are robust to extending the cutoff threshold of my instrument from changes of 50 employees to changes of 100 employees in a single firm in a single year.

Column 7 of table B.5 interacts new export job arrivals with a positive and a negative indicator dummy. Therefore, I allow for years of net new job losses (negative values of l_{mc}) to have potentially different effects from years of net new job gains (positive values of l_{mc}). In column 8, I separate new export job arrivals into the jobs created (firm expansions or openings) and the jobs destroyed (firm contractions or closures) in each municipality-year cell. I code both net new job losses in column 7 and the jobs destroyed in column 8 as negative numbers. In both cases, I find that job expansions lower education, and job contractions raise education (however the coefficient on years of net new job losses in column 7 is not significant). Finally, column 9 investigates the possibility that there are geographic spillovers. For example, a student may decide to drop out of school due to new export job opportunities in the State capital. I calculate the net new export job arrivals at ages 15 and 16 in all other municipalities in the state and divide this number by the working age population of those municipalities. I include this state-level job measure as an additional independent variable (and replace state-time dummies with region-time dummies to avoid collinearity). The coefficient on state-level net new export jobs per worker is small and insignificant, suggesting that youths educational choices are primarily affected by local labor market conditions.

In summary, there is a robust negative impact of new export-manufacturing jobs at ages 15 and 16 on cohort schooling.

Table B.2: Robustness: Fewer Fixed Effects

	(1)	(2)	(3)	(4)	(5)
	Baseline		*Fewer Fixed Effects*		
	Cohort Average Completed Years of Schooling (All RF, Large Δs)				
Net New Export Manufacturing Jobs/Worker at Age 15-16	-2.618*** (0.724)	-5.483*** (0.972)	-6.129*** (1.424)	-10.72*** (2.550)	6.547* (3.374)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	No
Municipality Linear Trends	Yes	No	No	No	No
State-Time Dummies	Yes	Yes	No	No	Yes
Time Dummies	Yes	Yes	Yes	No	Yes
Observations	23,484	23,484	23,484	23,484	23,484
R^2	0.944	0.905	0.893	0.876	0.329
Municipalities	1808	1808	1808	1808	1808

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 municipality at ages 15 and 16 attributable to firms that expand or contract their employment by 50 or more employees in a single year. Column 1 repeats the baseline specification with state-time dummies, municipality dummies and municipality linear trends. Columns 2-4 sequentially remove municipality linear trends, state-time dummies and time-dummies. Column 5 removes municipality fixed effects and linear trends. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Table B.3: Robustness: Different Geographical Samples

	(1)	(2)	(3)	(4)
	Baseline	*No Informal Municipalities*	*Progresa Dummy*	*No Monterrey or Guadalajara*
	Cohort Average Completed Years of Schooling (All RF, Large Δs)			
Net New Export Manuf. Jobs/Worker at Age 15-16	-2.618*** (0.724)	-2.423*** (0.720)	-2.433*** (0.728)	-2.477*** (0.725)
State-Time Dummies	Yes	Yes	Yes	Yes
Region-Time Dummies	No	No	No	No
Observations	23,484	13,350	23,484	23,458
R^2	0.944	0.941	0.944	0.937
Municipalities	1808	1027	1808	1806

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 municipality at ages 15 and 16 attributable to firms that expand or contract their employment by 50 or more employees in a single year. State-time dummies, municipality dummies and municipality linear trends included. Geographic coverage documented in column headings. Progresa dummy included in column 3 takes the value 1 in the 1998 and 1999 if more than 10 percent of municipality population reported receiving Progresa or Procampo payments in the 2000 Census. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Table B.4: Robustness: Different Geographical Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline With Region-Time FE	*Includes Mexico City*	*Metropolitan Zones*	*Non-Metropolitan Municipalities*	*Northern Region*	*Central Region*	*Southern Region*
	Cohort Average Completed Years of Schooling (All RF, Large Δs)						
Net New Export Manuf.	-2.127***	-2.447***	-0.295	-1.505**	-1.991**	-1.960*	-7.334
Jobs/Worker at Age 15-16	(0.705)	(0.761)	(1.476)	(0.682)	(0.927)	(1.055)	(4.527)
State-Time Dummies	No	No	No	No	No	No	No
Region-Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,484	23,497	702	22,782	4,302	9,789	9,393
R^2	0.941	0.956	0.966	0.908	0.931	0.926	0.932
Municipalities	1808	1809	54	1754	331	753	724

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 municipality at ages 15 and 16 attributable to firms that expand or contract their employment by 50 or more employees in a single year. Region-time dummies, municipality dummies and municipality linear trends included. Geographic coverage documented in column headings. Regression weighted by cell population, excludes Mexico City (except column 6) and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

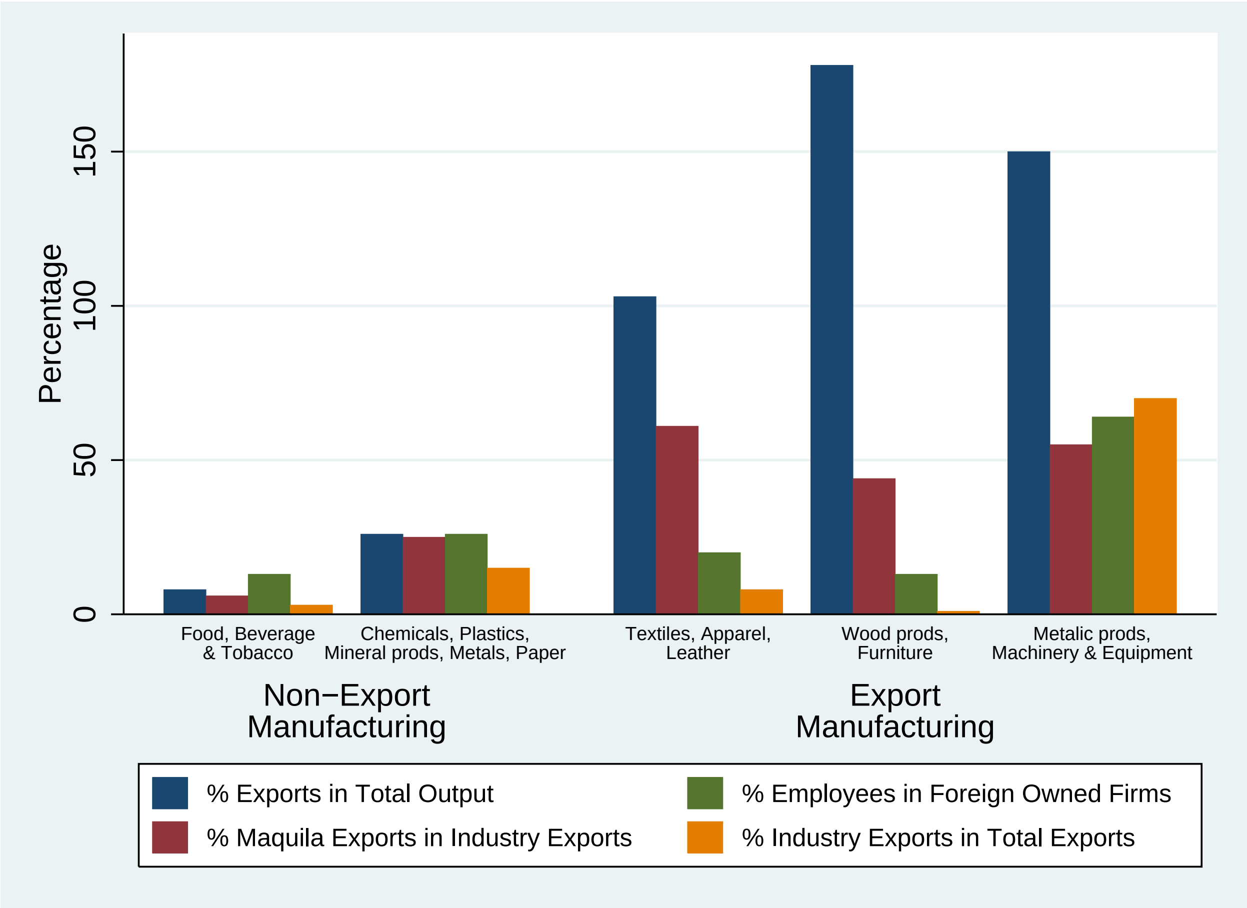
Table B.5: Robustness: Alternate Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	*Alternate School Measures*		*Sex Specific School*		$\lvert\Delta l_{mc}\rvert \geq 100$	*Pos/Neg*	*Hires/Fires*	*State Spillover*
				RF (Large Δs)					
	Cohort Schooling	Schooling Cap at 12	Schooling (Not at School)	Male Schooling	Female Schooling	Cohort Schooling	Cohort Schooling	Cohort Schooling	Cohort Schooling
Net New Export Manufacturing Jobs/Worker at Age 15-16	-2.618***	-2.018***	-2.751***			-2.683***			-2.629***
	(0.724)	(0.554)	(0.647)			(0.776)			(0.797)
Net New Male Export Manuf. Jobs/Worker at Age 15-16				-2.892***					
				(0.798)					
Net New Female Export Manuf. Jobs/Worker at Age 15-16					-1.354				
					(1.145)				
Positive Net New Export Manuf. Jobs/Worker at Age 15-16							-2.796***		
							(0.895)		
Negative Net New Export Manuf. Jobs/Worker at Age 15-16							-1.861		
							(1.874)		
New Hires in Export Manuf. Jobs/Worker at Age 15-16								-2.362***	
								(0.858)	
New Fires in Export Manuf. Jobs/Worker at Age 15-16								-3.135**	
								(1.248)	
State-Level Net New Export Manuf. Jobs/Worker at Age 15-16									0.864
									(1.823)
Observations	23,484	23,484	23,477	23,328	23,375	23,484	23,484	23,484	23,484
R^2	0.944	0.940	0.935	0.888	0.915	0.944	0.944	0.944	0.940
Municipalities	1808	1808	1808	1808	1808	1808	1808	1808	1808

Notes: Dependent variable is cohort average years of schooling in the year 2000, other than columns 2 to 5. Dependent variable in column 2 is cohort average schooling with schooling capped at 12 years. Dependent variable in column 3 is cohort average schooling for subset of students not currently attending school. Independent variable is net new export-manufacturing jobs per worker arriving in cohort's municipality at ages 15 and 16 attributable to firms that expand or contract their employment by 50 or more employees in a single year. Columns 4 and 5 focus on sex specific job arrivals. Column 6 raises the threshold to firms that expand or contract their employment by 100 or more employees in a single year. Columns 7 and 8 break up export job arrivals into years of positive and negative net new job arrivals (column 7) or net new job arrivals at firms expanding or contracting employment (column 8). Column 9 includes additional independent variable, the net new export job arrivals in other municipalities in the State. State-time dummies, municipality dummies and municipality linear trends not shown. Column 9 replaces state-time dummies with region-time dummies. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.

Appendix C Additional Figures

Figure C.1: Manufacturing Industry Features



Notes: These data cover the whole of Mexico and originate from Banco de Mexico, Nicita and Olarreaga (2007) and Ibarraran (2004). The measure of output used by Nicita and Olarreaga (2007) does not properly account for all the imported intermediate components that typify the Mexican export production, hence the major export assembly industries show export ratios of over 100 percent.

Figure C.2: Histogram of Employee Age by Industry

Note: The age distribution is calculated using the year 2000 Census for formal sector workers. A formal worker is defined as a worker insured by IMSS or equivalent insurance scheme.

Appendix D Heterogeneity Results Using Highly Agglomerated Industries

As noted in section 3.3, the demand for exports is driven by external demand shifts which are swept out by the time fixed effects. However, demand for domestic goods may be driven by local demand shocks which correlate with other time-location-varying omitted variables. For example, a positive shock to income may raise local demand and schooling, and upwards bias the job arrivals coefficient. In order to mitigate this concern, I use the IMSS employment data to calculate a Herfindahl index for state-level industrial concentration in the year 2000 at the lowest level of industrial classification in the database (129 manufacturing industries).

The Herfindahl index for industry j is equal to $\sum_r (s_{jr} - s_r)^2$, where s_{jr} is state r's share of total employment in industry j, and s_r is state r's share of total manufacturing employment. The values for the Herfindahl index range from less than 0.005 for food products made with cereals and for publishing, to a value of 0.65 for the manufacture of arms and ammunition and 0.75 for the manufacture of pens and pencils. 73 industries have Herfindahls above 0.1 and 53 industries have Herfindahls below that number.

If one of these industries is highly concentrated in a few states, demand is likely to be driven by national rather than local demand shifters. Therefore, as a robustness check, I repeat the analysis of section 5 but replace the non-export component of net new manufacturing jobs per worker with job growth only in the non-export industries which have Herfindahl indexes below 0.1. These results are reported in columns 1 through 12 of table D.6. Reassuringly, results for the skill level of jobs and the density interactions are similar to table 6 that uses the full sample of manufacturing jobs. The only exception is the wage premia result, where there is no longer a significant negative coefficient on the low-skill wage premia interacted with low-skill job arrivals.

Table D.6: High Herfindahl Sample: Exploring Heterogeniety Due to Job and Location Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Cohort Average Completed Years of Schooling, RF (Large Δs), High-Herfindahl i for Non-Export Manufacturing											
	Density$_c$		Density$_0$		Density$_c$	Density$_0$		Density$_c$	Density$_0$		Density$_c$	Density$_0$
Low Skill Net New Manuf.	-10.64***	14.13*	12.59*	-20.47*	-9.478	-5.859	-9.314**	17.03**	13.29*	-19.19*	-7.224	-5.715
Jobs/Worker at Age 15-16	(3.650)	(7.509)	(7.185)	(10.46)	(9.792)	(9.364)	(4.437)	(8.219)	(7.348)	(11.32)	(10.48)	(9.891)
Mid Skill Net New Manuf.	-1.695	-1.681	-1.511	-99.73	-120.9*	-96.80	-0.714	0.0909	-1.301	-98.25	-118.2*	-96.66
Jobs/Worker at Age 15-16	(2.875)	(5.206)	(5.609)	(69.36)	(67.53)	(63.24)	(3.055)	(5.508)	(5.854)	(69.35)	(67.69)	(63.10)
High Skill Net New Manuf.	4.230***	-0.740	1.780	-1.920	15.64	-7.332	4.496***	-0.687	1.757	-2.273	14.82	-7.367
Jobs/Worker at Age 15-16	(1.500)	(2.478)	(3.342)	(18.91)	(23.73)	(17.02)	(1.596)	(2.460)	(3.379)	(19.38)	(24.83)	(17.04)
Low Skill Net New Jobs×		-96.14***	-108.8**		-101.1***	-93.80**		-98.40***	-109.9***		-102.9***	-94.01**
Density of Marginal Youth		(32.89)	(42.69)		(34.07)	(40.70)		(33.00)	(42.43)		(34.09)	(40.12)
Mid Skill Net New Jobs×		-10.85	-15.18		-12.86	-32.05		-11.68	-14.16		-13.51	-31.85
Density of Marginal Youth		(26.54)	(37.97)		(25.75)	(38.41)		(26.63)	(37.88)		(25.84)	(38.06)
High Skill Net New Jobs×		26.97*	13.79		30.89**	17.50		29.20**	14.50		32.66**	17.63
Density of Marginal Youth		(14.11)	(22.39)		(14.62)	(23.16)		(14.69)	(22.89)		(15.20)	(23.61)
Low Skill Net New Jobs×				11.84	27.10**	17.56**				11.61	27.17**	17.55**
Wage Premia for Low Skill				(8.999)	(10.73)	(8.730)				(9.093)	(10.77)	(8.777)
Mid Skill Net New Jobs×				96.57	117.9*	96.70				95.88	116.7*	96.60
Wage Premia for Mid Skill				(68.82)	(66.09)	(65.00)				(69.01)	(66.48)	(64.87)
High Skill Net New Jobs×				6.197	-16.80	8.598				6.755	-15.93	8.629
Wage Premia for High Skill				(18.76)	(23.66)	(16.82)				(19.23)	(24.75)	(16.83)
Density of Marginal Youth		3.036***			3.051***			3.035***			3.050***	
		(0.149)			(0.148)			(0.149)			(0.148)	
Wage Premia for Low Skill				-0.044**	-0.041***	-0.045**				-0.044**	-0.041***	-0.045**
				(0.0183)	(0.0136)	(0.0185)				(0.0184)	(0.0136)	(0.0185)
Wage Premia for Mid Skill				0.0022	0.015	2.51e-05				0.0022	0.0152	3.07e-05
				(0.0180)	(0.0227)	(0.0189)				(0.0181)	(0.0228)	(0.0189)
Wage Premia for High Skill				0.0136	-0.0006	0.0134				0.0138	-0.000251	0.0134
				(0.0191)	(0.0220)	(0.0193)				(0.0191)	(0.0219)	(0.0193)
Net New Export Manuf.							-1.015	-1.759	-0.392	-0.807	-1.423	-0.0740
Jobs/Worker at Age 15-16							(1.563)	(1.673)	(1.494)	(1.518)	(1.634)	(1.472)
Observations	23,484	23,484	23,471	23,484	23,484	23,471	23,484	23,484	23,471	23,484	23,484	23,471
R^2	0.944	0.950	0.944	0.944	0.950	0.944	0.944	0.950	0.944	0.944	0.950	0.944
Municipalities	1808	1808	1807	1808	1808	1807	1808	1808	1807	1808	1808	1807

Notes: Dependent variable is cohort average schooling in the year 2000. Independent variables are net new jobs per worker arriving in cohort's municipality at ages 15 and 16 attributable to firms that expand or contract employment by 50 or more employees in a single year, as well as interactions for the density of marginal youth and the wage premia on offer. The job arrivals are categorized into low, mid and high skill based on the relative education level of employees in those municipality-subindustry pairs in the 2000 Census. Density$_c$ columns use the cohort-varying density proxy while Density$_0$ columns use the value of the density proxy for the two cohorts prior to the oldest cohort in the sample. State-time dummies, municipality dummies and municipality linear trends not shown. Regression weighted by cell population, excludes Mexico City and migrants. Municipality clustered standard errors in parentheses. * significant at 10 percent level, ** at 5 percent and *** at 1 percent.