Exports and Wage Premia: Evidence from Mexican Employer-Employee Data[*]

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Abstract

This paper draws on a new combination of employer-employee and plant-level data from Mexico to investigate the relationship between exports and wage premia, defined as wages above what workers would receive elsewhere in the labor market. We first use detailed information on individual workers' wage histories to decompose plant-level average wages into a component reflecting skill composition and a component reflecting wage premia. Our estimating procedure allows for changes in the return to ability and feedback from current idiosyncratic shocks to future mobility. We then use the peso devaluation of late 1994, which we argue generated an exogenous differential inducement to export within industries, to estimate the effect of export incentives on the two components. Comparing across plants within industries, we find that approximately two-thirds of the higher level of wages in larger, more productive plants is explained by higher levels of wage premia, and that nearly all of the differential within-industry wage change due to the export shock is explained by changes in wage premia. The findings argue against the hypothesis that sorting on individual ability is solely responsible for the well-documented correlation between exporting and wages.

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1 Introduction

Trade theories in the Ricardian and Heckscher-Ohlin traditions typically suppose that both product markets and labor markets are frictionless and perfectly competitive. Many advances in trade theory over the past three decades have derived from introducing elements of imperfect competition in product markets. But with a few notable exceptions, it has been common in the literature to carry along the assumption of frictionless, competitive — what we will refer to as "neoclassical" — labor markets.[1] Very recently, there has been a small surge of theoretical interest in introducing features of non-neoclassical labor markets — search frictions, bargaining, rent-sharing, and efficiency wages, among others — into trade models, in particular into the influential heterogeneous-firm, monopolistic-competition model of Melitz (2003).[2] The implications of these new models for the level and distribution of gains from trade can differ greatly from those of more standard treatments. To date, however, there has been relatively little work investigating the extent to which these non-neoclassical features are empirically important for the analysis of trade and labor-market outcomes.

In this paper, we draw on a new combination of employer-employee and plant-level datasets from Mexico to investigate the relationship between exporting and wage premia, defined as wages above what workers would receive elsewhere in the labor market. This relationship is particularly salient because models with neoclassical and non-neoclassical labor markets differ in their predictions for it. As we discuss in more detail in Section 2 below, while both sets of theories can explain a correlation between exports and average wages at the plant level, only models with non-neoclassical features are consistent with an effect of exporting — or, more precisely, of the incentive to export[3] — on wage premia. A test of whether a shock to the incentive to export has an effect on wage premia is thus a first step in evaluating the extent to which features such as search frictions or efficiency wages are important in explaining the labor-market consequences of

[1] Early work introducing non-neoclassical labor-market features into trade models includes Davidson, Martin, and Matusz (1988, 1999) on search; Copeland (1989), Brecher (1992), and Matusz (1996) on efficiency wages; and Agell and Lundborg (1995) on fair wages.

[2] Recent work in this area, discussed in more detail in Section 2 below, includes Helpman, Itskhoki, and Redding (2008), Felbermayr, Prat, and Schmerer (2008), Coşar, Guner, and Tybout (2009), Artuç, Chaudhuri, and McLaren (2008) and Sethupathy (2008) on search and mobility costs; Davis and Harrigan (2007) on efficiency wages; and Amiti and Davis (2008) and Egger and Kreickemeier (forthcoming) on fair wages.

[3] Since exports and wages in a given period are simultaneous outcomes of a single firm optimization problem, it is not clear that the relationship between them can be interpreted causally. As discussed in more detail below, we focus instead on the response of both to variation in the incentive to export, which is more amenable to causal interpretation.

international trade.

It is well established that exporting plants pay higher average wages on average than non-exporting plants in the same industry (Bernard and Jensen, 1995, 1997).[4] But identifying a causal relationship between exports and wage premia is made difficult by two forms of endogenous selection. First, workers select into plants, and higher wages in exporting plants may reflect higher skill that is unobservable to the econometrician. Second, plants select into the export market. Within each industry, larger, more productive plants, which tend to be higher-wage, are more likely to export than smaller, less-productive ones. Thus even if one were to establish that exporting plants paid higher wage premia than non-exporting plants, in general it would not be clear whether the relationship were due to exporting *per se* or to productivity (or other) differences between the two sets of plants.

Our strategy for addressing these issues is two-fold. To address the first form of selection, we use detailed information on individual workers' wage histories from the administrative records of the Mexican social security agency. Extending the work of Abowd, Kramarz, and Margolis (1999) to allow for changes in the return to individual ability, we decompose plant-level average wages in each year into an average "person" component, which reflects the skill composition of the workforce, and a plant component, which we interpret as a measure of average wage premia. To address the selection of highly productive plants into the export market, we use the late-1994 Mexican peso devaluation as a source of exogenous variation in the incentive to export. Following Verhoogen (2008), we argue that the devaluation generated a differential inducement to export within industries, with initially larger, more productive plants receiving a larger effective inducement to export than initially smaller, less productive ones. Using a variety of proxies for plant heterogeneity, we compare the differential trends between more and less productive plants within industries during the peso crisis period to the corresponding differential trends in a later period without a devaluation.[5]

We have two main findings. First, in *levels*, within industries, it appears that approximately two thirds of the correlation between plant-level average wages and plant size can be explained by wage premia and one third by workforce composition. Second, in *changes*, it appears that essentially all of the differential within-industry effect of the shock to export incentives on plant-

[4]Schank, Schnabel, and Wagner (2007) provide a list of studies documenting a correlation between exports and wages in 22 countries.

[5]When possible, we also compare to an earlier period without a devaluation, although data constraints prevent us from doing so in our baseline estimates.

level average wages is explained by changes in the plant component. These results argue against theories that rely solely on worker sorting to explain the response of plant-level wages to export shocks. Our data do not allow us to discriminate among the various non-neoclassical mechanisms that might generate such patterns but the results do appear to suggest that one or more mechanisms in the non-neoclassical category played an important role in shaping the response of the Mexican labor market to the export shock.

In addition to contributing to the trade-and-wages literature, we believe that this paper makes two contributions to the employer-employee literature that has grown out of the work of Abowd, Kramarz, and Margolis (1999) (which we also refer to as AKM).[6] First, as mentioned above, we allow the return to individual ability to change over time when estimating wage premia. The standard approach is to include a fixed effect for each individual, which implicitly assumes that the return to the individual's ability is constant over time. While this assumption is plausible in many settings, in our context it seems unattractive. If exporting plants employed higher-skilled workers than non-exporters before the export shock, and the export shock raised raised the return to individual skill, then the standard approach would misinterpret the rising return to ability as an effect of exporting on wage premia. In allowing for time-varying returns, we draw on techniques from the dynamic-panel literature, in particular Holtz-Eakin, Newey, and Rosen (1988). This approach has the additional benefit that we are able to weaken the standard assumption that worker mobility is random conditional on observables: we allow for feedback from current idiosyncratic shocks to future characteristics, in particular, to which plant a worker is employed in. (An important caveat is that we do not have a randomized experiment for worker mobility, and still require a form of conditional random mobility, as we discuss below.) Our estimates indicate that the return to ability varied significantly over the sample period, in a pattern broadly consistent with changes in overall wage inequality in Mexico. At the same time, we learn from the exercise that the qualitative conclusions are similar to those from a standard AKM-type model, suggesting that biases from assuming time-constant individual returns are relatively small in our context.

Second, we believe that ours is the first paper to relate estimates of wage premia at the plant level to quasi-experimental variation in product-market conditions. Our research design allows us

[6]For reviews, see Abowd and Kramarz (1999) and Abowd, Kramarz, and Woodcock (2008). The employer-employee literature itself builds on the earlier literatures on inter-industry wage differentials (Dickens and Katz, 1987; Murphy and Topel, 1987; Krueger and Summers, 1988; Katz and Summers, 1989; Groshen, 1991) and firm-size wage differentials (Brown and Medoff, 1989; Oi and Idson, 1999).

to identify the effects of the export shock separately from a number of potentially confounding omitted variables — in particular, idiosyncratic plant productivity shocks that may lead plants both to increase exports and to increase wages and wage premia.[7] Our design also allows us to reduce the potentially confounding influence of endogenous mobility at the individual level: given that we focus on within-plant changes in wage premia, any biases due to non-random sorting on unobservables that are stable over time are differenced out. While there may still have been changes over time in the extent of individual sorting, it is not obvious why such changes would have been systematically related to the exogenous variation in export incentives and hence not clear how endogenous individual mobility could explain our results.[8]

In addition to the papers cited above, our work is related to a number of papers in settings where it is not possible to follow individual workers from employer to employer. Abowd and Lemieux (1993) use sector-level terms-of-trade movements to identify the effect of foreign competition on union bargaining contracts in Canada. Lemieux (1998) and Gibbons, Katz, Lemieux, and Parent (2005) estimate wage equations with differing returns to ability across sectors, rather than over time. The growing body of research on the labor-market consequences of product-market changes due to to international trade includes Revenga (1992), Borjas and Ramey (1995), Bertrand (2004), Pavcnik, Blom, Goldberg, and Schady (2004), Goldberg and Pavcnik (2005), Söderbom, Teal, and Wambugu (2005), Biscourp and Kramarz (2007), Guadalupe (2007), Bustos (2007), Cuñat and Guadalupe (2009), Fafchamps (forthcoming), and Brambilla, Lederman, and Porto (2009). Menezes-Filho and Muendler (2007) use employer-employee data from Brazil to investigate a different issue, the extent of employment reallocation in response to trade reforms. For overviews of the trade-and-wages literature, readers are referred to the surveys by Goldberg and Pavcnik (2004, 2007) and Feenstra and Hanson (2003).

The paper is organized as follows. In the next section, we discuss briefly a number of theoretical mechanisms that may link exporting and wages at the plant level. Section 3 sets out our econometric strategy. Section 4 describes the datasets and the sample selection procedure.

[7]This approach differentiates our paper from two recent papers that also use employer-employee data to examine the association between exporting and wages, Schank, Schnabel, and Wagner (2007) and Munch and Skaksen (2008), both of which regress wages on export status and sets of indicator variables for each worker-firm match (or "spell"). In the absence of exogenous variation in exporting, both papers are vulnerable to the criticism that idiosyncratic productivity shocks may be responsible for the within-spell correlation between export status and wages.

[8]In the absence of a randomized experiment moving workers from one firm to another, an alternative approach is to estimate structural models of the search process, at the cost of stronger functional-form and statistical assumptions than we employ here (e.g. Postel-Vinay and Robin (2002)). We see our more reduced-form approach as complementary to studies in this vein.

In Section 5.1, we use the employer-employee data to decompose plant-level average wages into plant and person components. In Section 5.2, we merge the estimated wage components into a longitudinal plant panel and estimate the effect of the export shock on them. Section 6 examines the robustness of our main results and Section 7 concludes.

2 Theoretical Mechanisms

In this section, we review a non-exhaustive list of theoretical mechanisms that may link exporting and wages at the plant level within industries. A convenient organizing framework is the model of heterogeneous firms under monopolistic competition of Melitz (2003), on which several of the theories are based.[9] The Melitz framework has a highly stylized labor market in which workers are homogeneous and all paid the same wage, but the framework can be extended to account for wage differences across plants. The extensions we consider can be categorized in two groups: those with "neoclassical" labor markets that rely solely on worker sorting to explain the exporting-wage relationship, and those with non-neoclassical labor markets that allow for a role for wage premia.

In the neoclassical category, one important mechanism involves the technology-choice idea of Yeaple (2005), extended to a heterogeneous-firm context by Bustos (2007). The idea is that firms have a choice between a more skill-intensive, high-fixed-cost, low-marginal-cost, modern technology and a less skill-intensive, low-fixed-cost, high-marginal-cost, traditional technology. An increase in exports increases the scale of production and favors the adoption of the high-fixed-cost modern technology, leading to skill upgrading within plants.[10] Another possible mechanism is quality upgrading; in this view, plants in poor countries export higher-quality products to rich countries than they sell in domestic markets, hence increases in exports are associated with increases in the average quality of goods produced. Verhoogen (2008) considers several mechanisms through which this process may affect wages, one of which involves sorting: if producing high-quality goods requires highly skilled workers, as in the O-ring theory of Kremer (1993), then quality upgrading will lead to a skill upgrading of the workforce. Note that neither the

[9] In the Melitz framework, potential entrants pay a fixed cost to receive a productivity draw and plants with a low draw exit immediately. Ex post, the remaining plants have heterogeneous productivities, all but the marginal firm have positive profits, and only the most productive plants pay an additional fixed cost to enter the export market.

[10] A related idea is that more capital-intensive production requires more "industrious" workers supplying higher effort, as suggested by Leamer (1999). In this view, effort is contractible and industriousness of workers reflects their preferences for work vs. leisure. In our framework, industriousness can thus be thought of as an unobserved characteristic that should be equally rewarded in all plants.

technology-choice mechanism nor the quality-upgrading-with-worker-sorting mechanism implies an effect of exporting on wage premia; in both cases, workers are paid what they would be paid in the outside labor market.

In the non-neoclassical category, a leading theory is the model of search and bargaining in an open economy of Helpman, Itskhoki, and Redding (2008).[11] In their framework, because of hiring costs, workers outside a firm are not perfect substitutes for workers currently employed, and employed workers are able to bargain for a share of profits. Workers are ex-ante homogeneous but receive a firm-specific ability draw. Firms have a screening technology that allows them, at a cost, to determine whether a worker's ability is above some cut-off. In equilibrium, more productive firms sample more workers, impose higher ability cut-offs, and pay higher wages (to compensate workers for the risk of unemployment) than less productive firms in the same industry. As revenues increase, firms increase the number of workers sampled, the ability cut-off, and the wage paid. A decrease in the costs of exporting leads exporting firms to increase revenues and raise wages. Given that workers are ex-ante homogeneous, these wage increases represent increases in wage premia.

Another possibility is that firms imperfectly observe the effort supplied by workers and pay efficiency wages in order to give teeth to the threat of firing workers who are caught shirking, along the lines of Shapiro and Stiglitz (1984). Davis and Harrigan (2007) introduce this feature into a heterogeneous-firm model of trade. In their framework, the size of the wage premium depends only on the effort-monitoring technology of the firm, not the export status, but it is not difficult to imagine a similar model along lines suggested by Verhoogen (2008): if worker effort is more valuable to firms when they export — e.g. because exported goods are higher-quality goods that require more careful attention — then firms may optimally increase the efficiency wage when exports increase. Another possibility is that modern technologies are worse at monitoring effort than traditional technologies, and hence the exporting-induced adoption of modern technologies hypothesized by Yeaple (2005) leads to higher efficiency wages.

Finally, perhaps the simplest wage-premium mechanism is provided by fair-wage considerations (Amiti and Davis, 2008; Egger and Kreickemeier, forthcoming). In this view, workers withhold effort if wages fall below a level perceived to be fair, where the fair wage level is increasing

[11] Helpman and Itskhoki (2007) and Felbermayr, Prat, and Schmerer (2008) also extend Melitz (2003) to include search frictions in the labor market, but in both cases workers are homogeneous and the models predict no within-industry variation in wages. Coşar, Guner, and Tybout (2009) extend the literature by allowing for ongoing idiosyncratic productivity shocks and fitting the dynamic model to Colombian plant-level data.

in firm profitability. Firms optimally pay wages that increase with profitability. Since increases in exports raise profitability, they also raise wages, even though workers are homogeneous.

Despite the diversity of mechanisms, these various stories carry similar implications for the response of plant-average wages to a decline in the cost of exporting, due either to an exchange-rate movement or a reduction in tariffs of a trading partner. Figures 1-3 provide a stylized illustration.[12] In Figure 1, λ is a Melitz-type productivity term, $\lambda_d^{\min}$ is the cut-off for remaining in the domestic market, and $\lambda_x^{\min}$ is the cut-off for remaining in the export market.[13] The $w_d(\lambda)$ curve represents the wage that plants would pay if they sold only to the domestic market, and the $w_x(\lambda)$ curve the wage plants would pay if they entered both the domestic and export market. The $w(\lambda)$ curve, the solid black line, represents the wages actually paid, given entry patterns. Figure 2 illustrates the response of wages to either a bilateral reduction in tariffs or an exchange-rate devaluation accompanied by a sufficiently large domestic contraction: the lowest productivity plants exit[14] and intermediate-productivity plants initially just below the export-market cut-off enter the export market. Although the magnitudes of the shifts will depend on the details of the models, in general we would expect a relative increase of wages and profits among exporting plants compared to those that remain in the domestic market. The key implication, for our purposes, is captured by Figure 3, which plots the difference between the gray and black lines in Figure 2: changes in wages (and revenues) in response to the trade shock are larger in initially higher-productivity plants within each industry.[15]

While both categories of models would predict differential wage changes similar to those pictured in Figure 3 for plant-average wages, the two categories carry different predictions for changes in wage premia in response to the trade shock. The neoclassical models predict zero wage premia

[12]The figures do not provide an exact depiction of the predictions of all models. For instance, the fair-wage model of Amiti and Davis (2008) assumes that wage premia vary with firm profits, not revenues, and hence does not predict a discontinuity at the entry cut-off for the export market. But the figures do roughly capture the wage predictions of models such as Helpman, Itskhoki, and Redding (2008), Bustos (2007), or Verhoogen (2008).

[13]Here we implicitly assume that Mexican plants must pay the fixed cost of accessing the domestic market before becoming exporters, and hence that there are no plants that export all their output.

[14]In the case of an exchange-rate devaluation accompanied by a domestic contraction, there are two offsetting effects: on one hand, the domestic contraction reduces profits; on the other hand, the exchange-rate change favors sales of domestic producers over foreign ones. In theory, the direction of the shift in $\lambda_d^{\min}$ in ambiguous. In fact in response to the peso crisis there was increased exit of Mexican plants, suggesting that $\lambda_d^{\min}$ moved to the right as shown.

[15]Many theories predict especially large wage changes for intermediate-λ plants that switch from non-exporting to exporting. The discontinuous function depicted in Figure 3 is unlikely to hold exactly in the data, both because we will have at best noisy proxies for λ and because fixed export costs are likely to be heterogeneous across firms and sectors. In the empirical work below, we will approximate the relationship between λ and predicted log wages with a linear function and abstract from the larger predicted changes for switchers.

for all productivity levels both before and after the trade shock. Models in the non-neoclassical category generally predict changes in wage premia along the lines illustrated in Figure 3. With the available data, we are not able to discriminate among the models within the non-neoclassical category. Rather, this paper should be seen as seeking to discriminate between the two broad classes of theories.[16]

3 Econometric Strategy

Our estimation strategy has two parts. We first use the employer-employee data to decompose plant-level wages into a plant component due to wage premia and a person component due to skill composition. We then relate changes in those components to the export shock brought about by the peso devaluation. In this section we describe the econometric models for the two parts (subsections 3.1 and 3.2 respectively); the data and results are presented in Sections 4 and 5 below.

3.1 Decomposing Plant-Average Wages

The econometric model of Abowd, Kramarz, and Margolis (1999), which has become standard in the employer-employee literature, can be written as follows:

$$w_{it} = \eta_t + \alpha_i + \boldsymbol{x}'_{it}\boldsymbol{\gamma}_t + \boldsymbol{d}'_{it}\boldsymbol{\psi} + \varepsilon_{it} \tag{1}$$

where boldface indicates a vector; i, j, and t index individuals, plants, and years; w_{it} is log wage; α_i is an individual fixed effect; $\boldsymbol{x}_{it}$ is a vector of observable individual characteristics; $\boldsymbol{d}_{it}$ is a $J \times 1$ vector of indicators for presence of individual i in plant j in year t (where J is the total number of plants); and $\boldsymbol{\psi}$ is a $J \times 1$ vector of coefficients on those indicators. It is more common to write the plant indicators and coefficients together as $\boldsymbol{\psi}_{j(i,t)}$, where $j(i,t)$ is the plant in which worker i is employed in period t, but it will be convenient below to write the indicators and coefficients separately.

[16] Verhoogen (2008) argued that a particular mechanism — quality upgrading — generated the plant-level wage changes but remained agnostic about whether those changes were explained by wage premia or sorting of workers by unobserved ability. This paper remains agnostic about the mechanism generating the plant-average wage changes, but attempts to determine whether they reflect wage premia or sorting.

The identifying assumption for this model is:[17]

$$E(\varepsilon_{it}|\boldsymbol{x}_{i1}...\boldsymbol{x}_{iT}, \boldsymbol{d}_{i1}...\boldsymbol{d}_{iT}, \eta_1...\eta_T, \alpha_i) = 0 \tag{2}$$

In the terminology of the panel-data literature, the covariates, including the indicators for which plant an individual is employed in, are assumed to be *strictly exogenous*; that is, all past and future values of the covariates are uncorrelated with the idiosyncratic disturbance in each period.[18] In the employer-employee literature, (2) is referred to as a *conditional random mobility* assumption, since it requires that, conditional on observable variables, an individual's current-period idiosyncratic shock is uncorrelated with which plant she is employed in. Note that the coefficient vector on the vector of plant indicators, $\boldsymbol{\psi}$, is identified by workers who switch between plants; intuitively, if no workers move into or out of a particular plant, then it is not possible to identify the plant effect separately from the individual effects for the workers.

We make several modifications to the standard model. Our baseline model is the following:

$$w_{it} = \eta_t + \alpha_i\delta_t + \boldsymbol{x}_{it}'\boldsymbol{\gamma}_t + \boldsymbol{d}_{it}'\boldsymbol{\psi}_t + \sum_{m=1}^{M}\phi_{mt}w_{it-m} + \varepsilon_{it} \tag{3}$$

We will refer to (3) as the *levels* equation. The most important difference between this model and (1) is the multiplicative term, δ_t, which is allowed to vary by year. This is intended to capture changes in the general-equilibrium return to ability over time.[19] As discussed above, given that changes in the aggregate demand for skill is one of the main mechanisms through which trade shocks may affect labor-market outcomes, it seems important to consider a model that is flexible in this way. A second difference is that we allow the coefficients on the vector of plant indicators, $\boldsymbol{\psi}_t$, to vary by year, to capture potential changes in plant wage policies in response to the trade shock. A third difference is that we include a set of M lags of the dependent variable on the right-hand side. These lags are intended to capture the sluggish adjustment of wages to individual productivity shocks and absorb some of the serial correlation within individuals that

[17] See the statistical appendix of Abowd, Kramarz, and Margolis (1999).

[18] Such an assumption is typically necessary for fixed-effect estimation of panel-data models. Intuitively, after a within transformation removing the individual effect, the disturbance term includes the within-individual mean of errors from all years. Strict exogeneity is required for this term to be uncorrelated with the covariates.

[19] Note that the model assumes that the return to individual characteristics does not vary across sectors within a year. For discussions that take into account the possibility of within-year comparative advantage (but do not estimate plant effects), see Lemieux (1998), and Gibbons, Katz, Lemieux, and Parent (2005).

would otherwise appear in the disturbance term, for reasons that will become clear below.

Our identifying assumption is:

$$E(\varepsilon_{it}|\boldsymbol{x}_{i1}...\boldsymbol{x}_{it}, \boldsymbol{d}_{i1}...\boldsymbol{d}_{it}, \eta_1...\eta_t, \alpha_i, w_{i1}...w_{it}) = 0 \tag{4}$$

In the terminology of the panel-data literature, we require *sequential exogeneity* or *predeterminedness* of the covariates, but not strict exogeneity as in (2). This assumption still requires a form of conditional random mobility, in the sense that current-period idiosyncratic shocks are assumed to be uncorrelated with *current and past* employers. But the assumption is weaker than the standard conditional random mobility assumption, (2), in that it allows for feedback from current-period shocks to *future* employer and other covariates.[20] The assumption that mobility in the current period is sluggish in the sense that it does not respond to current-period innovations in individual productivity seems less objectionable that the assumption that mobility in all periods, including future periods, is uncorrelated with current-period innovations. It is also worth noting that (4), while restrictive, nonetheless allows for relatively rich patterns of selection — in particular, any form of selection based on individual characteristics, including unobserved characteristics such as motivation, that are constant over time.

The multiplicative term, δ_t, complicates the estimation, because the individual effect cannot be eliminated by a within transformation or simple first-differencing. But the model fits the framework of Holtz-Eakin, Newey, and Rosen (1988), and we can follow their approach of quasi-differencing to remove the individual effects.[21] Taking (3) for period $t-1$, solving for α_i, and substituting back into (3) for period t, we have:

$$w_{it} = \pi_{0t} + \boldsymbol{x}'_{it}\boldsymbol{\pi}_{1t} + \boldsymbol{x}'_{it-1}\boldsymbol{\pi}_{2t} + \boldsymbol{d}'_{it}\boldsymbol{\pi}_{3t} + \boldsymbol{d}'_{it-1}\boldsymbol{\pi}_{4t} + \sum_{m=1}^{M+1} w_{it-m}\chi_{mt} + \widetilde{\varepsilon}_{it} \tag{5}$$

[20] In this sense, the current approach differs from the standard value-added models used to estimate teacher effects in an education context, which have recently been criticized by Rothstein (forthcoming).

[21] Similar models with time-varying individual effects, outside of the employer-employee context, have been considered by Chamberlain (1992), Keane and Runkle (1992), Chay and Honoré (1998), Wooldridge (1997), Ahn, Lee, and Schmidt (2001), and Bai (forthcoming), among others. In an education context, Kramarz, Machin, and Ouazad (2008) estimate a teacher-student model in which individual effects vary over time, although the time-varying parameter is constrained to be an interaction of a time trend and the coefficient on the previous year's teacher, rather than allowed to vary freely over time, as in our model.

where:

$$\pi_{0t} = \eta_t - \frac{\delta_t}{\delta_{t-1}}\eta_{t-1} \tag{6a}$$

$$\boldsymbol{\pi}_{1t} = \boldsymbol{\gamma}_t \tag{6b}$$

$$\boldsymbol{\pi}_{2t} = -\frac{\delta_t}{\delta_{t-1}}\boldsymbol{\gamma}_{t-1} \tag{6c}$$

$$\boldsymbol{\pi}_{3t} = \boldsymbol{\psi}_t \tag{6d}$$

$$\boldsymbol{\pi}_{4t} = -\frac{\delta_t}{\delta_{t-1}}\boldsymbol{\psi}_{t-1} \tag{6e}$$

$$\chi_{1t} = \phi_{1t} + \frac{\delta_t}{\delta_{t-1}} \tag{6f}$$

$$\chi_{mt} = \phi_{mt} - \frac{\delta_t}{\delta_{t-1}}\phi_{m-1,t-1} \quad \text{for} \quad m = 2, ..., M \tag{6g}$$

$$\chi_{M+1,t} = -\frac{\delta_t}{\delta_{t-1}}\phi_{M,t-1} \tag{6h}$$

$$\widetilde{\varepsilon}_{it} = \varepsilon_{it} - \frac{\delta_t}{\delta_{t-1}}\varepsilon_{it-1} \tag{6i}$$

We will refer to (5) as the *quasi-differenced* equation. Our general strategy is to estimate the parameters of (5) and then use minimum-distance estimation to recover estimates of the parameters of the levels equation, (3). Because all parameters are allowed to vary by year, the estimation of (5) can be carried out separately by year, which helps to reduce the formidable computational burden.

As discussed by Abowd, Creecy, and Kramarz (2002) in the context of the standard model, the coefficients on the plant-year effects and lagged plant-year effects can be identified only for plants that are "connected" to other plants by workers switching between them.[22] In each year we focus on the largest connected subset of plants.

Another important issue is that the first lag of the dependent variable, w_{it-1}, is clearly correlated with ε_{it-1}, a component of $\widetilde{\varepsilon}_{it}$. A standard approach in such cases is to use further lags of wages as instruments and estimate by two-stage least squares (2SLS) (Anderson and Hsiao, 1982; Holtz-Eakin, Newey, and Rosen, 1988; Arellano and Bond, 1991).[23] A large literature has explored the optimal choice of instruments for efficient GMM estimation; see Arellano and Honoré

[22] In the context of (5), we see that "stayers", workers who do not move between periods $t-1$ and t, have $\boldsymbol{d}_{it-1} = \boldsymbol{d}_{it}$ and hence are not useful for identifying $\boldsymbol{\pi}_{3t}$ separately from $\boldsymbol{\pi}_{4t}$. Note, however, that stayers do help to identify $\boldsymbol{\pi}_{1t}$, $\boldsymbol{\pi}_{2t}$ and $\chi_{1t}, ..., \chi_{M+1,t}$, which in turn identify δ_t/δ_{t-1} and the other levels-equation parameters.

[23] Bernard and Jensen (2004) use an Arellano-Bond-type model in estimating determinants of plants' decisions to export.

(2001) and Arellano (2003) for overviews. The sheer size of our dataset and the large number of parameters to be estimated make the theoretically most efficient estimator computationally impractical. We must satisfy ourselves with inefficient, but still consistent, estimators. The choices of how many lags of wages to include as covariates and which lags to use as instruments must balance several issues. First, the number of included lags, M, must be large enough to absorb sufficient serial correlation that lags of wages available in the data are valid as instruments. Second, the larger is M the weaker will be the correlation between the excluded instrument(s) and the endogenous w_{it-1}; this may lead to poor performance of the 2SLS estimator in finite samples. Third, increasing M raises the number of lags required for each individual, thus reducing the size of the individual-level sample and the number of connected plants. Requiring additional lags can lead to a significant reduction in the number of plants with complete data in the plant-level analysis. In our baseline specification, we attempt to balance these objectives by setting $M = 2$ — that is, including w_{it-1} and w_{it-2} in (3) (hence w_{it-1}, w_{it-2}, and w_{it-3} in (5)) — and using w_{it-4} as an excluded instrument. We thus require that all individuals have at least four consecutive lags of wages.[24]

Two additional notes regarding identification are important. First, as is apparent from (6a)-(6i), the ratio δ_t/δ_{t-1} is identified, but δ_t is not identified separately from δ_{t-1}. To recover estimates of δ_t, we normalize the value of δ in a base year to be 1. Second, the year-specific intercepts, η_t, are not identified separately from one another. There is always one more intercept from the levels equation to identify than intercepts from the quasi-differenced equation with which to identify it. These intercept terms are incidental parameters, and it is not crucial to estimate them; we will simply need to be careful when making comparisons across years below.

Although the large number of individual effects have been quasi-differenced out of (5), the computational demands remain high, because the vectors $\boldsymbol{d}_{it}$ and $\boldsymbol{d}_{it-1}$ (and hence the coefficient vectors $\boldsymbol{\pi}_{3t}$ and $\boldsymbol{\pi}_{4t}$) are of high dimension (i.e. more than 50,000 $\times 1$). To carry out the estimation, we use an iterative algorithm due to Abowd, Creecy, and Kramarz (2002).[25] This iterative procedure does not produce a variance-covariance matrix and we need to bootstrap to calculate standard errors. A standard bootstrapping procedure would face the difficulty that the set of

[24] We have examined the robustness of our results to different choices for the set of lags and set of instruments, and have found that our qualitative conclusions for the relationship between exports and wage premia are relatively insensitive to these choices.

[25] To be specific, we use the a2reg command for Stata, Amine Ouazad's implementation of the Abowd, Creecy, and Kramarz (2002) algorithm.

connected plants — and hence the set of estimable parameters — would vary with each bootstrap sample. Instead, we use the "wild" bootstrap (Wu, 1986), as extended to instrumental-variables models by Davidson and MacKinnon (forthcoming), allowing for clustering at the level of individuals along lines suggested by Cameron, Gelbach, and Miller (2008). This procedure re-samples the residuals from the 2SLS estimation of (5) but not the covariates, and hence does not affect the "connectedness" of plants.[26]

Once we have recovered parameter estimates from the quasi-differenced model, (5), we can estimate the parameters of the levels equation, (3), by a minimum-distance procedure. The computational burden of estimating all parameters from the levels equation simultaneously is prohibitive, since $\boldsymbol{\psi}_t$ for each year contains more than 50,000 elements. Instead, we note from (6b), (6c) and (6f)-(6h) that a subset of the of the quasi-differenced equation parameters ($\boldsymbol{\pi}_{1t}$, $\boldsymbol{\pi}_{2t}$, and $\chi_{1t}, ..., \chi_{M+1,t}$ for all t) are sufficient to identify a subset of the levels-equation parameters ($\boldsymbol{\gamma}_t$, δ_t/δ_{t-1} , and $\phi_{1t}, ..., \phi_{Mt}$ for all t). We do minimum-distance estimation solely on these sub-blocks of parameters. This procedure is not efficient, in that there is information on δ_t/δ_{t-1} embedded in $\boldsymbol{\pi}_{3t}$ and $\boldsymbol{\pi}_{4t}$, and this information could in turn be used to improve estimates of $\boldsymbol{\gamma}_t$ and $\phi_{1t}, ..., \phi_{Mt}$. But the minimum-distance estimates will nonetheless be consistent, which is satisfactory for our purposes. Note also that the 2SLS estimate of $\widehat{\boldsymbol{\pi}}_{5t}$ is a consistent estimator for the vector of plant effects $\boldsymbol{\psi}_t$, and we use it as such.[27]

Equations (6b), (6c) and (6f)-(6h) can be rewritten in in matrix form as $\mathbf{\Pi} = f(\mathbf{\Theta})$, where:

$$\mathbf{\Pi} \equiv \begin{pmatrix} \boldsymbol{\pi}'_{1s} & \boldsymbol{\pi}'_{2s} & \chi_{1s} & \cdots & \chi_{M+1,s} \\ \ldots & \ldots & \ldots & \ldots & \ldots \\ \boldsymbol{\pi}'_{1T} & \boldsymbol{\pi}'_{2T} & \chi_{1T} & \cdots & \chi_{M+1,T} \end{pmatrix} \qquad \mathbf{\Theta} \equiv \begin{pmatrix} \boldsymbol{\gamma}'_{s-1} & \delta_{s-1} & \phi_{1s} & \cdots & \phi_{Ms} \\ \ldots & \ldots & \ldots & \ldots & \ldots \\ \boldsymbol{\gamma}'_{T} & \delta_{T} & \phi_{1T} & \cdots & \phi_{MT} \end{pmatrix}$$

[26] Briefly, our procedure is the following. For each individual, recover the vectors of first-stage and 2SLS residuals (not OLS residuals from the second stage!) for all years, call them $\hat{\boldsymbol{u}}_i$ and $\hat{\boldsymbol{\varepsilon}}_i$ respectively. Form $\hat{\hat{\boldsymbol{u}}}_i = a\hat{\boldsymbol{u}}_i$ and $\hat{\hat{\boldsymbol{\varepsilon}}}_i = a\hat{\boldsymbol{\varepsilon}}_i$ where $a = -1$ with probability .5 and 1 with probability .5. (These are the Rademacher weights advocated by Davidson and Flachaire (2008).) Use $\hat{\hat{\boldsymbol{u}}}_i$ to form $\hat{\hat{w}}_{it-1}$ for each year, using the first-stage covariates and parameter estimates from the full sample. Form $\hat{\hat{w}}_{it}$ for each year, using $\hat{\hat{\boldsymbol{\varepsilon}}}_i$, $\hat{\hat{w}}_{it-1}$ and the covariates and parameter estimates from the full sample. Then re-estimate (5) by 2SLS using $\hat{\hat{w}}_{it}$ and $\hat{\hat{w}}_{it-1}$ in place of w_{it} and w_{it-1}. Collect 50 such replications and calculate the standard deviations of the parameter estimates.

[27] A thorny practical issue in the estimation is that it is difficult to estimate δ_t/δ_{t-1} separately from the ϕ_{mt}'s in the final year of the sample. In previous years, information to estimate each ϕ_{mt} is contained in two sets of reduced-form parameters, from the current year and the subsequent year. (Refer to (6f)-(6h).) But in the final year, only one set of reduced-form parameters is available, and estimates can be erratic. To resolve this issue, we constrain the estimates of ϕ_{mt} in the final year, 2005, to be equal to the estimates in 2004. We note that that the plant-level panel ends in 2003, and the 2004 and 2005 parameters are not used subsequently in our analysis.

The minimum-distance estimator, $\widehat{\mathbf{\Theta}}$, is the matrix that minimizes:

$$Z = \left\{\mathrm{vec}\left(\widehat{\mathbf{\Pi}}\right) - \mathrm{vec}\left(f(\widehat{\mathbf{\Theta}})\right)\right\}' \mathbf{Q}^{-1} \left\{\mathrm{vec}\left(\widehat{\mathbf{\Pi}}\right) - \mathrm{vec}\left(f(\widehat{\mathbf{\Theta}})\right)\right\}$$

for an appropriate choice of weighting matrix $\mathbf{Q}$, where $\widehat{\mathbf{\Pi}}$ is the matrix of estimated parameters corresponding to $\mathbf{\Pi}$. There is a debate in the applied-econometrics literature about the best choice of the weighting matrix, $\mathbf{Q}$, in finite samples. Altonji and Segal (1996) show that the asymptotically efficient choice of $\mathbf{Q}$, which yields the optimal minimum-distance estimator (see Chamberlain (1984)), can be unreliable in small samples, and that the equally weighted minimum-distance estimator (EWMD), making $\mathbf{Q}$ an identity matrix, performs better for a wide range of distributions. A convention among applied researchers is to use the equally weighted minimum distance estimator, and we follow this convention.[28]

We now turn to the decomposition of plant-level wages into plant and person components. It will be convenient to define $\mathbf{\Omega}_{it} \equiv \mathbf{x_{it}}'\boldsymbol{\gamma}_t + \sum_{m=1}^{M} \phi_{mt} w_{it-m}$. From (3), the individual effect, α_i, can be expressed as:

$$\alpha_i = \frac{1}{\delta_t}\left(w_{it} - \mathbf{\Omega}_{it} - \mathbf{d_{it}}'\boldsymbol{\psi}_t\right) - \frac{\eta_t}{\delta_t} - \frac{\varepsilon_{it}}{\delta_t} \tag{7}$$

This individual effect is not identified, because the year-specific intercept, η_t, is not identified. But since η_t does not vary across individuals, we can identify the deviation of α_i from its year-specific expected value, call it $\widetilde{\alpha}_{it}$:

$$\widetilde{\alpha}_{it} \equiv \alpha_i - E_t(\alpha_i) = \tfrac{1}{\delta_t}\left\{w_{it} - \mathbf{\Omega}_{it} - \mathbf{d_{it}}'\boldsymbol{\psi}_t - E_t\left(w_{it} - \mathbf{\Omega}_{it} - \mathbf{d_{it}}'\boldsymbol{\psi}_t\right)\right\} - \tfrac{\varepsilon_{it}}{\delta_t}$$

where $E_t(\cdot)$ represents the expectation across individuals within year t and $E_t(\varepsilon_{it}) = 0$ by assumption. A natural estimator for this deviated individual effect is its sample analogue:

$$\widehat{\widetilde{\alpha}}_{it} = \frac{1}{\widehat{\delta}_t}\left\{w_{it} - \widehat{\mathbf{\Omega}}_{it} - \mathbf{d_{it}}'\widehat{\boldsymbol{\psi}}_t - \left(\overline{w}_t - \overline{\widehat{\mathbf{\Omega}}}_t - \overline{\widehat{\boldsymbol{\psi}}}_t\right)\right\} \tag{8}$$

using parameter estimates from the minimum-distance procedure described above, $\widehat{\mathbf{\Omega}}_{it} = \mathbf{x_{it}}'\widehat{\boldsymbol{\gamma}}_t + \sum_{m=1}^{M} \widehat{\phi}_{mt} w_{it-m}$, and the bar represents the sample average across all individuals in year t.[29]

[28] See, for instance, Abowd and Card (1989, appendix A) and Baker and Solon (2003, p. 302).

[29] That is, $\overline{w}_t = \frac{1}{N_t}\sum_{i=1}^{N_t} w_{it}$, $\overline{\widehat{\mathbf{\Omega}}}_t = \frac{1}{N_t}\sum_{i=1}^{N_t}\widehat{\mathbf{\Omega}}_{it}$, and $\overline{\widehat{\boldsymbol{\psi}}}_t = \frac{1}{N_t}\sum_{i=1}^{N_t}\mathbf{d_{it}}'\widehat{\boldsymbol{\psi}}_t$, where N_t is the number of individuals

We define the "person" component for each individual to be the component of the wage explained by the return to time-invariant individual ability plus the contribution of the other person-specific observables: $s_{it} = \alpha_i \delta_t + \mathbf{\Omega}_{it}$. This person component is not identified for the same reason the individual effect, α_i, is not identified, but we can identify the deviation of s_{it} from its year-specific expectation in terms of $\widetilde{\alpha}_{it}$: $\widetilde{s}_{it} \equiv s_{it} - E_t(s_{it}) = \widetilde{\alpha}_{it}\delta_t + [\mathbf{\Omega}_{it} - E_t(\mathbf{\Omega}_{it})]$. The sample-analogue estimator for s_{it} is then: $\widehat{\widetilde{s}}_{it} = \widehat{\widetilde{\alpha}}_{it}\widehat{\delta}_t + \left(\widehat{\mathbf{\Omega}}_{it} - \overline{\widehat{\mathbf{\Omega}}}_t\right)$. Combining this expression with (8) and rearranging, we have:

$$w_{it} - \overline{w}_t = \left(\mathbf{d_{it}}'\widehat{\boldsymbol{\psi}}_t - \overline{\widehat{\boldsymbol{\psi}}}_t\right) + \widehat{\widetilde{s}}_{it} \tag{9}$$

That is, in deviations from year means, the individual wage is the sum of the person component and the corresponding plant-year effect, which we interpret as a measure of the wage premium. We can then take averages of (9) over all individuals in a plant j in year t. Letting $\psi_{jt} = \mathbf{d_{it}}'\widehat{\boldsymbol{\psi}}_t$ for any individual in plant j in year t (and noting that it takes the same value for all such individuals), we have:

$$\underbrace{\left(\frac{1}{N_{jt}}\sum_{i=1}^{N_{jt}} w_{it}\right) - \overline{w}_t}_{\substack{\text{plant-avg. wage} \\ \text{(deviated)}}} = \underbrace{\psi_{jt} - \overline{\psi}_t}_{\substack{\text{plant component} \\ \text{(deviated)}}} + \underbrace{\frac{1}{N_{jt}}\sum_{i=1}^{N_{jt}} \widehat{\widetilde{s}}_{it}}_{\substack{\text{avg. person comp.} \\ \text{(deviated)}}} \tag{10}$$

As the notes below the terms indicate, once we deviate from year means, plant-level average wages can be neatly decomposed into a plant component, which we interpret as a measure of the plant-average wage premium, and an average person component, which we interpret as a measure of average skill of the workforce.

3.2 Identifying the Effect of the Export Shock on Wage Premia

Once we have estimated the average person and plant components of plant-average wages for each plant in each year, the next step is to relate those components to variation in the effective inducement to export. Following Verhoogen (2008), we argue that the peso devaluation of late 1994 generated a larger effective inducement to export for initially more-productive, larger plants than for initially less-productive, smaller plants in the same industry. Recall from Section 2 that

in year t.

both neoclassical and non-neoclassical theories considered would predict the pattern in Figure 3 for plant-average wages but that only the non-neoclassical theories would predict such a pattern for wage premia.

Our econometric model for investigating these predictions is the following:

$$\triangle y_j = \mu + \widehat{\lambda}_{0j}\beta + D_j\pi + u_j \tag{11}$$

where j indexes establishments, $\triangle y_j$ is a change in an outcome variable of interest, $\widehat{\lambda}_{0j}$ is a proxy for the latent Melitz-type productivity term in an initial year, and D_j is a vector of industry and state dummies. The key outcome variables we consider are the plant-level export share, plant-average wages, the plant component, and the average person component, although we also report regressions with capital intensity as an outcome variable. The coefficient of interest is β, which captures a differential change in the outcome by the initial value of the productivity proxy.

A key step in implementing this approach is to choose a proxy for the latent productivity variable, λ. Following Verhoogen (2008), as our preferred proxy we use log domestic sales. This proxy has the advantage that it is relatively well-measured and the theoretical advantage that it bears a smooth, continuously differentiable relationship to the latent Melitz-type productivity term in many of the theories we considered, since unlike other variables sales are measured separately for the domestic and export markets. Intuitively, the rationale is that productivity leads plants to become large, in the domestic market as well as overall, hence we can infer plants' underlying productivity from their size. When export share is the outcome variable, using log domestic sales as the productivity proxy is likely to generate spurious correlations, since domestic sales appears in the denominator of export share. In this case, we use an alternative measure of plant size, log employment, as the proxy for productivity.[30] To check robustness, we use four additional proxies — log employment, sales per worker, total factor productivity, and an index of export propensity — and show that the results do not depend on the choice of proxy.

Our strategy is to estimate β for the peso crisis period, 1993-1997, and compare the estimate to analogous estimates for a later period during which there was no devaluation of the peso, 1997-2001.[31] (Although data constraints prevent us from estimating our baseline model in the period prior to the devaluation, when possible we also compare to the 1989-1993 period.) For export

[30] When we use the IMSS data without linking to the plant panel, and in those cases the only proxy available is log employment.

[31] We have experimented with alternative periodizations, and have found results similar to those reported below.

share and plant-average wages, the theories discussed in Section 2 would lead us to expect $\beta > 0$ in 1993-1997 and $\beta = 0$ in the other periods, all else being equal. However, it is plausible that there are differential trends between larger, more-productive plants and smaller, less-productive ones, and that $\beta \neq 0$ even without a devaluation. For our purposes, the crucial prediction of the model is that β is *larger* in 1993-1997 than in the other periods. Our approach is analogous to a difference-in-difference-in-differences (D-in-D-in-D) strategy. The comparison of changes between high-λ and low-λ plants during the peso-crisis period is analogous to a simple difference-in-differences. We then compare this difference-in-differences between the "treated" period, 1993-1997, and a "control" period, 1997-2001. If λ were a discrete rather than a continuous variable, the analogy would be exact.[32]

The key assumption underlying our approach is that differences between the estimates of β in 1993-1997 and in other periods can be attributed to the differential shock to the incentive to export brought about by the peso devaluation. Verhoogen (2008) explicitly considered a number of reasons why this assumption might be violated, in particular that the devaluation created a macro shock that may have affected larger and smaller firms within industries differently. Readers are referred to that paper for more extensive discussion, but two points are worth re-emphasizing. First, the assembly-for-export (*maquiladora*) sector in Mexico exported essentially all of its output both before and after the devaluation, thus we would not expect the devaluation to have generated a differential within-industry shock to exporting. Consistent with this prediction, Verhoogen (2008) shows that there was no differential change in wages between larger and smaller *maquiladora* plants during the peso-crisis period. If the macro shock had had a differential within-industry impact through a channel other than the differential inducement to export, one would have expected it to show up in the *maquiladora* sector as well. Second, while there is evidence that exporting plants faced a lower cost of capital than non-exporters, likely due to greater access to foriegn capital, it is also true that they are a greater share of dollar-denominated loans before the crisis and hence their balance sheets were more adversely affected by the devaluation, and these

[32] It is worth noting that (11) can also be interpreted in an instrumental-variables context. The productivity proxy, $\widehat{\lambda}_{0j}$, interacted with an indicator for the devaluation, could be thought of as instrument for the change in exports, which could then be used to estimate the effect of exporting on other outcome variables. The difficulty with this interpretation is that a plant's decisions regarding how much to export and what wage to pay are outcomes of the same optimization problem, and it is not clear what it means to talk about the effect of one on the other. It is not clear, in other words, that the exclusion restriction that the instrument affects wages only through the effect on exports would be valid. Instead, we focus on the reduced-form relationships between exports and the instrument and the wage outcomes and the instrument, which avoids the difficulty of interpretation.

effects appear to offset. Verhoogen (2008) shows that there was no differential within-industry change in the cost of capital that would explain the differential wage changes.

4 Data

The employer-employee data we use are drawn from the administrative records of the *Instituto Mexicano del Seguro Social (IMSS)*, the Mexican social security agency.[33] 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. Table 1 reports aggregate statistics on the size of the Mexican labor force, to indicate of the coverage of the dataset. It is notable that roughly half of remunerated, private-sector employees in Mexico are not registered in IMSS, and hence should be considered informal; while the size of the informal sector seems large by developed-country standards, it is not out of line for countries at approximately Mexico's income level.[34] The IMSS data are available from 1985 to 2005. At the level of individuals, the IMSS data contain information on age, sex, daily wage (including benefits), and state and year of the individual's first registration with IMSS. At the establishment level, the data contain only industry and location.

An important practical issue is that the top- and bottom-codes in the IMSS data have changed over time. Prior to 1991, IMSS allowed establishments to report wages below the corresponding regional minimum wage, even though paying such wages was illegal.[35] Beginning in 1991, this practice was disallowed and thereafter no wages were reported below the minimum wage even if the actual wages paid were below the legal minimum. The top-code has also changed over time. Figure 4 displays the top- and bottom-codes, as well as several wage percentiles from the raw data. To reduce biases due to changes in top- and bottom-codes, we follow a recommendation of Angrist and Krueger (1999) and "winsorize" the wage data, by replacing wages reported below the 10th percentile by the wage at the 10th percentile, and, to maintain symmetry, wages above the 90th percentile with wages at the 90th percentile. This has the added benefit of reducing the influence of outliers generated by other forms of misreporting or measurement error. We have

[33] The data have been used by one of us (Kaplan) in several previous papers with coauthors (Castellanos, Garcia-Verdu, and Kaplan, 2004; Kaplan, Martinez Gonzalez, and Robertson, 2004, 2005).

[34] See e.g. Schneider and Enste (2000). The employment figures for manufacturing in the IMSS data differ by less than 10% from independently reported figures in the 1993 Industrial Census, suggesting that underreporting is not especially severe in the social security data (Kaplan, Martinez Gonzalez, and Robertson, 2004, 2005).

[35] There are three minimum-wage regions in Mexico, with the minimum wage in Mexico City and other urban areas generally 10-20% higher than in poorer rural areas.

also conducted the analysis simply dropping all observations above the 90th or below the 10th percentile and the main conclusions are unchanged.

As noted above, we require that four lags of wages be observed for each individual in a given year. This criterion reduces the sample size significantly, as a relatively large fraction of workers move into or out of the informal sector in each period.[36] As mentioned above, we also require that establishments be "connected" to the largest set of connected establishments. This criterion tends to select the largest, most stable plants. We apply a number of additional cleaning procedures and sample selection criteria; details are in the data appendix. Table 2 reports summary statistics on the baseline individual-level sample requiring four observed wage lags and winsorizing the top and bottom deciles. Even after our sample selection procedure, the number of individual-level observations in each year remains large, in excess of 1.5 million. Note that average real wages dropped significantly following the peso devaluation in late 1994. (A similar pattern can be observed in the raw data in Figure 4.) We estimate average plant and person components for all establishments, manufacturing or non-manufacturing, that appear in the unbalanced, individual-level panel described by Table 2.

The plant-level panel we use is the *Encuesta Industrial Anual (EIA)* [Annual Industrial Survey], conducted by the *Instituto Nacional de Estadísticas y Geografía (INEGI)*, the Mexican statistical agency.[37] The variables contained in the EIA are similar to those in plant-level surveys in other countries: employment, total wage bill, investment, capital stock, domestic and export sales, among others. 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 refreshing of the sample. The EIA does not include information on *maquiladora* plants, assembly-for-export plants located mainly along the U.S. border, which are covered by a different dataset. The EIA data have been linked to the IMSS employer-employee data using establishment name, location and address. We focus on the subset of plants with complete EIA information for which it is possible to estimate wage premia in the IMSS data.

[36] Table A.1 reports transition probabilities in the raw data, with initial status at left and status in the next year in Columns 2-5, reported as a share of the total for the row in Column 1. Approximately 33% of individuals with less than one year of tenure and 14% of individuals with at least one year of tenure leave the dataset in the following year.

[37] We participated in the first discussions between IMSS and INEGI to share the employer-employee and plant-level datasets in 2004. The two agencies signed a legal agreement to share the data in 2006, and we were granted permission to access the datasets in the INEGI offices in Aguascalientes in January 2008. To maintain confidentiality, the computer programs we have developed to carry out the analysis are run only by INEGI personnel.

Because of the "connectedness" criterion discussed above, a large number of linked plants are missing estimated wage premia for one or more years during the 1993-2003 period. Rather than require complete data over the entire period, which would reduce the number of plants significantly, we require only that plants have complete data in three key years: 1993, 1997 and 2001. Approximately 2,200 plants satisfy this criterion. Table 3 reports summary statistics from the the EIA-IMSS linked panel for 1993, broken down by export status. The plants in the linked panel are quite large on average, with average employment greater than 300. The difference between exporters and non-exporters 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, and higher-wage than non-exporters, and they make up a minority of plants in each industry on average.

It is worth emphasizing that, as a consequence of our sample-selection and linking procedures, the individual-level and plant-level estimation samples are skewed toward workers who have higher wages and longer-term attachments to the formal sector than the typical Mexican worker and toward plants that are larger, more stable and more likely to export than the typical Mexican non-*maquiladora* establishment. It is not clear to what extent the results from our non-representative panels generalize to the Mexican labor market as a whole. Nonetheless, we are able to estimate consistently the effect of the export shock on wage premia within our linked dataset. We would also argue that larger, more stable plants and workers with more stable attachments to the formal sector are the sets of plants and workers for whom the relationship between international integration and wages is likely to be most relevant.

5 Main Results

Subsection 5.1 reports results from the estimation of skill-composition and wage-premia components of average wages in the IMSS individual-level data, and Subsection 5.2 reports of the effect of the export shock on those components in the EIA-IMSS plant-level panel. We will consider a series of checks of robustness of the main results in the following section, Section 6.

5.1 Estimating Wage Premia

Tables 4 and 5 report the first-stage and second-stage results from year-by-year 2SLS estimation of equation (5), setting $M = 2$ and using w_{it-4} as an instrument for w_{it-1}. As mentioned above, the individual characteristics available in the IMSS data are quite limited. We include linear and quadratic terms in tenure;[38] for age, the linear term is not identified and we include only a quadratic term.[39] The large sample size makes the estimates quite precise; essentially all estimates are significant at the 1% level. In Table 4, the important point is that w_{it-4} still has explanatory power for w_{it-1} even conditional on w_{it-2} and w_{it-3}.

In order for w_{it-4} to be a valid instrument for w_{it-1}, it must be the case that w_{it-4} is uncorrelated with the second-stage error term, $\widetilde{\varepsilon}_{it} = \varepsilon_{it} - (\delta_t/\delta_{t-1})\varepsilon_{it-1}$. Table 6 reports the matrix of autocorrelations of the 2SLS residuals from estimation of (5). If the lagged dependent variables in (3) are successful in absorbing the time-series structure in wages and the ε's are uncorrelated, then the quasi-differenced errors should display an MA(1) correlation structure, since quasi-differencing generates a mechanical correlation of $\widetilde{\varepsilon}_{it}$ with $\widetilde{\varepsilon}_{it-1}$, but not with further lags. We see that there is strong correlation on the first off-diagonal term, but the autocorrelation drops essentially to zero on the second off-diagonal term, suggesting that using w_{it-4} as an instrument for w_{it-1} is not wildly inappropriate. It is also worth emphasizing that we are not arguing for a causal interpretation of the relationship between lagged and current wages; this instrumental-variable procedure is mainly seeking to reduce biases due to measurement error in wages.

The second-stage estimates in Table 5 serve mainly as a basis for the minimum-distance estimation of the parameters of the levels equation, which are easier to interpret and are reported in Table 7. Note that although the first year for which the reduced-form model (5) is estimated is 1989, the fact that lagged covariates are included allows us to estimate the structural parameters for 1988. Unsurprisingly, more years of tenure are associated with higher wages, and the returns to tenure are largely diminishing, as are the returns to age. The returns to tenure increased over the 1998-1994 period, consistent with the hypothesis that there was an increase in the return to

[38]Because the data begin in 1985, tenure is effectively top-coded and the top-coding changes each year (i.e. the maximum value of tenure is 4 in 1989, 5 in 1990, etc.) The relevant top-code for lag tenure is one year less than for tenure. To avoid introducing mechanical effects arising from changes in the effective top-code, we impose a top-code on tenure and lag tenure of three years.

[39]The difference between the linear term in age and its lag is collinear with the year effect. Note also that time-invariant individual characteristics (sex, place of first registration in IMSS) are collinear with the individual effects in (3) and their coefficients are not identified in (5).

skill, acquired either on the job or in school, over this period.[40]

Perhaps the most noteworthy results in Table 7 are those for the ratio of returns to individual ability, δ_t/δ_{t-1}. It is instructive to compare this pattern to the changes in aggregate inequality in Mexico. Figure 5 displays the estimates of δ_t implied by the estimates of δ_t/δ_{t-1} (normalizing $\delta_{1988} = 1$) against the evolution of the log 90-10 wage ratio from the Encuesta Nacional de Empleo Urbano, a household survey similar to the Current Population Survey in the U.S. The two series display similar broad patterns, with an increase in the late 1980s and early 1990s and a decrease later in the period. There is a difference in the timing of the peak: the log 90-10 ratio continue to increase until 1996, while the estimated δ_t reach a peak in 1993. This suggests that the increase in wage inequality during 1994-1996 may have been driven mainly by factors other than an increase in the return to skill. We do not want to push this interpretation too far: the ratio δ_t/δ_{t-1} is estimated with noise and the standard errors on the implied δ_t magnifies with each new period. The main lesson that we take away is that our model's estimates of changes in the return to ability accord in a rough sense to the time-path of overall inequality. It is also worth noting that our estimates of δ_t/δ_{t-1} are typically significantly different from 1, the value implicitly imposed by standard AKM-type models.

5.2 Estimating the Effects of the Export Shock

The devaluation of the peso in December 1994 represented an enormous shock to the Mexican economy. The Mexican peso lost approximately 50% of its nominal value in a matter of days. Figure 6 plots the real exchange rate over the 1989-2004 period; note that it took several years for the peso to re-appreciate to 1994 levels. GDP fell by 6.7% from 1994 to 1995. Exports rose sharply, with approximately 85% destined for the U.S. market. Using a balanced panel of 3,290 plants from the EIA (not all of which can be linked to the IMSS data), Figure 7 illustrates the shift toward the export market: the export share for the panel as a whole jumped sharply, and the number of plants with positive exports rose from approximately 30% to 45% of the sample.[41]

[40] There is some volatility in the return to tenure over the 1994-2005 period, but there does not appear to be a systematic trend.

[41] 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.

We begin with a set of graphs to illustrate the main results, before turning to statistical tests. Using the EIA-IMSS linked panel, Figure 8 presents a set of non-parametric regressions for outcome variables originally drawn from the EIA: export share, capital intensity, and log plant-average hourly wage (total wage bill divided by total hours worked). The first row of graphs, Figures 8a-c, plot cross-sectional regressions of these variables against plant size, measured as log employment in Figure 8a and log domestic sales in Figures 8b and 8c.[42] All variables have been deviated from industry-year means (and hence all regression curves pass through the origin). We see that larger plants are more likely to export, are more capital intensive, and pay higher wages. The second row of graphs, Figures 8d-f, plot *changes* in the same variables against initial levels of plant size, for two periods, 1993-1997 and 1997-2001.[43] Given that all variables have been deviated from industry means, the information in these graphs is contained in the relative slopes of the curves. We see that larger plants saw a greater increase in export share, capital intensity, and average hourly wages than smaller plants during the 1993-1997 period, and these differential changes were greater than the corresponding changes during the 1997-2001 period. It appears, in other words, that the peso crisis did affect plants differently within industries, consistent with the theoretical models discussed in Section 2. The results so far are similar to those of Verhoogen (2008), which used a broader EIA panel without linking to the IMSS data.

Using variables drawn from the IMSS employer-employee data, Figure 9 presents graphs in the same format as Figure 8 with average log wage, the plant component and the average person component as outcome variables. Note that the plant component and average person component sum to the plant-average log wage, as discussed above, and that the y-axes have the same scale in all graphs. In the first row, we see (a) that all three variables bear a positive relationship to log domestic sales, and (b) that the slope of the plant component vs. plant size curve is approximately twice the slope of the person component vs. plant size curve. We will estimate the slopes more precisely below, but intuitively this already provides the basis for the statement in the abstract that approximately two thirds of the within-plant correlation between plant-average wages and plant size appear to be attributable to wage premia.

The second row compares differential changes over the 1993-1997 and 1997-2001 periods. The patterns in Figure 9d, using plant-level wages averaged up from individual records, are similar to

[42]When export share is an outcome variable, we use log employment as the proxy for plant heterogeneity, to avoid mechanical biases arising from the fact that domestic sales appears in the denominator of export share.

[43]The x-axis variable is log plant size (employment of domestic sales) in 1993 for the 1993-1997 curve, and log plant size in 1997 for the 1997-2001 curve.

those in Figure 8f using the plant-level wages from the EIA data.[44] Figures 9e and 9f illustrate the main point of this paper: essentially all of the difference in differential changes in wages between the 1993-1997 and 1997-2001 periods can be explained by the difference in differential changes in wage premia. There is effectively no difference in differential changes in the person component, our measure of skill composition.

We now turn to parametric regression models in order to calculate standard errors and conduct hypothesis tests. Panel A of Table 8 reports simple linear cross-sectional regressions for 1993, analogous to Figures 8a-c and 9a-c, including both industry and state effects. All six outcome variables — export share, log capital-labor ratio, log average hourly wage (from the EIA), average log daily wage (from the IMSS data), the plant component, and the average person component — are significantly positively correlated with log plant size (log employment in Column 1, log domestic sales in other columns). In terms of magnitudes, the estimate for average log daily wage in Column 4 indicates that a 10% larger plant had 1% higher wages on average. Comparing the coefficient estimates in Columns 4-6 of Panel A, the slope of the plant component vs. log domestic sales is 70% of the slope of plant wages vs. log domestic sales, approximately the two-thirds figure mentioned above.[45]

Panels B.1 and B.2 of Table 8 report regressions of the form of (11) for the 1993-1997 period and 1997-2001 periods, respectively. These regressions are linear analogues to the non-parametric curves in Figures 8d-f and 9d-f. In terms of magnitudes, the estimates for plant-average wages in Column 4 indicate that a 10% larger plant saw approximately .3% greater wage change over the 1993-1997 and no greater wage change over 1997-2001. The crucial statistics for our hypothesis tests are the differences in estimated coefficients ($\widehat{\beta}$ for 1997-2001 minus $\widehat{\beta}$ for 1993-1997) and the standard errors on those differences, reported in Panel C. The results are consistent with the graphs above: there is strong evidence of a difference in differential changes in export share, capital intensity, average wages, and wage premia, but not in skill composition.

A somewhat subtle point is that the differential change in the plant component in the 1997-

[44] In the IMSS data, we observe daily, not hourly, wages, and it is conceivable that the differential wage changes reflect differential changes in hours worked, rather than in hourly wages. But the wages in the EIA are measured on an hourly basis, and the fact that the changes in daily wages in the IMSS in Figure 9d and similar to the changes in hourly wages in the EIA in Figure 8f argues against this hypothesis.

[45] While the 70% figure is greater than is typically found in the employer-employee literature, the magnitude is not completely out of line with previous estimates. For instance, using a different decomposition, Abowd, Finer, and Kramarz (1999) and Abowd, Creecy, and Kramarz (2002) found that person and firm heterogeneity could explain approximately equal shares of overall wage variance in Washington State in the U.S.

2001 period is negative and the differential change in the person component is positive and of equal magnitude. These coefficients are not significantly different from zero and one should be cautious in drawing inferences from them, but one possible interpretation is that the relative increase in wage premia in response to the shock is in part a response to short-term frictions, and that some of the increase in premia is unwound in the later period, as plants with relatively high wages are able to attract high-skill workers. We will see below, when estimating a standard AKM-type model, that this "overshooting" pattern is absent. This suggests that our model may be better at capturing plants' dynamic responses to the shock. Note, however, that the negative differential change in wage premia is significantly smaller than the positive differential change in the earlier period. The net effect is clearly positive, at least over the period for which the plant-level data are available. It remains an open question whether the wage premia would continue to be unwound over a longer time horizon.

6 Robustness

This section presents a number of checks of the robustness of our main findings. As a first check, in Table 9 we estimate our baseline model using a variety of different proxies in place of log domestic sales: log employment, log sales per worker, log TFP, and an index for predicted export share. The table reports four blocks of six regressions, with each block corresponding to Table 8, Columns 3-6, Panels B.1 and B.2. Log TFP is calculated as the coefficient on a plant fixed effect in a regression of log sales on log labor hours, log capital, log materials expenditures and plant fixed effect, estimated separately by 2-digit industry, using data from 1993-1994 (first row) or 1997-1998 (second row). The predicted export share index is calculated as $X\hat{\beta}$ from a tobit of export share on log employment, log hours, log sales, log capital-labor ratio, a foreign ownership indicator (=1 if $\geq$ 10% foreign capital, 0 otherwise) and 4-digit industry effects. The results are quite similar to the baseline results using log domestic sales in Table 8.

Next we estimate a standard AKM-type model with time-constant individual returns. In particular, we estimate the following model, similar to (1) but allowing plant effects to vary across years:

$$w_{it} = \eta_t + \alpha_i + \boldsymbol{x}'_{it}\boldsymbol{\gamma} + \boldsymbol{d}'_{it}\boldsymbol{\psi}_t + \varepsilon_{it} \tag{12}$$

The coefficients on age squared, tenure, and tenure squared (constrained to be equal across periods) are -.0004, .0824 and -.0127, respectively, all highly significant. Table 10 presents results analogous to Columns 4-6 of Table 8 using the plant and person components estimated from (12). The results are quite similar to the results in Table 8. The AKM-type decomposition attributes a slightly greater share of differences in wage levels and a slightly lower share of differences in wage changes to wage premia. But the results are qualitatively similar to those from our approach above. It appears that any biases due to imposing time-constant returns to ability are small, at least in our context. It is worth emphasizing, however, that it is only because we have estimated a model with time-varying returns that we are able to reach this conclusion.

In Table 11, we take a simple alternative approach. Rather than attempting to estimate plant and person components, we examine the changes in average wages over the 1993-1997 and 1997-2001 periods for stayers, individual workers continuously employed in a given plant. Workers who remain in a plant are clearly a selected subset of workers employed in the plant at the beginning of each period. But one would nevertheless expect results for wages of stayers to be broadly consistent with the results for changes in wage premia. In Column 1 of Panel A, we see that the average wage for stayers (defined in Panel A as workers who *will* stay in a plant for at least four years) is higher in larger plants. In Column 2, we include a fifth-degree polynomial in the fraction of stayers in each plant, to correct for the endogenous selection of stayers. Intuitively, the motivation for including this selectivity correction is to absorb as much of the variation between plants that can be attributed to differences in the share of stayers as possible; the coefficients on the other covariates will then be identified on the basis across plants subject to similar amount of selection bias, a common strategy in the non-parametric selection-correction literature (Ahn and Powell, 1993). Similar approaches have been implemented by Card and Payne (2002) and Mas (2008). We see in Panel A, Column 2 that including the selectivity-correction terms has little effect on the estimates. In Panels B.1 and B.2, the dependent variable is the change in wages for workers continuously employed in the plant over the corresponding four-year period. The estimated coefficients in Panel B.1 for 1993-1997 are quite similar to the corresponding coefficient from our model (Table 8, Panel B.1, Column 5) and the AKM-type model (Table 10, Panel B.1, Column 2). The coefficients in Panel B.2 for 1997-2001, are significantly positive; stayers in larger plants saw greater wage increases than stayers in smaller plants, even in the absence of a devaluation. There appear to be background differential trends in stayers' wages between initially larger and initially smaller plants. Nevertheless, as we argued in Subsection 3.2 above, the important question for our study is whether the differential changes were larger in the peso crisis period than in the later period, and here the stayers' regressions tell a similar story to our baseline model and the AKM-type model. Note in particular that the difference in coefficients reported in Panel C, -.023, is almost exactly the same as the corresponding difference in coefficients in Table 10, Panel C, Column 2.

Finally, Table 12 presents a comparison between the 1993-1997 and 1997-2001 periods and a period before the devaluation, 1989-1993. To make this comparison, we are limited to variables that appear in the IMSS administrative records.[46] We focus on establishments in the manufacturing sector, without attempting to link to the EIA, and use log employment as the proxy for plant heterogeneity. Table 12 reports the results, in a format similar to Table 8. We see that the differential change in average wages is significantly higher in 1993-1997 than in either the earlier or the later period. For wage premia, there is a positive differential change in the earlier period. This renders the difference between 1989-1993 and 1993-1997 less stark than the difference between 1993-1997 and 1997-2001, but the difference remains significant at the 10% level.

7 Conclusion

This paper has used a rich new combination of datasets from Mexico to estimate the heterogeneous within-industry effects of a shock to the incentive to export — the late-1994 peso devaluation — on wage premia at the plant level. We have shown that, in levels, approximately two thirds of wage differences between plants within industries can be attributed to differences in wage premia, and approximately one third to workforce composition. More importantly, we have shown that the exchange-rate devaluation generated a differential increase in plant-level wage premia within industries and that this differential increase can explain essentially all of the differential changes in plant-level wages in response to the shock. This result casts doubt on the hypothesis that sorting on individual worker ability can entirely explain the relationship between exporting and wages at the plant level. It appears that one or more features of non-neoclassical labor markets — search and bargaining, efficiency wages, rent-sharing or similar mechanisms — are required to explain the empirical patterns.

[46] The main EIA panel begins in 1993. Although there exists an earlier panel covering 1984-1994 (used in Verhoogen (2008)), the sample is smaller and the number of plants that can be linked to the IMSS data is insufficient for the analysis.

We do not claim to be able to distinguish between the various theories that would predict an effect of an export shock on wage premia. The relative importance of the various non-neoclassical mechanisms remains an important area for research. One question is whether the relative increases in wage premia among larger, more-productive plants in response to the export shock were a short-term response to supply frictions or a more permanent response to persistent changes in export patterns. Although the net differential effect over our sample period is clearly positive, it is possible that we would observe a complete reversal of the differential increase in wage premia if we had access to a longer plant panel. Nevertheless, our results suggest that some form of labor-market imperfection is playing a non-negligible role.

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A Data Appendix

A.1 IMSS individual-level data

As mentioned above, all private Mexican employers are legally required to report wages for their employees to the Mexican social security agency, *Instituto Mexicano del Seguro Social (IMSS)*. Not all employers comply; those that do not are commonly defined as being in the informal sector. The raw IMSS data can thus be considered a census of private, formal-sector establishments and their workforces for 1985-2005. (Public-sector workers and employees of the state-run oil company are covered by other insurance programs.)

The IMSS data contain information on the daily wage of individuals. The wages are a measure of total compensation, called the *salario base de cotización*, which includes both earnings and benefits, including payments made in cash, bonuses, commissions, room and board, overtime payments, and in-kind benefits. The data are reported as a sequence of spells for each worker, with beginning and end dates. In principle it is possible to recover a wage for every individual for every day of every year. We extracted data for September 30 for each year. At the level of individuals, the data also contain information on age, sex, and state and year of the individual's first registration with IMSS. At the establishment level, the data contain information only on location and industry (using the IMSS's own 4-digit industrial categories, of which there are 276.)

We impose the following criteria in cleaning the data. (1) In its internal records, IMSS classifies wage records according to different types, referred to as *modalidades*. We use only *modalidades* for which consistent, reliable wage figures are available.[47] (2) We require that an individual have a positive wage. (3) If two wages in different establishments are observed simultaneously for a given individual, we keep only the higher-wage observation. (4) We require that individuals be employed in an establishment with 5 or more workers. (5) We require that individuals be 14 years or older and 64 year or younger. (6) We require that wages be observed in each of the previous 4 years, in addition to the current year (for reasons described in Section 3.1 above). (7) We require that workers be employed in an establishment in the largest connected graph of establishments, as described in Section 3.1 above.

The total number of workers with wage data in the "raw" IMSS files (i.e. the sample size after step 3 of the cleaning procedure described in the previous paragraph) ranges from approximately 4 million in 1985 to approximately 10 million in 2005. The number in the cleaned data is given by Table 3.

Because the raw data begin in 1985, the tenure variable, which we construct by pooling data for individuals across years, is effectively censored from above, and the top-code changes over time. Given that lagged tenure also appears in the main specification, (5), the binding top-code (on lagged tenure) is 3 in 1989, 4 in 1990 etc. To avoid introducing mechanical biases due to changes in top-coding, we impose a uniform top-code of 3 in all years.

As illustrated in Figure 4, the top-code for wages changed over time. Prior to 1993, the top-code was 10 times the minimum wage in Mexico City; in 1994, it was 18 times; and since 1995 it has been 25 times the minimum wage in Mexico City. Additional details on the IMSS data are available in Castellanos, Garcia-Verdu, and Kaplan (2004) and Kaplan, Martinez Gonzalez, and Robertson (2004, 2005).

A.2 EIA plant-level data

The cleaning procedure for the plant-level data from the *Encuesta Industrial Anual (EIA)* [Annual Industrial Survey] is the same as described in detail in Appendix II (online) of Verhoogen (2008), and rather than repeat the entire description we focus here on key points. Refer to Appendix II (online) of Verhoogen (2008) for details. The main EIA panel used in this paper covers the period 1993-2003.[48]

[47] In the internal classification system, we use *modalidades* 10, 13 17, 34 and 36. This excludes rural casual laborers, self-employed individuals who are insured through IMSS, employees of rural agricultural cooperatives and credit unions, freelance workers, taxi drivers, miscellaneous public-sector workers insured through IMSS, and a number of smaller categories.

[48] An earlier panel is available for the 1984-1994 period, but the number of plants in the earlier panel that can be linked to the IMSS data is too small to draw meaningful statistical inferences. A later panel covering the period

The main EIA sample was drawn in 1993, to include the largest plants in 205 of the 309 6-digit industries (*clases*) in the Mexican industrial classification system, covering 85% of the value of production in each industry. These plants were followed over time, with minimal refreshing of the sample.

Capital stock was constructed using the perpetual-inventory method. Capital was classified into three types: machinery and equipment, land and buildings, and transportation equipment and other fixed assets. Following Olley and Pakes (1996), each type of capital was assumed to evolve according to $K_{jt} = (1 - \delta_j)K_{jt-1} + i_{jt-1}$, where j indexes the three types of capital. Following Levinsohn and Petrin (2003), the depreciation rates, δ_j for machinery and equipment, land and buildings, and transportation equipment were assumed to be 10%, 5% and 20% respectively. Total capital stock is the sum of the three types of capital. The book value of capital stock in 1993 was taken as the initial value.

The following cleaning procedures were implemented. (1) Plants in multi-plant firms for which complete information was not reported separately by plant were dropped. (2) Plants owned in whole or in part by government entities were dropped. (3) Establishments that appeared to be *maquiladoras*, because they derived more than 95% of their income from exports or subcontracting, were dropped. (4) Variables that changed within a plant by more than a factor of 10 from one year to the next were set to missing. (5) Missing values of variables were imputed following the procedure described in Appendix II of Verhoogen (2008). (6) After imputation, plants with incomplete information on any key variable (employment, hours, wage bill, total costs, domestic sales, total sales, capital stock) were dropped. (7) The key variables listed in the previous point were "winsorized" at the 1st and 99th percentiles, following a suggestion Angrist and Krueger (1999).

The EIA plant-level data were linked to the establishment-level information in the IMSS employer-employee data using information on plant name, address, industry, and employment. Plants were included in the final EIA-IMSS plant-level panel if they had complete information (included estimated wage premia) in the years 1993, 1997 and 2001. In the final panel, 2,211 plant satisfied this requirement.

A.3 *Encuesta Nacional de Empleo Urbano (ENEU)*

The *Encuesta Nacional de Empleo Urbano (ENEU)* [National Urban Employment Survey], used only in Figure 5 above, is a household survey modeled on the Current Population Survey (CPS) in the United States. Households are interviewed quarterly for five quarters, and then rotate out of the sample. The original ENEU sample focused on the 16 largest Mexican cities. Over time, the coverage of cities expanded but we focus on the original 16 cities in order to maximize comparability across years. The ENEU sample used in this paper consists of men, ages 16-64, who worked 35 or more hours in previous week. Self-employed workers are excluded.

The hourly wage figures were constructed as follows. (1) We recovered monthly wages for the job worked last week as converted from weekly or bi-weekly basis by INEGI enumerators. Top-coded reports were assigned 1.5 times the top-code value. Individuals who reported not working in previous week were dropped. (2) Monthly hours were calculated as 4.3 times hours worked in the previous week. Responses of "irregular hours, less than 35", "irregular hours, between 35 and 48" and "irregular hours, more than 48" were assigned values of 20, 42 and 60 hours per week, respectively. (3) Hourly wage was calculated as monthly wage/monthly hours. The wage was deflated to constant 1994 pesos using the main consumer price index (INPC) from *Banco de Mexico*, the Mexican central bank. All calculations use the sampling weights reported by INEGI.

2003 to the present is also available, but it is difficult to map IMSS establishments into it.

Figure 1. Theoretical prediction: wages vs. productivity in cross section

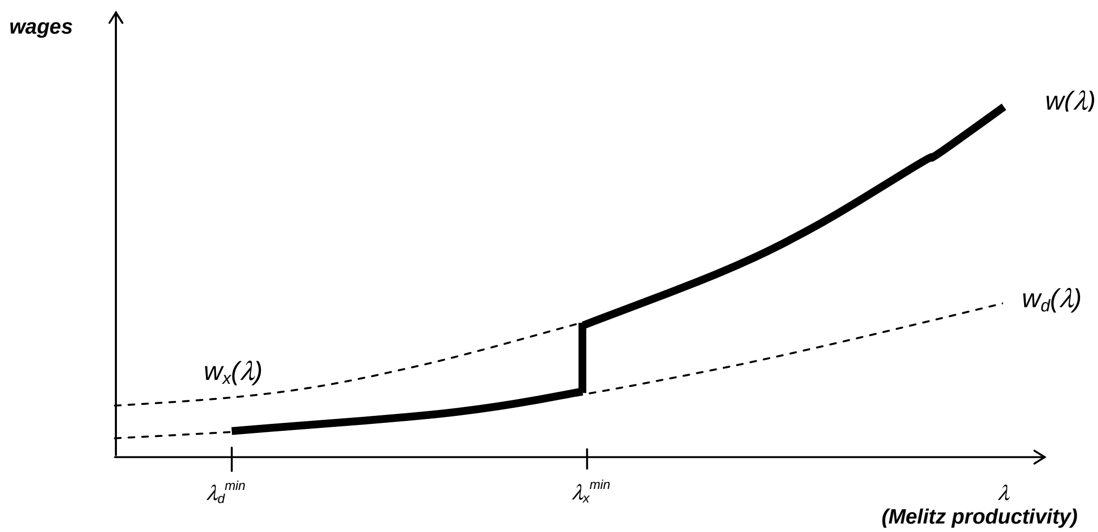


Figure 2. Theoretical prediction, wages vs. productivity, response to trade shock

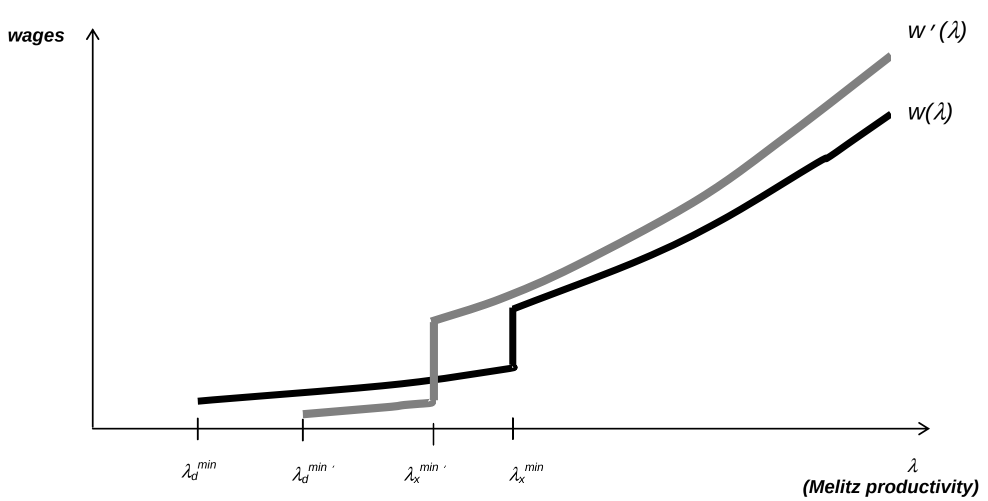


Figure 3. Theoretical prediction: change in wages vs. initial productivity

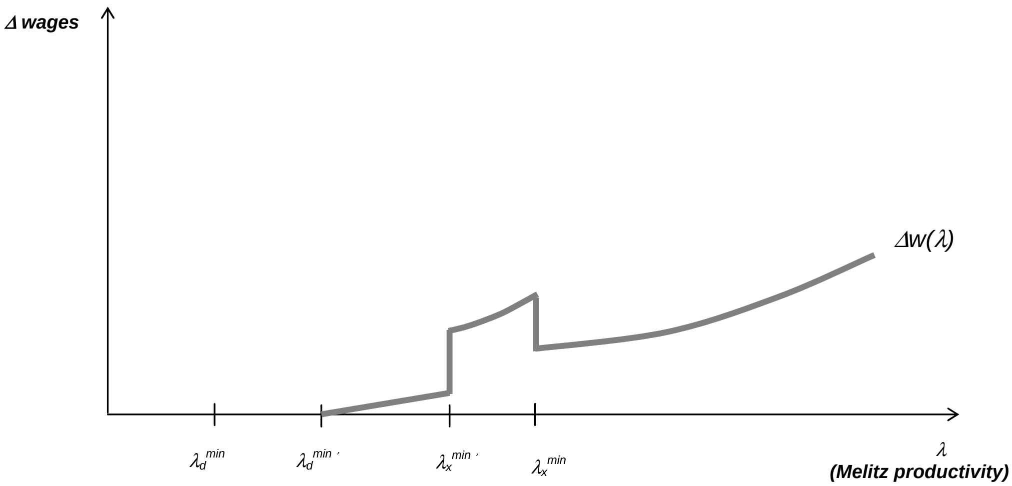
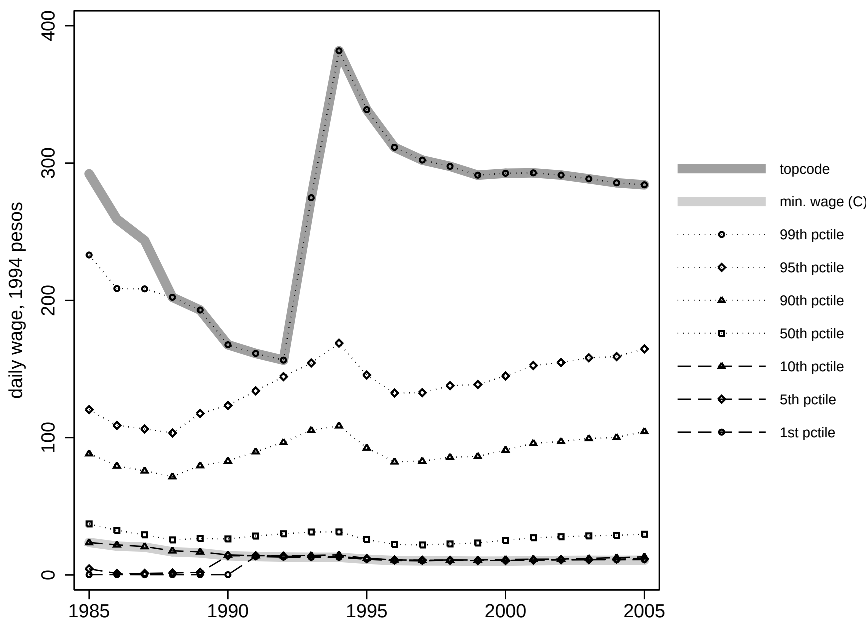
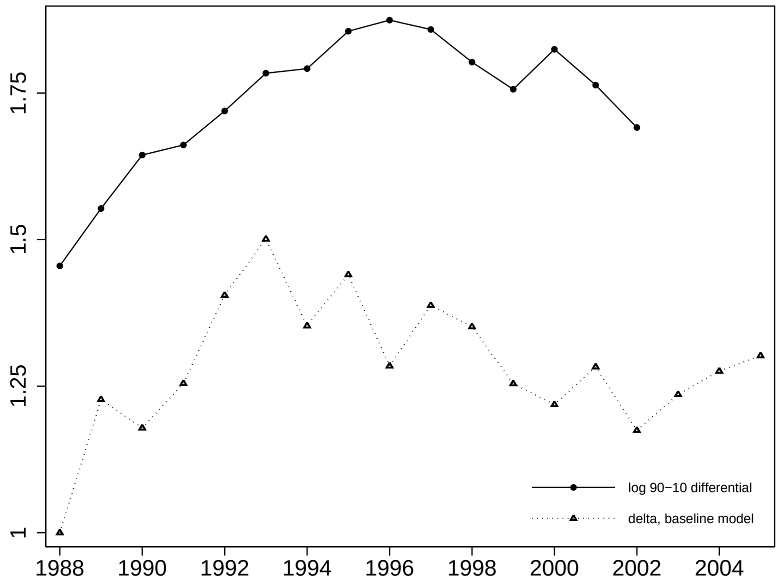


Figure 4. Wage percentiles, top- and bottom-codes, IMSS raw data, 1985-2005



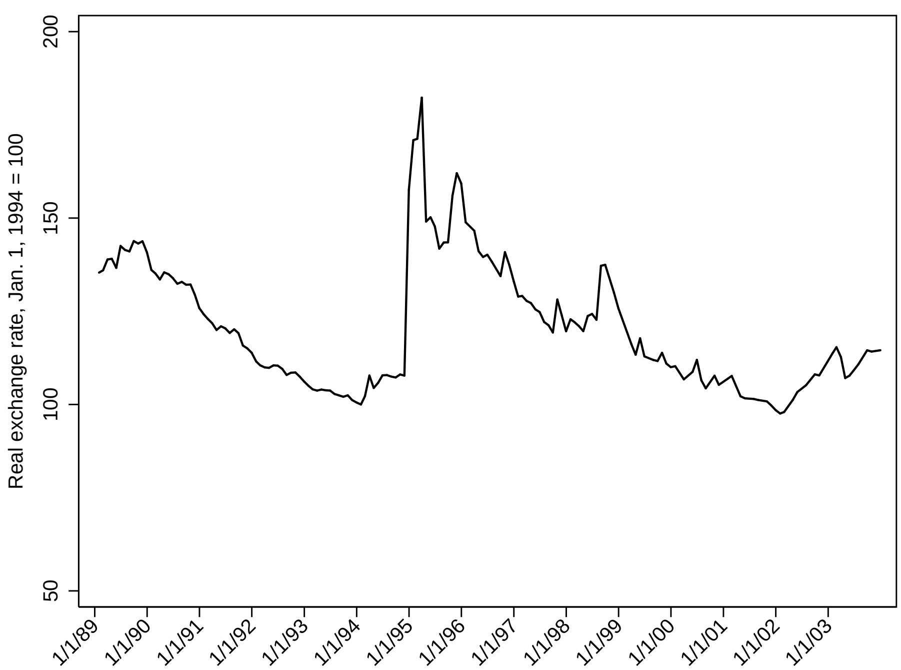
Notes: Wage percentiles calculated from raw IMSS data, before cleaning. There are three minimum wages in Mexico, corresponding to different geographic regions. Displayed is the minimum wage for Zone C, the lowest of the three. The top-code was 10 times the minimum wage in Mexico City (Zone A) from 1985-1993, 18 times in 1994, and 25 times from 1995-2005. Prior to 1991, establishments were allowed to report wages below the corresponding minimum wage to IMSS. Beginning in 1991, this practice was disallowed. Average 1994 exchange rate: 3.38 pesos/US$1.

Figure 5. Aggregate wage inequality and estimated δ_t



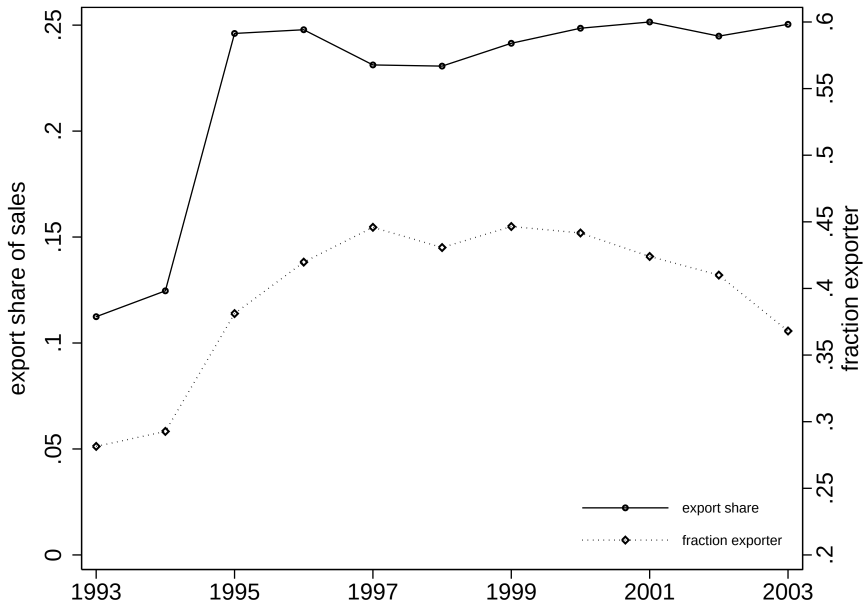
Notes: Estimated values of δ calculated from estimates of δ_t/δ_{t-1} reported in Table 5, with δ normalized to 1 in 1988. Log 90-10 ratio is for real hourly wages from ENEU household survey. For details of data processing, see data appendix.

Figure 6. Real exchange rate



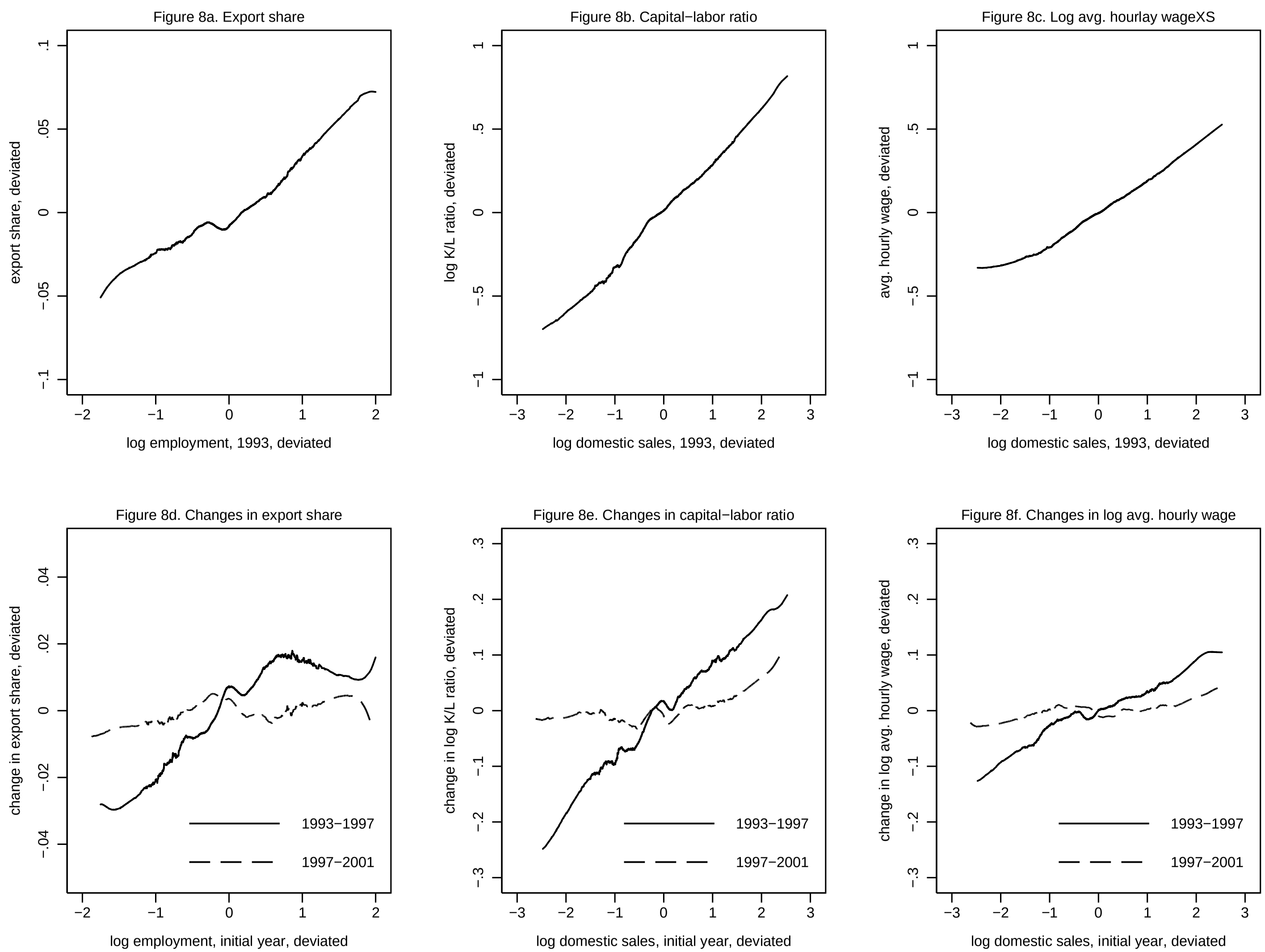
Notes: Real exchange rate calculated as $e\times$CPI(US)/CPI(Mexico), where e is peso/US$ nominal exchange rate. Data from IMF International Financial Statistics.

Figure 7. Shift to export market, EIA panel



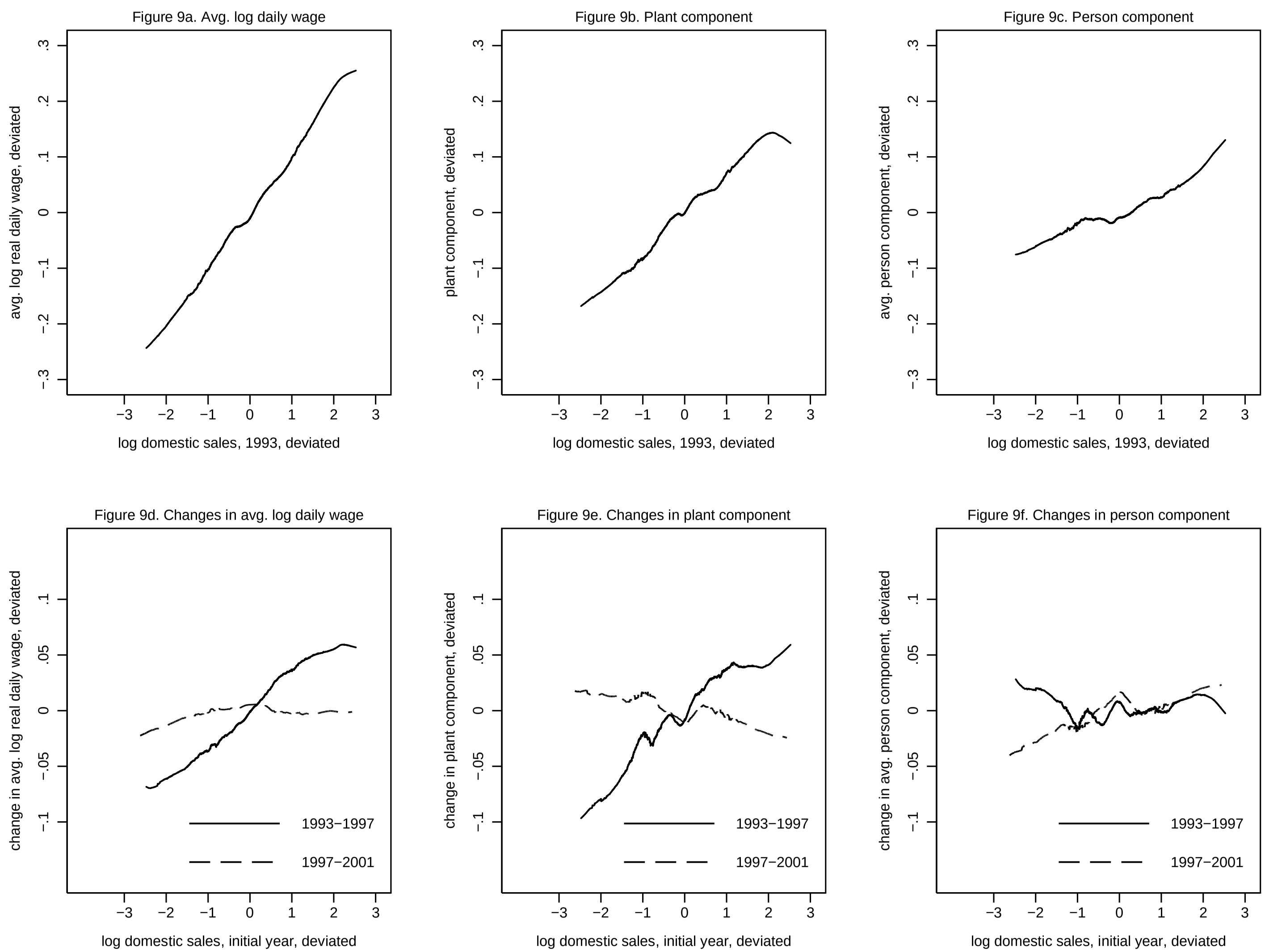
Notes: Data from 1993-2003 EIA balanced panel of 3,290 plants (including plants regardless of whether estimated wage premia available). Export percentage of sales calculated as (total exports for all plants)/(total sales for all plants). Plants with exports greater than zero classified as exporters. See data appendix for details.

Figure 8. Non-parametric regressions, plant-level variables



Notes: Non-parametric regressions estimated using Stata lowess command, bandwidth=.3 in first row, =.4 in second row. All variables deviated from industry-year means. All variables from EIA dataset. Log avg. hourly wage calculated as (total wage bill/total hours worked). See Section 4 of text and data appendix for further details on variables.

Figure 9. Non-parametric regressions, wage components



Notes: Non-parametric regressions estimated using Stata lowess command, bandwidth=.3 in first row, =.4 in second row. All variables deviated from industry-year means. Outcome variables calculated from IMSS individual-level data. See Section 4 of text and data appendix for further details on variables.

Table 1. Aggregate labor force statistics

	1990	2000
Total population	81.25	97.48
Eonomically active pop. age > 14	31.23	40.16
Remunerated workers	25.96	32.01
Remunerated workers, private sector	21.27	27.20
Workers registered in IMSS	10.76	15.24
Workers registered in IMSS, permanent	9.53	13.53

Notes: Numbers in millions. Figures drawn from *Anuario Estadístico de los Estados Unidos Mexicanos* [Statistical Yearbook of Mexico], 2005, based on decennial population censuses (total population), *Encuesta Nacional de Empleo* [National Employment Survey] for 1991 (economically active population age > 14), Mexican national accounts data (remunerated workers), and administrative records of IMSS (number of registered workers).

Table 2. Summary statistics, individual-level data, estimation sample

year	# individuals	# establishments	avg. age	fraction male	avg. daily wage (1994 pesos)
1989	1,680,437	53,755	35.82	0.77	45.47
1990	1,697,521	58,474	36.09	0.77	46.97
1991	1,753,425	62,921	36.36	0.75	50.73
1992	1,958,698	68,568	36.42	0.75	55.08
1993	2,206,920	69,974	36.08	0.74	58.84
1994	2,360,191	69,120	35.90	0.73	60.19
1995	2,476,975	63,829	35.83	0.72	50.32
1996	2,614,035	65,812	35.87	0.72	45.17
1997	2,749,303	73,666	36.04	0.71	45.29
1998	2,858,791	77,698	36.22	0.71	46.69
1999	2,926,678	81,267	36.56	0.71	47.41
2000	3,139,942	89,215	36.72	0.71	50.27
2001	3,285,524	93,166	36.87	0.71	52.38
2002	3,399,090	91,119	36.98	0.70	53.08
2003	3,471,483	89,925	37.01	0.70	54.28
2004	3,606,085	90,070	37.09	0.69	54.56
2005	3,653,018	80,137	37.34	0.69	56.73

Notes: Data from IMSS administrative records. Sample includes individuals who (a) are registered in only one job, (b) are employed in an establishment with at least 5 workers, (c) earn a positive wage, (d) are age 14-64, (e) have satisfied (a)-(d) for the previous four years, in addition to the current year; and (f) are employed in an establishment in the largest "connected" graph of establishments. See Section 4 and data appendix for further details. Average 1994 exchange rate: 3.38 pesos/US$1.

Table 3. Summary statistics, plant-level data, 1993

	non-exporters (1)	exporters (2)	all plants (3)
Total revenues	61.91	148.60	92.81
	(3.83)	(19.36)	(7.38)
Employment	253.96	448.36	323.07
	(9.61)	(28.27)	(11.96)
K/L	51.47	60.34	54.63
	(4.02)	(4.50)	(3.05)
Export share of sales		0.16	0.06
		(0.01)	(0.00)
Avg. daily wage	55.85	61.85	57.98
	(0.39)	(0.57)	(0.33)
N	1425	786	2211

Notes: Table reports statistics using 1993 data from EIA-IMSS linked panel of plants with complete data (including estimated wage premia) in 1993, 1997 and 2001. Standard errors of means in parentheses. Exporter defined as export sales > 0. Export share is fraction of total sales derived from exports. Sales are measured in millions of 1994 Mexican pesos, capital-labor ratio in thousands of 1994 pesos, and average daily wage in 1994 pesos. Average 1994 exchange rate: 3.38 pesos/US$1. Daily wage is plant-level average of individual wages from IMSS estimation sample. For further details, refer to Section 4 and the data appendix.

Table 4. First stage from 2SLS estimation of quasi-differenced equation

	Dependent variable: log real daily wage in year $t-1$ (w_{t-1})								
year	tenure	tenure^2	age^2	lag tenure	lag tenure^2	lag age^2	w_{t-2}	w_{t-3}	w_{t-4}
1989	0.052	-0.016	-0.002	0.035	-0.010	0.002	0.664	0.196	0.133
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.002)	(0.001)
1990	0.080	-0.025	-0.002	0.057	-0.015	0.002	0.794	0.059	0.156
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.002)	(0.002)	(0.001)
1991	0.087	-0.028	-0.001	0.057	-0.014	0.001	0.782	0.090	0.065
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.001)	(0.002)	(0.001)
1992	0.084	-0.026	0.001	0.064	-0.015	-0.001	0.770	0.073	0.058
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
1993	0.091	-0.029	0.001	0.074	-0.017	-0.001	0.806	0.043	0.033
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
1994	0.105	-0.034	0.001	0.109	-0.025	-0.001	0.722	0.144	0.029
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
1995	0.095	-0.031	-0.000	0.107	-0.025	0.000	0.690	0.138	0.062
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
1996	0.108	-0.035	-0.001	0.126	-0.029	0.001	0.678	0.132	0.072
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
1997	0.094	-0.030	-0.000	0.092	-0.022	0.000	0.670	0.134	0.061
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
1998	0.105	-0.034	-0.001	0.082	-0.020	0.001	0.666	0.115	0.080
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
1999	0.113	-0.036	-0.000	0.087	-0.022	0.000	0.639	0.143	0.071
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
2000	0.120	-0.038	-0.001	0.101	-0.025	0.001	0.642	0.133	0.068
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
2001	0.111	-0.035	-0.001	0.088	-0.022	0.001	0.628	0.142	0.067
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
2002	0.115	-0.036	-0.001	0.119	-0.029	0.001	0.622	0.139	0.079
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
2003	0.117	-0.038	-0.001	0.131	-0.030	0.001	0.630	0.135	0.081
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
2004	0.119	-0.038	-0.001	0.123	-0.028	0.001	0.655	0.125	0.076
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
2005	0.121	-0.039	-0.001	0.108	-0.025	0.001	0.659	0.131	0.070
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)

Notes: Table reports coefficients from first-stage estimation of equation (5) in text. Coefficient estimates correspond to co-variates indicated at top of each column. Standard errors estimated with 50 bootstrap replications, clustering at level of individuals. Asterisks to indicate significance omitted to reduce clutter; nearly all estimates are significant at 1% level.

Table 5. Second stage from 2SLS estimation of quasi-differenced equation

	Dependent variable: log real daily wage (w_t)								
year	tenure	tenure^2	age^2	lag tenure	lag tenure^2	lag age^2	w_{t-1}	w_{t-2}	w_{t-3}
1989	0.011	0.004	-0.001	-0.048	0.007	0.001	1.476	-0.390	-0.028
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.012)	(0.008)	(0.004)
1990	0.007	0.007	-0.002	-0.069	0.012	0.002	1.285	-0.238	0.019
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.006)	(0.006)	(0.001)
1991	0.001	0.010	-0.000	-0.081	0.015	0.000	1.330	-0.323	-0.001
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.017)	(0.014)	(0.003)
1992	0.014	0.006	-0.001	-0.096	0.019	0.001	1.333	-0.295	-0.028
	(0.003)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.017)	(0.013)	(0.002)
1993	0.027	0.005	-0.000	-0.106	0.020	0.000	1.233	-0.231	-0.008
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.017)	(0.014)	(0.001)
1994	0.042	0.001	-0.001	-0.112	0.022	0.001	1.109	-0.134	-0.004
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.011)	(0.008)	(0.002)
1995	0.032	0.005	-0.001	-0.130	0.026	0.001	1.217	-0.217	-0.026
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.013)	(0.010)	(0.002)
1996	0.038	0.002	-0.001	-0.119	0.023	0.001	1.097	-0.132	-0.015
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.010)	(0.006)	(0.002)
1997	0.021	0.006	-0.001	-0.117	0.023	0.001	1.222	-0.227	-0.018
	(0.003)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.015)	(0.010)	(0.003)
1998	0.027	0.004	-0.001	-0.107	0.020	0.001	1.111	-0.136	-0.009
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.010)	(0.007)	(0.002)
1999	0.034	0.003	-0.001	-0.108	0.020	0.001	1.095	-0.122	-0.014
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.011)	(0.007)	(0.002)
2000	0.025	0.006	-0.001	-0.118	0.023	0.001	1.140	-0.150	-0.016
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.010)	(0.007)	(0.002)
2001	0.029	0.006	-0.001	-0.126	0.024	0.001	1.193	-0.182	-0.024
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.011)	(0.007)	(0.002)
2002	0.057	-0.001	-0.001	-0.128	0.025	0.001	1.098	-0.120	-0.008
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.008)	(0.005)	(0.002)
2003	0.031	0.008	-0.001	-0.153	0.030	0.001	1.203	-0.189	-0.023
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.008)	(0.005)	(0.002)
2004	0.031	0.007	-0.001	-0.149	0.029	0.001	1.170	-0.169	-0.019
	(0.002)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.007)	(0.005)	(0.001)
2005	0.047	0.003	-0.001	-0.134	0.026	0.001	1.158	-0.161	-0.015
	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.008)	(0.006)	(0.002)

Notes: Table reports estimates of coefficients on covariates in quasi-differenced equation, (5) in text. First lag of log wage (w_{t-1}) instrumented by fourth lag (w_{t-4}); for first-stage estimates, see Table 4. Standard errors estimated with 50 bootstrap replications, clustering at level of individuals. Asterisks to indicate significance omitted to reduce clutter; nearly all estimates are significant at 1% level.

Table 6. Residual autocorrelation matrix

	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005
1989	1.000																
1990	-0.363	1.000															
1991	-0.011	-0.402	1.000														
1992	-0.024	0.008	-0.416	1.000													
1993	-0.021	-0.005	0.008	-0.341	1.000												
1994	-0.004	-0.004	-0.003	-0.000	-0.333	1.000											
1995	-0.010	-0.004	-0.007	-0.010	-0.007	-0.412	1.000										
1996	-0.008	0.001	-0.001	0.001	0.006	-0.005	-0.328	1.000									
1997	-0.026	0.000	0.004	-0.013	-0.029	0.003	-0.005	-0.375	1.000								
1998	0.014	-0.006	0.006	-0.005	0.004	-0.006	0.002	-0.012	-0.386	1.000							
1999	-0.008	0.009	-0.012	-0.000	-0.010	-0.006	-0.010	0.005	-0.026	-0.357	1.000						
2000	0.008	-0.003	-0.003	0.001	0.001	0.003	-0.001	0.003	0.010	0.006	-0.416	1.000					
2001	-0.001	0.003	0.000	-0.005	-0.002	-0.004	-0.005	-0.012	0.009	-0.009	0.008	-0.432	1.000				
2002	-0.001	-0.005	0.009	-0.003	-0.003	0.000	-0.001	-0.003	-0.010	-0.003	-0.005	-0.010	-0.380	1.000			
2003	-0.010	-0.001	-0.004	-0.004	0.007	-0.004	0.001	-0.009	-0.008	0.000	-0.018	0.002	0.001	-0.390	1.000		
2004	-0.001	0.001	0.008	0.009	-0.005	0.002	-0.004	-0.004	-0.000	0.000	-0.003	-0.002	0.010	-0.002	-0.402	1.000	
2005	-0.004	0.004	-0.006	-0.002	-0.002	0.005	-0.002	-0.007	-0.004	-0.009	-0.002	0.000	-0.007	0.007	-0.003	-0.359	1.000

Notes: Table reports autocorrelations of 2SLS residuals from estimation of quasi-differenced equation, (5) in text.

Table 7. Minimum-distance estimates of levels equation

year	age^2 (γ_{1t})	tenure (γ_{2t})	tenure^2 (γ_{3t})	δ_t/δ_{t-1}	w_{it-1} (ϕ_{1t})	w_{it-2} (ϕ_{2t})
1988	-0.0008	0.0389	-0.0060		0.3013	0.0230
	(0.0001)	(0.0016)	(0.0004)		(0.0043)	(0.0029)
1989	-0.0016	0.0402	-0.0038	1.2272	0.2491	-0.0198
	(0.0000)	(0.0015)	(0.0004)	(0.0133)	(0.0057)	(0.0014)
1990	-0.0012	0.0438	-0.0041	0.9606	0.3252	0.0014
	(0.0000)	(0.0016)	(0.0005)	(0.0109)	(0.0090)	(0.0025)
1991	-0.0006	0.0479	-0.0048	1.0645	0.2676	0.0243
	(0.0000)	(0.0014)	(0.0004)	(0.0190)	(0.0086)	(0.0017)
1992	-0.0006	0.0593	-0.0072	1.1198	0.2158	0.0060
	(0.0000)	(0.0017)	(0.0005)	(0.0189)	(0.0100)	(0.0010)
1993	-0.0005	0.0709	-0.0083	1.0682	0.1676	0.0015
	(0.0000)	(0.0013)	(0.0004)	(0.0185)	(0.0078)	(0.0022)
1994	-0.0007	0.0842	-0.0123	0.9013	0.2115	0.0209
	(0.0000)	(0.0011)	(0.0003)	(0.0114)	(0.0055)	(0.0020)
1995	-0.0008	0.0770	-0.0090	1.0645	0.1567	0.0119
	(0.0000)	(0.0017)	(0.0005)	(0.0182)	(0.0074)	(0.0023)
1996	-0.0007	0.0763	-0.0103	0.8920	0.2102	0.0125
	(0.0000)	(0.0010)	(0.0003)	(0.0106)	(0.0076)	(0.0023)
1997	-0.0008	0.0641	-0.0071	1.0803	0.1455	0.0043
	(0.0000)	(0.0016)	(0.0005)	(0.0155)	(0.0058)	(0.0022)
1998	-0.0011	0.0683	-0.0078	0.9738	0.1405	0.0101
	(0.0000)	(0.0014)	(0.0004)	(0.0124)	(0.0070)	(0.0024)
1999	-0.0013	0.0763	-0.0096	0.9281	0.1701	0.0127
	(0.0000)	(0.0012)	(0.0004)	(0.0118)	(0.0058)	(0.0022)
2000	-0.0012	0.0746	-0.0094	0.9715	0.1725	0.0194
	(0.0000)	(0.0013)	(0.0004)	(0.0117)	(0.0061)	(0.0019)
2001	-0.0011	0.0799	-0.0092	1.0528	0.1450	0.0040
	(0.0000)	(0.0014)	(0.0004)	(0.0116)	(0.0045)	(0.0017)
2002	-0.0008	0.1035	-0.0152	0.9156	0.1878	0.0179
	(0.0000)	(0.0010)	(0.0003)	(0.0083)	(0.0034)	(0.0014)
2003	-0.0009	0.0894	-0.0106	1.0521	0.1565	0.0135
	(0.0000)	(0.0012)	(0.0004)	(0.0102)	(0.0049)	(0.0013)
2004	-0.0010	0.0825	-0.0097	1.0323	0.1437	-0.0021
	(0.0000)	(0.0012)	(0.0003)	(0.0076)	(0.0063)	(0.0014)
2005	-0.0008	0.0467	0.0026	1.0205	0.1437	-0.0021
	(0.0000)	(0.0019)	(0.0006)	(0.0024)	(0.0063)	(0.0014)

Notes: Table reports estimates of parameters in levels equation, (3) in text, estimated by minimum distance on parameters of quasi-differenced equation reported in Table 5. Coefficients on plant-year effects and lagged plant-year effects not reported. Standard errors estimated by repeating minimum-distance estimation for each bootstrap replication of quasi-differenced model. Asterisks to indicate significance omitted to reduce clutter; nearly all estimates are significant at 1% level.

Table 8. Differential effect of devaluation, baseline estimates

	(1) export share	(2) log K/L	(3) log avg. hourly wage (EIA)	(4) avg. log daily wage (IMSS)	(5) plant component	(6) person component
A. Cross-sectional correlations, 1993						
log plant size, 1993	0.030***	0.316***	0.187***	0.101***	0.070***	0.031***
	(0.004)	(0.024)	(0.008)	(0.005)	(0.006)	(0.006)
	△ export share	△ log K/L	△ log avg. hourly wage (EIA)	△ avg. log daily wage (IMSS)	△ plant component	△ person component
B.1. Changes, 1993-1997						
log plant size, 1993	0.017***	0.082***	0.036***	0.033***	0.035***	-0.002
	(0.004)	(0.013)	(0.007)	(0.004)	(0.007)	(0.007)
B.2. Changes, 1997-2001						
log plant size, 1997	0.001	0.011	0.005	0.000	-0.011	0.011
	(0.003)	(0.013)	(0.005)	(0.003)	(0.007)	(0.007)
C. Differences in coefficients						
$\beta_{1997-2001} - \beta_{1993-1997}$	-0.015***	-0.071***	-0.031***	-0.032***	-0.045***	0.013
	(0.005)	(0.018)	(0.009)	(0.005)	(0.010)	(0.010)
6-digit industry effects	Y	Y	Y	Y	Y	Y
region (state) effects	Y	Y	Y	Y	Y	Y
N	2211	2211	2211	2211	2211	2211

Notes: Panels A, B.1, and B.2 report six regressions each, all including industry effects (6-digit) and state effects. Panel C reports differences in coefficients between Panel B.1 and B.2, with standard errors on differences allowing for correlation across time periods. Log plant size is log employment in Column (1), log domestic sales otherwise. (Domestic sales enters the denominator of export share, and we avoid regressing changes in export share on initial level of domestic sales to avoid a spurious negative correlation.) Export share is fraction of total sales derived from exports. Robust standard errors in brackets. *10% level, **5% level, ***1% level.

Table 9. Differential effect of devaluation, alternative proxies

Proxy	(1) △ avg. log daily wage	(2) △ plant component	(3) △ person component
log employment, 1993	0.038***	0.045***	-0.007
	(0.006)	(0.010)	(0.010)
log employment, 1997	-0.002	-0.023**	0.020**
	(0.004)	(0.010)	(0.010)
log sales per worker, 1993	0.042***	0.038***	0.004
	(0.007)	(0.013)	(0.013)
log sales per worker, 1997	0.005	-0.004	0.009
	(0.005)	(0.012)	(0.012)
log TFP, 1993	0.025***	0.032***	-0.007
	(0.004)	(0.009)	(0.008)
log TFP, 1997	0.002	-0.010	0.012*
	(0.003)	(0.008)	(0.007)
predicted export share, 1993	0.034***	0.035***	-0.002
	(0.004)	(0.008)	(0.008)
predicted export share, 1997	-0.000	-0.016**	0.016**
	(0.003)	(0.008)	(0.008)
6-digit industry effects	Y	Y	Y
region (state) effects	Y	Y	Y
N	2211	2211	2211

Notes: Table reports 24 separate regressions, of dependent variable at top of column on proxy for plant heterogeneity at left (and industry and state effects), as in Table 8, Panels B.1 and B.2, Columns 4-6. Within each set of 6 regressions using the same proxy, the dependent variable is the change over 1993-1997 in the first row and over 1997-2001 in the second row. log TFP calculated as coefficient on plant fixed effect in regression of log sales on log labor hours, log capital, log materials expenditures and plant fixed effect, estimated separately by 2-digit industry, using data from 1993-1994 (first row) or 1997-1998 (second row). Predicted export share calculated as $X\hat{\beta}$ from a tobit of export share on log employment, log hours, log sales, log capital-labor ratio, a foreign ownership indicator (=1 if $\geq$ 10% foreign capital, 0 otherwise) and 4-digit industry effects. Robust standard errors in brackets. *10% level, **5% level, ***1% level.

Table 10. Differential effect of devaluation, AKM-type model

	(1) avg. log daily wage	(2) plant component	(3) person component
A. Cross-sectional correlations, 1993			
log domestic sales, 1993	0.101***	0.084***	0.017***
	(0.005)	(0.004)	(0.003)
	△ avg. log daily wage	△ plant component	△ person component
B.1. Changes, 1993-1997			
log domestic sales, 1993	0.033***	0.027***	0.006***
	(0.004)	(0.003)	(0.002)
B.2. Changes, 1997-2001			
log domestic sales, 1997	0.000	0.002	-0.002
	(0.003)	(0.003)	(0.002)
C. Differences in coefficients			
$\beta_{1997-2001} - \beta_{1993-1997}$	-0.032***	-0.024***	-0.008***
	(0.005)	(0.004)	(0.003)
6-digit industry effects	Y	Y	Y
region (state) effects	Y	Y	Y
N	2211	2211	2211

Notes: Panels A, B.1, and B.2 report three regressions each, all including industry effects (6-digit) and state effects. Panel C reports differences in coefficients between Panel B.1 and B.2, with standard errors on differences allowing for correlation across time periods. Robust standard errors in brackets. *10% level, **5% level, ***1% level.

Table 11. Differential effect of devaluation, stayers only

	Dep. var.: avg. log wage of stayers	
	(1)	(2)
A. Cross-sectional correlations, 1993		
log domestic sales, 1993	0.099***	0.099***
	(0.005)	(0.005)
	Dep. var.: $\triangle$ avg. log wage of stayers	
B.1. Changes, 1993-1997		
log domestic sales, 1993	0.031***	0.030***
	(0.004)	(0.004)
B.2. Changes, 1997-2001		
log domestic sales, 1997	0.008**	0.007**
	(0.003)	(0.003)
C. Differences in coefficients		
$\beta_{1997-2001} - \beta_{1993-1997}$	-0.023***	-0.023***
	(0.005)	(0.005)
selectivity correction	N	Y
6-digit industry effects	Y	Y
region (state) effects	Y	Y
N	2175	2175

Notes: Panels A, B.1, and B.2 report four regressions each, all including industry effects (6-digit) and state effects. Panel C reports differences in coefficients between Panel B.1 and B.2, with standard errors on differences allowing for correlation across time periods. Dependent variable in Panel A is avg. log daily wage, from IMSS administrative records, of workers who will stay in plant for at least four years. Dependent variable in Panels B.1 and B.2 is average change in log daily wage for workers who stayed in plant over indicated four-year period. Number of observations is reduced from previous tables because not all plants employ workers that stayed in plant over four-year period. See Section 4 and the data appendix for details. Robust standard errors in brackets. *10% level, **5% level, ***1% level.

Table 12. Differential effect of devaluation, IMSS 1989-2005 panel

	(1) avg. log daily wage (IMSS)	(2) plant component	(3) person component
A. Cross-sectional correlations, 1993			
log employment, 1993	0.081***	0.061***	0.020***
	(0.006)	(0.005)	(0.005)
	△ avg. log daily wage (IMSS)	△ plant component	△ person component
B.1 Changes, 1989-1993			
log employment, 1989	0.006	0.012**	-0.006
	(0.005)	(0.005)	(0.006)
B.2 Changes, 1993-1997			
log employment, 1993	0.032***	0.028***	0.004
	(0.004)	(0.006)	(0.007)
B.3. Changes, 1997-2001			
log employment, 1997	-0.003	-0.011*	0.009
	(0.004)	(0.006)	(0.006)
C. Differences in coefficients			
$\beta_{1993-1997} - \beta_{1989-1993}$	0.026***	0.016*	0.010
	(0.006)	(0.008)	(0.009)
$\beta_{1997-2001} - \beta_{1993-1997}$	-0.035***	-0.039***	0.004
	(0.006)	(0.009)	(0.009)
6-digit industry effects	Y	Y	Y
region (state) effects	Y	Y	Y
N	2361	2361	2361

Notes: Panels A and B.1-B.3 report three separate regression each, of the dependent variable at the top of the column on initial log employment, industry and state effects. Panel C reports the indicated differences in coefficients from and the standard errors on the differences. Sample is manufacturing establishments for which plant and person components could be estimated in every year over the 1989-2003 period. Robust standard errors in brackets. *10% level, **5% level, ***1% level.

Table A.1. Transition matrix, IMSS data, 1985-2003

		Status in subsequent year (as share of row)			
	N	tenure < 1	tenure ≥ 1	in micro est.	out of dataset
Initial status	(1)	(2)	(3)	(4)	(5)
tenure < 1	54.2	0.17	0.49	0.02	0.33
tenure ≥ 1	88.8	0.08	0.76	0.01	0.14
in micro-establishment	14.8	0.07	0.07	0.59	0.27
out of dataset	334.8	0.11	.	0.01	0.88

Notes: Transition probabilities calculated from raw IMSS individual-level data (before cleaning) for all individuals age 14-64 who appear in dataset in any year over 1985-2004 period. (Year 2005 omitted because no transitions to subsequent year are observed.) Multiple transitions are observed for each individual. Micro-establishments are defined as having fewer than 5 employees (and are not included in cleaned sample.) Categories tenure$<$ 1 and tenure$\geq$ 1 refer to tenure in a non-micro establishment. Number of observations for initial status (N) measured in millions.