Exporting and Individual Wage Premia: Evidence from Mexican Employer-Employee Data

David S. Kaplan
ITAM
kaplan@itam.mx

Eric A. Verhoogen
Columbia University
ev2124@columbia.edu

Jan. 2006

PRELIMINARY

Abstract

This paper draws on employer-employee data from the Mexican social security agency to investigate the relationship between exporting and individual wage premia. Following Verhoogen (2004), we show that the peso crisis of late 1994 generated a differential inducement to export within manufacturing industries, with initially larger and more productive plants increasing exports relative to initially smaller and less productive plants, and that average wages at the plant level followed the same pattern. We then use the longitudinal information on individual workers to decompose the average wage changes into changes in the skill composition of the workforce and changes in plant-year effects, which we interpret as measures of wage premia. We compare the results for manufacturing during the peso crisis period (1993-1997) to results for a later period during which there was no devaluation (1997-2001) and to results for non-tradable sectors in both periods. We show that two-thirds or more of the difference in differential changes in average wages in manufacturing between the two periods can be attributed to changes in wage premia. There were no such changes in wage premia in non-tradable sectors. These results provide strong support for the argument that the differential inducement to export generated by the peso crisis led plants to increase individual wage premia, and are consistent with the hypothesis that exporting requires upgrading product quality, which in turn requires raising wage premia.

1 Introduction*

Exporting plants pay higher average wages than non-exporting plants within the same industry. This fact is robust across datasets and across countries, developed and developing.[1] But does this correlation reflect a causal relationship between exporting — or, more precisely, the incentive to export — and individual wage premia?[2] The correlation between exporting and average wages may reflect two types of endogenous selection. First, plants endogenously select into the export market. Larger, more productive plants, which tend to be higher-wage, are more likely to enter the export market than smaller, less-productive ones. Second, workers endogenously select into plants. In a textbook neoclassical labor market, higher wages in larger, more productive, exporting plants would simply reflect the skill of individual workers; those workers would receive the same wage elsewhere on the market, and there would be no wage premia. It is not possible to identify the causal effect of exporting on wage premia either in standard plant-level datasets — which contain little information on worker characteristics and are silent on whether average wage changes at the plant-level reflect changes in wage premia or changes in workforce composition — or in standard household-level datasets — which contain little information on employers, and in particular lack information on exports at the plant level.

* An earlier version of this paper was circulated under the title, "Quality Upgrading and Establishment Wage Policies: Evidence from Mexican Employer-Employee Data." We would like to thank Rita Almeida, David Card, Wojciech Kopczuk, Steven Lich-Tyler, Osmel Manzano, Marc-Andreas Muendler, Cristian Pop-Eleches, Jesse Rothstein, Matthias Schuendeln, Matthew Slaughter, Till von Wachter, and many seminar participants for helpful comments. All errors are ours.

[1] Bernard and Jensen (1995) first noted this fact for the U.S. For a list of studies documenting this pattern, see Schank, Schnabel, and Wagner (2004).

[2] We make the distinction between the effect of exporting and the effect of the incentive to export because whether to enter the export market, how much to sell on the export market, and wages can be seen as simultaneous outcomes of a plant or firm's single optimization problem. The effect we focus on here is the effect in changes in the environment external to plants on both the exporting decision and wages.

In this paper, we draw on employer-employee data from the Mexican social security agency and a longitudinal plant panel from the Mexican statistical agency to estimate the effect of a shock to exports on individual wage premia at the establishment level. Following Verhoogen (2004), we argue that the Mexican peso crisis of late 1994 generated exogenous variation within each industry in the incentive of Mexican producers to export, with initially larger, more productive establishments more likely to increase exports than initially smaller, less productive ones. We compare the differential trends between initially larger and smaller plants in manufacturing in the peso crisis period to the differential trends in other periods and to the differential trends in the non-tradable sector, and attribute the difference in differential trends to the shock to exporting. This strategy, comparing subsequent wage changes for plants with different initial characteristics, avoids the confounding influence of endogenous selection into the export market. We use the information on individual wage histories in the employer-employee data to control for worker skill — including time-invariant unobservable dimensions of skill — and estimate plant-year effects on wages conditional on those characteristics. We interpret the estimated plant-year effects as measures of wage premia. Under standard assumptions about the mobility of workers between establishments, this strategy allows us to avoid the confounding influence of selection of highly skilled workers into exporting plants.

Why would exporting have an effect on individual wage premia? Our primary hypothesis, following Verhoogen (2004), is that the differential inducement to export of the peso crisis led to *differential quality upgrading* within industries in the Mexican manufacturing sector, as exporting plants shifted their within-plant product mix toward higher-quality varieties to appeal to richer U.S. consumers, and that this product quality upgrading required an upgrading of the workforce as well. This upgrading of the workforce may plausibly have taken two forms: attracting new workers with higher levels of human capital, or improving the productivity of existing workers, for instance by increasing efficiency wages or by investing in training. The innovation of this paper is to estimate the extent to which average wage increases in upgrading plants — documented in plant-level data by Verhoogen (2004) and confirmed in this paper — can be attributed to increasing wage premia. We consider several other hypotheses for the relationship between exporting and wage premia but argue that they are less plausible explanations for the empirical patterns we uncover than the quality-upgrading hypothesis.

Our argument proceeds in several steps. First, we present a simple theoretical model to spell out our argument. A key assumption in the model is that firms (or plants) differ in a single latent variable, λ, which we refer to as entrepreneurial ability. The value of this variable shapes firms' product-quality and wage decisions, as well as their response to the exchange-rate shock. Second, using plant-level data from the manufacturing sector we construct a variety of proxies for the latent entrepreneurial-ability variable and show that the patterns of differential within-industry increases in exports, upgrading of product quality, and increases in wages are robust to the choice of proxy. Third, using the employer-employee data for manufacturing and the only proxy available in those data, log employment,[3] we confirm the existence of what might be termed the "quality upgrading" effect of the peso crisis on plant-level average wages: initially larger plants saw relatively greater wage growth

[3] The employer-employee data and the plant-level data are potentially linkable, and negotiations between the Mexican social security agency and the Mexican statistical agency about sharing the data have begun. Once the data have been linked, a wider variety of proxies will be available.

than initially smaller plants within the same industry during the peso crisis period (1993-1997), and this differential wage change was greater than in a subsequent period with no devaluation (1997-2001).[4] Fourth, we decompose the plant-level average wages into changes in plant-year effects (i.e. plant-year specific wage premia) and changes in average skill of the workforce. We show that 67%-89% of the difference in differential changes in plant-level average wages is accounted for by changes in plant-level wage premia, depending on the specification we use. Fifth, we show that the pattern of changes in wage premia we observe in manufacturing is not present in the non-tradable sector, supporting our argument that the changes in wage premia are linked to increased exporting. Sixth, we show that our results are robust to a number of modifications that address concerns with our basic specification. Overall, we take these results as strong support for the hypothesis that the increased inducement to export led Mexican manufacturers to raise wage premia, and we argue that the most plausible explanation for this result is quality upgrading.

In addition to the papers cited above, our work is related to a number existing papers. It is most closely related to Schank, Schnabel, and Wagner (2004), who use linked employer-employee data from Germany and show that the export status of plants is no longer a significant predictor of individual wages once a large set of plant and individual characteristics have been controlled for. The advantage of our study is that we have an arguably exogenous shock to exports, and are able to avoid the possibility that an omitted variable correlated with export status is biasing our estimate of the effect of exporting on wages.

[4] It is important to note that this "quality upgrading" effect is based entirely on a within-industry comparison. A variety of between-industry and macro-level factors are clearly crucial in accounting for the overall pattern of aggregate wage changes in Mexico, but we do not focus on them here. For more on wage changes in Mexico, see Cragg and Epelbaum (1996); Feenstra and Hanson (1997); Revenga (1997); Hanson and Harrison (1999); Meza (1999); Feliciano (2000); Esquivel and Rodriguez-Lopez (2003); Robertson (2000, 2003, 2004).

Our paper is related more generally to the literature on heterogeneity of wage policies across plants. In both developed and developing countries, it has long been established that, controlling for observable individual characteristics, some industries consistently pay more than others (Dickens and Katz, 1987; Krueger and Summers, 1986; Moll, 1993) and large firms pay more than small firms (Brown and Medoff, 1989; Velenchik, 1997; Schaffner, 1998). A perennial issue is whether this heterogeneity is due to differences in the unobserved skills of workers or to differences in wage premia across establishments (Murphy and Topel, 1987). The growing availability of matched employer-employee data has made it possible to address this question, and a series of recent papers has found (under a the same assumption on worker mobility that we impose here) that a significant portion of inter-industry and firm-size wage differentials in France and the U.S. can be attributed to differences in firm- and plant-level wage premia (Abowd, Kramarz, and Margolis, 1999; Abowd, Finer, and Kramarz, 1999; Abowd and Kramarz, 1999b,a). Abowd, Kramarz, and Moreau (1996) find a correlation between worker quality and product quality — proxied by product price — in French data, but they do not address the issue of causality. Our contribution to this literature is a research design that allows us to relate arguably exogenous variation in product market conditions to establishment-level premia. Also, to our knowledge, ours is the first paper to use employer-employee data to estimate wage premia at the plant level in a developing country.[5]

Our paper is related to papers on the relationship between trade and skill upgrading in developing countries (Feenstra and Hanson (1996); Davis (1996); Robbins (1996); Kremer and Maskin (2003); Pavcnik (2003); Zhu and Trefler (2005); Antras, Garicano, and Rossi-

[5] Haltiwanger and Vodopivec (2003) analyze employer-employee data from Slovenia, but focus on worker and job flows. Menezes and Muendler (2005) use employer-employee data from Brazil to analyze labor reallocation following trade reform.

Hansberg (forthcoming); see Goldberg and Pavcnik (2004) for a review, and Bernard and Jensen (1997) on the U.S.); to papers exploring the role of product quality in trade (Shaked and Sutton, 1982; Flam and Helpman, 1987; Stokey, 1991; Copeland and Kotwal, 1996; Murphy and Shleifer, 1997; Brooks, 2003; Schott, 2004; Hallak, forthcoming; Hallak and Schott, 2005); and to papers relating international trade to labor effort (Copeland, 1989; Brecher, 1992; Matusz, 1996; Leamer, 1999). Our paper is complementary to research on the effect of foreign ownership on wages (Aitken, Harrison, and Lipsey, 1996; Lipsey and Sjöholm, 2004); it may be that one reason why foreign-owned firms pay more than domestically owned ones is that they are more "able", and produce higher quality goods for the export market.

2 Theory

This section develops a model of monopolistic-competition model of trade with heterogeneous producers, combining ideas from Melitz (2003); McFadden (1974); Anderson, de Palma, and Thisse (1992); Burenstam Linder (1961); Kremer (1993) and others. A number of caveats apply: the model is partial-equilibrium, static, and with special functional forms. The model differs from the model of Verhoogen (2004) in that it distinguishes between (contractible) skill and (non-contractible) effort, but is otherwise similar in structure to that model, and readers are referred there for a less terse exposition.

There are two countries, North (n) and South (s). Firms are constrained to produce no more than one good for each market. Firms can sell a different quality product and charge a different price in each market. They can also make different optimization decisions on the production line for goods destined for North than the line for goods destined for South. Let $c = s, n$ index the country in which a plant is located; and $d = s, n$ index the destination market for a particular production line. In each country, there is a mass of potential entrepreneurs, heterogeneous in a (fixed) productivity parameter λ, which can be thought of as representing entrepreneurial ability, with distribution $g_c(\lambda)$. As is standard in monopolistic competition models, it will turn out that each producer differentiates her good and has a monopoly over the production of her own variety; hence λ will also uniquely identify goods within the set of goods produced by firms in a particular location for a particular destination market, and we use λ to index both goods and entrepreneurs.

Assume that aggregate demand for differentiated good produced by entrepreneur with ability λ in country c and sold in domestic market d is given by:

$$x_{cd}(\lambda) = \frac{N_d}{D_d} \exp\left\{\theta_d q_{cd}(\lambda) - \frac{p_{cd}(\lambda)}{\delta_{cd}}\right\} \tag{1}$$

where

$$D_d \equiv \sum_{c=n,s} \int_{\lambda_{cd} \in \Lambda_{cd}} \exp\left\{\theta_d q_{cd}(\lambda) - \frac{p_{cd}(\lambda)}{\delta_{cd}}(\lambda)\right\} g_c(\lambda)\, d\lambda \tag{2}$$

N_d is the number of consumers in destination market d, taken to be exogenous. θ_d is the willingness of consumers in country d to pay for quality, assumed to be greater in North than in South: $\theta_n > \theta_s$. This difference can be derived from differences in income, and is the only difference between Northern and Southern consumers. $q_{cd}(\lambda)$ is the quality of good produced by entrepreneur with ability λ in country c for market d. $p_{cd}(\lambda)$ is the price of the good, *denominated in terms of goods in country c*. δ_{cd} is the ratio of goods prices in country d to goods prices in country c, otherwise known as the real exchange rate. The term $\frac{p_{cd}(\lambda)}{\delta_{cd}}$ thus represents the price charged by producer λ in location c for market d, denominated in terms of goods in country d, which is the appropriate measure in the consumer demand equation in country d. Λ_{cd} represents the set of all producers from country c that enter destination market d. D_d thus represents a sum over all producers (from either country) that enter market d. This expression for aggregate demand follows from a standard multinomial-logit specification of discrete choices by individuals, where each individual purchases one unit of a good in the industry and the utility of the good includes a random term with an extreme-value distribution.[6]

Production of one unit of output is assumed to require one worker. Production of quality is assumed to depend on three different inputs: entrepreneurial ability, λ, human capital, h, and effort, e, in Cobb-Douglas fashion:

$$q_{cd} = A\lambda(h_{cd})^{\beta_h}(e_{cd})^{\beta_e} \tag{3}$$

where $\beta \equiv \beta_h + \beta_e$ and we assume $\beta < 1$ to ensure an interior solution in the choice of quality. A is a constant term that captures the general level of technology. This specification borrows from Kremer (1993) the insight that when producing high-quality goods, a large number of low-quality workers cannot substitute for one high-quality one.[7] Note that this specification nests two special models: setting $\beta_e = 0$ yields a model in which quality depends only on the inherent skill of workers, as in Kremer (1993); setting $\beta_h = 0$ yields a model in which quality depends only on effort, as in Dalmazzo (2002).

We assume that there exists an exogenously given wage-skill schedule in the outside labor

[6] See Verhoogen (2004) for details.

[7] A situation in which high-quality workers are assigned to high-quality jobs may also arise in an assignment model of the type reviewed by Sattinger (1993).

market, linear and passing through the origin:

$$h_{cd} = z_h \widetilde{w}_{cd} \tag{4}$$

where z_h is a positive constant. The wage $\widetilde{w}_{cd}$ represents the minimum required to induce a worker of human capital h_{cd} to show up for work; we refer to it as the *market wage* for a worker with human capital h_{cd}. We assume that the wage-skill schedule has the same slope z_h in both countries, although this assumption could easily be relaxed. We assume that effort is a function of the gap between what a worker receives in the firm and her market wage. Let v represent the gap, or quasi-rent, where $v \equiv w_{cd} - \widetilde{w}_{cd}$. For simplicity, we assume that effort is a linear function of the quasi-rent passing through the origin:

$$e_{cd} = z_e v_{cd} = z_e \left(w_{cd} - \widetilde{w}_{cd} \right) \tag{5}$$

where z_e is a positive constant.[8]

The key distinction between human capital and effort is that the former is contractible and transferable across firms such that it earns the same return in a particular firm as on the outside market, while the latter is imperfectly transferable or subject to contracting problems such that workers earn a premium above what they would earn in the outside market. If workers have firm-specific skills that are not valued on the market but that generate benefits to the firm, some of which are in turn shared with workers, those firm-specific skills are best interpreted as part of "effort" in our model. Similarly, if effort is observable and verifiable, as for instance in Leamer (1999), such that firms and workers can write contracts over the

[8] The assumptions that the wage-skill and wage-effort schedules are linear and passing through the origin are clearly simplistic, but more flexible specifications would seem to impose a greater algebraic burden with few compensating insights.

amount of effort that workers supply, then it is best interpreted as part of "human capital."

Firm choose the price, (p), the level of human capital, (h), and the wage (w), which in turn determine worker effort. Marginal cost per unit of output is constant, and equal to the wage of the single worker producing a single unit of output. The fixed cost to produce in the domestic market is f; the fixed cost to produce for the export market is f_x, where the domestic fixed cost has to be paid prior to exporting. It is convenient to write these fixed costs as f_{cd} where $f_{cd} = f + f_x$ for exporters and f for domestic producers.

The combination of constant marginal cost (conditional on quality) and the fixed cost of entry generates increasing returns to scale. There is no cost to differentiation. As a consequence, all firms differentiate and have a monopoly in the market for their particular variety. Profit is given by:

$$\pi_{cd}(\lambda) = (p_{cd} - w_{cd})\, x_{cd} - f_{cd} \tag{6}$$

Each firm is assumed to treat the aggregate quantity in the denominator of the expression for demand, D_d in equation 1, as unaffected by its own choices. Optimizing over the choice of p, h, and w, and solving for q, we have:

$$q_{cd}(\lambda) = \eta(\lambda)^{\frac{1}{1-\beta}} (\delta_{cd}\theta_d)^{\frac{\beta}{1-\beta}} \tag{7}$$

where $\eta \equiv (A)^{\frac{1}{1-\beta}} (z_h\beta_h)^{\frac{\beta_h}{1-\beta}} (z_e\beta_e)^{\frac{\beta_e}{1-\beta}}$. The optimal quality of good produced is increasing in both λ and θ. An entrepreneur with higher ability will produce higher quality for a given market. Conditional on a given level of ability, an entrepreneur will produce a higher quality good for the Northern than for the Southern market.

The optimal choices of human capital, h, and wage, w, and the corresponding level of effort, e, can be summarized in terms of the optimal quality level:

$$\begin{aligned} h_{cd}(\lambda) &= \theta_d \delta_{cd} z_h \beta_h q_{cd}(\lambda) \\ w_{cd}(\lambda) &= \theta_d \delta_{cd} \beta q_{cd}(\lambda) \\ e_{cd}(\lambda) &= \theta_d \delta_{cd} z_e \beta_e q_{cd}(\lambda) \end{aligned} \tag{8}$$

These variables follow the same pattern as quality: all are increasing in both λ and θ. Note further that the Cobb-Douglas form of the production function for quality implies that the shares of the wage made up of the "market wage", $\widetilde{w}$, and the quasi-rent, v, are constant:

$$\begin{aligned} \widetilde{w}_{cd}(\lambda) &= \frac{\beta_h}{\beta} w_{cd}(\lambda) \\ v_{cd}(\lambda) &= \frac{\beta_e}{\beta} w_{cd}(\lambda) \end{aligned} \tag{9}$$

Thus the market wage and quasi-rent components of wages also follow the same cross-sectional pattern as product quality.

Although we have modeled plants as making separate wage decisions on each production line, we do not observe wages by production line. The wages (and hence market wages and quasi-rents) we observe correspond to weighted averages across production lines, where the weights are given by the export and domestic-market shares of production.

As is standard in logit demand models, the mark-up is constant: $p_{cd}(\lambda) - w_{cd}(\lambda) = \mu \delta_{cd}$. Profit can then be written:

$$\pi_{cd}(\lambda) = \frac{\mu \delta_{cd} N_d}{D_d} \exp\left\{ \frac{\theta_d q_{cd}(\lambda) - p_{cd}(\lambda)}{\mu} \right\} - f_{cd} \tag{10}$$

where D_d is defined as in equation (2). It is straightforward to show that profit is increasing in λ and θ_d. The fact that profitability is increasing in λ implies that for each location-destination pair there is a single equilibrium cut-off value of productivity, call it $\lambda_{cd}^{\min}$, above which all plants will enter and earn positive profits, and below which no plants will enter. The cut-off is defined implicitly by the requirement that the marginal plant have zero profits. The fact that plants must enter the domestic market before entering paying the export market means that $\lambda_{sn}^{\min} > \lambda_{ss}^{\min}$ and $\lambda_{ns}^{\min} > \lambda_{nn}^{\min}$. Within the set of potential entrepreneurs in each country at a given time, we have three distinct groups of plants: plants that do not enter either market (non-entrants), plants that enter only the domestic market (non-exporters), and plants that enter both markets (exporters).

Figure 1 summarizes the relationship between product quality and λ in the cross-section of Southern plants, prior to an exchange-rate shock. The $q_{ss}(\lambda)$ curve represents the quality that a Southern plant with ability λ produces for the Southern market; the $q_{sn}(\lambda)$ curve represents the corresponding quality for the Northern market. As mentioned above, both are increasing in λ, and for a given λ, $q_{sn}(\lambda) > q_{ss}(\lambda)$. The $\overline{q}_s(\lambda)$ (dotted) curve represents the counterfactual average quality that would obtain if all plants entered both markets. Not all plants enter both markets: entrepreneurs with $\lambda < \lambda_{ss}^{\min}$ enter neither market; those with $\lambda_{ss}^{\min} \leq \lambda < \lambda_{sn}^{\min}$ enter only the domestic market; those with $\lambda_{sn}^{\min} \leq \lambda$ enter both markets. What we actually expect to observe, both in product quality and in wages, is given by the curve in bold. Note that we would expect to observe the same qualitative pattern for average wages at the plant-level, plant-level average "market wages", and plant-level average quasi-rents.

We are now in a position to consider again the argument from the introduction that a wage difference between exporters and non-exporters does not necessarily reflect an positive effect of exporting on wage premia. Suppose for the moment that $\theta_n = \theta_s$. In this case, the q_{sn} curve would lie on top of the q_{ss} curve in Figure 1, and the extent of exporting would have no effect on wages. We would nevertheless observe that higher-λ plants would pay higher average wages overall, higher "market wages," and higher quasi-rents than non-exporters.

We model an exchange-rate shock as having two exogenous effects: first, the real exchange rate, δ_{sn}, rises; second, total consumer demand in South, represented by the number of Southern consumers, N_s, falls. The analysis of the effect of the shock on plants' entry decisions (and hence the shares of export and domestic production that enter the wage averaged across production lines) is somewhat tedious and is the same as that presented in Verhoogen (2004), so we do not reproduce it here. Instead, we sketch the intuition informally. Figure 2 summarizes the effect of the exchange rate shock on the level average quality within each plant. The $q_{sn}(\lambda)$ curve shifts up in response to the shock, since quality is cheaper to produce relative to what consumers are willing to pay for it. The dotted counterfactual $\overline{q}_s$ curve also shifts up, reflecting both the increase in quality on the export line and the increase in the export share of output. The thinner, dark solid line reproduces the solid curve in Figure 1. The thicker, gray solid line represents the actual average quality curve post-crisis. Of the plants that were only in the domestic market before the crisis, some exit,[9] some remain only in the domestic market, and some enter the export market. Plants that were exporting before the crisis increase exports. Figure 3 depicts the *change* in average quality as a function of λ, that is, the difference between the thick gray and the thin black curves in Figure 2.

[9] This is theoretically ambiguous: although the domestic market shrinks, domestic producers have a greater cost advantage than before the crisis. Empirically, however, it is the case that exit of low-productivity plants increased sharply during the peso crisis.

It can be shown that the change in the plant-level average wage, $d\overline{w}_s(\lambda)$, follows the same pattern as the change in average quality illustrated in Figure 3. From equation 9, it follows directly that the changes in market wages and quasi-rents are proportional to the overall wage change:

$$
\begin{aligned}
d\overline{\widetilde{w}}_s(\lambda) &= \frac{\beta_h}{\beta} d\overline{w}_s(\lambda) \qquad (11) \\
d\overline{v}_s(\lambda) &= \frac{\beta_e}{\beta} d\overline{w}_s(\lambda)
\end{aligned}
$$

A central goal of our empirical work will be to estimate the parameters β_h and β_e, and thereby decompose the differential plant-level wage changes (observed in Verhoogen (2004) and confirmed in our data) into market wage and quasi-rent components. Note that if we only had data on average wages at the plant level, these parameters would not be separately identified, and we would not be able to distinguish sorting by unobserved skill from changes in wage premia at the plant level. An important caveat to our empirical implementation is that imprecision in our proxies for λ prevent us from estimate the exact functional form illustrated in Figure 3. We must content ourselves with estimating linear relationships between initial λ and subsequent changes in product quality and wage measures.

3 Econometric Approach

The model discussed above suggests an empirical approach based on comparisons of differential changes between initially higher-λ and initially lower-λ plants across time periods and sectors: if the quality-upgrading story is correct, we expect to see greater differential product quality and wage changes in the period of the peso crisis than in a period without a devaluation, and greater differential changes in tradables (manufacturing) than in non-tradables (construction, retail, services, transportation). These comparisons form the basis of our identification strategy.

Our basic econometric model has the following form:

$$\triangle y_j = \mu + \hat{\lambda}_{0j}\gamma + D_j\pi + \epsilon_j \tag{12}$$

where j indexes establishments, $\triangle y_j$ is a change in an outcome variable of interest, $\hat{\lambda}_{0j}$ is a proxy for the latent entrepreneurial ability variable taken from an initial year (discussed in more detail in Section 5 below), and D_j is a vector of industry and state dummies. The outcome variables we focus on are the export share, an indicator of product quality, and wage measures. In Section 6.2 below, we describe how we estimate measures of the "market wage" and quasi-rent components of wages, and include them as outcome variables in this equation.

The coefficient of interest in these regressions is γ, which captures a differential change in the outcome by the initial value of the entrepreneurial ability proxy. We estimate the model separately for the peso crisis period (1993-1997) and a later period without a devaluation (1997-2001), and separately for tradables and non-tradables.[10] If there were no other differential influences on large and small plants besides the exchange rate, we would expect $\gamma > 0$ in 1993-1997 in tradables and $\gamma = 0$ in 1997-2001 or in tradables. However, a more plausible specification would allow for some differential evolution between high-λ and low-λ plants, as would be predicted by an industrial evolution model such as that of Jovanovic (1982). For our purposes, the crucial prediction of the model is that γ is larger in 1993-1997

[10] Our choice of the periods 1993-1997 and 1997-2001 is determined largely by availability of the EIA plant-level data. The main EIA panel begins in 1993, and we have access until 2001. We have experimented with alternative periodizations, and have found results similar to those reported here.

and in tradables than in the other period or sector.

The key assumption underlying our approach is that differences between the estimates of γ in tradables in 1993-1997 and the estimates for non-tradables or the non-tradables sector can be attributed to the peso crisis and the quality-upgrading mechanism outlined above. Our approach is analogous to a difference-in-difference-in-differences (D-in-D-in-D) strategy: we compare differences in changes in outcomes between high-λ and low-λ plants in a "treated" period, 1993-1997, and a "control" period, 1997-2001. When we add the comparison with the non-tradables sector, our approach is analogous to a quadruple-difference design. The analogy is not fully accurate in the sense that λ is a continuous rather than a binary variable, but it points out that our approach shares the vulnerability of nearly all difference-in-difference-type designs that there may be unrelated differential trends confounding our results. We address what we see as the main counter-arguments as we proceed below.

4 The Data

The employer-employee data used in this paper are drawn from the administrative records of the *Instituto Mexicano del Seguro Social (IMSS)*, the Mexican social security agency.[11] All private, formal-sector Mexican employers are required to report wages for their employees, and pay social-security taxes on the basis of their reports. The raw data can be considered a census of private, formal-sector establishments and their workforces for 1985-2001. The number of workers in the raw data at a given point in time ranges from approximately

[11] The data have been obtained and cleaned by David Kaplan, and used in several previous papers (Castellanos, Garcia-Verdu, and Kaplan, 2004; Kaplan, Martinez Gonzalez, and Robertson, 2004, 2005).

5 million in 1985 to approximately 11 million in 2001. Kaplan, Martinez Gonzalez, and Robertson (2004, 2005) compared IMSS employment figures for manufacturing to the 1993 Industrial Census, and found that approximately 90% of manufacturing workers in private firms were included in the IMSS data.

The IMSS data contain information on the age, sex and daily wage of individuals, in addition to the state and year of the individual's first registration with IMSS and an individual identifier. The wage figures are based on a measure of total compensation, called the *salario base de cotización*, which includes both wages and benefits, including payments made in cash, bonuses, commissions, room and board, overtime payments, and in-kind benefits. The raw data include a start date and an end date for each wage earned for each individual in each establishment. When an individual's wage changes, the record for the old wage is closed and a record for the new wage is opened. We extracted data for September 30 for each year. At the establishment level, the data contain information only on location and industry (using the IMSS's own 271 4-digit industrial categories). Further details are in the data appendix.

We supplement the employer-employee data with information from two plant-level surveys of the manufacturing sector conducted by the *Instituto Nacional de Estadísticas, Geografía e Información (INEGI)*, the Mexican statistical agency. The *Encuesta Industrial Anual (EIA)* [Annual Industrial Survey], a yearly plant panel, contains basic balance sheet information as well as data on employment, hours worked, and total wage bill for two separate occupational categories, *empleados* and *obreros*, corresponding to white-collar and blue-collar workers.[12] The EIA does not include information on the *maquiladoras*, the assembly-for-export plants

[12] The white-collar/blue-collar breakdown comes from the *Encuesta Industrial Mensual (EIM)*, a less extensive but higher-frequency survey conducted on the same sample as the EIA.

located mainly along the U.S. border. The sampling design of the EIA is somewhat unorthodox: the largest plants in 205 6-digit industries were selected deterministically in 1993, and those plants were followed over time, with minimal and haphazard refreshing of the sample. After cleaning, we are left with a balanced panel of 3,255 plants over the period 1993-2001.[13] For details, refer to the data appendix of Verhoogen (2004).

The *Encuesta Nacional de Empleo, Salarios, Tecnología y Capacitación (ENESTyC)*, [National Survey of Employment, Wages, Technology and Training], contains information on a variety of qualitative plant characteristics. The ENESTyC includes information on whether a plant has ISO 9000 certification, an international production standard commonly associated with product quality. We draw on information from four waves of the ENESTyC, in 1992, 1995, 1999 and 2001, each of which mainly contains information from the previous calendar year (1991, 1994, 1998 and 2000). A subset of plants from the EIA 1993-2001 panel can be linked to the ENESTyC: 1791 in 1994; 850 in all three years 1994, 1998 and 2000. The ENESTyC includes some maquiladoras, but we exclude them since they have no matching record in the EIA.

The employer-employee data and the plant-level data are potentially linkable. On our initiative, IMSS and INEGI have entered negotiations about sharing the data, and we are hopeful that we will be able to link the datasets in the near future, although we have not been able to do so for this version of the paper. In anticipation of being able to link the IMSS employer-employee data to the INEGI plant-level data, we construct balanced panels of establishments in the employer-employee data that are comparable to the EIA plant-level

[13] An earlier panel with a similar sampling design is available for the 1984-1994 period. After linking and cleaning the 1984-1994 and 1993-2001 panels, it is possible to follow approximately 1,000 plants over the entire 1984-2001 period. Results from this EIA long panel will be incorporated in future drafts.

panel. We have also been guided by the goal of maintaining the same sample size in our various specifications. In what we refer to as the IMSS manufacturing panel, we include manufacturing plants that have at least 50 workers in all years 1993-2001, that have at least one leaver (worker who switches out) and one entrant (worker who switches in) in each year, and that have at least one worker who has stayed and one who will stay for four years or the maximum possible given our time period, whichever is smaller. We also construct an IMSS non-tradables panel, using the same criteria for establishments in non-tradable sectors: construction, retail trade, transportation and services. For the purposes of identifying plant effects, we further require that each establishment be part of the largest group of "connected" establishments, where connected means sharing at least one worker at some point during the relevant period with another connected firm. The manufacturing and non-tradables panels contain 3628 and 3659 establishments respectively.

Summary statistics on the panels appear in Table 1. Within each sector, larger establishments tend to pay higher wages. Worker separation rates in our data are somewhat larger than the rates in the U.S., but of the same order of magnitude. (For further details, see Kaplan, Martinez Gonzalez, and Robertson (2004).) Importantly, the cumulative turnover rates are large enough to be consistent with significant changes in the composition of the workforce in each establishment. Summary statistics for the EIA panel for 1993, broken down by export status, appear in Table 2. The patterns are similar to those documented for the U.S. by Bernard and Jensen (1999), and subsequently for many other countries: exporters are larger, more capital-intensive, higher-wage, more likely to be foreign-owned than non-exporters, and are a minority of plants in each industry. In addition, the second panel of the table presents information from the ENESTyC and shows that exporters are more likely to have ISO 9000 certification and more likely to have a formal employee training program than non-exporters.

5 Results from EIA Plant-Level Data

A crucial first step in our econometric approach is to choose a proxy for the latent entrepreneurial-ability variable, λ. The model suggests that a number of observable variables at the plant level — the export share, indicators of product quality, sales in each market (export and domestic), employment, and wages — will be correlated with the underlying latent variable in cross-section. In a model with capital, we would also expect capital-intensity to be correlated with λ. If receiving foreign direct investment (FDI) affords a plant access to the technical knowledge of the parent firm, we might expect FDI to be correlated with λ as well. In principle, there are a variety of ways in which we might combine these variables to construct a proxy for λ.

In this version of the paper, we have not yet been able to link the IMSS employer-employee data to the EIA plant-level data, and hence we just have one proxy available in the employer-employee data: log employment.[14] In this section, we use information from the EIA plant-level data to show that log employment is highly correlated with a number of other plausible proxies, and that the basic results on plant-level differential changes in exports, product quality and wages are robust to the choice of proxy.

In the EIA panel, we construct six different proxies. In all cases, we use information from an initial year (or years), i.e. 1993 or 1997 (1993-94 or 1997-98 in the case of TFP).

[14] We avoid using wage levels as a proxy for λ, since changes in wages are our primary outcome variable.

- Log domestic sales: This proxy has the practical advantage that it is relatively well-measured and the theoretical advantage that it bears a smooth, continuously differentiable relationship to λ, since, unlike other variables, sales are measured separately by production line.

- Export share index: We run a tobit of the export share of sales on log hours worked (separately by occupation), log domestic sales, log capital-labor ratio, and an indicator for whether the plant has $\geq$ 10% foreign ownership; we then recover the predicted values ($x_j'\hat{\beta}$ where x_j is a vector of the independent variables and $\hat{\beta}$ are the estimated coefficients), deviate from industry means, and standardize the variable.

- ISO 9000 index: Using the subset of observations that can be linked between the EIA and the ENESTyC, we run a probit of whether or not a plant has ISO 9000 certification on log hours worked (separately by occupation), log domestic sales, log capital-labor ratio, the FDI indicator, and export share of sales.[15] We recover the estimated coefficients, and calculate the predicted $x_j'\hat{\beta}$ for all plants in our EIA panel, deviate from industry means and standardize.

- First principal component: We take the first principal component (the vector capturing the maximum proportion of joint variance) of log total hours worked (separately by occupation), log domestic sales, log capital-labor ratio, the FDI indicator, and export share of sales. We deviate from industry means and standardize.

- Total factor productivity (TFP): We regress log value-added (sales minus costs) on log

[15] We regress the ISO 9000 certification information from the 1995 ENESTyC (pertaining to 1994) on information from the 1993 EIA, and ISO 9000 from the 1999 ENESTyC (pertaining to 1998) on information from the 1997 EIA.

hours worked (separately by occupation), log capital stock,[16] year dummies and plant dummies for the two initial years of a period (i.e. 1993 and 1994, for the 1993-1997 period), separately for 9 two-digit industries. We then recover the coefficient estimates on the plant dummies, deviate them from industry means, and standardize.[17] This approach has the advantage that year-specific measurement error in value-added is not treated as part of TFP.

- Log employment: This proxy has the advantage of being well-measured, and, as mentioned above, the only proxy available in the IMSS employer-employee data.

Table 3 presents bivariate correlations between these proxies for 1993. Five of these six proxies are highly correlated. The exception is the TFP measure, but even for TFP we can reject the hypotheses of no bivariate correlations with ease.

We now turn to the evidence on the differential effect of the peso crisis within industries, using our various proxies for the entrepreneurial-ability variable, λ. It is important to note, first of all, that the crisis represented an enormous shock. The Mexican peso lost approximately 50% of its nominal value, and nearly as much of its real value, in a matter of days at the end of December, 1994. Figure 4 plots the real exchange rate over the 1984-2001 period. GDP fell by 6.7% from 1994 to 1995. Exports rose sharply, with approximately 85% destined for the U.S. market. Figures 5a-c, which are based on the EIA 1993-2001 balanced panel, illustrate that domestic sales dipped and export sales rose, with the result that the export share of sales rose sharply. The number of plant with positive exports rose from

[16] Calculated following the permanent-inventory method described in Olley and Pakes (1996).

[17] We thank Matthias Schuendeln for suggesting this approach.

approximately 30% to 45% of the sample, but the new entry was slower than the increase in the export share of sales.[18]

Consider the differential export response to the shock. Columns 1 and 2 of Table 4 present results of 12 regressions of the form of equation 12 (six proxies, two periods each), where the change in the export share is the dependent variable and the coefficients on the industry and region dummies have been omitted. (Column 3 presents the differences between the 1993-1997 and 1997-2001 coefficients.) We see that in the 1993-1997 period, higher-λ plants within each industry saw greater export growth than lower-λ plants, and this differential change was significantly greater than in the 1997-2001 period. This basic pattern is robust to the choice of proxy. To gain a sense of the magnitudes of the effects, note that if we compare two plants, one of which was a factor e larger than the other in domestic sales terms, the larger plant saw export growth approximate 1.9% greater than the smaller plant over the 1993-1997 period, and .6% greater over the 1997-2001 period.

One concern with the specification in Columns 1-2 is that domestic sales, which appear in the denominator of the export share, also enter the initial level of the various proxies on the right-hand side; a spuriously large coefficient on the entrepreneurial-ability proxy may result. To address this concern, we also estimate an instrumental variables model, where we use the value of the proxy from 1993 as an instrument for the 1994 value, and use the change in export share from 1994-1997 as the dependent variable. Columns 4-6 present the results.

[18] It is worth emphasizing that the peso crisis was a much larger shock than the North American Free Trade Agreement (NAFTA), which took effect in January 1994. Mexico's main trade liberalization came with its entrance into the General Agreement on Tariffs and Trade in the mid-1980s, and by 1994 the vast majority of Mexican imports were covered by tariffs of 20% or less. Average U.S. tariffs on goods from Mexico were on the order of 3-5%. In the majority of cases, NAFTA phased out existing tariffs slowly over time. Relative to the exchange-rate devaluation, the year-by-year tariff changes were quite small.

The qualitative message is unchanged.

Product quality is difficult to capture in large datasets. The best measure we have of product quality for a large set of plants is ISO 9000 certification. The ISO 9000 certification process is largely focused on how systematized and rationalized management processes are, but the certification is commonly believed to reflect product quality and reliability as well. We have information on 844 plants that appear both in the EIA and in the three waves of the ENESTyC in which ISO 9000 information is available. Table 5 reports regressions of the form of equation 12, with the change in an ISO 9000 indicator variable as the outcome, estimated as simple linear probability models. Because of the reduction in sample size, we include dummies for 4-digit industries (of which there are 50) rather than 6-digit industries. The point estimates are generally consistent with the quality upgrading hypothesis: initially higher-λ plants were more likely to acquire ISO 9000 certification over the 1994-1998 period than initially lower-λ plants, and this difference in rate of acquisition was greater in the 1994-1998 period than in the 1998-2000 period.[19] These results must be accompanied by the caveat that although the coefficients on the λ proxies for 1994-1998 are mostly statistically different from zero, they are not significantly different from the coefficients for 1998-2000. In addition, one should note that the earlier period is longer than the later period, and we would naturally expect the coefficient for the earlier period to be larger. For these reasons, we regard the evidence from ISO 9000 certification to be suggestive rather than definitive.

Finally, consider wage changes. Table 6 is organized like Table 4, with the change in

[19] These results are consistent with case study evidence suggesting that the shift in production toward the export market was accompanied by an increase in the average quality of good produced in exporting plants. For instance, Verhoogen (2004) presents evidence that the Volkswagen plant in Puebla, Mexico, shifted production from Old Beetles for the Mexican market to Jettas and Golfs for the U.S. market, with a corresponding increase in technical sophistication and skill requirements.

the log average wage at the plant as the outcome variable, using the full EIA panel. The results are again consistent with the quality-upgrading story: the coefficients for 1993-1997 are significantly greater than for 1997-2001, and the results are not sensitive to the choice of proxy.[20] The motivation for including the IV results is again measurement error. Average wages in the EIA are calculated by dividing the wage bill by employment; measurement error in the initial level of employment would induce a spurious positive correlation between initial employment and the change in wages. A related concern is that establishments were heterogeneous and inconsistent in how they reported low-wage temporary blue-collar workers. If an establishment reported temporary workers as employees in an initial year but not in a later year, we would expect a similar bias. The IV results address both of these concerns, and are similar to the OLS results.

6 Results from Employer-Employee Data

6.1 Replicating Plant-Level Results

We now turn to the IMSS employer-employee data, using log employment as our proxy for entrepreneurial ability. We first attempt to replicate the results from the plant-level data in the employer-employee dataset. Part A of Table 7 reproduces (with some reorganization) the results from the bottom row of Table 6. Part B of the table presents analogous results using the IMSS employer-employee tradables panel. The results are quite similar across the two datasets. There is again a statistically significant difference between the results for 1993-1997 and 1997-2001 periods. The hypothesis that the estimates of the differences between the 1993-1997 and 1997-2001 coefficients are equal across datasets cannot be rejected at conventional

[20] Note that this is a purely within-industry comparison. Real wages overall fell sharply in the 1993-1997 period; the results indicate that they fell less in higher-λ plants within each industry.

levels of confidence. The similarity of the results across datasets is especially reassuring for two reasons. First, establishments have an incentive to under-report employees' wages to the IMSS. The fact that the results are similar to results using reported wages in the EIA, which have no bearing on social security taxes or other government payments, suggests that the bias from under-reporting is small. Second, the IMSS data report only a daily wage; we do not know how many hours of work the daily wage reflects. In the EIA data, by contrast, we know the number of hours of work; the average wage variable is an average hourly wage. The fact that the results using the different datasets are so similar again suggests that the bias from using the daily rather than the hourly wage is small.

6.2 Decomposing Plant-Level Average Wages

The results for plant-level average wages leave open the question of whether the differential wage changes are due to sorting of workers of different skill into plants or to changing wage premia. We now turn to the task of decomposing the plant-level average wage changes into these two components. The basic econometric model we use to conduct this decomposition is the following:

$$w_{it} = \alpha_i + \psi_{j(i,t)t} + x'_{i,t}\phi + u_{it} \tag{13}$$

where w_{it} is the log wage of individual i in year t, α_i is a person fixed effect, $j(i,t)$ indicates the plant at which individual i is employed in period t, $\psi_{j(i,t)t}$ is a plant-year effect, $x'_{i,t}$ is a vector of observable time-varying characteristics of individual i, and u_{it} is a mean-zero disturbance. We use the years 1993, 1997 and 2001.

Three notes about this model are important. First, the model allows plant effects to vary across years, but constrains individual effects to be fixed; we relax this assumption below. Second, the person effects α_i and the plant-year effects ψ_{jt} are separately identified in this regression only by individuals switching between establishments. (This is the reason for the requirement that all establishments in our panels be part of a "connected" group of establishments; for details on identification of plant effects, see Abowd, Creecy, and Kramarz (2002).) Third, estimating this model by OLS requires the assumption that the error term is uncorrelated with the co-variates. In particular, we assume that individual innovations in productivity (which enter the error term) are uncorrelated with the plant-year effects. This assumption is often referred to as the assumption of conditional random mobility, or simply exogenous mobility. Note that it does not require that mobility be completely random; it requires that mobility be random conditional on time-invariant individual characteristics and the other co-variates. In imposing this assumption, we are following Abowd, Kramarz, and Margolis (1999) and the subsequent employer-employee literature. Nevertheless, the assumption is clearly a strong one, and we plan to investigate its validity in future work.

We estimate this model on individual-level data for the years 1993, 1997 and 2001. We have few individual-level observables to include in the $x'_{i,t}$ vector: tenure and tenure squared, age squared, and experience squared. (Age and experience in levels are not identified separately from year dummies; age squared and experienced squared are separately identified from each other because we observe the date an individual first registered with the IMSS, and do not have to calculate experience as a function of age.) We implement the regression as a simple "within" regression, where the outcome and covariates are deviated from individual-specific means over the period. The computational problems discussed in Abowd, Kramarz, and Margolis (1999) and Abowd, Creecy, and Kramarz (2002) do not arise here because we estimate approximately 11,000 (approximately 3,600 plants * 3 years) plant-year effects, and matrices of this size can be inverted by standard software packages.

Once we have estimated this model, we recover the estimated plant-year effects, $\hat{\psi}_{jt}$, and treat them as measures of wage premia. We also construct measures of average skill of the workforce at the plant level. Under our assumptions, the estimated person effects, $\hat{\alpha}_i$, are estimates of the value of all time-invariant individual characteristics, including what is commonly thought of as unobserved skill as well as observed skill.[21] The estimates $x'_{i,t}\hat{\phi}$ are estimates of the value of the available time-varying observable characteristics. Thus $\hat{\alpha}_i + x'_{i,t}\hat{\phi}$ is an estimate of the overall value of the skills of individual i at time t. We take the average of this measure at the plant level in each year:

$$\bar{\hat{s}}_{jt} = \frac{1}{N_{jt}} \sum_{i|j(i,t)=j} \left(\hat{\alpha}_i + x'_{i,t}\hat{\phi}\right) \tag{14}$$

where N_{jt} is the number of workers in plant j at time t. We take this measure as an estimate of the average skill of the workforce at the plant-year level.

To illustrate the role of the time-varying observables and to check the robustness of our results, we also estimate equation 13 imposing the restriction that $\phi = 0$, i.e. excluding the time-varying observables in equation 13 and assuming that the individual fixed effects capture all relevant dimensions of employee skill.

[21] Note that this measure of skill should be interpreted as a measure of *general* skill — the skill that earns equal rewards within a given plant or in the outside market. Workers may develop plant-specific skills, and may reap some of the benefits of those skills. In our framework, the returns to plant-specific skills would count as part of the wage premium, where the wage premium should be interpreted as the difference between what a given worker earns in the plant and what he or she would earn on the outside market.

6.3 Estimating Changes in Wage Premia and Average Workforce Skill

With our plant-year-specific measures of wage premia and average skill, we estimate models of the form of equation 12, with the average log wage, the estimated plant-year effect, $\hat{\psi}_{jt}$, and the average skill of the workforce, $\overline{\hat{s}_{jt}}$, as dependent variables.

Before presenting tables of regression results, we present a set of graphs that illustrate the main results.[22] Figures 6a-c present locally smoothed non-parametric regression lines of the levels of average log wages, wage premia, and average skill in 1993 and 1997 against log domestic sales in 1993. The dark, bold curves are for the pre-crisis year, the gray curves for the post-crisis year. All variables have been deviated from year-specific industry means; hence the relevant information is contained in the slopes of the curves, not the levels. Figure 6a reveals (1) that in cross-section larger plants have higher wages on average, and (2) that there was a larger increase in average wages in plants with initially greater employment. Figures 6b and 6c plot similar graphs for the estimated plant effects (wage premia) and average skill. These figures reveal that both the fact that wages are higher in larger plants and the fact that wages increased more in initially larger plants are attributable largely to wage premia, rather than average skill.

Figures 7a-c compare *changes* in our key variables over 1993-1997 period (dark, bold curves) to changes over the 1997-2001 period (gray curves). The changes in average wages over the 1993-1997 period are visibly larger for higher-$\widehat{\lambda}$ plants. The key point is that the difference in slopes in the two periods is explained almost entirely by differences in the slopes of the plant effects. Although initially higher-employment plants did increase average skill

[22] These graphs correspond to the specifications allowing ϕ to vary freely, that is, corresponding to Part B of Table 8; see the discussion below.

more than initially lower-employment plants during both 1993-1997 and 1997-2001, there is essentially no difference in the differential changes in the two periods. To get a better sense of the statistical significance of these patterns, we now turn to the regression results.

Part A of Table 8 reports the results under the assumption that $\phi = 0$ in equation 13. The advantage of beginning with this more restrictive specification is that the decomposition of plant-level average wages into wage premia and average skill is particularly transparent. The results for the average log wage in Column 1, which do not reflect the decomposition described above, are (unsurprisingly) similar to the results for the log average wage in Part B of Table 7. Part A, Columns 2-3 indicate that initially higher-employment plants have a greater change in both wage premia and average workforce skill than initially lower-employment plants in both periods, but that the differential change was significantly larger in 1993-1997 than in 1997-2001. The key message of Part A, Columns 1-3, however, is in the magnitudes: for plant-level average wages, the difference in the 1993-1997 and 1997-2001 coefficients on log employment is 0.036; approximately two-thirds (0.024/0.036) of this difference is explained by the changing wage premia. As a further test, we calculate the average log wage at the establishment-year level for stayers, employees continuously employed in an establishment.[23] Note that the stayers results are subject to a potential bias due to selection on observables (i.e. individual fixed effects or time-varying observed characteristics) in addition to the potential bias due to selection on unobservables (i.e. period-specific shocks to individual productivity) mentioned above. Nonetheless, the stayers results are a simple, transparent check on the decomposition of average wage changes into wage premia changes and average

[23] We keep track of the set of stayers from 1993-1997 and from 1997-2001 separately.

workforce skill changes. In the absence of extensive selection on observables, we would expect the stayers results to resemble the results for the changes in wage premia, and they indeed are reassuringly similar to the wage premia results of Column 2.

Part B of Table 8 reports the results where ϕ in equation 13 is unrestricted. Part B, Column 1 reproduces the results in Part A, Column 1, which are not sensitive to the restriction on ϕ. The key point to notice in Part B, Column 2 is that the coefficient representing the differential change in wage premia over 1997-2001 goes essentially to zero. In other words, the significant differential change in wage premia in 1997-2001 in Part A, Column 2 can be attributed to almost entirely to changes in observable workforce characteristics. The coefficient for average skill for 1997-2001 in Part B, Column 3 reflects this fact: it is roughly twice as large as when time-varying observable characteristics were excluded. The results in Part B suggest that an even larger proportion of the difference in differential changes in average wages — approximately 89% — can be attributed to changes in wage premia.

Once we include time-varying observables in equation 13, the results for changes in wage premia (Part B, Column 2) are no longer directly comparable to the results for raw changes in average wages for stayers (Part A, Column 4). To construct comparable figures for stayers, we regress the change in the average wage of stayers on initial log employment and changes in the average values of the time-varying observables. The coefficients on the log employment term in each period are reported in Part B, Column 4. The results for stayers are again not statistically different from the wage premia results.

6.4 Results for Non-Tradable Sectors

A possible objection to our approach is that the difference in differential changes in our dependent variables was indeed due to the peso crisis, but that the effect operated through a channel other than the quality-upgrading channel we have emphasized here. For example, the peso devaluation was accompanied by a major crisis in the Mexican banking sector, which resulted in a contraction of the availability of credit. If this credit crunch adversely affected small firms, then we might again observe a greater coefficient on initial employment in 1993-1997, even in the absence of quality upgrading. To address this objection, we compare the manufacturing sector to a collection of non-tradable sectors: construction, retail trade, transportation and services. If the differential wage changes were a consequence of the credit crunch or another aspect of the macro shock unrelated to trade and product quality, then we would expect to see similar results in non-tradables as in manufacturing.[24]

The results for the non-tradable sectors appear in Table 9. As in Table 8, Panel A reports results constraining $\phi = 0$ in 13, and Panel B reports results where ϕ is unrestricted. The results in Column 1 indicate that there was a significant difference in differential average wage changes between 1993-1997 and 1997-2001. The key point, however, is that this difference in differential changes is *not* attributable to differences in differential changes in wage premia. The results for changes in wage premia, in both Part A and Part B, indicate that we cannot reject the hypothesis of no differential change in wage premia in the non-tradables sector,

[24] Additional evidence that the differential wage changes were driven by quality upgrading and not some other aspect of the macroeconomic crisis is provided by Kandilov (2005), who explicitly tests the predictions of Verhoogen (2004) in the context of an export subsidy in Chile. He finds that industries offered export subsidies displayed greater wage growth for white-collar workers than non-subsidized industries, and that within subsidized industries initially larger and more productive plants saw greater wage increases for both white-collar and blue-collar workers than initially smaller or less productive plants.

in either period. The majority of the difference in differential changes in average wages can be attributed to changes in average skill of the workforce. Interestingly, the results for the differential changes in average workforce skill (Column 3) are similar to the results for manufacturing. One possible interpretation of this result is that the peso crisis threw many relatively high-skill workers into unemployment, and that large establishments generally were better-positioned than small establishments to attract these workers, independent of export status. The stayers results, calculated in the same way as in Table 8, reinforce the results for wage premia.

6.5 Alternative Hypotheses

In this section, we consider two alternative hypotheses that may explain the pattern of differential wage changes we observe: rent-sharing and changes in the economy-wide return to skill.

The rent-sharing hypothesis is that the differential wage changes were indeed due to the increase in exporting brought about by the peso crisis, but that the wage changes reflect sharing of profits from export operations, rather than quality upgrading (Abowd and Lemieux, 1993; Blanchflower, Oswald, and Sanfey, 1996; Hildreth and Oswald, 1997; Budd and Slaughter, 2004). Note that two distinct mechanisms are often lumped together under the single heading of rent-sharing: (1) profit-maximizing rent-sharing, where firms optimally share rents in order to improve the morale and productivity of the workforce, and (2) non-profit-maximizing rent-sharing, where managers are motivated by objectives other than profits — for instance, to have a quiet life or to be generous to employees for whom they have some sort of affection. We consider (1) to be a variant of efficiency-wage theory, and a friendly interpretation of our basic story: the new product market conditions led exporting firms to seek to improve employee productivity, relative to non-exporting firms, by paying higher wages. Here we focus on the hypthesis (2), non-profit-maximizing rent-sharing, which is more of a challenge for our argument.

It is difficult to investigate the non-profit-maximizing rent-sharing hypothesis directly, since the measures of profitability that can be constructed from plant-level panels are accounting profits, which are typically poor measures of economic profits, and since the employer-employee data and the plant-level data have not yet been linked. Given these constraints, one straightforward way to investigate the rent-sharing hypothesis is to control for the employment growth of establishments. Establishments presented with new profit-making opportunities typically expand; as a result, employment growth can be interpreted as a proxy, albeit imperfect, for the growth of profitability. Table 10 reports results for our basic model including the change in employment as an additional co-variate. Two patterns are notable. First, the change-in-scale term is associated with a significant *decrease* in the average person effect. This is consistent with the idea that establishments face a limited supply of skill in their local labor markets, and must hire lower-skill workers as they expand. Second, the change-in-scale term is associated with a significant increase in the plant effect. This is consistent with the rent-sharing hypothesis. It is also consistent with the hypothesis that plants face internal equity constraints or institutional constraints that require them to pay low-productivity workers a wage above their marginal product; in this case, newly hired low-productivity workers may receive higher wage premia than existing workers. The first association is stronger than the second, hence the overall association of the change in scale with average log wages is negative. But the key point for our argument is that the results for plant effects are largely unaffected — indeed, they are strengthened — by the inclusion of the scale term. Comparing Tables 8 and 10, we see that the difference in differential changes in wage premia is *larger* when we control for scale effects than when we do not, which undermines the non-pofit-maximizing rent-sharing hypothesis.

In another approach to investigating the non-profit-maximizing rent-sharing hypothesis, we re-run our basic sepcification in the plant-level data separately for plants that had at least 10% foreign ownership vs. plant that had less than 10% foreign ownership in 1994, the only year for which foreign ownership information is available. Our motivation is the following: if the differential wage changes we observe were due to non-profit-maximizing rent-sharing, then we would expect to see less of an effect in foreign-owned plants, since the financial officers in the foreign headquarters of those plants presumably care less about the peacefulness of the lives of local managers and have no emotional attachment to the local workforce. Table 10A presents the results for our basic specification (the specification reported in Table 6) separately for foreign-owned and non-foreign owned. We see that the difference in differential wage changes in manufacturing between 1993-1997 and 1997-2001 is positive and significant within both subsets, but the effect is markedly *stronger* within the subset of foreign-owned plants. This also casts doubt on the non-profit-maximizing rent-sharing hypothesis. We conclude that the non-profit-maximizing hypothesis is unlikely to be the entire explanation for the greater differential change in wages and plant effect during 1993-1997.

The other alternative hypothesis that we consider is that the return to skill varies over time. Equation 13 imposes the assumption that the individual effects, α_i are fixed, and that the coefficients on the time-varying observable variables are constant over time. But if high-λ plants employ higher-skill workers than low-λ plants and the return to skill rises over time, equation 13 will interpret the rising wages of high-skilled workers as an increase in wage premia in high-λ plants.

To address this concern, we estimate a model of the following form, a modified version of equation 13:

$$w_{it} = \left(\alpha_i + x'_{i,t}\phi\right)\eta_t + \psi_{j(i,t)t} + u_{it} \tag{15}$$

where η_t can be interpreted as the general-equilibrium return to skill. There are at least two approaches one could take to estimating a model of this form. One approach would be to estimate the model with non-linear least squares, estimating η_t simultaneously with the other parameters. The large number of individual and plant-year effects makes this a computationally difficult task.[25] A second approach would be first to construct estimates of η_t, and then to use them to estimate the other parameters. In this paper, we follow the second approach.

Our procedure is the following:

1. We take data from the four years prior to the initial year of our period of interest (i.e. 1989-1992 if we are interested in 1993-1997) and estimate a model of the form of equation 13, to generate preliminary estimates of α_i and ϕ, call them $\tilde{\alpha}_i$ and $\tilde{\phi}$. We then mechanically generate $x'_{i,t}\tilde{\phi}$

[25] We are pursuing a non-linear version of a Chamberlain (1982) correlated random effects model, where we linearly project the individual effects, α_i on the value of the covariates from all periods before estimating. This approach has the advantage that we avoiding estimating directly the million-plus individual effects for each year. We thank Steven Lich-Tyler for suggesting it.

2. We take data for stayers between two periods (call them t and $t+1$),[26] and estimate equation 15 in differences:

$$\triangle w_i = \left(\tilde{\alpha}_i + x'_{i,t}\tilde{\phi}\right) \triangle \eta + \triangle \psi_{j(i)} + \triangle u_i \tag{16}$$

This yields an estimate of $\widehat{\triangle \eta}$. Normalizing η_t for an initial year (i.e. 1993) to 1, we can recover estimates $\hat{\eta}_t$ for other years (i.e. 1997, 2001).

3. We substitute $\hat{\eta}_t$ into equation 15. The model can then be estimated on the full sample by a quasi-differencing procedure as described in Wooldridge (2002, pp. 317-322). Stacking observations for a given individual and multiplying both sides of equation 15 by a quasi-differencing operator, we can write:

$$[I_T - P_\eta]\, w_i = [I_T - P_\eta] \left(\alpha_i + x'_{i,t}\phi\right) \eta_t + [I_T - P_\eta]\, \psi_{j(i,t)t} + [I_T - P_\eta]\, u_i \tag{17}$$

where $P_\eta = \eta\, (\eta'\eta)^{-1}\, \eta'$ and $\eta \equiv (\eta_1\ \eta_2\ \ldots\ \eta_T)'$. The individual-effect term, α_i, is differenced out, and the equation can be estimated by OLS, yielding estimates $\hat{\alpha}_i$, $\hat{\phi}$, and $\hat{\psi}_{jt}$. We then construct an average workforce skill measure and estimate a model of the form of equation 12, as above.

The values we calculate for η_t in step 2 for 1993, 1997 and 2001 are 1, 0.94 and 0.87 respectively. Table 11 presents results for differential changes in plant-year effects and average skill from this procedure. The results are nearly identical to the results in Part B of Table 8, where the general-equilibrium return to skill is held constant. This approach must be accompanied by a number of caveats. The estimates $\tilde{\alpha}_i$ and $\tilde{\phi}$ in step 1 assume that η_t is

[26] The advantage of focusing on stayers here is that we can capture the change in wages due to changing plant-year effects with a plant dummy.

held constant, and serial correlation in the measurement error in wages from the pre-period data used in step 1 to wages in the initial year of the period used in step 2 may lead to spurious underestimates of η_t. Nevertheless, the fact that the results of this procedure so closely resemble the results above reassures us that the difference in differential changes in wage premia is not being driven entirely by changes in the general-equilibrium return to skill.

7 Conclusion

In this paper, we have confirmed in a new dataset the result of Verhoogen (2004) that initially larger manufacturing plants increased wages relative to initially smaller plants within each industry during the peso crisis period, and that this differential change was greater than in a subsequent period without a devaluation. We have shown further that, under the assumption of conditional random mobility, 67%-89% of the difference in differential wage changes can be attributed to changes in wage premia, depending on the specification we use. This pattern holds in manufacturing but not in a set of non-tradable sectors, which supports our argument that the changes in average wages and wage premia were driven by the differential inducement to increase exports. Our results are robust to a variety of modifications of our econometric model, addressing possible concerns with our approach.

These results are consistent with the hypothesis that quality upgrading in response to the late-1994 peso crisis led Mexican manufacturing plants to increase wage premia. It remains an open question precisely why higher product quality requires higher wage premia. One possibility is that high-quality production requires particularly motivated workers, and that plants pay higher wages to increase motivation, along the lines of the efficiency-wage models of Bowles (1985); Shapiro and Stiglitz (1984) or Akerlof (1982). A second possibility is that quality upgrading leads to investments by plants or by workers in plant-specific skills, and that the gains to such investments are subsequently shared with workers (Hashimoto, 1981). A third possibility is that quality upgrading leads plants or workers to invest in *general* training, and then plants raise wages to prevent workers from leaving for other establishments. This third possibility may be distinguished from the first two by examining what happens to workers who have been employed by upgrading plants after they leave the plants. The first two possibilities may be distinguishable from one another using information on training programs in the ENESTyC survey. We leave the resolution of this question to future work.

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A Data Appendix

Several aspects of our data cleaning procedure for the IMSS employer-employee data merit explanation.

A.1 Minimum Wages

Mexico has three regional minimum wages, with a higher minimum in Mexico City than in outlying areas. Prior to 1990, it was common for establishments to report wages below the corresponding regional minimum wage. In 1990, IMSS initiated a campaign to require establishments to report at least the minimum wage (whether or not they were in fact paying the minimum wage), and many establishments appear to have complied. A small fraction (on the order of 1/10 of 1%) continued report wages lower than the applicable minimum wage. In all years, we replace values below the minimum wage with the corresponding minimum wage.

A.2 Top Codes and Outliers

The wage data were top-coded over the period we study, at values ranging from 10 to 25 times the Mexico City minimum wage. Nonetheless, a number of individuals have reported wages above the top-code. In an effort to minimize the effects of the changing top-code and the outliers, we "winsorized" our data, setting wage values above the 95th percentile for a given year equal to the value at the 95th percentile, and values below the 5th percentile to the value at the 5th percentile. The 95th percentile was below the top-code in all years. In several years, more than 5% of individuals in our data were receiving the lowest minimum wage.

A.3 Geographical Selection

We dropped establishments in municipalities along the Northern border with the U.S. The vast majority of such plants are maquiladoras and since maquiladoras are not included in the EIA panels, for reasons of comparability we also dropped them from the IMSS panels.

Table 1: Summary Statistics, IMSS Panels

	Manufacturing panel			Non-tradables panel		
	emp. <200	emp. >=200	all	emp. <200	emp. >=200	all
	(1)	(2)	(3)	(4)	(5)	(6)
average daily wage (1994 pesos)	43.59	54.91	52.78	53.26	61.75	59.74
	(0.045)	(0.025)	(0.022)	(0.060)	(0.039)	(0.033)
average daily wage (current US $)	13.70	17.43	16.72	16.77	19.57	18.91
	(0.014)	(0.008)	(0.007)	(0.019)	(0.013)	(0.011)
Fraction male	0.71	0.72	0.72	0.72	0.63	0.65
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Age	32.0	31.4	31.5	32.7	34.6	34.1
	(0.013)	(0.006)	(0.005)	(0.012)	(0.007)	(0.006)
% stayers (previous 4 years)	0.43	0.44	0.44	0.39	0.41	0.41
	(0.0007)	(0.0003)	(0.0003)	(0.0007)	(0.0004)	(0.0003)
N establishments	2425	1203	3628	2800	879	3679

Notes: Panels include years 1993, 1997, 2001. Standard errors of means in parentheses. Stayers calculated as (# employees continuously in establishment over previous 4 years)/(current employment); only available in 1997, 2001.

Table 2: Summary Statistics by Export Status, EIA Panel, 1993

	Non-exporters	Exporters	All
A. EIA Panel			
Employment	183.3	335.0	227.5
	(5.0)	(12.2)	(5.1)
Revenues	43.1	91.1	57.1
	(1.8)	(4.8)	(1.9)
Domestic sales	41.2	71.6	50.1
	(1.7)	(3.7)	(1.6)
K/L ratio	42.8	55.6	46.6
	(1.4)	(2.6)	(1.3)
Avg. hourly wage	12.1	16.5	13.4
	(0.1)	(0.3)	(0.1)
white-collar % of employment	30.4	33.5	31.3
	(0.4)	(0.5)	(0.3)
export % of sales		15.4	
		(0.6)	
% with foreign ownership (1994)	8.8	29.7	14.9
	(0.6)	(1.5)	(0.6)
TFP	-0.09	0.21	0.00
	(0.02)	(0.03)	(0.02)
N	2306	949	3255
B. EIA-ENESTyC Panel (1994)			
Has ISO 9000	8.6	14.1	10.5
	(0.8)	(1.4)	(0.7)
Has formal training program	65.6	75.8	69.2
	(1.4)	(1.7)	(1.1)
N	1166	625	1791

Notes: Standard errors (of means) in parentheses. Exporter defined as having exports greater than zero. Having foreign ownership means at least 10% FDI. Data on foreign ownership, ISO 9000, training from 1994; all other data from 1993. Revenues and domestic sales measured in millions of 1994 pesos, K/L ratio in thousands of 1994 pesos, wages in 1994 pesos per hour. Average 1994 exchange rate: 3.38 pesos/US$1. TFP is estimate of firm fixed effect in regression of log value-added on log inputs for 1993-94, separately for 9 two-digit industries; estimates then deviated from 6-digit industry means.

Table 3: Proxies for λ, 1993, EIA Panel

	log (employment)	log (domestic sales)	exp. share index	ISO 9000 index	1st princ. comp.	log tfp (fixed effect)
log (employment)	1					
log (domestic sales)	0.8014*	1				
export share index	0.8719*	0.8322*	1			
ISO 9000 index	0.6987*	0.6090*	0.8049*	1		
1st princ. component	0.9020*	0.8789*	0.9660*	0.8398*	1	
TFP (fixed effect)	0.3326*	0.4754*	0.3875*	0.2720*	0.4123*	1

Notes: All variables deviated from industry means. Asterisk indicates significance at the 99% level. Export share index is Xb from tobit of export share on log employment, log revenues, log K/L ratio, foreign ownership indicator. ISO 900 index is Xb from probit of ISO 9000 on log employment, log revenues, log K/L ratio, foreign ownership indicator, and export share of sales. First principal component is of the following variables: log employment, log revenues, log K/L ratio, foreign ownership indicator, and export share of sales. TFP is estimate of firm fixed effect in regression of log value-added on log inputs for 1993-94, separately for 9 two-digit industries; estimates then deviated from 6-digit industry means. * = 1% level.

Table 4: Results on Export Shares, EIA Panel

	Dependent variable: Δ export share of sales					
	OLS			IV		
	1993-1997	1997-2001	Difference	1994-1997	1998-2001	Difference
	(1)	(2)	(3)	(3)	(4)	(5)
Proxy for λ:						
log dom. sales	0.019**	0.006**	0.013**	0.014**	0.004**	0.010**
	[0.002]	[0.002]	[0.003]	[0.002]	[0.001]	[0.002]
export share index	0.017**	0.003	0.014**	0.013**	0.003	0.010**
	[0.002]	[0.002]	[0.003]	[0.002]	[0.002]	[0.003]
ISO 9000 index	0.008**	-0.004	0.012**	0.007**	-0.002	0.009**
	[0.002]	[0.002]	[0.003]	[0.002]	[0.002]	[0.003]
1st prin. component	0.013**	-0.002	0.015**	0.011**	-0.001	0.012**
	[0.002]	[0.002]	[0.003]	[0.002]	[0.002]	[0.003]
TFP (fixed effect)	0.009**	0.002	0.007*			
	[0.002]	[0.002]	[0.003]			
log employment	0.016**	0.004	0.012**	0.013**	0.003	0.010**
	[0.003]	[0.002]	[0.004]	[0.003]	[0.002]	[0.004]

Notes: Each coefficient reflects a separate regression. Robust standard errors in brackets. R-squareds omitted (all are between .10 and .17). All regressions include industry and state fixed effects (coefficients omitted). N = 3255. IV uses initial year value (1993 or 1997) as instrument for subsequent value (1994 or 1998) of proxy, and takes change over three years (1994-1997, 1998-2001) as outcome variable. See text or notes to Table 3 for details on proxies. ** = 1% level, * = 5% level.

Table 5: Results on ISO 9000 Certification, EIA-ENESTyC panel

		Dependent variable: Δ ISO 9000		
		OLS		
		1994-1998	1998-2000	Difference
		(1)	(2)	(3)
Proxy for λ:				
	log domestic sales	0.072**	0.023	0.049
		[0.018]	[0.018]	[0.026]
	export share index	0.056**	0.017	0.039
		[0.020]	[0.020]	[0.028]
	ISO 9000 index	0.038	0.026	0.012
		[0.020]	[0.019]	[0.028]
	1st prin. component	0.067**	0.034	0.033
		[0.019]	[0.019]	[0.027]
	TFP (fixed effect)	0.030	0.002	0.028
		[0.018]	[0.016]	[0.024]
	log employment	0.076**	0.030	0.046
		[0.028]	[0.027]	[0.038]

Notes: Each coefficient reflects a separate regression. Each regression includes industry and state fixed effects (coefficients omitted). N = 844. Robust standard errors in brackets. R-squareds=.17 for all (stacked) regressions. ** = 1% level, * = 5% level.

Table 6: Results on Plant-Level Average Wages, EIA Panel

	Dependent variable: Δ avg. log hourly wage					
	OLS			IV		
	1993-1997	1997-2001	Difference	1994-1997	1998-2001	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Proxy for λ:						
log dom. sales	0.044**	0.012**	0.032**	0.041**	0.009*	0.032**
	[0.006]	[0.004]	[0.007]	[0.005]	[0.004]	[0.006]
export share index	0.051**	0.023**	0.029**	0.049**	0.019**	0.030**
	[0.006]	[0.005]	[0.008]	[0.005]	[0.005]	[0.007]
ISO 9000 index	0.035**	0.006	0.029**	0.034**	0.001	0.033**
	[0.006]	[0.006]	[0.008]	[0.006]	[0.005]	[0.008]
1st prin. component	0.048**	0.017**	0.031**	0.046**	0.013**	0.033**
	[0.006]	[0.005]	[0.008]	[0.005]	[0.005]	[0.007]
TFP (fixed effect)	0.016**	0.002	0.014			
	[0.006]	[0.005]	[0.008]			
log employment	0.070**	0.024**	0.046**	0.058**	0.020**	0.038**
	[0.007]	[0.006]	[0.009]	[0.007]	[0.005]	[0.009]

Notes: Each coefficient reflects a separate regression. Robust standard errors in brackets. R-squareds omitted (all are between .10 and .17). All regressions include industry and state fixed effects (coefficients omitted). N = 3255. IV uses initial year value (1993 or 1997) as instrument for subsequent value (1994 or 1998) of proxy, and takes change over three years (1994-1997, 1998-2001) as outcome variable. See text or notes to Table 3 for details on proxies. ** = 1% level, * = 5% level.

Table 7: Comparing EIA and IMSS Manufacturing Panels

		Dependent variable:	Δ log(avg. wage)	
			OLS	IV
			(1)	(2)
A. EIA 1993-2001 panel				
1993-1997	log employment 1993		0.070**	0.058**
			[0.007]	[0.007]
			0.156	0.154
1997-2001	log employment 1997		0.024**	0.020**
			[0.006]	[0.005]
			0.11	0.112
Difference			0.046**	0.038**
			[0.009]	[0.009]
B. IMSS 1993-2001 panel				
1993-1997	log employment 1993		0.056**	0.042**
			[0.004]	[0.004]
			0.185	0.189
1997-2001	log employment 1997		0.022**	0.015**
			[0.004]	[0.003]
			0.117	0.105
Difference			0.034**	0.027**
			[0.006]	[0.005]

Notes: All regressions include industry and state fixed effects (coefficients omitted). N = 3255 in part A, N = 3628 in part B. Robust standard errors in brackets. R-squareds below standard errors. IV uses initial year value (1993 or 1997) as instrument for subsequent value (1994 or 1998) of proxy, and takes change over three years (1994-1997, 1998-2001) as outcome variable. ** = 1% level, * = 5% level.

Table 8: Baseline Results, IMSS Manufacturing Panel

	Dependent variable:	Δ avg. log(wage)	Δ plant effect	Δ avg. skill	Δ avg. log(wage) of stayers
		(1)	(2)	(3)	(4)
A. Excluding time-varying observables					
1993-1997	log employment 1993	0.055**	0.034**	0.021**	0.031**
		[0.004]	[0.004]	[0.003]	[0.004]
		0.197	0.179	0.164	0.174
1997-2001	log employment 1997	0.019**	0.010**	0.009**	0.010**
		[0.004]	[0.003]	[0.003]	[0.003]
		0.124	0.143	0.091	0.147
Difference		0.036**	0.024**	0.012**	0.021**
		[0.006]	[0.005]	[0.004]	[0.005]
B. Including time-varying observables					
1993-1997	log employment 1993	0.055**	0.033**	0.022**	0.026**
		[0.004]	[0.004]	[0.003]	[0.004]
		0.197	0.186	0.183	0.2201
1997-2001	log employment 1997	0.019**	0.001	0.018**	-0.004
		[0.004]	[0.003]	[0.003]	[0.003]
		0.124	0.135	0.098	0.2271
Difference		0.036**	0.032**	0.004	0.030**
		[0.006]	[0.005]	[0.004]	[0.005]

Notes: All regressions include industry and state fixed effects (coefficients omitted). N = 3628. Robust standard errors in brackets. R-squareds below standard errors. Part B, Column (4) includes plant-level averages of tenure squared, age squared, and experience squared (coefficients omitted). ** = 1% level, * = 5% level.

Table 9: Baseline Results, IMSS Non-Tradables Panel

		Dependent variable:	Δ avg. log(wage)	Δ plant effect	Δ avg. skill	Δ avg. log(wage) of stayers
			(1)	(2)	(3)	(4)
A. Excluding time-varying observables						
1993-1997	log employment 1993		0.014**	-0.002	0.016**	-0.005
			[0.004]	[0.004]	[0.003]	[0.004]
			0.156	0.157	0.106	0.162
1997-2001	log employment 1997		-0.002	-0.007	0.005	-0.006
			[0.006]	[0.006]	[0.003]	[0.006]
			0.100	0.128	0.118	0.137
Difference			0.017*	0.005	0.012**	0.001
			[0.007]	[0.007]	[0.004]	[0.007]
B. Including time-varying observables						
1993-1997	log employment 1993		0.014**	0.000	0.014**	0.001
			[0.004]	[0.004]	[0.003]	[0.004]
			0.156	0.152	0.082	0.2068
1997-2001	log employment 1997		-0.002	-0.006	0.004	-0.002
			[0.006]	[0.006]	[0.003]	[0.004]
			0.100	0.117	0.100	0.2405
Difference			0.017*	0.006	0.010	0.003
			[0.007]	[0.007]	[0.004]	[0.006]

Notes: Non-tradables include construction, retail trade, transportation, and services. All regressions include industry and state fixed effects (coefficients omitted). N = 3679. Robust standard errors in brackets. R-squareds below standard errors. Part B, Column (4) includes plant-level averages of tenure squared, age squared, and experience squared (coefficients omitted). ** = 1% level, * = 5% level.

Table 10: Controlling for Change in Scale, IMSS Manufacturing Panel

	Dependent variable:	Δ avg. log(wage)	Δ plant effect	Δ avg. skill
		(1)	(2)	(3)
1993-1997	log employment 1993	0.043**	0.043**	0.000
		[0.004]	[0.004]	[0.002]
	Δ log employment	-0.086**	0.069**	-0.154**
		[0.010]	[0.008]	[0.007]
	R-squared	0.225	0.208	0.388
1997-2001	log employment 1997	0.014**	0.006	0.008**
		[0.003]	[0.003]	[0.002]
	Δ log employment	-0.072**	0.066**	-0.138**
		[0.011]	[0.007]	[0.009]
	R-squared	0.15	0.162	0.292
Difference		0.030**	0.037**	-0.008**
		[0.005]	[0.005]	[0.003]

Notes: All regressions include industry and state fixed effects (coefficients omitted). N = 3628. Robust standard errors in brackets. ** = 1% level, * = 5% level.

Table 10A: Differential Changes in Plant-Level Average Wages, by Foreign Ownership

		Dependent variable: Δ avg. log hourly wage		
		1993-1997	1997-2001	Difference
		(1)	(2)	(3)
Panel A: Foreign ownership >10% in 1994 (N=485)				
Proxy for λ:	log dom. sales	0.072***	-0.006	0.078***
		[0.021]	[0.019]	[0.028]
	export share index	0.102***	-0.008	0.110***
		[0.024]	[0.024]	[0.034]
	log employment	0.124***	-0.024	0.148***
		[0.025]	[0.026]	[0.036]
Panel B: Foreign ownership <10% in 1994 (N=2770)				
Proxy for λ:	log dom. sales	0.035***	0.013***	0.022***
		[0.006]	[0.005]	[0.008]
	export share index	0.043***	0.029***	0.014
		[0.008]	[0.006]	[0.01]
	log employment	0.060***	0.029***	0.031***
		[0.008]	[0.007]	[0.01]

Notes: Each coefficient reflects a separate regression. Robust standard errors in brackets. R-squareds omitted (all are between .10 and .17). All regressions include industry and state fixed effects (coefficients omitted). N = 3255. IV uses initial year value (1993 or 1997) as instrument for subsequent value (1994 or 1998) of proxy, and takes change over three years (1994-1997, 1998-2001) as outcome variable. See text or notes to Table 3 for details on proxies. ** = 1% level, * = 5% level.

Table 11: Allowing for Changing Return to Skill, IMSS Manufacturing Panel

		Δ avg. log(wage)	Δ plant effect	Δ avg. skill
		(1)	(2)	(3)
1993-1997	log employment 1993	0.055**	0.034**	0.021**
		[0.004]	[0.004]	[0.003]
		0.197	0.194	0.176
1997-2001	log employment 1997	0.019**	0.003	0.016**
		[0.004]	[0.003]	[0.003]
		0.124	0.141	0.121
Difference		0.036**	0.031**	0.005
		[0.006]	[0.005]	[0.004]

Notes: All regressions include industry and state fixed effects (coefficients omitted). N = 3628. Robust standard errors in brackets. R-squareds below standard errors. ** significant at 1% level, * at 5% level.

Fig. 1: Average Quality as a Function of Know-how Parameter

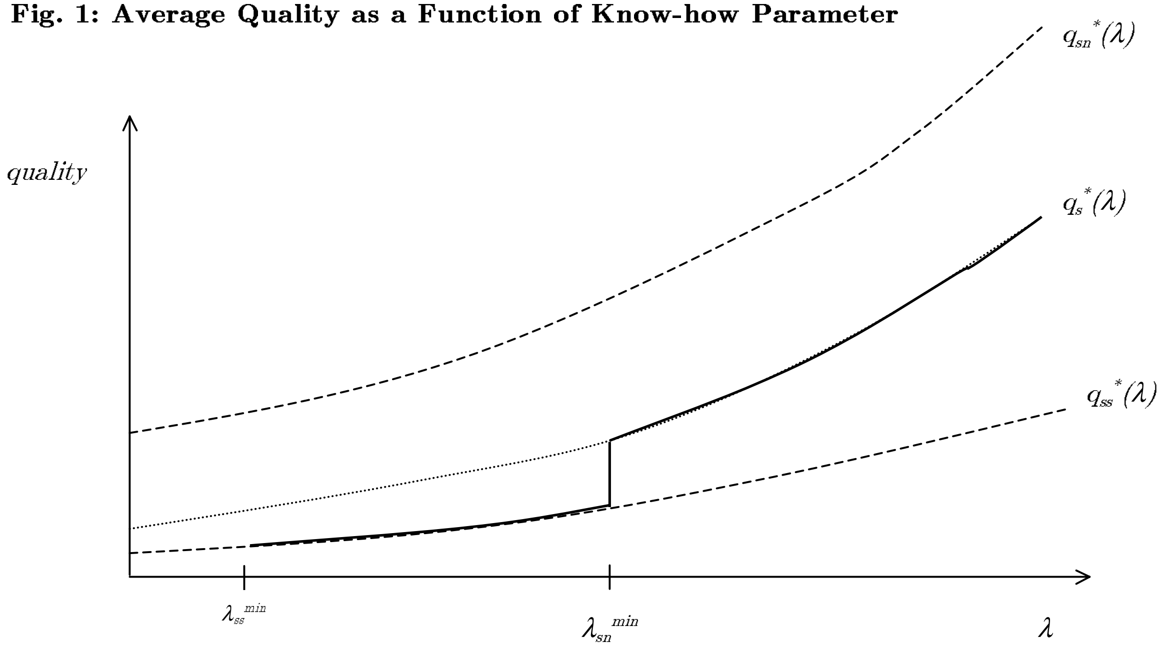


Fig. 2: Change in Average Quality as a Function of Know-how Parameter

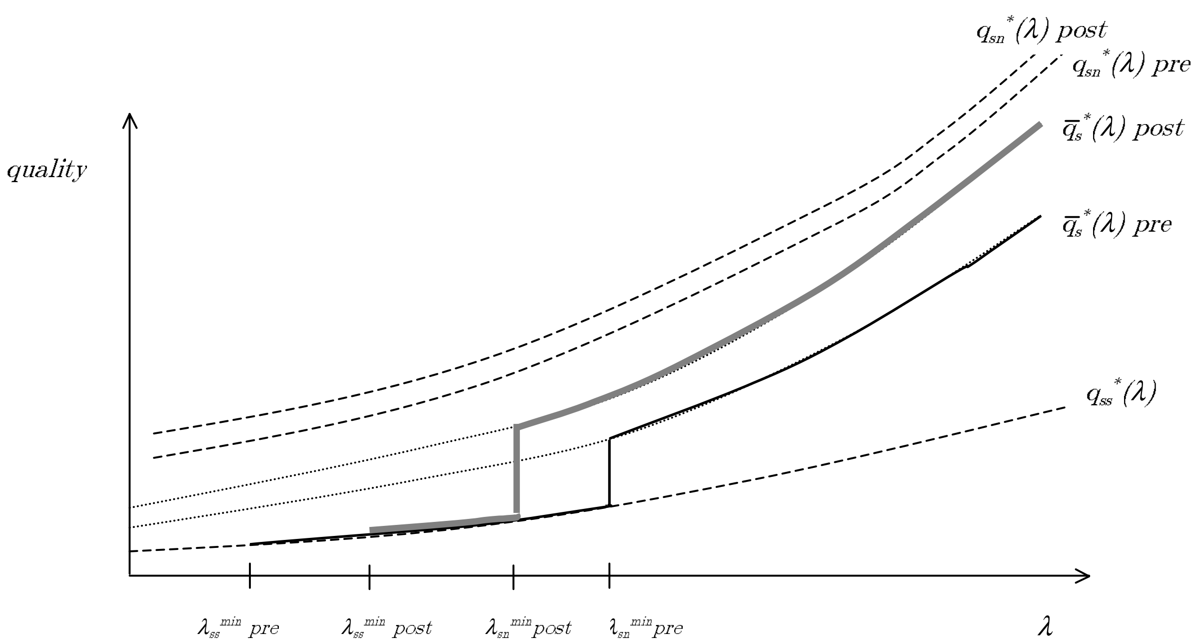


Fig. 3: Change in Average Quality as a Function of Know-how Parameter

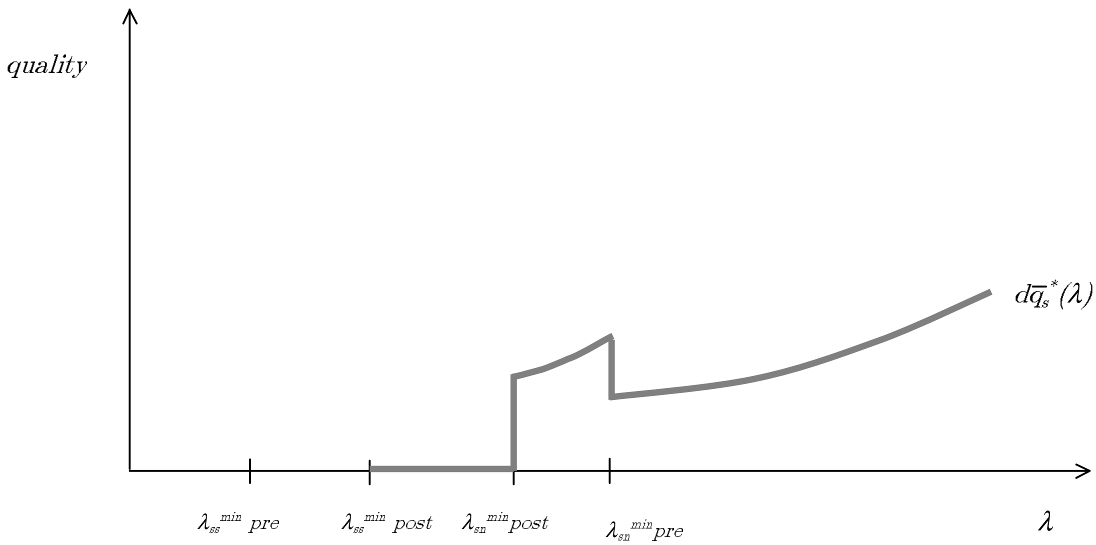


Fig. 4: Real Exchange Rate

Note: Price levels and exchange rate from Penn World Table 6.1.

Fig. 5a: Sales, Domestic and Export, 1993-2001

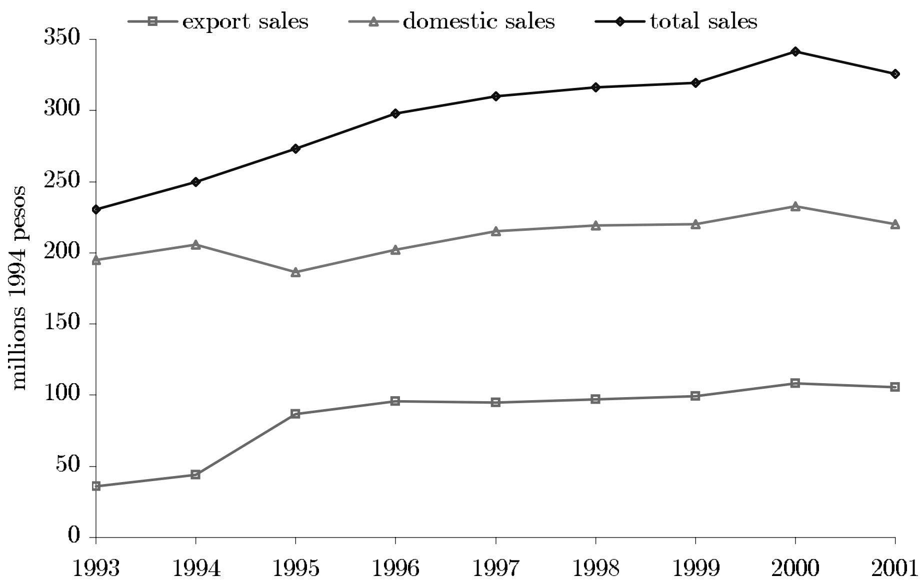


Fig. 5b: Export percentage of total sales

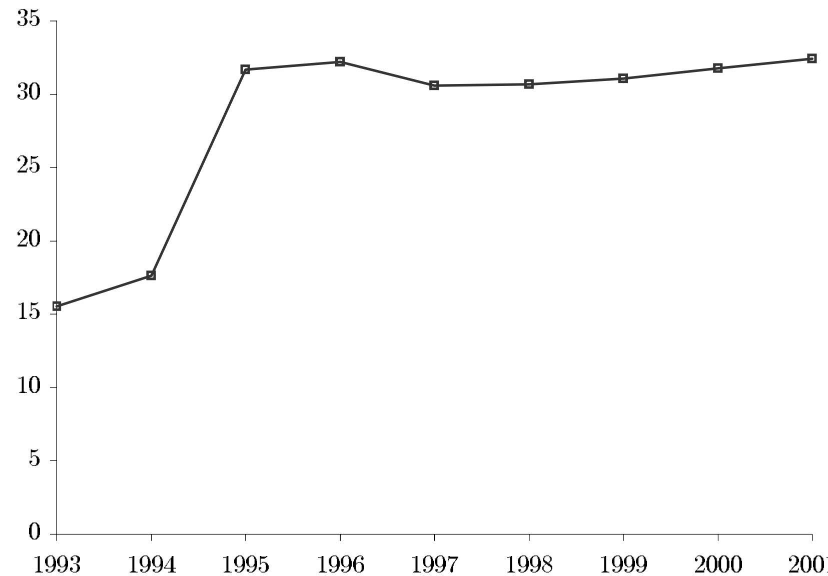
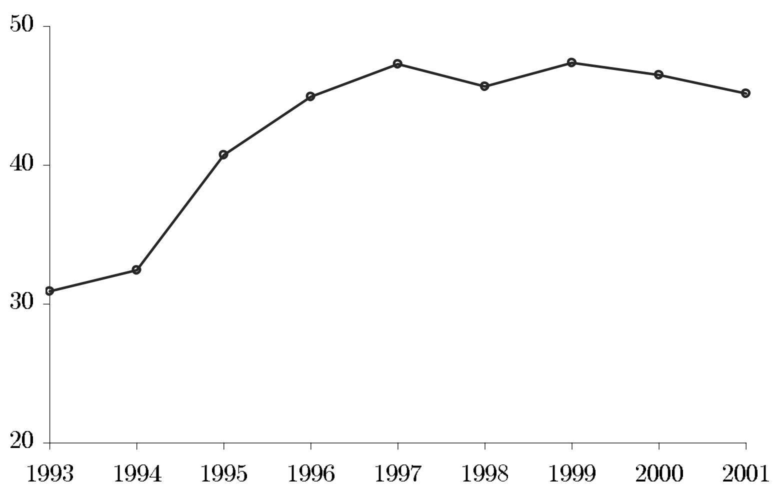


Fig. 5c: Percentage of Plants Exporting



Notes: Data from EIA 1993-2001 Panel. Export percentage of total sales calculated as (total exports for all plants)/(total sales for all plants). Plants with exports greater than zero classified as exporting.

Fig. 6: Non-Parametric Regressions, 1993 and 1997, IMSS Mfg. Panel

Fig. 6a: Avg. log wage

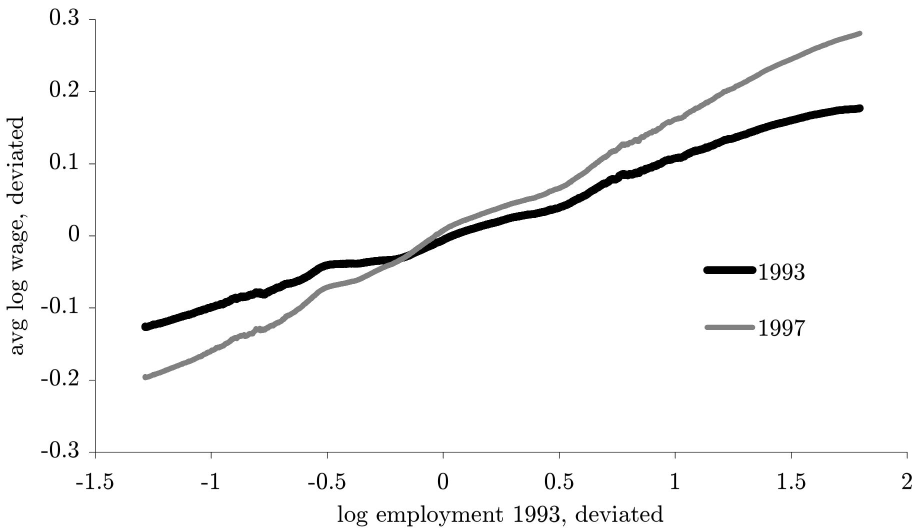


Fig. 6b: Plant effect

Fig. 6c: Avg. skill

Notes: All variables deviated from industry-year means. Graphs are locally smoothed non-parametric bivariate regressions (bandwidth = .3), of y-axis variable on log employment 1993, using IMSS 1993-2001 panel, trimmed at 2nd and 98th percentiles.

Fig. 7: Non-Parametric Regressions, 1993-1997 and 1997-2001, IMSS Mfg. Panel

Fig. 7a: Change in Avg. Log Wage

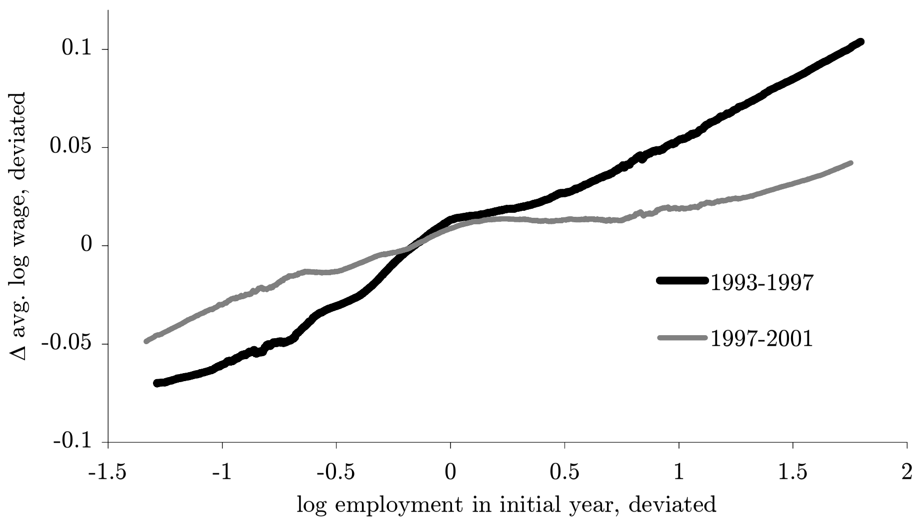


Fig. 7b: Change in Plant Effect

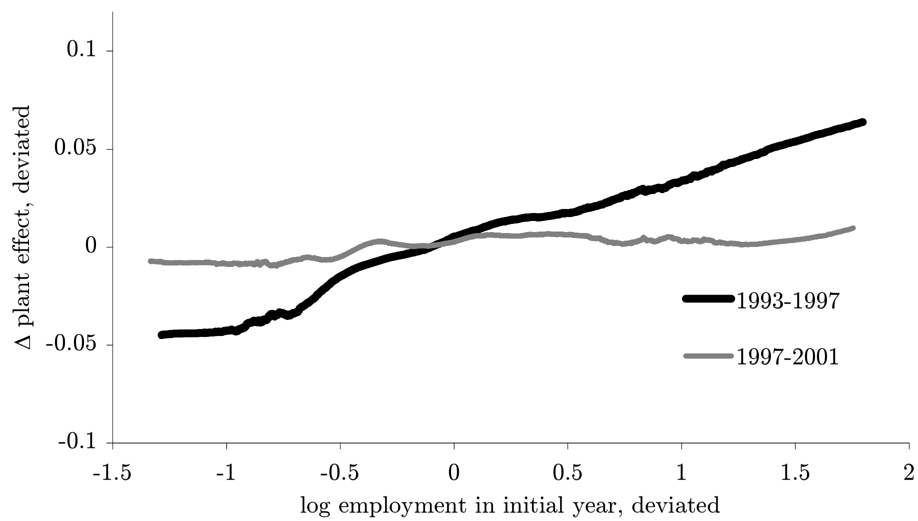
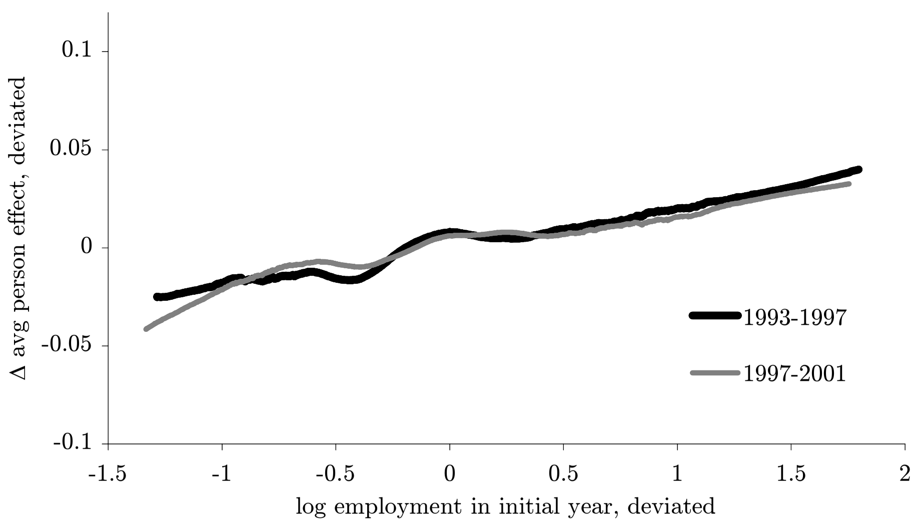


Fig. 7c: Change in Avg. Skill



Notes: All variables deviated from industry-year means. Graphs are locally smoothed non-parametric bivariate regressions (bandwidth = .3), of y-axis variable on log employment 1993, using IMSS 1993-2001 panel, trimmed at 2nd and 98th percentiles.