Essays on Development Economics

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ABSTRACT

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This dissertation uses individual-level and aggregate data to study topics in development economics. The first chapter analyzes the impact of Chinese competition on Mexican labor markets. Over the past two decades, the growth of manufacturing in China has significantly impacted international trade. This paper explores how the dramatic rise in Chinese manufacturing exports influences employment in Mexico through product competition in the US export market. Following Autor, Dorn, and Hanson (2013), we compare employment outcomes for formal workers living in different Mexican labor markets to changes in Chinese import competition from 1993 to 2003. We find that exposure to Chinese competition has negative labor market effects for formally employed Mexican workers. In particular, a formally employed individual working in a labor market that experiences the average change in exposure to Chinese competition is 2.0 to 2.7 percentage points less likely to be formally employed in five years.

The second chapter examines the under-reporting of workers wages in Mexico. Non-compliance of firms with tax regulations is a major constraint on state capacity in developing countries. We focus on an arguably under-appreciated dimension of non-compliance: under-reporting of wages by formal firms to evade payroll taxes. We compare two sources of wage information from Mexico — firms' reports of individuals' wages to the Mexican social security agency and individuals' responses to a household labor-force survey — to investigate the extent of wage under-reporting and how it responded to an important change in the social security system. We document that under-reporting by formal firms is extensive, and that compliance is better in larger firms. Using a difference-in-differences strategy based on the 1997 Mexican pension reform, which effectively tied pension benefits more closely to reported wages for younger workers than for older workers, we show that

the reform led to a relative decline in under-reporting for younger workers. The empirical patterns suggest that giving employees incentives and information to improve the accuracy of employer reports can be an effective way to improve payroll-tax compliance.

The third chapter studies the effect of trade liberalization on fertility, sex ratios, and infant mortality in India. We compare women and births in rural Indian districts more or less exposed to tariff cuts. For low socioeconomic status women, tariff cuts increase the likelihood of a female birth and these daughters are less likely to die during infancy and childhood. On the contrary, high-status women are less likely to give birth to girls and their daughters have higher mortality rates when more exposed to tariff declines. Consistent with the fertility-sex ratio trade-off in high son preference societies, fertility increases for low-status women and decreases for high-status women. An exploration of the mechanisms suggests that the labor market returns for low-status women (relative to men) and high-status men (relative to women) have increased in response to trade liberalization. Thus, altered expectations about future returns from daughters relative to sons seem to have caused families to change the sex-composition of and health investments in their children.

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For Mom and Dad

For Mark,
we did it.

Chapter 1

Chinese Competition and Mexican Labor Markets[1]

Todd Kumler

Judith Frías [2]

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[2] Instituto Mexicano del Seguro Social (IMSS)

1.1 Introduction

Over the past two decades, the growth of manufacturing in China has significantly impacted international trade. Since the 1990s, Chinese exports have surged. This can be seen particularly in the US import market, where US imports of Chinese products grew from 27 billion US dollars in 1991 to nearly 298 billion US dollars by 2006 (Figure 1.1). As the dollar share of US imports from China increased from less than 5 percent at the beginning of the 1990s to over 15 percent by 2006, China has displaced Mexico as the number two source of US imports (after Canada, Figure 1.2).

Several recent papers have analyzed the impact of the growth in Chinese exports on employment and earnings in the US. In particular, a recent paper by Autor, Dorn, and Hanson (2013) concludes that US labor markets with a greater share of industries that compete with Chinese imports face decreased wages, increased unemployment, and lower labor force participation. A second study using individual-level social security records finds that workers are more likely to exit the labor force and receive lower cumulative earnings if they were initially employed in industries that experienced high import growth rates from China (Autor, Dorn, Hanson, and Song, 2013).

Despite the growing evidence showing the negative employment effects for US workers as a result of increased Chinese exports to the US, there is less evidence on how the dramatic rise in Chinese manufacturing exports influences employment in other developing countries. Since the composition of manufacturing and exported products is arguably more similar between China and certain developing economies than between China and the US, understanding how Chinese competition influences employment in other low- and middle-income countries is a question of important policy interest. Furthermore, understanding the impact of the growth in Chinese imports can shed light on the impact of competition between developing countries more generally.

In this paper, we explore the impact of increased US imports from China on formal employment and wages in Mexico. This is a particularly apt comparison, since China and Mexico are the second and third largest exporters to the US. While total US imports from China have skyrocketed in the last two decades, growth in US imports from Mexico has been more modest (Figure 1.1). While the dollar share of US imports from China has been increasing (especially after 2001), the share of US imports from Mexico has been relatively stagnant or in decline (Figure 1.1).

We follow an empirical methodology similar to Autor, Dorn, and Hanson (2013) by comparing employment outcomes for formal workers living in different Mexican labor markets to changes in Chinese import competition from 1993 to 2003. To measure a local labor market's exposure to Chinese import competition, we combine changes in US imports from China by industry with differences in initial industrial composition across local labor markets. Since US imports from China can be driven by US import demand as well as Chinese supply, we exploit an instrumental variable approach to isolate the supply-driven component of Chinese exports. Using longitudinal, individual-level employment data for formal sector workers, we are able to determine how exposure to Chinese import competition in a local labor market impacts a worker's daily wage, the probability of exiting the labor force, and the mobility of the worker to other labor markets in the medium-run.

We find that exposure to Chinese competition has negative labor market effects for formally employed Mexican workers. A formally employed individual working in a labor market that experiences the average change in exposure to Chinese competition is 2.0 to 2.7 percentage points less likely to be formally employed in five years. This result is robust to inclusion of various individual controls, and the findings hold regardless of the trade data sets used or the definition of local labor markets. Furthermore, we find that while exposure to Chinese competition does not influence daily wages, this competition does influence labor reallocation. Formal workers facing greater exposure to Chinese competition are less likely to be working in manufacturing in the future, and conditional on being formally employed in the future, workers have an increased probability of relocating to a different labor markets or a different industry.

There are several mechanisms explaining why increased import competition in the US could influence Mexican employment. First, if Mexican manufacturers are unable to compete with Chinese producers, the quantity of exports from Mexico in uncompetitive industries may decline. In turn, Mexican producers may reduce output or exit the market entirely, leading to job losses. Mexican manufacturers may also respond to Chinese competition by changing the types of products exported or the means of production. In this case, changes in employment depends on how labor appears in the production function. Furthermore, changes in the production process may lead to changes in employment for certain categories of workers but not others (for example, skilled or unskilled workers).

If labor reallocates sluggishly to other labor markets or other industries, any job losses due to Chinese competition would be sustained in the medium-run. Job losses among manufacturers could then spillover to other sectors of a local economy. On the other hand, if labor is highly mobile and workers reallocate to comparative advantage industries, employment declines and forgone earnings may be muted. Ultimately, the impact of Chinese competition on local labor markets is an empirical question; distinguishing between the mechanisms driving any changes in local economics is left to future work.

Thia paper measures competition between China and Mexico in the context of the US import market. However, the rise of Chinese manufacturing could also influence Mexican labor markets through a rise in Chinese competition in the domestic Mexican product market. From 1991 to 2006, the proportion of imports to Mexico from China also increased. To the extent that Chinese exports the same types of products to Mexico that it exports to the US, then our industry-level measures of Chinese competition in the US import market may also capture Chinese competition in the Mexican import market. While it is possible to construct a similar measure of exposure to Chinese competition in the Mexican market, this measure is highly correlated with exposure to

Chinese competition in the US import market. Thus, it is not possibly to separately identify the impact of competition with China in the US market and in the Mexican market with the current data and regression framework. However, given that the US import market is much larger than the Mexican import market, it seems plausible that much of the effect on Chinese competition on labor market outcomes in Mexico is driven by competition in the US import market.

This paper relates to several areas of research. In addition to Autor, Dorn, and Hanson (2013) and Autor, Dorn, Hanson, and Song (2013), other authors have analyzed the impact of Chinese exports on workers and firms in other countries. Using firm-level data from Europe, Bloom, Draca, and Reenen (2011) find that firms more impacted by Chinese competition increased technological innovation within these firms; in addition, the report labor reallocation to more technologically advanced firms. Bugamelli, Fabiani, and Sette (2010) find that Chinese imports increase competition and reduce prices among manufacturing firms in Italy. Moreover, Hanson and Robertson (2010) analyze how rising exports from China impacts export demand from ten developing countries (including Mexico); they determine that demand for exports from these countries would have been 0.8 percent to 1.6 percent higher had China's exports remained constant from 1995 to 2005.

In a forthcoming paper, Utar and Ruiz (2013) study the response of maquiladoras (Mexican export processing establishments) to Chinese competition using plant-level data. The authors determine that maquiladoras in industries with the greatest exposure to Chinese competition in the US market experience a decline in employment and plant growth as well as an increased likelihood of plant exit. Additional evidence suggests that increased exposure to Chinese competition prompts movement toward higher-skilled and more technologically-intensive production. My analysis differs from Utar and Ruiz (2013) in that we use individual-level data to look at how outcomes such as employment, wages, and worker mobility are impacted by Chinese import competition. In addition, we measure exposure to Chinese competition at the labor market level, which allows me capture any general equilibrium effects of competition within a local economy.

This paper also relates to several studies that employ a similar methodology to measure exposure to trade shocks at the labor market level. Literature in this area has looked at the district-level impact of India's 1991 trade liberalization (Topalova (2010); Edmonds, Pavcnik, and Topalova (2010a); and Anukriti and Kumler (2014)), regional impacts of NAFTA in Mexico (Chiquiar, 2008), provincial impacts of bilateral trade agreements with the US in Vietnam (McCaig, 2011), and regional impacts of trade liberalization in Brazil (Kovak, 2013).

Finally, this paper relates to the literature analyzing labor mobility resulting from trade exposure. Artuc, Chadhuri, and McLaren (2010) use a structural approach to estimate workers' costs in switching to jobs in different sectors; their parameter estimates imply a slow reallocation of workers in response to trade shocks. Menezes-Filho and Muendler (2011) and Dix-Carneiro (2014) find similarly sluggish reallocation of labor in response to trade shocks in Brazil. More generally, this paper relates to research on reallocation of workers in response to economic shocks; more recent papers in this area include Notowidigdo (2013) and Walker (2013).

The paper proceeds as follows. Section 1.2 describes the methodology for measuring a labor market's exposure to Chinese competition. Section 1.3 discusses the various sources of data, while Section 1.4 details the empirical strategy. Section 1.5 presents the results, and Section 1.6 concludes.

1.2 Measures of Trade Competition

Since the mid 1990s, the value of Chinese exports to the United States has increased dramatically, especially after China's entrance into the WTO in 2001 (Branstetter and Lardy, 2006). The solid blue line in Figure 1.1 plots the real value of Chinese exports to the US from 1991 to 2006. Over this time period, the real value of Chinese exports to the US increased over ten-fold, rising from approximately 27 billion dollars in 1991 to nearly 298 billion dollars in 2006. The penetration of

Chinese exports also increased as a share of total US imports, as shown by the solid blue line in Figure 1.2. The increase in Chinese exports to the US, however, was not uniform across industries: while US imports from China increased significantly for many industries, other industries saw sluggish growth. The real value of US imports from China increased by 15000 percent for some industries, such as tires, socks, and office furniture; other industries had modest or even negative growth (Table 1.1). For some industries, China became the dominant producer of goods for the US market by taking over a large share of the US import market (Table 1.2).

In addition to the rise in US imports from China, there is also substantial spatial heterogeneity in the industrial composition of local labor markets in Mexico. Table 1.3 lists the share of manufacturing employment in the largest industry for the twenty largest Mexican labor markets. While some metro areas have a diversified industrial base (such as Mexico City and Monterrey), many others have a large proportion of employment in one industry. For example, over 50 percent of manufacturing workers are employed in the footwear industry in Leon, while over a quarter of manufacturing workers are employed in one industry in La Laguna (the apparel industry) and Chihuahua (the electric automobile parts industry).

To measure a local labor market's exposure to Chinese competition, we combine the fact that industrial composition differs across Mexican labor markets with the variation across industries in the growth of US imports from China. Intuitively, if Mexican manufacturers are competing with Chinese manufacturers in the US market, they will only compete in industries that have experienced growth in Chinese exports to the US. In turn, the Mexican labor markets where these industries are concentrated will face employment pressure due to Chinese competition.

Specifically, we interact the share of total Mexican employment in industry j working in a given labor market m at the start of time period t ($\frac{E_{mjt}}{E_{jt}}$) with the change in the real value of imports from China to the US over the time period for the industry, ΔM_{jt}^{US}. We then divide this interaction by the total employment in the labor market at the start of the time period (E_{mt}) and sum over all industries:

$$Exposure_{mt} = \sum_{j} \frac{E_{mjt}}{E_{jt}} * \frac{\Delta M_{jt}^{US}}{E_{mt}} \tag{1}$$

One concern with this measure of a local labor market's exposure to Chinese competition is that imports to the US from China and Mexico could be driven by US demand shocks. For example, increased US demand for computers could increase imports of computers from both Mexico and China. As a result, the realized increased in imports of computers from China would be correlated with the realized growth in the imports of computers from Mexico; areas in Mexico that produce computers would witness a potential increase in production and labor demand. OLS estimates would therefore underestimate the true causal impact of Chinese competition in the US import market on Mexican employment.

Previous papers, such as Autor, Dorn, and Hanson (2013), have instrumented for Chinese competition with industry-level imports from China for other advanced economies.[3] One threat, however, to the validity of this type of instrument (which we will explore later) is that demand shocks are in fact correlated across high-income countries. This correlation between product demand shocks and increasing US imports from China would once again lead us to understate the true impact of Chinese competition on Mexican labor markets.

We therefore implement a different instrumental variables (IV) strategies to determine the true causal impact of Chinese competition on Mexican labor markets. This instrument exploits the fact that much of the increase in US imports from China over the sample period was driven

[3] Autor, Dorn, and Hanson (2013) use imports for Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland.

by changing supply-side factors in China. During the sample period, China became increasingly integrated into world trade, culminating in China's ascension to the WTO in 2001 (Branstetter and Lardy, 2006). Other internal reforms in China, combined with large-scale rural-to-urban migration, further increased Chinese ability to manufacture products for export markets as it transitioned to a market-based economy (Chen, Jin, and Yue, 2010).

In addition, industries which experienced the greatest increase in US imports were those in which China initially had a comparative advantage, before the dramatic increase in Chinese exports. This is apparent in Figures 1.3 and 1.4. The red line indicates industries that were in the top third of industries based on the 1991 Chinese import penetration ratio (measured as the total value of US imports from China for an industry divided by the total value of US imports from the entire world for the industry), while the blue line indicates industries in the bottom third for this measure. The top third of industries witnessed dramatic growth in the real value of US imports from China (Figure 1.3) as well as share of the US import market (Figure 1.4). At the same time, there was very little growth in US imports for the bottom third of industries.

Using this observation, we predict the real value of US imports from China for an industry by interacting the import penetration ratio in 1991 with the total value of Chinese imports in time t:

$$MPred_{jt}^{US} = \frac{M_{j1991}^{US,china}}{M_{j1991}^{US,world}} * M_t^{US} \tag{2}$$

We then substitute these predicted values into equation (1) to create the IV:

$$Exposure_{mt}^{pred} = \sum_i \frac{E_{mjt}}{E_{jt}} * \frac{\Delta MPred_{jt}^{US}}{E_{mt}} \tag{3}$$

By using the predicted value of US imports from China, this measure of import competition with China focuses on the arguably exogenous supply-side factors driving the increase in US imports from China. However, it is likely that these supply-side factors also led to an increase in Mexican imports from China over this time period. In addition, it is likely that certain Chinese industries had a high import penetration ratio in 1991 for both the US and for Mexico. As a result, this instrument may also capture Chinese competition with Mexico in the domestic Mexican product market. While US imports from China are much higher than Mexican imports from China, it is important to keep in mind that this measure could reflect both types of Chinese competition when interpreting the resutls.

1.3 Data

This paper combines data from several sources in order to analyze the effects of Chinese import penetration in the US on local labor markets in Mexico. We conduct our analysis using data on international trade flows from the United Nations Commodity Trade Statistics Database (UN Comtrade), which compiles import and export data from government agencies in close to 200 countries. We extract data from the UN Comtrade database on annual US Imports from China and Mexico (as well as total US imports from the whole world); these data are reported at the six-digit Harmonized System (HS) product level.

As a robustness check, we also construct our measure of exposure to Chinese competition using a second data set on trade flows from the Center for International Data at UC Davis. This data set cleans and standardizes US import and export data collected by the U.S. Bureau of the Census. These data are reported in nominal US dollars at the HS ten-digit product level; for consistency with the UN Comtrade database, we use data starting in 1991. Compared to the UN Comtrade database, the trade flow data from the Center for International Data at UC Davis is available at a more disaggregate level. However, these data are only available for US imports.

To construct a measure of exposure to Chinese competition for each Mexican labor market, we combine the trade flow data with variation in the industrial composition of employment across labor markets. Industry employment data are calculated from aggregate employment statistics published in conjunction with the Mexican Economic Census conducted every five years. In theory, the Economic Census surveys all firms operating in Mexico, collecting information on a variety of economic topics. While the microdata from the Economic Census are not publicly available, for this study we make use of aggregate employment statistics published at the industry by municipality level. In particular, we use these data from the 1994, 1999, and 2004 Economic Census, which contain aggregate employment information for the preceding year (i.e. 1993, 1998, and 2003).

An important consideration for the analysis is how to define local labor markets in Mexico. Mexico is divided into 2,438 municipalities, the administrative division below Mexican states.[4] However, in many large urban areas, individuals living in one municipality may commute to a job in a nearby municipality. To account for this fact, the Mexican government has defined various metropolitan areas which combine two or more municipalities into one continuous geographic entity. For municipalities contained within a metropolitan area, we combine these municipalities into the corresponding metropolitan areas and define the relevant labor market as the metropolitan area. We combine all remaining municipalities existing outside of a metropolitan area within a state into a local labor market. Since both municipalities and metropolitan areas have changed over time, we utilize municipality and metropolitan area classifications from the 1990 Mexican Census of Population to ensure that borders of local labor markets are not impacted by population and employment changes during the period of study. This leads to 86 separately defined local labor markets (54 metro areas and 32 states).[5]

[4]Municipalities in Mexico are similar to counties in the US.

[5]The results are robust to using other definitions of local labor markets. This includes defining each municipality

The Economic Census has employed different industry classifications over time. Industries in the 1994 Census are classified according to the six-digit Clasificación Mexicana de Actividades y Productos (CMAP, Mexican Classification of Activities and Products); there are 305 different manufacturing industry classifications in the 1994 Census. Industries in the 1999 and 2004 Census, meanwhile, are classified according to the 2002 revision of the Sistema de Clasificación Industrial de América del Norte (SCIAN, Industrial Classification System of North America), also at the six-digit level. There are 308 different manufacturing industry classifications in the 2002 SCIAN classification. We use a concordance between CMAP 1994 and SCIAN 2002 to re-classify all industries in all years using the 2002 SCIAN. A final challenge in measuring Mexican labor market exposure to Chinese competition is integrating the industries in the trade flow data (reported at the HS ten-digit or HS six-digit level) with the industries in the aggregate employment data (reported at the SCIAN six-digit level). Our procedure for converting the industries in the trade flow data to SCIAN industries is as follows.[6] First, we use a concordance created by Pierce and Schott (2009) to convert the more disaggregated ten-digit HS code trade flow data from the Center for International Data at UC Davis to US NAICS codes, which are similar to Mexican SCIAN codes. We then use the resulting mapping between US imports at the ten-digit HS level and the six-digit SCIAN level to determine weights (calculated as a share of total world imports to the US over the sample period) for each six-digit HS product code and each six-digit SCIAN code combination. These weights are then applied to the UN Comtrade data to calculate trade flows at the six-digit SCIAN level. Finally, with the trade flow data sets now at the SCIAN level, we are able to combine the trade flow data with the employment composition data from the Census.

To analyze employment outcomes, we rely on data from the Instituto Mexicano del Seguro

outside of a metropolitan area as its own labor market, as well as using metropolitan areas based on the 2000 Mexican Census of Population. In addition, the results are robust to grouping municipalities into labor markets based on whether a significant portion of individuals living in a municipality report working in a different municipality, similar to the strategy used by Atkin (2012).

[6] This is based in large degree on the method used by Autor, Dorn, and Hanson (2013).

Social (IMSS), the Mexican social security agency. These underlying data contain linked employer-employee records for all formal workers at private firms in Mexico that are covered by IMSS.[7] Each record in the microdata includes information on the firm's industry and location as well as the individual worker's daily wage, birth year, and state of birth. Since each worker receives a unique individual ID, it is possible to follow workers over time and across jobs.[8]

The IMSS records are reported in wage spells for each worker-firm match. It is therefore possible to determine a worker's wage and employment for any particular day; we extract employment and wages for June 30 of each year. These data are available from 1985 to 2005 although we only make use of data from 1990 to 2003.

For workers that are observed working at two or more firms simultaneously, we select the worker-employee match that has the highest wage. We also exclude any workers for whom a zero daily wage is reported. Finally, we include working age individuals between the ages of 25 and 60.[9] For additional information on the IMSS data set, see Castellanos, Garcia-Verdu, and Kaplan (2004) and Kaplan, Martinez Gonzalez, and Robertson (2005).

1.4 Empirical Analysis

Using the measures of Chinese competition and the data previously described, we run the following basic regression models:

$$Y_{imt} = \alpha + \beta Exposure_{mt} + \gamma_m + \delta_t + age_i + male_i + \rho X_{it} + \epsilon_{mt} \tag{4}$$

[7] In theory, all private sector workers in Mexico are covered by IMSS. However, firms may not report workers to IMSS to avoid the payroll taxes associated with financing IMSS. Unreported workers are considered informal. Frías, Kaplan, and Verhoogen (2009a) calculate that in 2000, 15.2 million workers were reported as working to IMSS out of 27.2 million remunerated private sector workers. In the remainder of this paper, we refer to workers registered in IMSS as employed in the formal sector.

[8] One limitation of the IMSS data is that it does not include information on hours worked.

[9] Covered IMSS workers can receive pension benefits starting at age 60. There is a significant increase in the probability that a worker exits the IMSS data set after age 60.

where Y_{imt} is the outcome of interest (employment, wages, etc) for individual i employed in labor market m at the beginning of time t, $Exposure_{mt}$ is the exposure to Chinese trade competition measure for labor market m and time period t, γ_m is a labor market fixed effect, δ_t is a time period fixed effect, age_i is an age fixed effect, $male_i$ is an indicator for a male worker, and X_{it} are other controls. To account for correlation across individuals employed in the same local labor market at the start of each time period, standard errors are clustered at the metro area by time period level.

The main regressor of interest is $Exposure_{mt}$. This regressor is constructed as previously described in order to reflect Chinese competition in the US import market; however, as previously noted, it may also capture Chinese competition in the domestic Mexican product market. $Exposure_{mt}$ is measured as changes over a five-year time period, corresponding to the Economic Census (i.e. 1993 to 1998 and 1998 to 2003). In the IV regressions, the related instruments are constructed and measured similarly. The inclusion of labor market fixed effects (γ_m) and time period fixed effects (δ_t) control for any unobserved, time-invariant labor market characteristics as well as any Mexico-wide trends.

All regressions include fixed effects for a worker's age (in years) at the start of the time period. Additional controls X_{it} are added to regressions to check robustness. These controls include wage-quartile fixed effects based on a worker's daily wage at the start of the time period,[10] a worker's industry at the start of the time period, fixed effects for the size of a worker's employer at the start of the time period,[11] and indicators for whether a worker was formally employed for the past one, two or three years.

One additional control that is included in some regressions is the share of the local labor force employed in manufacturing. Variation in $Exposure_{mt}$ is the result of differences in the composition of manufacturing employment across industries as well as the share of employment in manufacturing

[10]Daily wage quartiles are determined for all of Mexico.

[11]Firm size categories are firms with one employee, firms with 2 to 20 employees, firms with 21 to 100 employees, and firms with greater than 100 employees.

and non-manufacturing in the labor market. Including the share of employment in manufacturing as a control variable isolates the variation in $Exposure_{mt}$ coming only from differences in the composition of manufacturing across labor markets.

For computational ease, we run equation (4) at the cell level. We weight each cell by the size of the cell at the beginning of the time period.

Table 1.4 presents variable means from the individual-level IMSS employment data. Since the main sample consists of all workers employed at the beginning of each period, Column 1 shows statistics for workers employed in 1993 and Column 2 shows statistics for workers employed in 1998. In general, formal workers employed in 1998 are more likely to be female and are slightly younger than in 1993. In addition, formal labor market turnover is lower in the period from 1998 to 2003 than in the period from 1993 to 1998. Over time, real formal sector wages increased dramatically. It is less clear whether the national share of employment in manufacturing has changed over time, although the firm size of the average worker appears to have decreased.

Table 1.5 presents descriptive statistics for the variables measuring exposure to Chinese competition. For ease in displaying and interpreting results, $Exposure_{mt}$ is calculated in 100,000 of dollars of exposure per worker. For example, the mean of 0.0476 in the pooled sample indicates a mean level of exposure of $4,760 per worker.

1.5 Results

1.5.1 Main Results

Table 1.6 displays results for the main outcome of interest, which is an indicator for whether a formally employed worker in t is also formally employed in $t + 5$. These regressions therefore capture whether exposure to Chinese competition influences the probability that someone formally employed in a given year remains formally employed in five years. The six columns represent regression models with different control variables, such as a worker's initial industry, tenure, daily wage quartile and firm size, as well as the initial manufacturing share of employment in a labor market.

OLS results are indicated in Panel A. The estimated coefficients are consistently negative across all columns; however, none of the coefficients are statistically significant. Furthermore, the point estimates are relatively small. The largest estimate in Panel A (-0.086 in Column (1)) indicates that an individual living in the average labor market (which experienced an increase of $4760 in Chinese competition per worker) would be 0.4 percentage points less likely to be formally employed in five years.

As previously discussed, OLS regressions may be underestimates of the true effect if $Exposure_{mt}$ is driven by US import demand shocks. We therefore instrument for exposure to Chinese competition using $Exposure_{mt}^{pred}$. Panel B in Table 1.6 indicates the coefficients from first stage regressions of $Exposure_{mt}$ on $Exposure_{mt}^{pred}$ while Panel C shows the instrumental variable coefficients on $Exposure_{mt}$.

The first stage coefficients indicate a consistently positive relationship between predicted exposure and actual exposure to Chinese competition. An increase in predicted exposure of $100000 per worker is correlated with an $1100 per worker increase in actual exposure to Chinese competition. This relationship is statistically significant, and the first stage F-statistics are sufficiently high, regardless of the individual control variables included in the regression.

Panel C of Table 1.6 provides the paper's main finding: formally employed workers in areas more exposed to Chinese competition are less likely to be formally employed in five years. The coefficient estimates suggest an economically meaningful impact of Chinese competition. A formally employed individual living in the average labor market (which experienced an increase of $4760 in Chinese competition per worker) is between 2.0 and 2.7 percentage points less likely to be employed in five years.

The IV point estimates are approximately six to ten times larger than the OLS estimates in Panel A. Since the instrument seeks to isolate the component of Chinese competition driven by exogenous, supply-related changes in Chinese exports, the larger IV estimates suggest an upward bias in the OLS results due to US demand for imports. However, since the instrument may also capture Chinese competition in the Mexican market, the larger IV estimate could also reflect the impact of this domestic competition that is less reflected in the OLS results.

In addition, the results in Panel C are generally robust to the inclusion of different control variables. In particular, the inclusion of a labor market's initial manufacturing share causes the magnitude of the effect of Chinese competition to decline; however, the coefficients typically remain economically meaningful and statistically significant. Thus, while part of the employment effect of Chinese competition is driven by variation in manufacturing share across labor markets, the employment effect is also driven by variation in the composition of manufacturing across labor markets (as indicated by the coefficients in the even columns of Table (1.6)).

Having established that exposure to Chinese competition increases job losses in Mexican labor markets, Table 1.7 provides IV results for other outcomes of interest. In Panels A and B, we present coefficients from regressions of an individual's real formal daily wage on exposure to Chinese competition. While the coefficients are negative (i.e. Chinese competition leads to lower wages), the coefficients are generally insignificant. This is the case whether workers that are not formally employed in $t+5$ are included with zero wages in the regressions (Panel A) or whether these workers are excluded (Panel B). Although we do not attempt to determine the mechanism behind the lack of an effect of Chinese competition on daily wages, possible explanations include downwardly sticky wages as a result of collective bargaining agreements or minimum wages provisions.[12]

[12] In addition, mismeasurement in daily wages could lead to imprecisely estimated coefficients.

Panels C and D of Table 1.7 present results of IV regressions of an indicator for being formally employed in manufacturing in five years on exposure to Chinese competition. Panel C (which includes individuals that are not formally employed in $t + 5$) indicates that exposure to Chinese competition reduces the likelihood that an individual is formally employed in manufacturing in five years. However, conditional on being formally employed in $t+5$ as in Panel D, exposure to Chinese competition does not have a statistically significant effect on whether an individual is employed in a manufacturing industry.

Finally, Panels E and F of Table 1.7 displays results for outcomes of whether a formally employed worker reallocates to another labor market or another industry in five years. Overall, it appears that part of the impact of Chinese competition is mitigated by reallocation of labor to other labor markets and industries, as exposure to Chinese competition increases the probability of being formally employed in another labor market (Panel E) or another industry (Panel F).

1.5.2 Additional Results

In this section, we seek to validate and extend our main results. First, we confirm our results using a different source of data for trade flows. Table 1.8 presents coefficients for OLS and IV regressions of an employment indicator on Chinese competition using the UC Davis import data. These results largely corroborate our previous findings. While OLS coefficients are negative, small, and insignificant, IV coefficients indicate that Chinese competition has a statistically significant and economically meaningful impact on whether a formally employed individual remains employed five years in the future.

To test the validity of the results, Table 1.9 displays the results of a falsification test. We run regressions of past employment outcomes on the measure of exposure to Chinese competition. Specifically, for workers formally employed in time t, we look at an indicator for being employed in $t-3$. If the research design is valid, then our measure of the change in exposure to Chinese competition from t to $t+5$ should not influence the probability a worker is formally employed in $t-3$. Indeed, this is what we find in Table 1.9 . While the point estimates are consistently negative across the different regression models, they are always small and statistically insignificant.

We explore heterogeneity in the effect of Chinese competition on formal employment in Tables 1.10, 1.11 and 1.12.[13] Each panel in these tables displays results for a different sub-group. Table (1.10) displays regressions by a worker's sex (Panels A and B) and age (Panels C and D). First, it appears that exposure to Chinese competition has a larger effect on whether a male worker remains formally employed than on a female worker. The point estimates for males are consistently higher than the point estimates for females, and only the coefficients for males are statistically significant.

When comparing the results by age (Panels C and D), the results appear to be driven by older workers. Formal workers age 40 to 60 are more likely than workers age 25 to 40 to exit the formal labor force as a result to exposure to Chinese competition. This differential effect could arise for several reasons. For example, older workers may be less willing or able to move to a new employer, industry, or labor market. Workers closer to retirement age may also choose to retire early and drop-out of the formal labor force in response to job separations. It could also be the case that firms are more likely to layoff older workers as a result of Chinese competition.

Regressions by manufacturing and non-manufacturing industries are shown in Panels A and B of Table 1.11. Since manufacturing firms are the most likely to face competition with Chinese firms in the US import market (as well as in the domestic Mexican market), it is not surprising that manufacturing workers are more likely to exit the formal labor force in five years as a result of Chinese competition. While coefficient estimates are negative for formal workers in non-manufacturing industries, the results are insignificant.

[13]While results by subgroup are informative, any differences between subgroups should not be interpreted as a causal relationship. Differential effects may occur for other reasons that cannot be controlled for; for example, different groups may work in different occupations or work different hours.

Finally, we divide the sample based on whether the labor market is in a border state or a non-border state. Many maquiladoras, or plants which assemble or process imported raw inputs and which export finished products back to the US, are geographically clustered in states adjacent to the US-Mexico border. If the impact of Chinese manufacturing is truly driven by competition in the US import market, we might expect a larger effect of exposure to Chinese competition in border states. However, the results in Table 1.12 are inconclusive for several reasons. The IV regression for non-border states (Panel A) suffers from a weak first stage. In addition, there are only 19 labor markets in the border region regressions (Panel B), leading to an insufficient number of clusters (38 labor market by time clusters). Ultimately, nearly all of the coefficients in Table 1.12 are statistically insignificant.

As a final robustness check, we confirm the plausibility of our results by running industry-level regressions. We regress US imports from Mexico on US imports from China, as well as total US imports (excluding China) on US imports from China:

$$Y_{jt} = \alpha + \beta USImpChina_{jt} + \omega_j + \phi_t + \epsilon_{jt} \tag{5}$$

In these regressions, the regressor of interest is $USImpChina_{jt}$, which is a measure of US imports from China for industry j in year t. The inclusion of industry fixed effects (ω_j) and year fixed effects (ϕ_t) isolates within-industry variation in $USImpChina_{jt}$ over time. Y_{jt} captures either US imports from Mexico or total US imports excluding China for industry j in year t. If Mexican industries compete with Chinese industries in the US import market or in the Mexican import market, we should expect $\beta < 0$. If $\beta > 0$, we would be concerned that demand factors or other variables are driving the results.

The coefficients for OLS regressions (Table 1.13) indicate a positive relationship between industry-level US imports from Mexico and US imports from China (Column (1)). The relationship between total industry-level imports to the US (excluding imports from China) and US imports from China (Column (2)) is also positive, although the coefficient is insignificant. As previously discussed, these relationships could be driven by several factors; one likely explanation may be that certain industries experienced large increases in US demand over the time period (such as computers and electronics). As a result, exports from China and Mexico (and potentially many other countries) to the US simultaneously increased. This coordinated demand increase for imports from both China and Mexico would cause an upward bias in the coefficient, potentially overwhelming any competitive relationship between China and Mexico in the US import market. As a result, the industry-level regressions in Table 1.13 provide one explanation for the small and insignificant point estimates in Table 1.6.

Table 1.14 shows industry-level regressions using an instrumental variables approach. Columns (1) and (2) of Table 1.14 display results from IV regressions using predicted US imports from China as an instrument. These coefficients indicate that increased US imports from China reduce US imports from Mexico as well as US imports from the rest of the world excluding China. In this case, the industry level regressions using the predicted US imports from China instrument seems to validate the use of $Exposure_{mt}^{pred}$ as an instrument for exposure to Chinese competition in the labor market regressions.

Since US imports from China are commonly instrumented using imports for eight other advanced economies from China as an instrument for US import from China, we also present IV regressions using this instrument.[14] Interestingly, when using this instrument, we find a a positive and statistically significant relationship between US imports from China and US imports from Mex-

[14]The eight countries are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland. This instrument is analogous to one used by Autor, Dorn, and Hanson (2013).

ico (Column (3) of Table 1.14) as well as a positive and statistically significant relationship between US imports from China and US imports from the rest of the world (Column (4)).[15] As previously discussed, correlated import demand shocks across advanced economies provides one explanation for the positive coefficients when instrumenting based on Chinese exports to other countries.[16] Ultimately, these industry-level regressions suggest that instrumenting US imports from China using imports for other advanced countries is not a credible instrument and predicted US imports from China based on China's initial comparative advantage is a more reliable estimation strategy.

1.6 Conclusion

The growth in manufacturing in China represents one of the biggest shifts in the world economy over the past two decades. The resulting rise in exports from China has potentially pronounced effects on manufacturers located in other countries that compete with Chinese manufacturers on the global export market. This paper adds to the existing literature documenting how competition with Chinese manufactured goods influences workers employed in competiting industries in a developing country. In particular, this paper explores how Chinese competition influences formal employment and wages in Mexico. We find that exposure to Chinese competition has negative labor market effects for formally employed Mexican workers. A formally employed individual working in a labor market that experiences the average change in exposure to Chinese competition is 2.0 to 2.7 percentage points less likely to be formally employed in five years. This result is robust to including various individual controls and to different definitions of local labor markets. In addition, we find that while Chinese competition does not influence daily wages, formally employed workers that

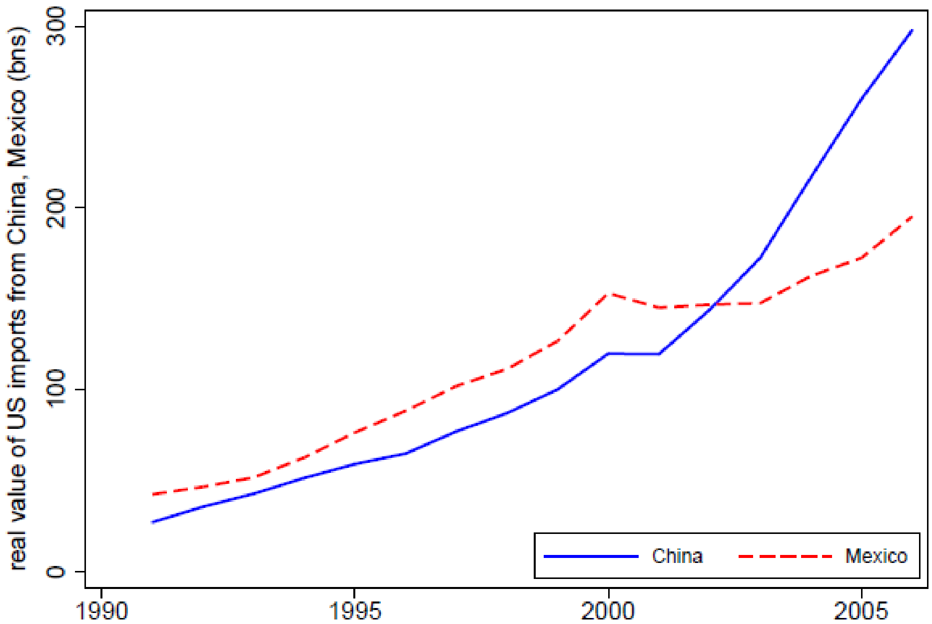
[15] Appendix Table 1.A1 presents similar findings for regressions using the SIC-level trade flow data from Autor, Dorn, and Hanson (2013). These results indicate a positive and significant relationship between Chinese exports to the eight other advance countries and Mexico/CAFTA exports to the US, low-income country exports to the US, and the rest of the world's exports to the US. In addition, the results indicate a negative and significant relationship between Chinese exports to the eight other advance countries and US exports to these same eight countries.

[16] Other hypotheses could also explain the positive coefficients. For example, if Chinese exports to advanced economies caused Mexican firms to produce higher quality and more expensive products, we would see a positive relationship.

face greater exposure to Chinese competition are less likely to be working in manufacturing in the future. Thus, we find that labor reallocates to other sectors in Mexico in response to competition with Chinese exports.

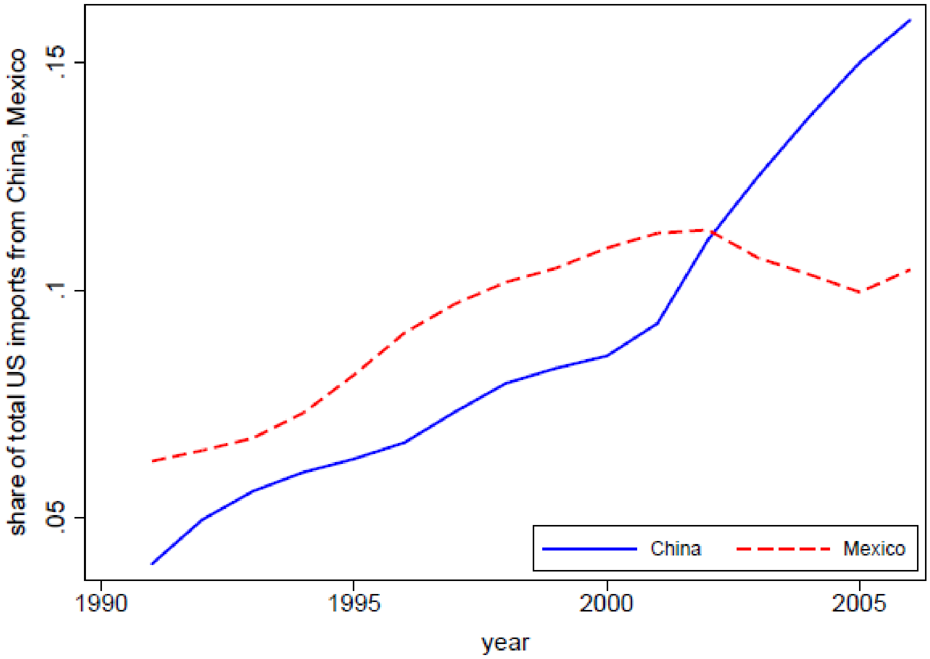
1.7 Figures

Figure 1.1. Real Value of US Imports from China and Mexico, 1991-2006



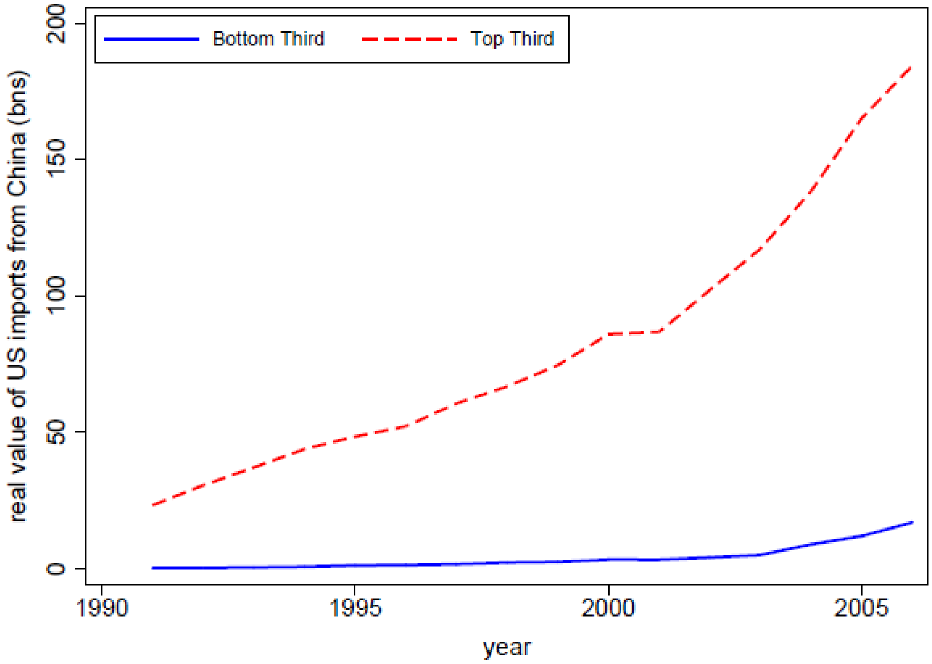
Notes: Data from UN Comtrade database.

Figure 1.2. Share of US Imports from China and Mexico, 1991-2006



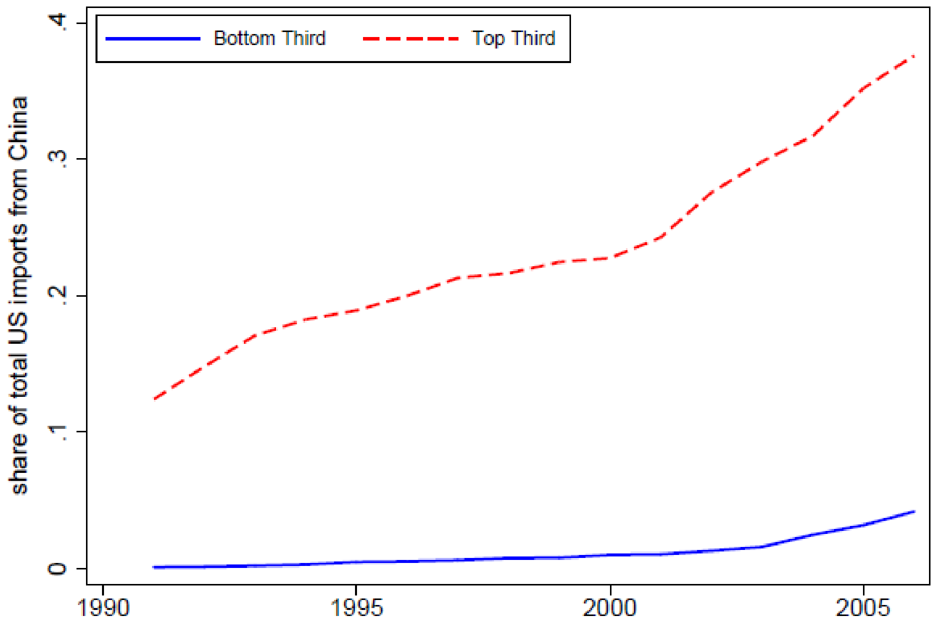
Notes: Data from UN Comtrade database.

Figure 1.3. Real Value of US Imports from China, 1991-2006, By Initial Penetration



Notes: Data from UN Comtrade database.

Figure 1.4. Share of US Imports from China, 1991-2006, By Initial Penetration



Notes: Data from UN Comtrade database.

1.8 Tables

Table 1.1. Change in Real Value of US Imports from China, 1991-2006

Ten Biggest		Ten Smallest	
	A. Change in Real Value (billions)		
Computer, Peripherals	39278.0	Breweries	-2.7
AV Equipment	17481.1	Wineries	0.7
Semiconductors	14870.6	Explosives	0.9
Apparel	14752.0	Thread Mills	6.3
Radio, TV, Wireless Comm.	13032.7	Motor Vehical Metal Stamping	9.4
Household Furniture	12109.6	Cookie, Cracker, Pasta	14.3
Doll, Toy, Games	11999.0	Snack Foods	20.6
Footwear	10560.3	Fiber, Yarn	24.7
Commercial, Service Machinery	6539.1	Chocolate, Confectionary	30.1
Small Appliances	5280.7	Coffee and Tea	35.5
	B. Percentage Change in Real Value		
Tires	57664.70	Breweries	-22.79
Millwork	36939.29	Wineries	60.08
Paper Mills	36452.70	Explosives	92.59
Cooper Rolling, Alloying	28819.29	Coffee and Tea	94.67
Gasoline Engine	28504.51	Broadwoven Fabric	107.73
Sawmill, Woodworking Machinery	19659.89	Carpet and Rug Mills	129.75
Hosiery, Socks	16933.39	Chocolate, Confectionary	156.63
Motor Vehical Seating	16923.04	Thread Mills	164.07
Metal Tank	16760.06	Cookie, Cracker, Pasta	185.00
Office Furniture	16097.67	Primary Nonferrous Smelting	228.35

Notes: Data from UN Comtrade database.

Table 1.2. Change in Share of Total US Imports from China, 1991-2006

Ten Biggest		Ten Smallest	
	A. Change in Share		
Sporting Goods	0.5601	Carpet and Rug Mills	-0.0252
Household Furniture	0.5302	Coffee and Tea	-0.0091
Lighting Fixtures	0.4909	Breweries	-0.0071
Doll, Toy, Games	0.4817	Snack Foods	-0.0032
Computer, Peripherals	0.4686	Pharmaceuticals	-0.0013
Hat, Caps	0.4655	Explosives	-0.0010
Blind and Shade	0.4563	Wineries	-0.0002
Footwear	0.4535	Petroleum Refineries	0.0003
Apparel Accessories	0.4301	Cookie, Cracker, Pasta	0.0041
Commercial, Service Machinery	0.4194	Primary Nonferrous Smelting	0.0045
	B. Percentage Change in Share		
Paper Mills	27156.86	Breweries	-74.51
Tires	20679.69	Wineries	-40.38
Gasoline Engine	11993.26	Explosives	-14.63
Photo Film, Paper	10126.41	Coffee and Tea	-13.51
Motor Vehical Seating	9749.54	Carpet and Rug Mills	-13.26
Railroad Rolling Stock	8239.69	Pharmaceuticals	-8.40
Sawmill, Woodworking Machinery	6998.06	Snack Foods	-3.88
Computer, Peripherals	6205.25	Petroleum Refineries	6.57
Metal Tank	6107.83	Cookie, Cracker, Pasta	10.76
Brake Systems	5746.85	Chocolate, Confectionary	23.35

Notes: Data from UN Comtrade database.

Table 1.3. Largest Industries in 20 Biggest Metro Areas, 1993

Metro Area	Largest Industry	Share of Manufacturing Employment
Valle de México	Apparel	0.072
Monterrey	Apparel	0.057
Guadalajara	Footwear	0.087
Juárez	Motor Vehicle Electric	0.222
Puebla-Tlaxcala	Broadwoven Fabric	0.091
Tijuana	Semiconductors	0.122
León	Footwear	0.541
Toluca	Bread, Bakery	0.067
La Laguna	Apparel	0.299
San Luis Potosí-Soledad de Graciano Sánchez	Major Appliacnes	0.072
Chihuahua	Motor Vehicle Electric	0.274
Mérida	Apparel	0.159
Aguascalientes	Apparel	0.182
Querétaro	Transmission, Power Train	0.071
Saltillo	Clay Building Material	0.075
Mexicali	Semiconductors	0.132
Cuernavaca	Auto, Light Truck	0.130
Reynosa-Río Bravo	AV Equipment	0.262
Matamoros	Semiconductors	0.165
Tampico	Petroleum Refineries	0.184

Notes: Data from 1994 Mexican Economic Census. Twenty largest metro areas as measured by total employment in 1993. Share calculated as employment in industry in metro area divided by total manufacturing employment in metro area.

Table 1.4. Summary Means

	Employed in 1993	Employed in 1998
male	0.71	0.69
age, t	35.87	35.92
employed, t+5	0.59	0.61
employed, t-3	0.65	0.66
real daily wage, t	49.78	119.25
real daily wage, t+5	87.96	148.83
real daily wage, t-3	18.51	47.22
employed in manufacturing, t	0.35	0.36
employed in manufacturing, t+5	0.21	0.21
employed in manufacturing, t-3	0.25	0.24
real daily wage, if employed, t+5	150.31	245.57
real daily wage, if employed, t-3	28.59	71.60
employed in manufacturing, if employed, t+5	0.36	0.34
employed in manufacturing, if employed, t-3	0.39	0.37
employed in new labor market, if employed, t+5	0.08	0.06
employed in new labor market, if employed, t-3	0.05	0.09
firm size, t	1169.64	1143.66
N	5254740	6430079

Notes: Samples shown are formally employed workers in 1993 (first column) and formally employed workers in 1998 (second column).

Table 1.5. Summary Statistics, Exposure to Chinese Competition

	Δ 1993-1998	Δ 1998-2003	Pooled
$Exposure_{mt}$	0.0497	0.0454	0.0476
	(0.0741)	(0.0568)	(0.0658)
N	86	86	172

Notes: Standard deviations shown in parentheses. $Exposure_{mt}$ defined as in equation (1) and measured in \$100,000 per worker.

Table 1.6. Main Employment Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
			A. OLS Regressions			
$Exposure_{mt}$	-0.086	-0.054	-0.085	-0.057	-0.083	-0.048
	(0.141)	(0.125)	(0.143)	(0.128)	(0.155)	(0.138)
			B. first stage Regressions			
$Exposure^{pred}_{mt}$	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
			C. Instrumental Variable Regressions			
$Exposure_{mt}$	-0.552*	-0.488*	-0.572**	-0.516*	-0.505*	-0.431
	(0.288)	(0.274)	(0.290)	(0.278)	(0.296)	(0.284)
industry effects	N	N	Y	Y	N	N
daily wage quartile effects	N	N	N	N	Y	Y
firmsize effects	N	N	N	N	Y	Y
tenure effects	N	N	N	N	Y	Y
manuf share	N	Y	N	Y	N	Y
F-stat	10.24	10.24	10.33	10.34	10.32	10.33
N	11684819	11684819	11684819	11684819	11684819	11684819

Notes: Outcome variable is an indicator for whether a formally employed worker in time t is also formally employed in time $t+5$. Each cell contains a coefficient from a different regression. All regressions include time period effects, labor market effects, age effects (in years), and a male indicator. Standard errors are clustered at the labor market by time period level. Trade flow data from UN Comtrade database and employment data from IMSS records. Regressions are run at the cell level due to computational constraints and weighted by cell size. $Exposure_{mt}$ defined in Equation (1) and $Exposure^{pred}_{mt}$ defined in Equation (3). *** 1%, ** 5%, *10% level.

Table 1.7. IV Regressions, Other Outcome Variables

	(1)	(2)	(3)	(4)	(5)	(6)
			A. dep. var.: real daily wage, t+5			
$Exposure_{mt}$	-255.716*	-171.922	-245.823*	-169.068	-306.645	-216.928
	(152.041)	(119.904)	(135.295)	(110.240)	(196.237)	(171.467)
			B. dep. var.: real daily wage, if employed, t+5			
$Exposure_{mt}$	-230.463	-132.732	-235.217	-141.038	-377.047	-268.759
	(171.988)	(124.667)	(158.605)	(115.284)	(264.043)	(227.300)
			C. dep. var.: employed in manufacturing, t+5			
$Exposure_{mt}$	-0.339*	-0.376**	-0.382*	-0.331	-0.285*	-0.311*
	(0.178)	(0.175)	(0.220)	(0.202)	(0.162)	(0.160)
			D. dep. var.: employed in manufacturing, if employed, t+5			
$Exposure_{mt}$	-0.104	-0.208	-0.041	-0.022	-0.027	-0.120
	(0.248)	(0.207)	(0.110)	(0.102)	(0.232)	(0.194)
			E. dep. var.: moved labor market, if employed, t+5			
$Exposure_{mt}$	3.301**	2.977*	3.607**	3.173*	3.278*	2.801
	(1.680)	(1.599)	(1.828)	(1.723)	(1.918)	(1.840)
			F. dep. var.: changed industry, if employed, t+5			
$Exposure_{mt}$	3.249*	2.876*	3.563*	3.086*	3.228	2.692
	(1.718)	(1.606)	(1.865)	(1.730)	(1.963)	(1.855)
industry effects	N	N	Y	Y	N	N
daily wage quartile effects	N	N	N	N	Y	Y
firmsize effects	N	N	N	N	Y	Y
tenure effects	N	N	N	N	Y	Y
manuf share	N	Y	N	Y	N	Y
N (employed, t)	11684819	11684819	11684819	11684819	11684819	11684819
N (employed, t and $t+5$)	6972206	6972206	6972206	6972206	6972206	6972206

Notes: Sample of workers in Panels A and C are workers formally employed in time t, while sample of workers in Panels B, D, E and F are workers formally employed in t and $t+5$. Each cell contains a coefficient from a different regression. All regressions include time period effects, labor market effects, age effects (in years), and a male indicator. Standard errors are clustered at the labor market by time period level. Trade flow data from UN Comtrade database and employment data from IMSS records. Regressions are run at the cell level due to computational constraints and weighted by cell size. $Exposure_{mt}$ defined in Equation (1). first stage coefficients are shown in Panel B of Table (1.6). *** 1%, ** 5%, *10% level.

Table 1.8. Main Employment Regressions, UC Davis Data

	(1)	(2)	(3)	(4)	(5)	(6)
			A. OLS Regressions			
$Exposure_{mt}$	-0.092	-0.061	-0.091	-0.063	-0.091	-0.057
	(0.146)	(0.130)	(0.148)	(0.133)	(0.162)	(0.145)
			B. first stage Regressions			
$Exposure^{pred}_{mt}$	0.011***	0.010***	0.011***	0.010***	0.011***	0.010***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
			C. Instrumental Variable Regressions			
$Exposure_{mt}$	-0.463*	-0.430*	-0.484**	-0.455*	-0.419	-0.381
	(0.244)	(0.240)	(0.245)	(0.242)	(0.259)	(0.261)
industry effects	N	N	Y	Y	N	N
daily wage quartile effects	N	N	N	N	Y	Y
firmsize effects	N	N	N	N	Y	Y
tenure effects	N	N	N	N	Y	Y
manuf share	N	Y	N	Y	N	Y
F-stat	10.02	10.24	10.13	10.35	10.12	10.34
N	11684819	11684819	11684819	11684819	11684819	11684819

Notes: Outcome variable is an indicator for whether a formally employed worker in time t is also formally employed in time $t + 5$. Each cell contains a coefficient from a different regression. All regressions include time period effects, labor market effects, age effects (in years), and a male indicator. Standard errors are clustered at the labor market by time period level. Trade flow data from UC Davis database and employment data from IMSS records. Regressions are run at the cell level due to computational constraints and weighted by cell size. $Exposure_{mt}$ defined in Equation (1) and $Exposure^{pred}_{mt}$ defined in Equation (3). *** 1%, ** 5%, *10% level.

Table 1.9. Falsification Test, Employed in $t-3$

	(1)	(2)	(3)	(4)	(5)	(6)
	A. dep. var.: employed, t-3					
$Exposure_{mt}$	-0.039	-0.040	-0.047	-0.048	-0.045	-0.047
	(0.293)	(0.308)	(0.308)	(0.321)	(0.057)	(0.060)
industry effects	N	N	Y	Y	N	N
daily wage quartile effects	N	N	N	N	Y	Y
firmsize effects	N	N	N	N	Y	Y
tenure effects	N	N	N	N	Y	Y
manuf share	N	Y	N	Y	N	Y
N	11684819	11684819	11684819	11684819	11684819	11684819

Notes: Outcome variable is an indicator for whether a formally employed worker in time t is also formally employed in time $t-3$. Each cell contains a coefficient from a different regression. All regressions include time period effects, labor market effects, age effects (in years), and a male indicator. Standard errors are clustered at the labor market by time period level. Trade flow data from UN Comtrade database and employment data from IMSS records. Regressions are run at the cell level due to computational constraints and weighted by cell size. $Exposure_{mt}$ defined in Equation (1). first stage coefficients are shown in Panel B of Table (1.6). *** 1%, ** 5%, *10% level.

Table 1.10. IV Employment Regressions, By Sub-Group

	(1)	(2)	(3)	(4)	(5)	(6)
			A. males			
$Exposure_{mt}$	-0.640**	-0.593**	-0.685**	-0.640**	-0.574**	-0.515*
	(0.291)	(0.285)	(0.301)	(0.296)	(0.292)	(0.288)
N	8201533	8201533	8201533	8201533	8201533	8201533
			B. females			
$Exposure_{mt}$	-0.358	-0.250	-0.319	-0.236	-0.369	-0.254
	(0.312)	(0.270)	(0.293)	(0.263)	(0.337)	(0.301)
N	3483286	3483286	3483286	3483286	3483286	3483286
			C. 25 to 40 years old, t			
$Exposure_{mt}$	-0.479*	-0.418	-0.507*	-0.452*	-0.427	-0.353
	(0.278)	(0.261)	(0.284)	(0.270)	(0.292)	(0.276)
N	8470695	8470695	8470695	8470695	8470695	8470695
			D. 40 to 60 years old, t			
$Exposure_{mt}$	-0.689**	-0.624**	-0.683**	-0.632**	-0.658**	-0.588**
	(0.298)	(0.292)	(0.287)	(0.284)	(0.298)	(0.299)
N	3534599	3534599	3534599	3534599	3534599	3534599
industry effects	N	N	Y	Y	N	N
daily wage quartile effects	N	N	N	N	Y	Y
firmsize effects	N	N	N	N	Y	Y
tenure effects	N	N	N	N	Y	Y
manufacturing share	N	Y	N	Y	N	Y

Notes: Outcome variable is an indicator for whether a formally employed worker in time t is also formally employed in time $t-3$. Each cell contains a coefficient from a different regression. All regressions include time period effects, labor market effects, age effects (in years), and a male indicator. Standard errors are clustered at the labor market by time period level. Trade flow data from UN Comtrade database and employment data from IMSS records. Regressions are run at the cell level due to computational constraints and weighted by cell size. $Exposure_{mt}$ defined in Equation (1). first stage regressions not shown. *** 1%, ** 5%, *10% level.

Table 1.11. IV Employment Regressions, By Sub-Group

	(1)	(2)	(3)	(4)	(5)	(6)
			A. non-manufacturing, t			
$Exposure_{mt}$	-0.253	-0.220	-0.252	-0.232	-0.231	-0.193
	(0.192)	(0.199)	(0.187)	(0.195)	(0.199)	(0.212)
N	7517134	7517134	7517134	7517134	7517134	7517134
			B. manufacturing, t			
$Exposure_{mt}$	-0.876*	-0.733	-0.903*	-0.765*	-0.796	-0.643
	(0.528)	(0.455)	(0.532)	(0.464)	(0.539)	(0.474)
N	4167685	4167685	4167685	4167685	4167685	4167685
			C. employed $<$ 3 consecutive years, t			
$Exposure_{mt}$	-0.280	-0.206	-0.384	-0.316	-0.336	-0.243
	(0.268)	(0.264)	(0.268)	(0.263)	(0.311)	(0.299)
N	4901486	4901486	4901486	4901486	4901486	4901486
			D. employed $\geq$ 3 consecutive years, t			
$Exposure_{mt}$	-0.637**	-0.576**	-0.604**	-0.552*	-0.653**	-0.590*
	(0.299)	(0.286)	(0.293)	(0.285)	(0.319)	(0.306)
N	6783333	6783333	6783333	6783333	6783333	6783333
industry effects	N	N	Y	Y	N	N
daily wage quartile effects	N	N	N	N	Y	Y
firmsize effects	N	N	N	N	Y	Y
tenure effects	N	N	N	N	Y	Y
manufacturing share	N	Y	N	Y	N	Y

Notes: Outcome variable is an indicator for whether a formally employed worker in time t is also formally employed in time $t-3$. Each cell contains a coefficient from a different regression. All regressions include time period effects, labor market effects, age effects (in years), and a male indicator. Standard errors are clustered at the labor market by time period level. Trade flow data from UN Comtrade database and employment data from IMSS records. Regressions are run at the cell level due to computational constraints and weighted by cell size. $Exposure_{mt}$ defined in Equation (1). first stage regressions not shown. *** 1%, ** 5%, *10% level.

Table 1.12. IV Employment Regressions, Border versus Non-Border

	(1)	(2)	(3)	(4)	(5)	(6)
			A. non-border, t			
$Exposure_{mt}$	-1.283	-1.164	-1.143	-1.054	-0.950	-0.802
	(1.627)	(1.465)	(1.476)	(1.365)	(1.301)	(1.194)
F-stat	0.83	0.79	0.84	0.79	0.84	0.80
N	8474305	8474305	8474305	8474305	8474305	8474305
			B. border, t			
$Exposure_{mt}$	-0.179	-0.170	-0.221*	-0.211	-0.164	-0.150
	(0.113)	(0.132)	(0.129)	(0.147)	(0.119)	(0.143)
F-stat	10.44	11.73	10.64	11.95	10.64	11.95
N	3210514	3210514	3210514	3210514	3210514	3210514
industry effects	N	N	Y	Y	N	N
daily wage quartile effects	N	N	N	N	Y	Y
firmsize effects	N	N	N	N	Y	Y
tenure effects	N	N	N	N	Y	Y
manufacturing share	N	Y	N	Y	N	Y

Notes: Outcome variable is an indicator for whether a formally employed worker in time t is also formally employed in time $t-3$. Each cell contains a coefficient from a different regression. All regressions include time period effects, labor market effects, age effects (in years), and a male indicator. Standard errors are clustered at the labor market by time period level. Trade flow data from UN Comtrade database and employment data from IMSS records. Regressions are run at the cell level due to computational constraints and weighted by cell size. $Exposure_{mt}$ defined in Equation (1). first stage regressions not shown. *** 1%, ** 5%, *10% level.

Table 1.13. Industry-level Regressions, OLS

dep. var.:	*mex_exp_US*	*world_exp_US*
	(1)	(2)
chn_exp_US	0.151***	0.052
	(0.009)	(0.054)
year fe	Y	Y
ind fe	Y	Y
N	2688	2688

Notes: Each column represents a different regression. *chn_exp_us*, *mex_exp_us* and *world_exp_us* measured in inflation-adjusted US dollar. *** 1%, ** 5%, * 10% level.

Table 1.14. Industry-level Regressions, IV

	(1)	(2)	(3)	(4)
	A. First Stage Regressions			
dep. var.:	*chn_exp_US*	*chn_exp_US*	*chn_exp_US*	*chn_exp_US*
chn_exp_pred	0.063***	0.063***		
	(0.005)	(0.005)		
chn_exp_other			1.216***	1.216***
			(0.012)	(0.012)
	B. IV Regressions			
dep. var.:	*mex_exp_US*	*world_exp_US*	*mex_exp_US*	*world_exp_US*
chn_exp_US	-0.045	-0.844***	0.188***	0.194***
	(0.035)	(0.214)	(0.009)	(0.058)
year fe	Y	Y	Y	Y
ind fe	Y	Y	Y	Y
F-stat	178.93	178.93	10996.31	10996.31
N	2688	2688	2688	2688

Notes: Each column represents a different regression. *chn_exp_us*, *mex_exp_us*, *world_exp_us*, *chn_exp_pred* and *chn_exp_other* measured in inflation-adjusted US dollar. *** 1%, ** 5%, * 10% level.

1.9 Appendix: Tables

Table 1.A1. Industry-level Regressions, Autor, Dorn, and Hanson (2013) data

	(1)	(2)	(3)	(4)	(5)	(6)
dep. var.:	*chn_exp_US*	*can_exp_US*	*cen_exp_US*	*low_exp_US*	*row_exp_US*	*US_exp_oth*
			A. Reduced Form Regressions			
chn_exp_other	1.401***	0.016	0.312***	0.048***	0.445***	-0.056***
	(0.008)	(0.014)	(0.012)	(0.003)	(0.033)	(0.008)
			B. IV Regressions			
chn_exp_US		0.011	0.223***	0.034***	0.318***	-0.040***
		(0.010)	(0.009)	(0.002)	(0.024)	(0.005)
year fe	X	X	X	X	X	X
ind fe	X	X	X	X	X	X
N	6749	6749	6749	6749	6749	6749

Notes: Each cell contains a coefficient for a different regression. Data from Autor, Dorn, and Hanson (2013). Industries with no exports for a given importer/exporter pair are assigned zero exports; results are robust to excluding these observations. chn = China, can = Canada, cen = Mexico and CAFTA countries, low = Low-income countries, row = Rest of world, and oth = eight other advanced economies. See Appendix of Autor, Dorn, and Hanson (2013) for country classifications. *** 1%, ** 5%, *10% level.

Chapter 2

Enlisting Employees in Improving Payroll-Tax Compliance: Evidence from Mexico[1]

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2.1 Introduction

A growing body of research suggests that lack of state capacity — in particular, difficulty in raising taxes to fund public goods — is an important constraint on the growth of developing countries (Burgess and Stern, 1993; Besley and Persson, 2013). Developing countries generally have low ratios of tax revenues to GDP and large informal sectors. Mexico, the focus of our study, is no exception: it has the lowest tax revenue share of GDP in the OECD, between 15 and 20 percent during the period we study, and the informal sector has been estimated to make up 40 percent or more of total output (OECD, 2011b; IMF, 2010; Schneider and Enste, 2000). Given weak enforcement institutions and widespread evasion, the task of improving the fiscal capacity in developing countries is a difficult one, and there is acute interest among researchers and policy-makers in potential remedies.

A key element of the general weakness of fiscal capacity is non-compliance of firms with tax regulations. A large literature has focused on one dimension of non-compliance: the failure of firms to register with tax authorities. Researchers have argued that this form of non-compliance generates a variety of market distortions, including limits on informal firms' employment growth and access to formal credit markets (Gordon and Li, 2009; La Porta and Shleifer, 2008; Levy, 2008; Busso, Fazio, and Levy, 2012).[4] Governments in a number of countries have implemented programs to reduce registration costs and induce firms to formalize, and there is an active empirical literature on the effects of formalization.[5]

In this paper, we focus on a different dimension of non-compliance by firms, less appreciated but arguably no less important: the under-reporting of wages by registered firms to evade payroll taxes. This form of non-compliance has received surprisingly little empirical attention. One reason may be that it has been shown not to be a significant issue in developed countries. For instance,

[4] Other notable theoretical work on firms' decisions about whether to formalize includes Rauch (1991), De Paula and Scheinkman (2011), and Galiani and Weinschelbaum (forthcoming).

[5] See e.g. Bruhn (2011), Kaplan, Piedra, and Seira (2011), Fajnzylber, Maloney, and Montes-Rojas (2011), Monteiro and Assunção (2012), McKenzie and Sakho (2010), de Mel, McKenzie, and Woodruff (2012).

using audits of individual tax returns in Denmark, Kleven, Knudsen, Kreiner, Pedersen, and Saez (2011) find little evasion when incomes are reported by employers or other third parties.[6] The view that third-party reporting is effective in ensuring compliance is widespread among practitioners and government agencies in developed countries (see e.g. Plumley (2004) and OECD (2006)). Another reason for the limited attention to wage under-reporting may be that it is difficult to study. It has been rare for researchers to have micro-level information on firms' wage reports, and rarer still to have access to an alternative source of wage information at a sufficiently disaggregated level to permit inferences about the extent of non-compliance (Slemrod and Yitzhaki, 2002; Slemrod, 2007). As a consequence, it has not been clear to what extent the accuracy of third-party reporting carries over to developing-country settings.

In this paper, we compare two sources of wage information from Mexico — firms' reports of individuals' wages to the Mexican social security agency and individuals' responses to a household labor-force survey (the *Encuesta Nacional de Empleo Urbano* (ENEU)) — to draw inferences about the extent of wage under-reporting and how it has responded to changes in the social security system. In particular, we focus on a major pension reform that introduced a system of personal retirement accounts, passed by the Mexican Congress on December 21, 1995 and implemented on July 1, 1997. As discussed in more detail below, prior to the reform the social security benefits of most workers were largely insensitive to the wages reported by firms on their behalf. The reform tied individual pensions more closely to firms' wage reports and made it easier for employees to observe those reports. Workers already in the traditional system prior to July 1, 1997 retained the right to choose, at the time of retirement, the pension that they would have received under the pre-reform regime. Because older workers had little time to accumulate sufficient balances in their personal

[6] In another example, Saez (2010) finds significant bunching around the first kink point of the Earned Income Tax Credit, suggesting misreporting, only among the self-employed. The Internal Revenue Service has documented that compliance is higher for income groups with greater third-party reporting in the U.S. (Internal Revenue Service, 1996, 2006).

accounts, their expected pension was higher under the old regime. Younger workers had a greater expectation of being better off under the new regime and hence had stronger incentives to ensure accurate reporting. We use this differential impact by age as the basis for a difference-in-differences estimation strategy.

To guide our empirical investigation, we have developed a simple partial-equilibrium model of endogenous compliance by heterogeneous firms, summarized briefly in Section 2.3 below and presented in full in the Appendix. Firms are assumed to be monopolistically competitive and to differ in productivity (in a setting similar to Melitz (2003) but without international trade). The cost of evasion is assumed to be increasing both in the unreported part of the wage per worker and in firm output, for reasons that may include the greater difficulty of maintaining collusion in larger firms, as argued in a recent paper by Kleven, Kreiner, and Saez (2009), or simply the greater visibility of larger firms to auditors. One consequence of this assumption is that evasion is declining in firm size in cross-section. The model further predicts that a social-security reform like the one that occurred in Mexico, which raises the sensitivity of social-security benefits to the reported wage, will lead to a decline in evasion for affected workers.

A key limitation of our study is that, although evasion decisions are taken at the level of individual firms, the household labor-force survey does not contain firm identifiers and we are not able to construct measures of evasion at the firm level. Instead, we construct measures of evasion at the level of cells defined by different combinations of metropolitan areas, sectors, firm-size categories and age groups, depending on the specification. Within each cell, we compare the distributions of wage measures that in the absence of evasion should coincide — the post-tax wage from the administrative records of the social security agency and the take-home wage reported on the household survey — and measure evasion either as the difference in log median or log mean wage or as the excess mass in the administrative-records distribution at the left tail. We focus on male workers, for reasons discussed below.

Using our evasion measures, we first document a set of cross-sectional relationships prior to the reform. Evasion is substantial, as one might expect given the weak relationship between wage reports and social security benefits for most workers. We also show that evasion is declining in firm size, although evasion remains non-trivially positive even among quite large firms.

We then turn to the difference-in-differences strategy using the pension reform. We focus on outcomes by age group at the level of local labor markets. We show that evasion rates by age group followed similar trends prior to the reform, but that the oldest age group, ages 55-65, saw a significant relative increase following the passage of the reform, a pattern that is robust across our measures of evasion.

Overall, the results support the view that tying benefits to reported wages and making it easier for workers to observe firms' reports can be an effective way to improve payroll-tax compliance. It seems likely that the change in incentives was more important than the change in information: if incentives had not changed, and benefits had remained largely insensitive to wage reports, it is not clear why information alone would have led to changes in compliance. But the "experiment" we consider combined both elements, and the effects we estimate should be interpreted as combined effects of incentives and information.[7]

This paper is related to a number of different literatures. There is a small literature on misreporting of social security contributions, including Nyland, Smyth, and Zhu (2006), Tonin (2011), and Bérgolo and Cruces (2012), and Mao, Zhang, and Zhao (2013). This paper appears to be the first to analyze how tying benefits more closely to reported wages can contribute to improved compliance.[8] Relative to this literature, the paper is also distinctive in its explicit consideration of firm

[7]Our argument should not be interpreted as advocating a system of personal accounts *per se*; one could imagine a change in pension benefits under the traditional pay-as-you-go system that would have had similar effects.

[8]Bailey and Turner (2001) suggest verbally that tying pension benefits to contributions would have the effect of reducing evasion.

heterogeneity and in using the comparison of firms' administrative reports to workers' responses to a household survey to measure evasion.[9]

This paper is related to recent work on incentivizing decentralized agents to improve tax enforcement. Kopczuk and Slemrod (2006), Keen and Lockwood (2010), and Pomeranz (2013) argue that value-added taxes (VATs) have attractive enforcement properties in part because they give each party in a supply-chain transaction greater incentive to ensure that the other reports accurately. A recent paper by Naritomi (2013) analyzes a Brazilian program to give consumers incentives to ask for receipts from retail establishments and finds positive effects on compliance. In an ongoing project, Khan, Khwaja, and Olken (2013) have randomized incentives to tax inspectors in Pakistan. More broadly, this paper is in the spirit of a growing empirical literature in development economics examining how corruption and other forms of illegal behavior respond to economic incentives, recently surveyed by Olken and Pande (2012). It is part of a small but growing literature using administrative records from developing countries to document various aspects of taxpayer behavior (Pomeranz, 2013; Kleven and Waseem, 2013; Best, Brockmeyer, Kleven, Spinnewijn, and Waseem, 2013). It is also related to an active recent literature on the role of firms in tax systems (Kopczuk and Slemrod, 2006; Gordon and Li, 2009; Dharmapala, Slemrod, and Wilson, 2011) and to a voluminous literature on tax evasion and avoidance, reviewed by Andreoni, Erard, and Feinstein (1998), Slemrod and Yitzhaki (2002), and Saez, Slemrod, and Giertz (2012).

2.2 Institutions: The Mexican Social Security System

Because our empirical strategy relies crucially on incentives in the Mexican social insurance system, this section describes the system and the pension reform in some detail. Because of data constraints,

[9] Papers using the general strategy of comparing information from more than one data source to infer illicit behavior (in other contexts) include Pissarides and Weber (1989), Fisman and Wei (2004), Olken (2006), Gorodnichenko, Martinez-Vazquez, and Peter (2009), Marion and Muehlegger (2008), Hurst, Li, and Pugsley (2011), Braguinsky, Mityakov, and Liscovich (2010), and Niehaus and Sukhtankar (2013).

discussed in more detail below, we focus on the years 1988-2003. In describing the characteristics of the social security system and in the empirical work below, we will focus primarily on male workers.[10]

The *Instituto Mexicano del Seguro Social* (IMSS), the Mexican social security agency, is the primary source of social insurance for private-sector workers in Mexico. It administers pension benefits, disability insurance, work injury compensation, childcare centers, and a large number of clinics and hospitals, which are the primary source of health care for the formal, private-sector Mexican workforce.[11] Beginning with its creation in 1944, IMSS operated as a pay-as-you-go (PAYGO) scheme financed by payroll taxes. By the late 1980s, however, rising health care costs and an increase in the number of pensioners relative to the working-age population led to projected shortfalls in the IMSS financial accounts. On Dec. 21, 1995, because of concerns about the financial viability of the system, the congress enacted a comprehensive pension reform, to take effect on July 1, 1997.[12] This reform replaced the entire PAYGO pension system with a system of personal retirement accounts (PRA). More extensive discussions of the pension reform are provided in Grandolini and Cerda (1998), Sales-Sarrapy, Solis-Soberon, and Villagomez-Amezcua (1996), and Aguila (2011).

[10]The incentives and empirical patterns for women are complicated by the facts that women's labor force participation changed relatively rapidly over the study period and that many women receive IMSS benefits through their spouses, which provides an incentive to remain in the informal sector. In addition, because of relatively low labor force participation by older women, sample sizes in the ENEU household survey (described below) are often inadequate, especially when analyzing the data separately by metropolitan area (or metropolitan area, firm size and sector), as explained below. We present the main tables and figures for women in the Appendix. To preview the results, the cross-sectional patterns are robust for women, but the difference-in-differences results are not, possibly for the reasons just discussed.

[11]Public-sector workers and workers for PEMEX, the state-owned oil company, are covered by separate systems. In 2003, the government created an alternative system called *Seguro Popular*, which provides basic health coverage for all individuals and is not tied to formal employment. In this paper, we focus on the IMSS system and sectors with minimal government employment.

[12]This change followed an unsuccessful partial reform in 1992, described in the Appendix.

2.2.1 Contribution Rates

IMSS requires contributions from both employers and employees based on reported wages; these are supplemented by government contributions. Figure 2.1 presents the contribution schedule for employers as a function of the reported real daily wages of each employee, for selected years. The schedule reflects a complicated set of formulas determining contributions to the various components of the IMSS system, principally health care, pensions, and child care.[13] The figure illustrates that the most significant changes in the schedule are for the highest-wage workers, earning above 500 pesos per day, due to changes in the maximum taxable income over the period, from 10 times to 25 times the minimum wage in Mexico City.[14] The topcodes apply to no more than 5 percent of wage-earners in any year and will play little role in our analysis. The total employer contribution varied between 18 percent and 22 percent of the wage over the range in which almost all workers fall. There was an increase in the employer contribution from 1990 to 1993, and then the reform in 1997 introduced a kink in the schedule, which raised contributions disproportionately on the lowest-wage workers. Figures 2.2 displays worker contributions, which vary between 2 percent and 5 percent over the relevant range and declined with the 1997 reform. Overall, while there were changes in the contribution schedules, these were relatively modest over the relevant wage range.[15] Looking ahead to the empirical strategy, we also note that the changes in contributions were the same for all age groups and their effects will be differenced out in our difference-in-differences procedure.

[13]Full details are presented in Appendix Tables 2.A1 and 2.A2.

[14]There are three minimum wage zones in Mexico, corresponding to higher-, medium- and lower-wage municipalities, respectively. The minimum wage in Mexico City is typically used for indexing purposes, and where we refer to the minimum wage (without specifying zone) we are referring to the minimum wage in Mexico City.

[15]Although the kink introduced in 1997 appears promising for econometric analysis, we do not find evidence of significant bunching at the kink point. While the changes in top-codes also appear promising, we lack sample size, particularly in the household data, to exploit them, at least using the basic strategy of the current paper.

2.2.2 Non-pension Benefits

Any worker on whose behalf contributions are made to the system is entitled to free health care at IMSS hospitals and clinics, for himself or herself, as well as for members of his or her immediate family, independent of the reported wage. In addition, working mothers and widowed or divorced working fathers are entitled to free child care during workdays for children under four years old.[16] It is difficult to estimate workers' valuations of these non-pension benefits. Conveniently for our empirical strategy, however, the health care and child care benefits did not change with the 1997 pension reform. Under the assumption that employees' valuations of the constant set of benefits did not change differentially by age group over the study period, the valuations will be differenced out in our difference-in-differences procedure.[17]

2.2.3 Pension Benefits

2.2.3.1 Pre-reform (pay-as-you-go) system Under the pre-reform regime, workers became vested in the system after 10 years of contributions, and were then entitled to receive at least the minimum pension. Pensions were calculated on the basis of the final average wage, defined as the average nominal wage in the five years preceding retirement. Panel A of Figure 2.3 illustrates the expected daily pension as a function of the final average wage for workers with 10, 20 and 30 years of contributions in selected years. The schedules combine a minimum pension guarantee with a benefit proportional to an individual's wage. At first glance, the pension values illustrated in Panel A appear to be sensitive to the reported final average wage, but it is important to note that in the years leading up to the reform inflation had severely eroded the real value of wages and pensions, such that a large majority of workers had final average wages in the region in which the

[16] IMSS also provides an individual savings account for housing expenditures, which in some cases can be used to contribute to an individual pension. See the Appendix for details.

[17] There has been a secular decline in the number of IMSS hospital and clinic beds per covered individual, but there was no trend break in 1997 (IMSS, 2011, ch. 11). Below we will find no pre-trend in under-reporting prior to 1997.

minimum was binding. Inflation exceeded 50 percent in every year in the volatile 1982-1988 period, and exceeded 100 percent in 1987 and 1988; it was above 25 percent in a number of subsequent years (1990-1991 and 1995-1996). (See Appendix Table 2.A4.) In response to public pressure, the Mexican Congress in 1989 increased the minimum pension to 70 percent of the minimum wage and indexed it to the minimum wage going forward, without raising the value of pensions greater than the minimum.[18] The congress subsequently raised the value of the minimum pension relative to the minimum wage, until it reached 100 percent of the minimum wage in Mexico City in 1995.

As a consequence of the erosion of the real value of pensions above the minimum and the legislative interventions to raise the minimum, the fraction of workers who expected to receive the minimum pension remained high throughout the pre-reform period. Panel B of Figure 2.3 plots the real value of the pension for male workers with 10, 20 or 30 years of contributions against the final average wage percentile of 60-65 year old men in the IMSS data, for selected years.[19] In 1990, approximately 80 percent of male retirees with 10 years of contributions received the minimum pension. The corresponding numbers for male workers with 20 or 30 years of contributions were 70 percent and 60 percent respectively. In 1997, just prior to the implementation of the pension reform, nearly all workers with 10 years of contributions, roughly 50 percent of those with 20 years, and 40 percent of those with 30 years could expect to receive the minimum pension.[20] Unfortunately, the data to which we have access do not contain total years of contributions by each individual worker, and hence we are not able to calculate the precise number of workers receiving the minimum pension. But analysts with access to this information report that approximately 80

[18]In 1991, benefits were indexed to the minimum wage, which slowed the erosion of the values of pensions above the minimum. That is, if a worker's final average wage was twice the minimum wage in 1991, the pension payment in 1992 was calculated on the basis of twice the minimum wage. The real minimum wage declined steadily over the period (see Appendix Table 2.A4) so the slowing of the erosion of pensions as a result of this change was modest.

[19]To calculate the final average wage percentile, we calculate the nominal wage at each percentile of the IMSS wage distribution for 60-65 year old men in each of preceding five years, then take the average for each percentile.

[20]In addition, there was a penalty for retirement before age 65 of 5 percent per year (i.e. a worker who retired at age 60 would have his or her pension reduced by 25 percent), but this penalty was not allowed to reduce the pension below the minimum. This reduced the disincentive to retire early to workers with pensions near the minimum (Aguila, 2011).

percent of retirees were receiving the minimum pension prior to the reform (Grandolini and Cerda, 1998).[21]

Strictly speaking, pension values were insensitive to final wages only for infra-marginal workers whose *true* final wage corresponded to the minimum pension. If wages were under-reported to IMSS, as we argue below, then the graphs in Panel B of Figure 2.3 likely overstate the fraction of workers whose pensions were insensitive to under-reporting. To address this, in Panel C of Figure 2.3 we plot similar graphs using final average wage percentiles calculated from the ENEU household data (described in Section 2.4 below), which should not be subject to under-reporting. We see that somewhat smaller fractions of workers with 10, 20 and 30 years of contributions would have received the minimum pension. But the key point is that the graph for 1997 resembles quite closely the corresponding graph in Panel B: essentially all workers with 10 years of contributions would have received the minimum pension, as well as more than 40 percent of workers with 20 years and more than 20 percent of workers with 30 years.

2.2.3.2 Post-reform (personal retirement accounts) system Under the personal retirement account (PRA) system, employees, employers and the government are required to make contributions to workers' personal retirement accounts in each period.[22] Each worker is required to choose an investment institution, known as an *Administrador de Fondos de Ahorro para el Retiro* (AFORE) [Retirement Savings Fund Administrator], to manage his or her account.[23] The reform

[21]In addition, because pensions were calculated only on the basis of the last five years of employment, any worker who was certain that he or she would work for more than five years in covered employment could also be certain that the current reported wage would not affect the pension benefit. In unreported results, we have investigated whether we see an increase in reported wages five years before retirement, as one might expect if workers were being sophisticated in adjusting strategically to the five-year rule, but we do not find a significant change.

[22]Over the 1997-2003 period, employers were required to contribute 5.15 percent of each employee's wage, and employees 1.125 percent; the government contributed 0.225 percent, as well as a "social quota" equal to 5.5 percent of the current minimum wage in Mexico City. See Appendix Tables 2.A1 and 2.A2 for details. Employees also have the option to contribute to a voluntary retirement savings account. See Lara-Ibarra (2011) for an analysis of the effects of a change in the tax rate on these contributions.

[23]The AFORE management fees are in many cases substantial, and it is not clear that workers choose AFOREs optimally. Duarte and Hastings (2010) investigate the role of behavioral issues in employees' choices of AFOREs.

also specified a minimum pension equal to the minimum wage on July 1, 1997, with further increases in the minimum pension indexed to the Consumer Price Index. Eligibility for the minimum pension was raised from 10 years of contributions to 25 years of contributions.

The establishment of the new pension regime created two categories of workers: "transition" workers who first registered with IMSS before July 1, 1997, and new workers who first registered after July 1, 1997. At retirement, transition workers are given a choice between receiving pension benefits under the PAYGO scheme or the PRA scheme. The PAYGO pension is calculated as if workers' post-reform contributions were under the old regime. If a transition worker opts for the PAYGO pension, IMSS appropriates the balance of his or her personal retirement account. The only option for new workers is the PRA.[24]

To illustrate the impact of the reform on pension wealth, we conduct a simulation of pension wealth under the two regimes, based on a similar simulation by Aguila (2011). In carrying out the simulation, we choose a relatively optimistic annual return on the personal accounts: 8.59 percent, the average return from 1998-2002, as in the more optimistic of the two scenarios considered by Aguila (2011). We also assume that participants expected the real value of the minimum wage to decline, as it had done for more than a decade (see Appendix Table 2.A4). Assumptions of lower interest rates and less rapid declines in the real minimum wage would be less favorable to the PRAs. Details of the simulation are in the Appendix.

One way to see the differences in incentives by age in the system is to compare pension wealth for workers of different ages in 1997. Table 2.1 displays the real present value of pension wealth by wage level for male workers of different ages in 1997, all of whom began working at age 25

[24] Under the personal-account system, individuals have three options upon retirement. One is to receive programmed withdrawals from the individual's AFORE, where the withdrawal amount is calculated based on the account balance as well as the age and life expectancy of the individual and dependents. (A worker who receives the minimum pension must choose this option.) A second option is to purchase an annuity from a private insurance company that guarantees a fixed monthly pension. A third option, available to workers with a personal-account balance exceeding 130 percent of the cost of an annuity providing a monthly payment equal to the minimum pension, is to take a lump-sum payment upon retirement.

and expect to continue working until age 60, assuming real wages are constant over their lifetimes. Numbers in italics indicate that the PRA pension is more valuable than the PAYGO pension. The message of the simulation is clear: the PRA pension is expected to be more valuable only for younger workers who expect to contribute to the personal account for 25 or more years, and among these workers the PRA pension is relatively more attractive for higher-wage workers.[25]

We do not attempt to infer from the simulation exact crossing points at which the PRA becomes preferable to the PAYGO pension; any such calculation would be sensitive to assumptions about the path of interest and inflation rates, and it is not clear that workers are sophisticated in calculating the precise values of pensions under the different systems. The basic message of the simulation, which we believe was understood by participants at the time of the reform, is that for most workers, conditional on qualifying for the minimum pension under the old regime, the personal accounts could be expected to be relatively more attractive only for workers with a significant number of years of contributions after 1997.

Another aspect of the pension reform, which may have been important in practice, is that the law requires AFOREs to send an account statement to each holder of a personal retirement account every four months. A redacted example of such an account statement appears as Figure 2.4. The account statement reports previous balances *(saldo anterior)*, new contributions *(aportaciones)*, withdrawals *(retiros)*, interest earned *(rendimientos)*, AFORE commissions charged *(comisiones)*, and final balances *(saldo final)* for the pension account as well as for the voluntary savings account (see footnote 22) and the housing savings account (see footnote 16). The bottom section reports 3-year returns and commissions for each AFORE, as well as the average 5-year net return (at

[25] Another way to see the effect of the reform is to consider the values of the pensions for different numbers of years of expected contributions, for a worker who entered then system on June 30, 1997, as presented in Appendix Table 2.A5. Note that workers with fewer than 10 years of contributions are better off under the new regime, since they receive no pension under the old regime but a small pension under the new regime. But conditional on a worker having at least 10 years of contributions, we again see that the attractiveness of the PRA pension is increasing in the number of years of contributions and the wage. The median wage for male workers is just above 100 pesos/day, and for a worker at this level the PRA only becomes more attractive if he expects to contribute for more than 25 years.

left). It appears that these account statements made it significantly easier for workers to discover how much employers were contributing on their behalf. This mechanism would not be expected to reduce evasion if employers and employees were colluding in under-reporting wages, but it may have reduced evasion in cases in which workers were unaware that their employers were under-reporting their wages.

Neither before nor after the reform was there a reward to employees for revealing evasion by their employers, beyond ensuring accurate reporting of their own wages. The social security law provides for fines if establishments are caught evading taxes. The fines ranged from 70-100 percent of the amount of evasion over the 1995-2001 period and have ranged from 40-100 percent, with most exactly at 40 percent, since 2001.[26] Although we argue that evasion has been widespread, at least one aspect of IMSS reporting requirements does appear to have been strictly enforced. By law, firms in Mexico are required to pay the relevant minimum wage and a holiday bonus called an *aguinaldo*, worth two weeks of salary — approximately 4.5 percent of annual earnings. In order to avoid fines, establishments are required to report wages of at least the corresponding minimum wage plus 4.5 percent throughout the year.[27]

2.2.4 Other Dimensions of Tax System in Mexico

One reason that firms in developed countries engage in relatively little under-reporting of wages may be that it does little to reduce their overall tax burden. If corporate or personal income taxes are as high as payroll taxes and difficult to evade, then lower payroll taxes due to under-reporting will be offset by higher taxes on corporate or personal income. In Mexico, the corporate income tax are generally higher than the payroll tax on paper: it went from 39 to 34 percent over the 1988-2003

[26] In addition, the law requires employers with 300 or more employees are required to submit an audit by a certified public accountant to IMSS (since 1993) as well as to the Mexican tax authority (since 1991).

[27] Prior to 1991, there are a scattered few reports of wages below this level; beginning in 1991, IMSS stepped up enforcement of this rule and such wages have no longer been observed.

period.[28] But corporate tax evasion and avoidance are rife in Mexico. For instance, the OECD in 1992 found that, in part due to various loopholes, 70 percent of corporate tax declarations reported no taxable income (OECD, 1992). By all accounts, tax evasion remains high (OECD, 2011a). In addition, the social security agency and the Mexican tax authority first signed an agreement to share data in June 2002; thus for almost all of the period under study, there was no chance that information reported to the social security agency would affect the corporate tax burden. It appears, in other words, that evaded payroll taxes were not offset by increases in other taxes.

Also, it does not appear that individual income taxes provided a strong disincentive to most workers to have their wages reported accurately. Mexico provides extensive tax credits for low-wage workers, originally instituted to offset the regressive effects of VATs, with the consequence that many workers legally pay no income tax, or even receive funds from the tax authority (i.e. face a negative income tax.) In 1997, for instance, individuals making less than 3.2 times the minimum wage in Mexico City faced a zero or negative tax rate (OECD, 1999, p. 80).

2.3 Conceptual Framework

There is a growing literature on the role of firms in tax systems, but to our knowledge there exists no off-the-shelf model to guide the analysis both of cross-sectional differences in firm behavior and of differential responses across firms to tax changes.[29] To organize our empirical analysis, we have developed a simple partial-equilibrium model of the compliance decisions of heterogeneous firms, in which employees and firms collude in under-reporting (as in Yaniv (1992)) and firms are monopolistically competitive and differ in productivity (as in Melitz (2003)). To save space in the

[28] Source: OECD Tax Database, www.oecd.org/ctp/taxdatabase.

[29] Kleven, Kreiner, and Saez (2009) consider a particular mechanism that generates greater compliance among larger firms — the increasing difficulty of maintaining collusion as the number of employees increases — but do not focus on differential responses to tax or benefit changes. Besley and Persson (2013, pp. 103-105) note that if compliance costs depend on firm size, then firm heterogeneity will matter for compliance, without taking a position on the source of the firm heterogeneity or on the implication for responses to tax changes. Dharmapala, Slemrod, and Wilson (2011) consider the optimal taxation of firms in a setting with firm heterogeneity and the implications for firm size distributions, but do not focus on wage under-reporting.

main text, we have put the full model in the Appendix; here we briefly summarize the main ideas.

If we let w_r be the pre-tax wage reported by a firm to the government, w_u be the unreported wage (paid "under the table"), and τ be the tax rate (the sum of firm and worker contributions), then the the net take-home wage received by workers is $w_{net} = w_u + (1-\tau)w_r$. Rearranging,

$$w_u = w_{net} - (1-\tau)w_r \tag{1}$$

In the empirics, w_r will correspond to the wage reported by the firm in the administrative records of the social security agency and w_{net} to the take-home pay reported by workers in the ENEU household survey. As mentioned above and discussed in more detail below, we do not observe w_{net} at the firm level, but we will be able to construct measures of the unreported wage at a more aggregated level.

We assume that the cost of evasion is given by $xc(w_u)$, where $c'(w_u) > 0$, $c''(w_u) > 0$ and x is the output of the firm. One possible justification for this assumption is simply that auditors are more likely to audit larger firms because their operations are more visible, as suggested by Besley and Persson (2013, p. 66) — a conjecture that appears anecdotally to be relevant in Mexico. Another is the argument of Kleven, Kreiner, and Saez (2009) that collusion in under-reporting is more difficult to sustain in larger firms. Whatever the underlying mechanism, the assumptions on the cost-of-evasion function give us our first key theoretical implication: in equilibrium, more productive firms, which are larger, choose to evade less.

In our static setting, we model the per-period value of the future pension benefit as bw_r, where we call b the "benefit rate." The total effective wage, inclusive of pension benefits, which we denote by w_e, is then:

$$w_e = w_{net} + bw_r = w_u + (1-(\tau-b))w_r \tag{2}$$

We assume that the labor market is competitive and that workers' labor supply responds to the effective wage, w_e.[30] It can be shown that an increase in the benefit rate, b, will lead firms to rely more heavily on the reported wage, w_r, in the compensation package to achieve a given market-clearing effective wage. This is our second key theoretical implication: an increase in the pension benefit rate will lead to a decrease in the unreported wage, w_u, within each firm. The model considers homogeneous workers, but could be easily extended to consider more than one type of worker, which differ in the benefit rate they face. An additional implication of the model is that a decrease in the tax rate, τ, has an analogous effect to an increase in the benefit rate on compliance; we return to this below.

An important issue in this context is the incidence of the change in the pension benefit rate on wages. Theoretically, it is possible to show that, for a finite labor-supply elasticity, the effective wage, w_e, is increasing in the benefit rate, b. The government ends up paying a larger share of the effective wage and some of this increased contribution redounds to workers. But in general it is not possible to sign the effects of the reform on the observable wage measures, the firm-specific reported wage, w_r, or the firm-specific take-home wage, w_{net}, for reasons discussed in the Appendix. It is worth emphasizing, however, that in the model the response of w_u to the policy change does not depend on the incidence of the policy change on w_e, w_r or w_{net}. In this sense, the model suggests that it is reasonable to examine the effect of the policy change on evasion separately from the question of incidence, which is how we proceed in the empirical analysis.

2.4 Data

The establishments' wage reports are drawn from IMSS administrative records. All private Mexican employers are in principle legally obligated to report wages for their employees, and pay social-

[30]We assume that workers observe both w_{net} and w_r, and hence w_u and w_e. In this sense, workers "collude" in under-reporting in that they are aware of it and do not report it.

security taxes on the basis of the reports. The IMSS dataset contains the full set of wage reports for employees in registered, private-sector establishments over the period 1985-2005.[31] The dataset contains a limited set of variables: age, sex, daily wage (including benefits), state and year of the individual's first registration with IMSS, an employer-specific identifier, and industry and location of the employer. Wages are reported in spells (with a begin and end date for each wage level) and in theory we could construct a day-by-day wage history for each individual. To keep the dataset manageable, we extract wages for a single day, June 30, in each year. Prior to 1997, records for temporary workers were not collected in digital form. To ensure comparability before and after 1997, we focus on workers identified in the IMSS data as permanent, defined as having a written contract of indefinite duration.

We select ages 16-65. To maintain consistency across years, we impose the lowest real value of the IMSS topcode for wage reporting (which occurred in 1991) in all years. We drop establishments with a single insured worker, since these are likely to be self-employed workers. In the interests of comparability with the ENEU household data, we include only the metropolitan areas included in the ENEU samples (described below). We also focus on sectors for which we are confident that IMSS is the only available formal-sector social insurance program: manufacturing, construction, and retail/hotel/restaurants. Other sectors contain a substantial share of public employees, who are covered by a separate system.[32] We focus primarily on men, for the reasons discussed in Section 2.2 above. (Results for women are reported in the Appendix.) We refer to the sample selected following these criteria as our IMSS baseline sample. Further details on sample selection and data processing in the Appendix.

The household data we use are from the *Encuesta Nacional de Empleo Urbano* (ENEU) [Na-

[31] The data have been used in several previous papers, including Castellanos, Garcia-Verdu, and Kaplan (2004), and Frías, Kaplan, and Verhoogen (2009b).

[32] We focus on manufacturing, construction, and retail/hotel/restaurants in part so that we can be confident that respondents to the household survey are not mistaking coverage under the public-sector system for IMSS coverage.

tional Urban Employment Survey], a household survey modeled on the Current Population Survey (CPS) in the United States, collected by the *Instituto Nacional de Estadísticas y Geografía* (INEGI), the Mexican statistical agency. The original ENEU sample, beginning in 1987, focused on the 16 largest Mexican metropolitan areas; although the coverage expanded over time, to maximize the number of pre-reform years we focus on the original 16 areas. As in the IMSS data, we include male workers ages 16-65, focus on the second quarter of each year, exclude self-employed workers, impose the 1991 IMSS topcode in all years, and include only manufacturing, construction, and retail/hotels/restaurants. All calculations below use the sampling weights provided by INEGI.

A very useful feature of the ENEU for our purposes is that it asks respondents whether they receive IMSS coverage as an employment benefit. Beginning in the third quarter of 1994, the ENEU also asked respondents whether they had a written contract of indefinite duration, the legal definition of a permanent employee used by IMSS. Hourly wages are calculated as monthly wages divided by 4.3 times hours worked in the previous week, and daily wages as 8 times hourly wages. The ENEU wage measures are based on respondents' reports of take-home pay, after social security taxes have been paid. They also exclude bonuses paid less frequently than monthly, and hence exclude the yearly *aguinaldo* bonus. The differences between the IMSS and the ENEU wage measures are discussed further in the Appendix.

Although the ENEU survey does not contain a firm identifier, it does ask respondents about the size of the firm at which he or she works. We use this information to generate a firm-size indicator taking on values 1-10, 11-50, 51-100, 101-250, or 250+ employees.[33] In addition, we drop workers with reported daily wages below 30 pesos (in 2002 constant pesos, approximately US$3, about 50 percent of the lowest legal minimum wage.) In principle, both the IMSS and the ENEU data are available over the 1987-2005 period. But there appear to be a number of data inconsistencies in the

[33]The survey allows for eight responses, 1, 2-5, 6-10, 11-15, 16-50, 51-100, 101-250 and 250+. To ensure that sample sizes within cells are sufficiently large, we create the five categories listed above.

ENEU in 1987, the first year of the survey. In addition, the ENEU sampling scheme was redesigned in the third quarter of 2003.

Our goal in the preparation of the datasets is to construct samples in the IMSS and ENEU data that are as similar as possible. Table 2.2 presents summary statistics for the IMSS baseline sample and various ENEU samples for 1990 and 2000, for a set of variables that are common between the sources: daily (post-tax) wage, age, and share in large establishments (with more than 100 employees). Column 2 contains the "full" ENEU sample, containing all non-self-employed men satisfying the age and sector criteria. Comparing columns 3 and 4, we see that ENEU workers with IMSS coverage tend to be higher-wage and more likely to work in large establishments than workers without IMSS coverage. Column 5 contains the sample that in principle should be the best match for the IMSS baseline sample: ENEU workers who report receiving IMSS coverage and having a written contract of indefinite duration — that is, who satisfy the definition of "permanent" used by IMSS. The average wage for this ENEU sample is greater than for the IMSS baseline sample, consistent with our argument below that there is under-reporting of wages in the IMSS data. Because the contract-type variable is available only beginning in 1994, however, we have prohibitively few years of pre-reform data for this sample. Instead, we will focus hereafter on the Column 6 sample, ENEU workers who report receiving IMSS coverage and working full-time (i.e. at least 35 hours in the previous week), which can be defined consistently over the entire period. We refer to the Column 6 sample as our ENEU baseline sample.

The ENEU baseline sample is not an ideal comparison group, for several reasons. Some temporary workers may work full-time, and some permanent workers may work part-time. Comparing Columns 5 and 6 for the year 2000, we see that average wages are significantly lower in the Column 6 sample; this is attributable to the facts that temporary full-time workers earn relatively low wages and that permanent part-time workers earn relatively high wages on average. It may also be that firms interpret "permanent" to mean something different from the legal definition (i.e written contract of indefinite duration) when reporting wages. In addition, patterns of non-response may differ between the IMSS and ENEU samples. It is well known, for instance, that richer households tend to be less likely to respond to income questions in household surveys (Groves and Couper, 1998). The weighted employment totals from the ENEU data in Columns 5 and 6 are below the IMSS totals in Column 1; this may in part reflect such non-response. These potential discrepancies recommend caution in interpreting cross-sectional differences between the IMSS and ENEU baseline samples. It is worth emphasizing, however, that our difference-in-difference strategy will focus on changes over time in the discrepancies between the samples, and any time-invariant sources of discrepancy will be differenced out.

As a check on the comparability of the baseline IMSS and ENEU samples (Columns 1 and 6 of Table 2.2), Table 2.3 compares the distributions in each sample across two dimensions that will be important in our analysis, age and firm size. In order to ensure that we have sufficient sample size in the ENEU to calculate the evasion measures below, we group individuals into five age categories (ages 16-25, 26-35, 36-45, 46-55, 56-65). Comparing the rightmost columns for the two panels, which indicate the share of employment in each firm size category as a share of total employment, it appears that firm sizes in the ENEU are skewed slightly away from the smallest and toward the largest size category (although there is non-monotonicity at intermediate sizes.) This may be because respondents in the household survey do not distinguish between employees directly hired by their employer and sub-contracted employees, or simply that respondents systematically overestimate employment. It may also be that firms under-report employment to IMSS, although the patterns of employment differences in Table 2.2 and Figure 2.5 tend to cast doubt on this interpretation. The distributions of employment across age groups conditional on a particular firm-size category also reveal some differences. In general, in the ENEU it appears that employment in smaller firms is shifted a bit toward younger workers relative to the IMSS (with the opposite shift among larger firms). But the overall distributions across age categories (in the "all firm sizes" rows) appear to be fairly similar. Given the issues in reconciling the samples discussed above, it is perhaps not surprising that formal Kolmogorov-Smirnov or Wilcoxon tests reject the null hypothesis that the samples are drawn from the same underlying distribution. Caution is thus warranted in interpreting our results below. We nonetheless feel that the samples appear to be sufficiently similar that it is not unreasonable to use the wage discrepancy between them as a measure of evasion. We also note again that any differences between the samples that are constant over time will be differenced out in our difference-in-differences procedure.

As a further comparison, Figure 2.5 plots employment totals over the 1988-2003 period for the same samples as in Table 2.2. Perhaps surprisingly, we see that over most of the period the number of workers in the IMSS sample is slightly *greater* than the numbers in any of the ENEU samples. There are several potential explanations. The difference may reflect non-response by households in the ENEU (perhaps varying systematically with income, as mentioned above). It may be that some respondents are unaware that they receive IMSS coverage from their employer, or believe that they are covered by the public-sector social security agency (known by the acronym ISSSTE) when in fact they are covered by IMSS. It may also be that individuals live outside of the boundaries of the metropolitan area in which they work, and hence are included in our IMSS sample but not our ENEU sample. For our purposes, however, the most important lesson of the figure is that there does not appear to have been a large change over time in the extent of the employment discrepancy between the IMSS and ENEU samples in response to the pension reform. Nor does it appear that there was a significant large inflow to (or outflow from) formal employment in response to the pension reform.

2.5 Cross-Sectional Comparisons of Wage Distributions

In this section, we consider cross-sectional differences in wage distributions between the IMSS and ENEU baseline samples prior to the 1997 pension reform. We focus on the year 1990, in part because (in unreported results) we have been able to validate the ENEU sample against the population census from that year. To begin, Figure 2.6 plots simple histograms of raw data: *pre-tax* daily wages from the IMSS baseline sample (gray bars) and daily take-home wages the ENEU baseline sample (bars with black borders and no fill color), using bins that are 5 pesos wide. The three vertical lines between 50 and 70 pesos (approximately US$5-US$7/day) represent the the three minimum wages in Mexico, with the rightmost corresponding to the minimum wage in Mexico City. Figure 2.7 plots similar histograms using the same samples but using only observations below 200 pesos (approximately US$20), with bins 2 pesos wide. The pattern is clear: there is bunching in the IMSS sample slightly above the three minimum wages. These bunches correspond to 104.5 percent of the minimum wages in each zone — the minimum reports to IMSS that did not incur penalties. It is also evident that the IMSS distribution lies largely to the left of the ENEU distribution.[34] The bunching and shift to the left of the distribution is precisely what one would have expected, given that, for most workers, social security benefits were insensitive to reported wages, as long as their firms made the minimum contributions on their behalf.[35]

A key empirical implication of our model, as well as of the previous theoretical work by Kleven et al. (2009), is that there is less evasion in larger firms. Figure 2.8 presents figures similar to Figure 2.7 (focused on daily wages below 200 pesos), separately for five firm sizes. Caution is warranted in interpreting these figures, since observed establishment size in the IMSS data may itself be affected

[34]The exception to this generalization is at the far right tail. In Figure 2.6, we see that there is relatively more weight at the topcode in the IMSS sample; there is also slightly more weight at high wage values just below the topcode. This appears to reflect non-response by high-income households in the ENEU — a common pattern in household surveys, as mentioned above.

[35]Note that we are showing the raw, pre-tax IMSS data here, to illustrate the bunching at the top-code and the minimum reportable values; when we use the post-tax IMSS wage below, the discrepancy in the distributions is even more evident.

by firms' compliance decisions. Subject to this caveat, it appears that there is less bunching on the minimum allowable wage reports at larger firm sizes, suggesting greater compliance. Even in establishments with 250 workers or more, however, there is evidence of bunching at the minimum allowable wage report, suggesting some under-reporting even in quite large firms.

To quantify the extent of non-compliance, we construct three measures of evasion. Recall from equation (1) that the unreported wage is the difference between the worker's net wage and the post-tax wage reported by the firm: $w_u = w_{net} - (1-\tau)w_r$. As noted above, the ENEU survey asks individuals their take-home wage, which corresponds to w_{net}, and the raw IMSS administrative records contain the reported wage, w_r. We know the social security tax scheduled in each year, discussed in Section 2.2, and hence can calculate the IMSS post-tax wage. The ENEU data do not contain firm identifiers, but we can construct an estimate of w_u at the level of cells defined by metropolitan area, sector, firm size categories and/or age groups.

At the cell level, our first measure of evasion is the log median ENEU take-home wage minus the log median IMSS post-tax wage. Our second measure is defined analogously, using the mean instead of the median. We refer to the first measure as the "wage gap (medians)" and to the second as the "wage gap (means)". Our third measure of evasion is an estimate of the excess mass at the left tail of the IMSS wage distribution. Figure 2.9 illustrates the calculation. The dotted (blue) curve is a non-parametric estimate of the ENEU distribution, the same one that underlies the hollow-rectangle histogram in Figure 2.6. The solid (red) curve is a non-parametric estimate of the *post-tax* IMSS distribution. (Note that this differs from the distribution underlying the sold-gray histogram in Figure 2.6, which presents the *pre-tax* IMSS distribution.[36]) In principle, the ENEU take-home wage and the IMSS post-tax wage are an "apples-to-apples" comparison; in the absence of evasion, they should coincide. We calculate the excess mass as the fraction of the IMSS sample

[36]This is the reason why the spikes at the right tails of the solid (red) and dotted (blue) distributions, which correspond to the IMSS and ENEU topcodes, no longer coincide.

minus the fraction of the ENEU sample to the left of 15^{th} percentile of the ENEU distribution.[37] Intuitively, our excess mass measure reflects the share of the sample that has to be moved from right to left across the vertical line in order to transform the dotted (blue) distribution into the solid (red) distribution.[38]

The level of aggregation is an important issue when constructing these evasion measures. Although sample size is not a severe constraint in the IMSS administrative records, the ENEU contains on the order of 10,000-14,000 raw observations on male full-time workers in each quarter in the country as a whole. When we divide these by age group, metropolitan area, firm-size category and sector, cell sizes in the ENEU can become prohibitively small. We cannot avoid doing some aggregation. As discussed above, we focus on five age categories and five firm size categories. We also aggregate four-digit industries into three broad sectors: manufacturing, construction, and retail/services. In addition, when constructing the evasion measures, we pool all four quarters within a given year in the ENEU data.[39] In this section, we present cross-sectional statistics using the measures of evasion calculated at the metro area/sector/firm size category/age group level. Below we will conduct the analyses are higher levels of aggregation, as appropriate to the questions being investigated.

Table 2.4 reports simple cross-sectional regressions of our three evasion measures on age-group,

[37] In choosing the critical value for the excess mass calculation, we face a trade-off. On one hand, we want a value that is clearly to the right of the region of bunching in the IMSS data. On the other hand, we do not want a value so far to the right that the measure misses under-reporting behavior. (If, for instance, the under-reporting of wages occurs only for workers below the median, using the 50^{th} percentile as the critical value will completely miss the under-reporting behavior.) The results using other higher percentiles than the 15^{th} are qualitatively similar, although slightly weaker in some years.

[38] This excess mass measure differs from other excess mass measures that have been used in the literature, for instance by Saez (2010), Chetty, Friedman, Olsen, and Pistaferri (2011), and Kleven and Waseem (2013), in two ways. First, in our setting we do not have to construct a counterfactual distribution, requiring assumptions about how individuals in the region of bunching are distributed in the counterfactual; here effectively we observe the counterfactual distribution in the ENEU household data. Second, it is common to scale the excess mass by the density of the counterfactual distribution in the region of bunching; in our setting at the left tail the density of the ENEU wage distribution is near zero, and dividing by this density might introduce significant errors, so we do not re-scale.

[39] Because the survey follows households for five quarters, observations in different quarters are not independent, but our econometric tests do not require an independence assumption.

firm-size and sector indicators in 1990. For each evasion measure, we report simple regressions on a set of age-group or firm-size indicators without controls (Columns 1-2, 4-5, and 7-8) and then a regression including sector indicators and metro-area indicators (Columns 3, 6, and 9). For age groups, there is clear evidence that evasion is higher on average for the youngest age group, ages 16-25 (the omitted category), perhaps not surprisingly, since the youngest workers tend to have lower labor-force attachment and higher mobility across jobs. The differences in coefficients among the over-25 age groups are generally not significantly different from one another. For firm size, the general pattern is that evasion is declining in employment, consistent with the pattern in the raw histograms in Figure 2.8. There appears to be some non-monotonicity in the relationship between evasion and firm size for the intermediate size categories (51-100 and 101-250 employees), but it appears robust that evasion is lower in 11-50 employee firms than in 1-10 employee firms (the omitted category), and lower still in 250+ employee firms. The estimates are largely unaffected by controlling for age group, metro area, and sector, which suggests that the pattern we observed in the raw data in Figure 2.8 is not due to differing age or metro area composition in different firm size categories. Finally, evasion follows a consistent pattern across broad sectors, with construction displaying the greatest extent of evasion, followed by manufacturing, followed by retail/services.

2.6 Effect of Pension Reform on Compliance

We now consider how evasion varied over time in response to the pension reform. A simple set of graphs illustrates our main finding. Figure 2.10 plots non-parametric estimates of the male wage distributions, similar to those in Figure 2.9 but for wages levels below 200 pesos/day, by age group, for three years, 1990, 1997, and 2003.[40] Each column of graphs corresponds to an age group (indicated in the x-axis titles) and each row to a year. The key empirical pattern is illustrated by the

[40]We present non-parametric density estimates, rather than histograms, because they are easier to interpret visually. Histograms would convey a qualitatively similar message.

contrast between the first column, corresponding to ages 16-25, and the last column, corresponding to ages 56-65. There is a clear decline in bunching and shift to the right of the IMSS distribution for the youngest age group. For the oldest age group, there is little evident decline in bunching or shift to the right in the IMSS distribution.

A possible concern with Figure 2.10 is that the differential changes by age group may reflect shocks to local labor markets which differ in their age composition. To remove the effects of local labor-market shocks, we calculate the wage gap (medians) measure at the age group-metro area-year level, regress it on a full set of metro area-year indicators, and average the residuals at the age group-year level. Figure 2.11 plots these averages, with observations corresponding to the 2^{nd} quarter of each year. One point to notice is that, as we saw in Table 2.4, evasion is highest for the youngest age group. But the more important point is that the gap for the oldest age group, 56-65, increased relative to the gaps for the other groups. To the extent that there is another group that appears to have seen a relative increase, it is the second-oldest age group, 46-55. For the oldest age group, it appears that the differential increase may have begun between the 2^{nd} quarter of 1995 and the 2^{nd} quarter of 1996, which corresponds to the time of passage of the law; there was significant discussion of the pension reform in the popular press around the time of passage, and it is possible that employees anticipated the effects of the reform and started paying attention to under-reporting even before the reform took effect in July 1997. However, to be conservative, in our regression analysis below we will take July 1, 1997 as the date of the reform. Even using this more conservative choice, it appears that there was a differential increase in evasion for the oldest two age groups relative to the younger ones.

Table 2.5 reports regressions that capture the pattern illustrated in Figure 2.11. Motivated by the figure, we look for a divergence in the time-path of evasion for the oldest age group relative to the younger groups. We interact an indicator for the oldest age group with year effects for each year over the 1988-2003 period, omitting the interaction with 1995, the year the reform was passed. We control flexibly for metro area-year interactions and age group-metro area interactions. The key finding is that, across the three measures of evasion, we see little evidence of a differential pre-trend but robust evidence of a relative increase in evasion for the oldest age group following the passage of the reform. It takes a bit longer for the relative increase in the wage gap (medians) measure to become statistically significant, but the fact that we see a similar pattern across the three measures is reassuring.[41] Overall, the results are supportive of our second main theoretical implication: evasion for older age groups declined relatively less than evasion for younger age groups.

The results for our evasion measures do not seem to be driven by discrepancies in the reporting of employment in the two data sources. Table 2.6 reports regressions similar to those in Table 2.5, but where the outcome variable is the difference in log employment — what we can call the "employment gap" — between the ENEU and IMSS baseline samples within an age group-metro area cell, rather than one of the evasion measures. There is no evidence of a differential change in the employment gap for older workers in response to the reform.[42]

To conclude this section, we briefly consider the incidence of the reform on market-level net wages, using the ENEU household survey alone. Table 2.7 reports specifications similar to those in Table 2.5, where the outcome is the log net wage reported on the ENEU survey, and we are able to control flexibly for individual characteristics. Column 1 includes just age-group and metro area-year effects, then moving across the columns we add sets of dummy variables for schooling categories, marital status, occupation, industry and firm size. The consistent message is that there

[41] As mentioned above, the results for the excess mass measure when using different critical values are slightly weaker. This is particularly true in 2003, a year in which we have consistent ENEU data only for the first two quarter. But for all critical values between the 10^{th} and 35^{th} percentiles the excess mass measures show a significant differential change by 2001-2002.

[42] Note that the employment gap here captures discrepancies in the number of workers reported to the IMSS system and the number who report on the household survey that they receive IMSS coverage; it does not capture changes in the propensity of firms to offer formal (i.e. covered by IMSS) employment. As mentioned above, we leave the investigation of the effects of the reform on the informal/formal employment margin for future work.

is no systematic differential effect of the reform on net wages of the oldest age group. While this non-result may appear surprising, it is consistent with recent evidence from other countries on the effects of tax reforms on take-home wages. For instance, Saez, Matsaganis, and Tsakloglou (2012) find little evidence of differences in take-home pay for workers of different cohorts facing different payroll-tax rates. The authors speculate that institutional factors such as fairness norms may constrain firms' ability to offer different wages to otherwise similar workers facing different effective payroll tax rates. Such factors may also be present in Mexico and may similarly prevent firms from offering different take-home wages to different age groups. Also, as discussed briefly in Section 2.3 and in more detail in the Appendix, in our theoretical framework the relationship between the pension reform and the net wage is ambiguous, even without such institutional factors. In short, based both on prior empirical work and on our model, it is not clear that we should have expected to find a differential effect of the pension reform on take-home wages.

2.7 Conclusion

Improving firms' compliance with tax regulations is a first-order policy issue in many developing countries. Much of the debate has focused on how to induce firms to register with tax authorities in the first place — what we might call the *extensive* margin of non-compliance. In this paper, we have shown that under-reporting of wages among firms that are already registered — non-compliance on an *intensive* margin — is also substantial and responds to incentives and the availability of information in the social security system.

The results suggest that providing incentives to workers to ensure accurate reporting, as well as information about firms' reports, should be a consideration in the design of social-insurance systems. Conceptually, our theoretical model suggests that an increase in such incentives and a commensurate reduction of payroll taxes should have equivalent effects on evasion, other things equal. But the effects on government revenues are decidedly non-equivalent. If the policy goal is to increase the fiscal capacity of the state, it appears that the tying benefits more closely to wage reports may be the preferable option.

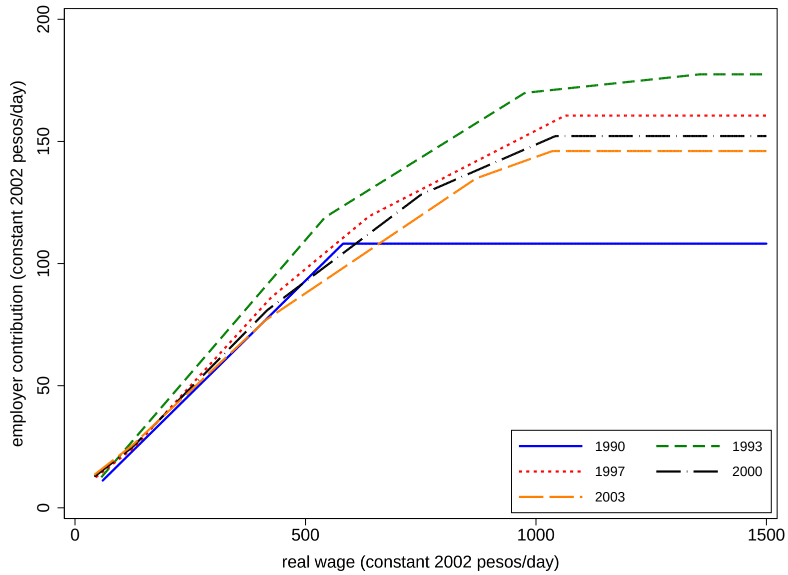
A number of interesting questions remain open. One is to what extent workers are aware of under-reporting by their employers and, relatedly, to what extent the effects of the pension reform we observe are due to the change in incentives versus the change in information. It seems unlikely that the results we observe could be due solely to the increased availability of information post-reform; if benefits had remained truly insensitive to wage reports, then workers would have had no reason to act on the information once they received it.[43] But it remains possible that there was an important interaction between the change in incentives and the change in information. Separating the two effects definitively will require a research design in which incentives vary separately from costs of information.

Another important open question is whether increased pressure on firms to report accurately (which increases compliance on the intensive margin) induces more firms to remain informal (reduces compliance on the extensive margin). Because of the nature of the IMSS data, we are not able to observe firms as they move from formality to informality or vice-versa. But clearly a full accounting of the costs and benefits of policies to increase intensive-margin compliance will have to take such a response into account.

[43]Relatedly, a reduction of the cost of acquiring information about firms' reports would not be expected to have an effect, as long as the cost remains positive, as workers would not be willing to pay even an ε cost to acquire it.

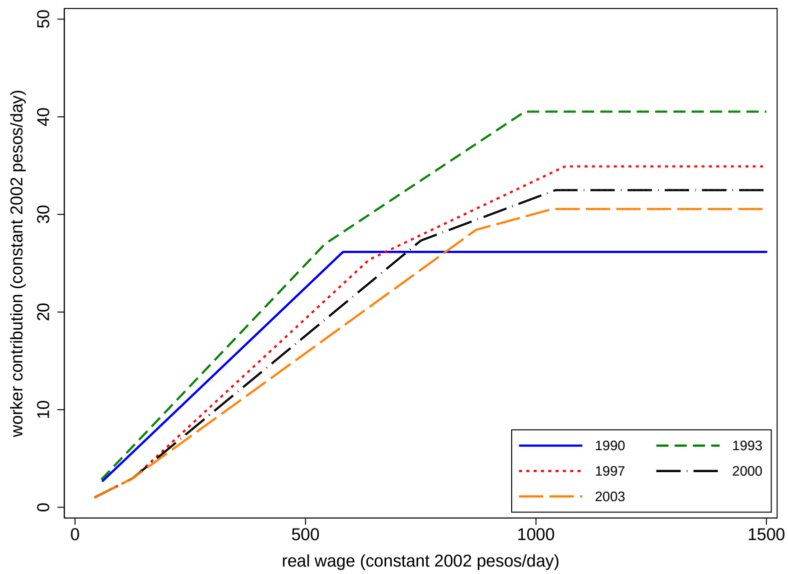
2.8 Figures

Figure 2.1. Employer Contributions



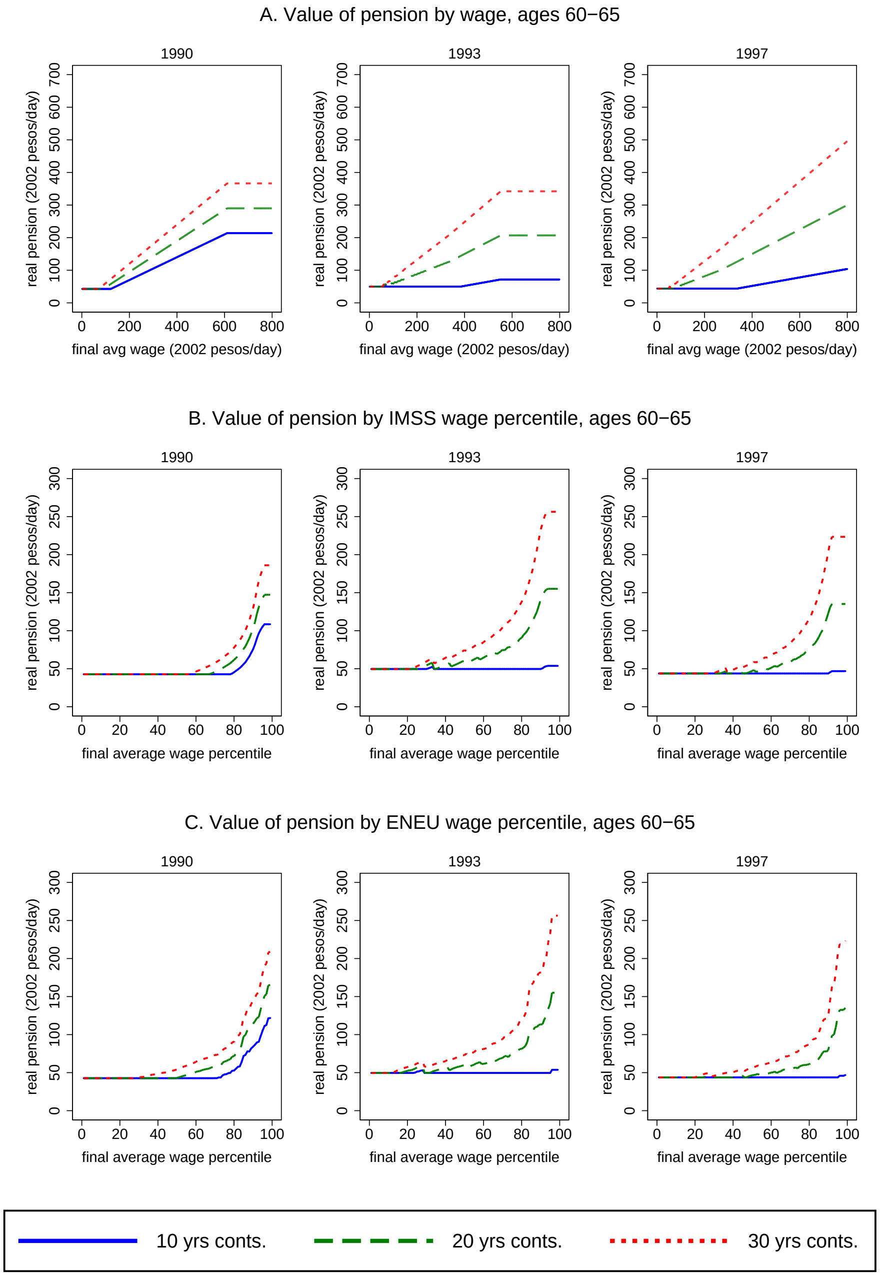
Notes: Variation in IMSS employer contribution rates at levels above 500 pesos/day are primarily due to changes in topcodes, which varied from 10 to 25 times the minimum wage in Mexico City over the period. Average 2002 exchange rate: 9.66 pesos/dollar.

Figure 2.2. Employee Contributions



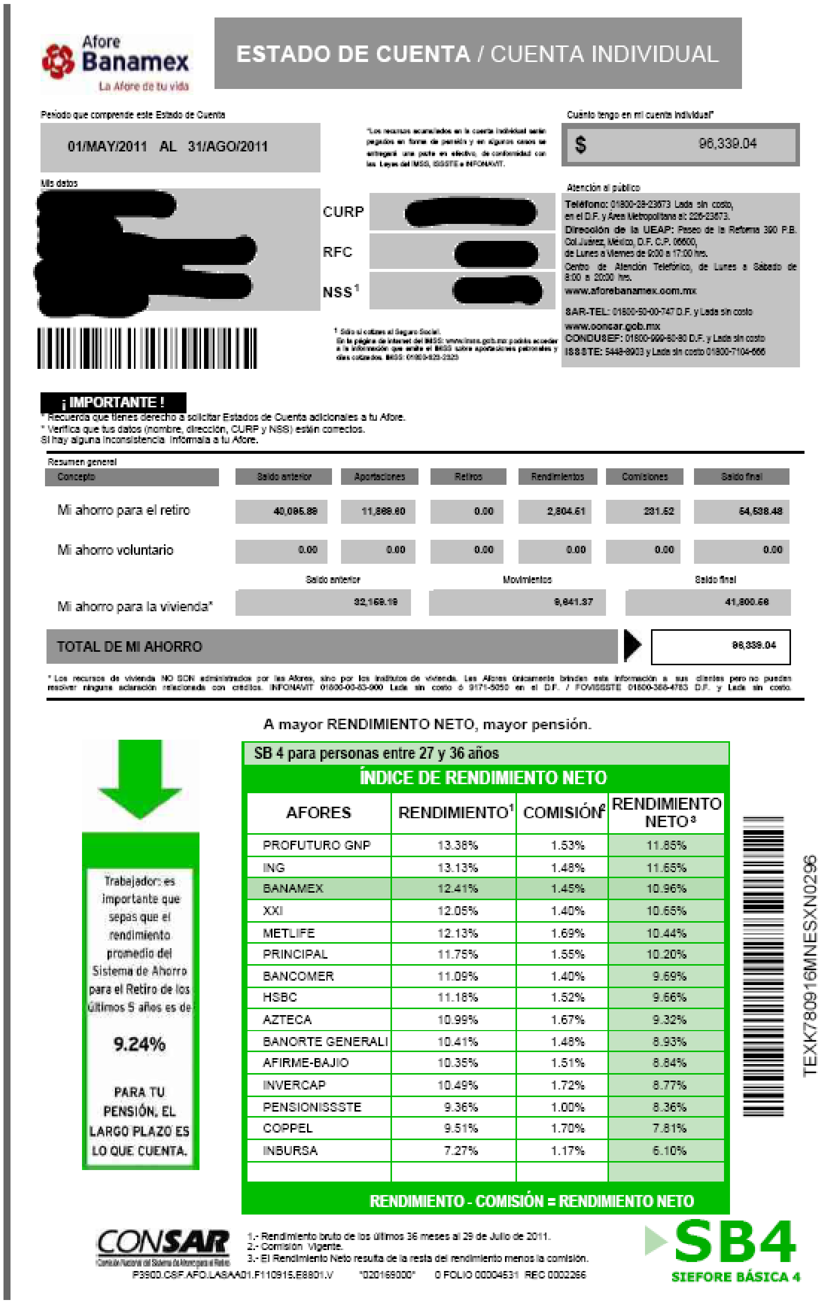
Notes: Variation in IMSS worker contribution rates at levels above 500 pesos/day are primarily due to changes in topcodes, which varied from 10 to 25 times the minimum wage in Mexico City over the period. Average 2002 exchange rate: 9.66 pesos/dollar.

Figure 2.3. Pension Values, Selected Years, Men



Notes: Final average wage (2002 pesos/day) is average nominal daily wage over five years prior to retirement, deflated to constant 2002 pesos. Figure indicates pension values for individuals with 10, 20 and 30 years of contributions to IMSS. In Panel B, we calculate the nominal wage at each quantile of the IMSS wage distribution for 60-65 year old men in each year and take the average for that quantile over the preceding five years. Panel C is constructed similarly using wage distributions from the ENEU baseline samples. See Section 2.4 for details of samples and Section 2.2.3 for details on pension benefits. Average 2002 exchange rate: 9.66 pesos/dollar.

Figure 2.4. Account Statement


Afore Banamex
La Afore de tu vida

ESTADO DE CUENTA / CUENTA INDIVIDUAL

Periodo que comprende este Estado de Cuenta

01/MAY/2011 AL 31/AGO/2011

"Los recursos acumulados en la cuenta individual serán pagados en forma de pensión y en algunos casos se entregará una parte en efectivo, de conformidad con las Leyes del IMSS, ISSSTE e INFONAVIT.

Cuánto tengo en mi cuenta individual*

$ 96,339.04

Mis datos

[redacted]

CURP [redacted]

RFC [redacted]

NSS[1] [redacted]

[1] Sólo si cotizas al Seguro Social.

Atención al público

Teléfono: 01800-28-23673 Lada sin costo, en el D.F. y Área Metropolitana al: 226-23673.
Dirección de la UEAP: Paseo de la Reforma 390 P.B. Col.Juárez, México, D.F. C.P. 06600, de Lunes a Viernes de 9:00 a 17:00 hrs.
Centro de Atención Telefónico, de Lunes a Sábado de 8:00 a 20:00 hrs.
www.aforebanamex.com.mx

SAR-TEL: 01800-50-00-747 D.F. y Lada sin costo
www.consar.gob.mx
CONDUSEF: 01800-999-80-80 D.F. y Lada sin costo
ISSSTE: 5448-8903 y Lada sin costo 01800-7104-666

¡ IMPORTANTE !
* Recuerda que tienes derecho a solicitar Estados de Cuenta adicionales a tu Afore.
* Verifica que tus datos (nombre, dirección, CURP y NSS) están correctos.
Si hay alguna inconsistencia infórmala a tu Afore.

Resumen general

Concepto	Saldo anterior	Aportaciones	Retiros	Rendimientos	Comisiones	Saldo final
Mi ahorro para el retiro	40,095.89	11,889.60	0.00	2,804.61	231.62	54,538.48
Mi ahorro voluntario	0.00	0.00	0.00	0.00	0.00	0.00

	Saldo anterior	Movimientos	Saldo final
Mi ahorro para la vivienda*	32,159.19	9,641.37	41,800.56

TOTAL DE MI AHORRO 96,339.04

* Los recursos de vivienda NO SON administrados por las Afores, sino por los Institutos de vivienda. Las Afores únicamente brindan esta información a sus clientes pero no pueden resolver ninguna aclaración relacionada con créditos. INFONAVIT 01800-00-83-900 Lada sin costo ó 9171-5050 en el D.F. / FOVISSSTE 01800-368-4783 D.F. y Lada sin costo.

A mayor RENDIMIENTO NETO, mayor pensión.

Trabajador: es importante que sepas que el rendimiento promedio del Sistema de Ahorro para el Retiro de los últimos 5 años es de

9.24%

PARA TU PENSIÓN, EL LARGO PLAZO ES LO QUE CUENTA.

SB 4 para personas entre 27 y 36 años

ÍNDICE DE RENDIMIENTO NETO

AFORES	RENDIMIENTO[1]	COMISIÓN[2]	RENDIMIENTO NETO[3]
PROFUTURO GNP	13.38%	1.53%	11.85%
ING	13.13%	1.48%	11.65%
BANAMEX	12.41%	1.45%	10.96%
XXI	12.05%	1.40%	10.65%
METLIFE	12.13%	1.69%	10.44%
PRINCIPAL	11.75%	1.55%	10.20%
BANCOMER	11.09%	1.40%	9.69%
HSBC	11.18%	1.52%	9.66%
AZTECA	10.99%	1.67%	9.32%
BANORTE GENERALI	10.41%	1.48%	8.93%
AFIRME-BAJIO	10.35%	1.51%	8.84%
INVERCAP	10.49%	1.72%	8.77%
PENSIONISSSTE	9.36%	1.00%	8.36%
COPPEL	9.51%	1.70%	7.81%
INBURSA	7.27%	1.17%	6.10%

RENDIMIENTO - COMISIÓN = RENDIMIENTO NETO

CONSAR

1.- Rendimiento bruto de los últimos 36 meses al 29 de Julio de 2011.
2.- Comisión Vigente.
3.- El Rendimiento Neto resulta de la resta del rendimiento menos la comisión.

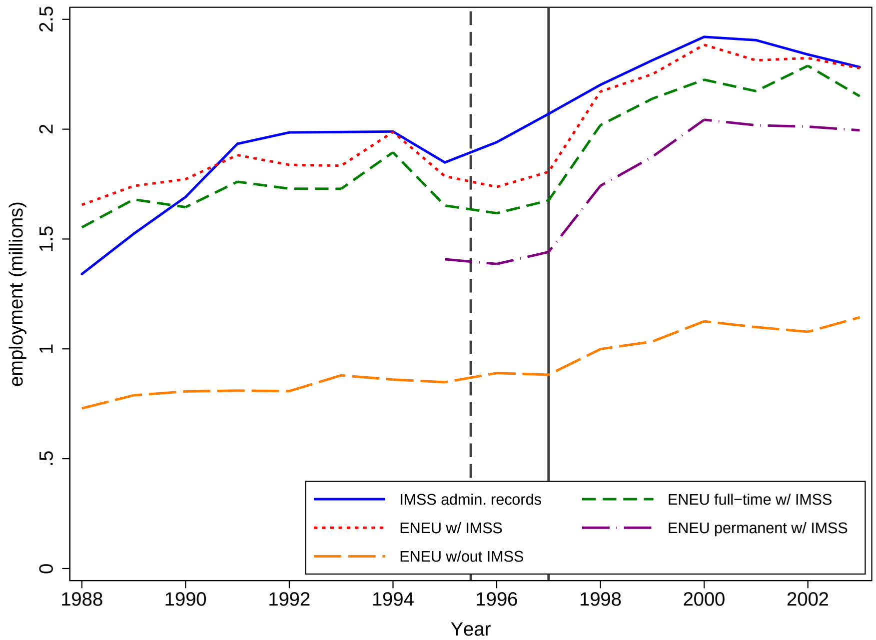
P3900.CSF.AFO.LASAA01.F110915.E8801.V "020169000" 0 FOLIO 00004531 REC 0002266

SB4 SIEFORE BÁSICA 4

TEXK780916MNESXN0296

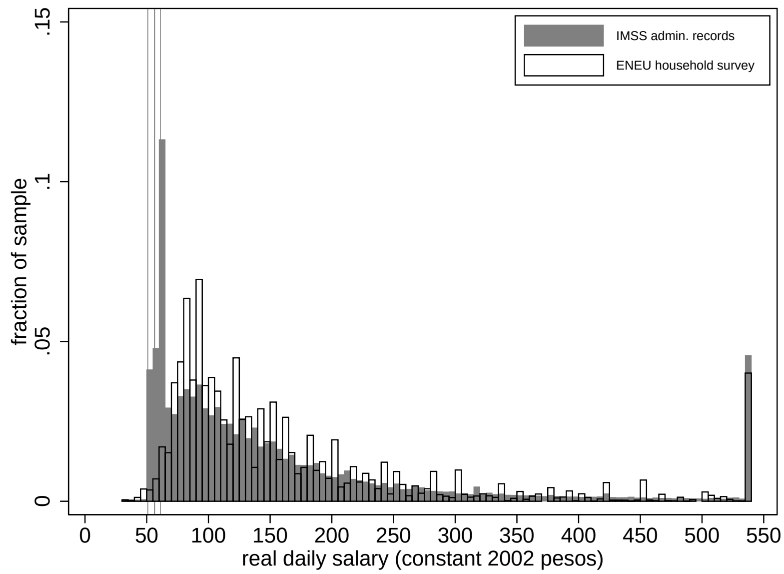
Notes: The box at top right ("Cuánto tengo en mi cuenta individual") reports total balance. The first row of boxes in the middle section ("Mi ahorro para el retiro") pertains to the retirement pension and reports previous balance ("Saldo anterior"), new contributions ("Aportaciones"), withdrawals ("Retiros"), interest earned ("Rendimientos"), AFORE commission charged ("Comisiones"), and final balance ("Saldo final"). The second and third rows in the middle section report balances in the individual's voluntary savings account and housing account. The bottom section reports 3-year returns and commissions for each AFORE, as well as the average 5-year net return (at left).

Figure 2.5. Employment, IMSS Admin. Records vs. ENEU household data, Men



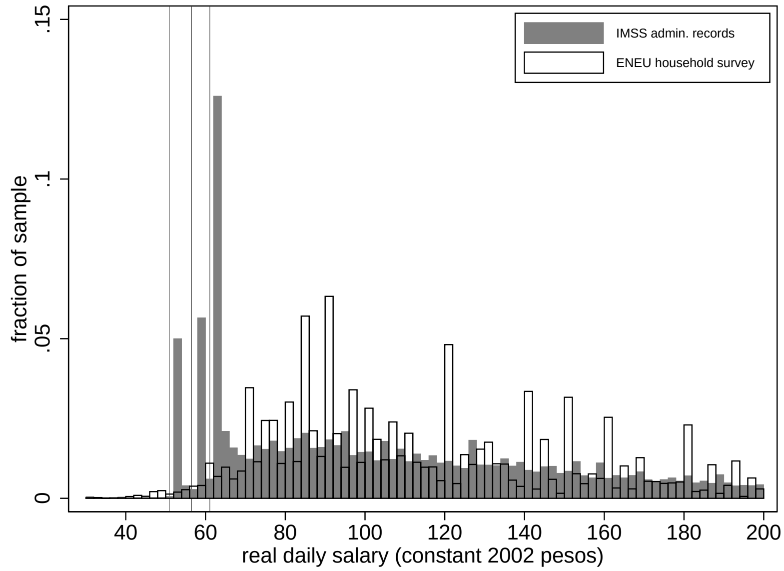
Notes: Samples are the same as those in Columns 1 and 3-6 of Table 2.2; refer to that table for details. ENEU totals are calculated using sampling weights. The dashed vertical line indicates the date the pension reform was passed by Congress (Dec. 21, 1995); the solid vertical line indicates the date the reform took effect (July 1, 1997). Observations correspond to the second quarter of each year. See Section 2.4 and the Appendix for details of sample selection.

Figure 2.6. Wage Histograms, Men, 1990



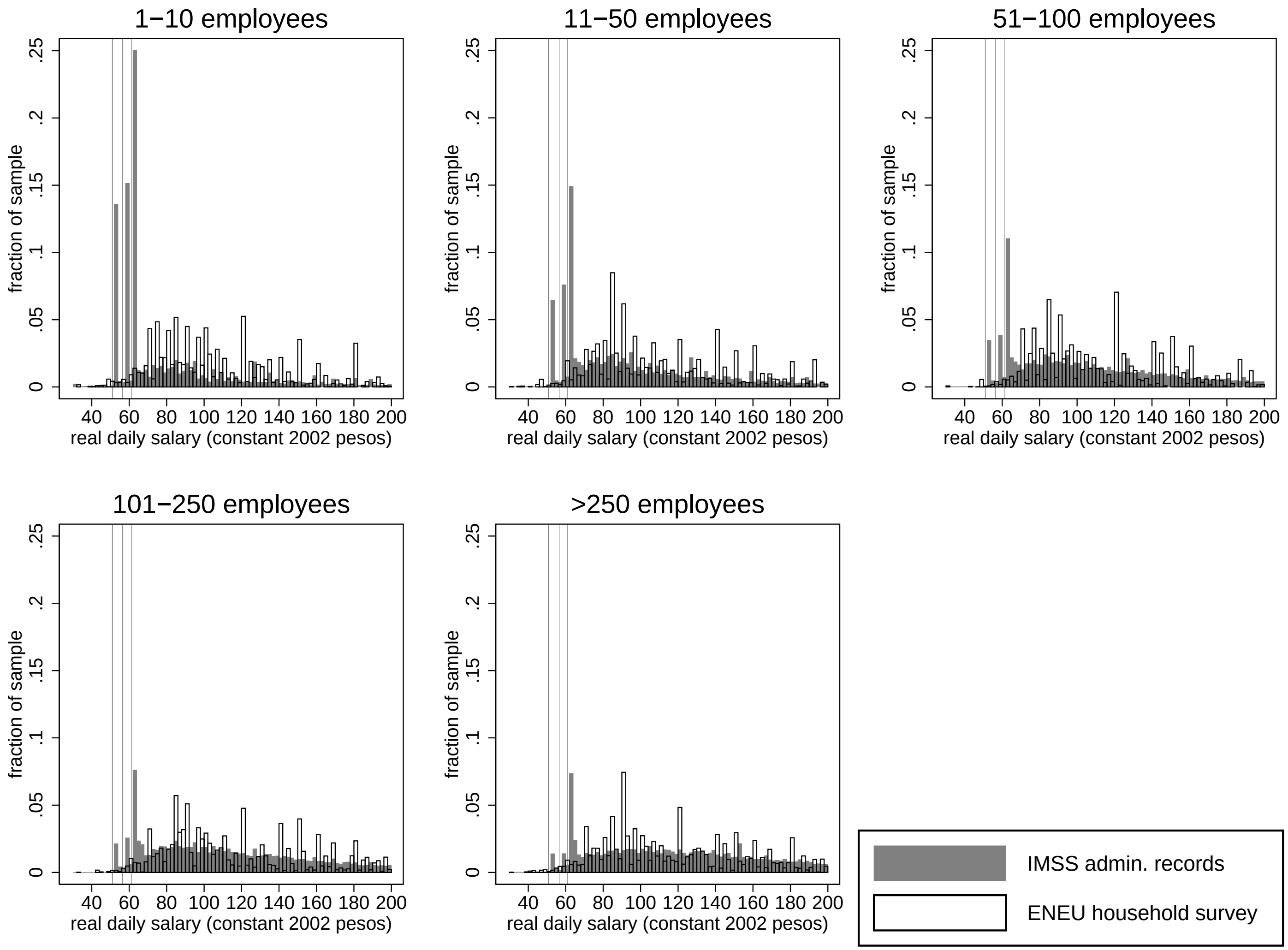
Notes: The ENEU wage is the real daily take-home wage reported to the ENEU household survey. The IMSS wage is the real daily *pre-tax* reported wage from the IMSS administrative records. Wages in 2002 pesos. Average 2002 exchange rate: 9.66 pesos/dollar. Samples are IMSS and ENEU "baseline" samples of men. Data in both samples are from second quarter of 1990. Vertical lines indicate minimum wages in the three minimum-wage zones in Mexico (A, B, C). Bins are 5 pesos wide. The rightmost bin captures all individuals with reported wages at or above the minimum IMSS topcode over the study period (from 1991). See Section 2.4 and the Appendix for further details.

Figure 2.7. Wage Histograms, Men, 1990, Low Wage Levels



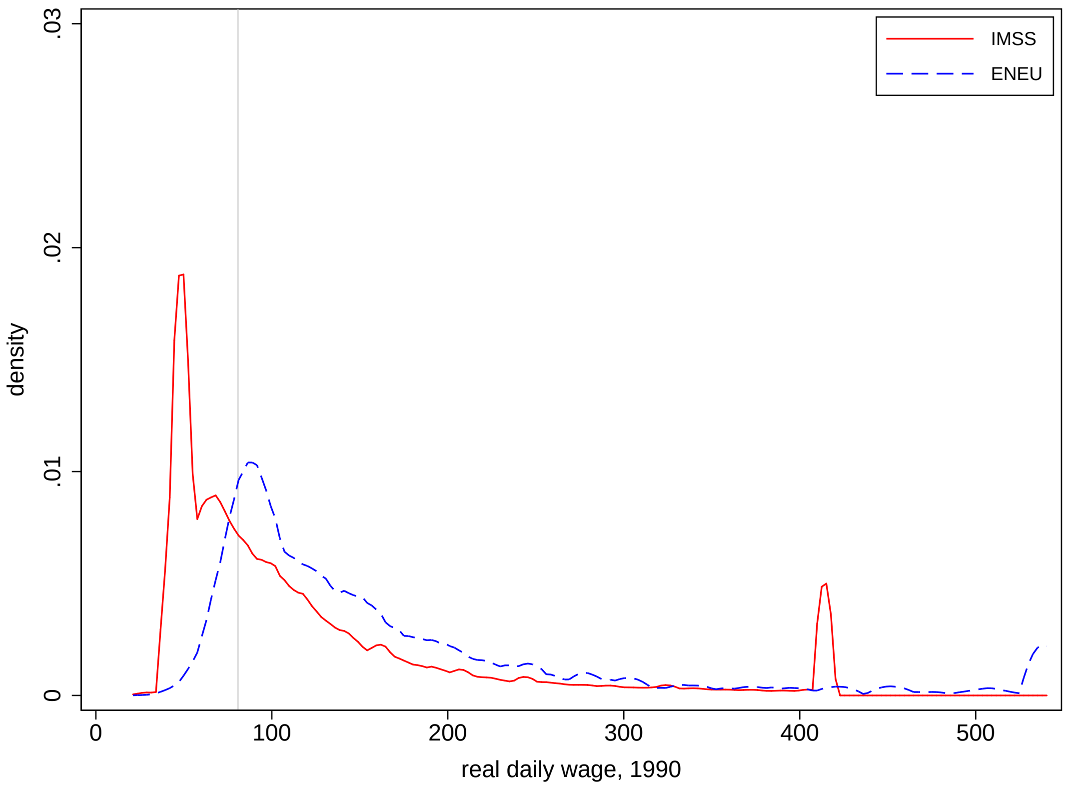
Notes: Histogram is similar to Figure 2.6 but only includes workers with wages less than 200 pesos/day (approx. $20/day) in constant 2002 pesos. Bins are 2 pesos wide.

Figure 2.8. Wage Histograms by Firm Size, Men, 1990, Low Wage Levels



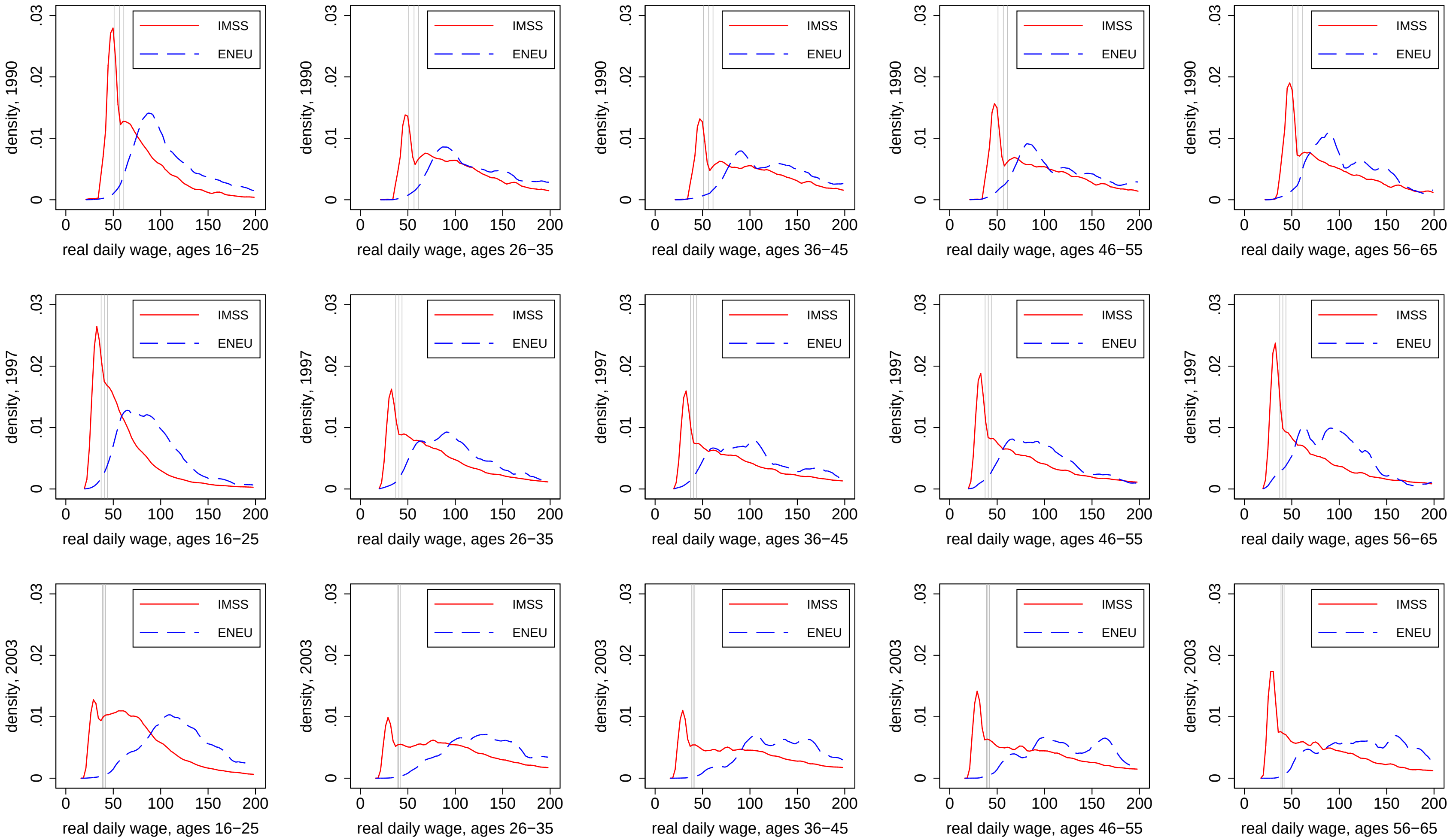
Notes: Histograms are similar to those in Figure 2.7. Vertical lines indicate minimum wages in the three minimum-wage zones in Mexico (A, B, C). Bins are 2 pesos wide. Average 2002 exchange rate: 9.66 pesos/dollar. See Section 2.4 and the Appendix for further details.

Figure 2.9. Excess Mass Calculation



Notes: The wage variables are the real daily take-home wage from ENEU and real daily *post-tax* reported wage from IMSS. Densities are estimated using 1990 data and an Epanechnikov kernel with bandwidth 3 pesos for IMSS data and 6 pesos for ENEU data (using Stata kdensity command). Wages are in 2002 pesos. Average 2002 exchange rate: 9.66 pesos/dollar. (Densities are smoothed versions of histograms in Figure 2.6.) Vertical line is at 25th percentile of the ENEU wage distribution. Data are from second quarter of 1990. Excess mass for 25th percentile defined as (area under red, left of vertical line) - (area under blue, left of vertical line). Excess mass defined analogously for other percentiles. See Section 2.4 and the Appendix for further details of data processing.

Figure 2.10. Wage Densities by Age Group, 1990, 1997, 2003, Men



Notes: The wage variables are the real daily take-home wage from ENEU and real daily *post-tax* reported wage from IMSS. Densities are estimated and an Epanechnikov kernel with bandwidth 3 pesos for IMSS data and 6 pesos for ENEU data (using Stata kdensity command). Wages are in 2002 pesos. Average 2002 exchange rate: 9.66 pesos/dollar. Rows correspond to years 1990, 1997, 2003; columns to age groups 16-25, 26-35, 36-45, 46-55, 56-65. Samples are baseline samples of men, only including workers with wages less than 200 pesos/day. Data in both samples are from second quarter. See Section 2.4 and the Appendix for further details.

Figure 2.11. Wage Gaps (Medians) by Age Group, Men, Deviated from Metro-Year Means

Notes: Each wage gap is the difference between the log median net wage from the ENEU survey and the log median post-tax reported wage from the IMSS administrative records, using the ENEU and IMSS baseline samples. To calculate deviated wage gaps, we calculate wage gaps separately by age group-year-metro area, regress them on a full set of metro area-year dummies, and average the residuals at the age-group level. The dashed vertical line indicates the date the pension reform was passed by Congress (Dec. 21, 1995); the solid vertical line indicates the date the reform took effect (July 1, 1997). Observations correspond to the second quarter of each year. See Section 2.4 and the Appendix for details of sample selection.

2.9 Tables

Table 2.1. Pension Wealth Simulation, by Age in 1997, Male Worker with 35 Years of Expected Contributions

			Real Daily Wage					
Age in 1997	Years of Expected PRA Contributions	Plan	43	100	200	300	500	1079
25	35	PRA	398.6	*815.0*	*1626.2*	*2437.3*	*4059.7*	*8751.9*
		PAYGO	398.6	*398.6*	*603.8*	*890.2*	*1483.6*	*3200.1*
30	30	PRA	398.6	*523.4*	*1044.3*	*1565.3*	*2607.1*	*5620.5*
		PAYGO	398.6	*398.6*	*603.8*	*890.2*	*1483.6*	*3200.1*
35	25	PRA	398.6	398.6	*659.1*	*987.8*	*1645.3*	*3546.9*
		PAYGO	398.6	398.6	*603.8*	*890.2*	*1483.6*	*3200.1*
40	20	PRA	398.6	398.6	403.9	605.4	1008.4	2173.9
		PAYGO	398.6	398.6	603.8	890.2	1483.6	3200.1
45	15	PRA	398.6	398.6	398.6	398.6	586.6	1264.7
		PAYGO	398.6	398.6	603.8	890.2	1483.6	3200.1
50	10	PRA	398.6	398.6	398.6	398.6	398.6	662.6
		PAYGO	398.6	398.6	603.8	890.2	1483.6	3200.1
55	5	PRA	398.6	398.6	398.6	398.6	398.6	398.6
		PAYGO	398.6	398.6	603.8	890.2	1483.6	3200.1

Notes: Values are real present discounted value of the future stream of pension benefits in thousands of 2002 pesos, for a male worker who began contributing at age 25 and expects to continue until age 60. Numbers in italics indicate that personal retirement account (PRA) has a higher expected payoff than the pre-reform pension (PAYGO). Average 2002 exchange rate: 9.66 pesos/dollar. 43 pesos is real daily minimum wage (in Mexico City) in 1997, 1079 pesos is the topcode we impose (corresponding to the lowest real value of IMSS topcode over study period.) See Section 2.2.3 and the Appendix for further details.

Table 2.2. Comparison of IMSS Baseline Sample and Various ENEU Samples, Men

	IMSS baseline sample (1)	full ENEU sample (2)	ENEU w/ IMSS (3)	ENEU w/o IMSS (4)	ENEU permanent w/ IMSS (5)	ENEU full-time w/ IMSS (6)
A. 1990						
real avg. daily post-tax wage	121.02	163.88	172.98	143.88		166.73
	(0.07)	(1.58)	(1.94)	(2.62)		(1.85)
age	31.75	31.46	32.13	29.98		32.22
	(0.01)	(0.15)	(0.17)	(0.29)		(0.17)
fraction employed in ests >100 employees	0.52	0.43	0.55	0.18		0.55
	(0.00)	(0.01)	(0.01)	(0.01)		(0.01)
N (raw observations)	1691417	16169	11592	4577		10978
N (population, using weights)	1691417	2578847	1772523	806324		1645229
B. 2000						
real avg. daily post-tax wage	123.60	148.20	161.15	120.78	166.42	155.80
	(0.07)	(1.31)	(1.60)	(2.16)	(1.80)	(1.59)
age	32.70	32.22	32.82	30.94	33.22	32.88
	(0.01)	(0.14)	(0.16)	(0.28)	(0.17)	(0.16)
fraction employed in ests >100 employees	0.58	0.44	0.59	0.10	0.63	0.59
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N (raw observations)	2420307	19171	14063	5108	11918	13246
N (population, using weights)	2420307	3509828	2384267	1125561	2042988	2225318

Notes: All columns focus on wage-earning male workers ages 16-65 in manufacturing, construction, and retail/hotel/restaurant sectors in 16 metropolitan areas from the original ENEU sample. Column 1 reports statistics for IMSS baseline sample; Column 2 for full ENEU (household survey) sample (satisfying aforementioned criteria); Column 3 for employees in ENEU who report receiving IMSS benefit in current employment; Column 4 for employees in ENEU who report not receiving IMSS benefit; Column 5 for employees in ENEU who report receiving IMSS benefit and having a written contract of indefinite duration; and Column 6 for employees in ENEU who report receiving IMSS benefit and working at least 35 hours in previous week (the ENEU baseline sample). Standard errors of means in parentheses. In IMSS data, the fraction in establishments with >100 employees variable refers to permanent employees. In the ENEU survey, the establishment-size question asks the total number of employees (without specifying permanent vs. temporary.) For further details, see Section 2.4 and the Appendix.

Table 2.3. Age Composition by Firm Size Category, 1990, Men

	Age category (employment as % of row total)					employment as % of column total
	16-25	26-35	36-45	46-55	56-65	
A. IMSS						
1-10 employees	29.9	32.6	19.8	11.9	5.8	14.5
11-50 employees	33.6	32.2	18.7	10.6	4.9	22.6
51-100 employees	35.0	32.5	18.5	9.8	4.2	10.8
101-250 employees	36.3	33.3	17.8	9.0	3.5	14.7
> 250 employees	37.7	34.8	17.5	7.6	2.5	37.5
all firm sizes	35.1	33.4	18.3	9.3	3.8	
B. ENEU						
1-10 employees	35.9	28.3	18.0	12.5	5.3	12.4
11-50 employees	33.5	33.3	18.4	10.3	4.5	21.0
51-100 employees	35.6	33.4	15.2	10.7	5.1	11.6
101-250 employees	30.2	31.2	21.5	12.4	4.7	10.5
> 250 employees	34.0	33.4	21.5	8.5	2.7	44.5
all firm sizes	33.9	32.5	19.7	10.1	3.9	

Notes: Data are from IMSS and ENEU baseline samples. Percentages are calculated based on employment (using sampling weights, in the case of the ENEU) in each cell. Panel B drops observations in ENEU baseline sample that are missing the firm-size variable (which make up less than 1% of sample). For further details, see Section 2.4 and the Appendix.

Table 2.4. Cross-Sectional Patterns of Evasion, 1990, Men

	wage gap (medians)			wage gap (means)			exc. mass (15th percentile)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
age 26-35	-0.054*		-0.053**	-0.081***		-0.081***	-0.154***		-0.154***
	(0.029)		(0.021)	(0.024)		(0.019)	(0.017)		(0.013)
age 36-45	-0.072**		-0.072***	-0.150***		-0.150***	-0.170***		-0.170***
	(0.034)		(0.027)	(0.028)		(0.024)	(0.017)		(0.014)
age 46-55	-0.029		-0.025	-0.154***		-0.151***	-0.154***		-0.152***
	(0.035)		(0.031)	(0.031)		(0.027)	(0.018)		(0.015)
age 56-65	-0.020		-0.026	-0.167***		-0.174***	-0.117***		-0.119***
	(0.044)		(0.040)	(0.038)		(0.035)	(0.020)		(0.017)
11-50 employees		-0.333***	-0.333***		-0.177***	-0.176***		-0.155***	-0.154***
		(0.026)	(0.024)		(0.025)	(0.023)		(0.011)	(0.010)
51-100 employees		-0.475***	-0.469***		-0.283***	-0.280***		-0.247***	-0.242***
		(0.033)	(0.031)		(0.030)	(0.028)		(0.015)	(0.014)
101-250 employees		-0.395***	-0.374***		-0.245***	-0.233***		-0.235***	-0.224***
		(0.039)	(0.037)		(0.035)	(0.032)		(0.018)	(0.016)
> 250 employees		-0.500***	-0.464***		-0.233***	-0.200***		-0.288***	-0.268***
		(0.035)	(0.034)		(0.030)	(0.030)		(0.018)	(0.017)
construction			0.134***			0.122***			0.064***
			(0.029)			(0.025)			(0.014)
retail/services			-0.074***			-0.110***			-0.043***
			(0.024)			(0.021)			(0.011)
constant	0.559***	0.855***	0.633***	0.501***	0.577***	0.506***	0.519***	0.578***	0.566***
	(0.017)	(0.018)	(0.047)	(0.016)	(0.018)	(0.039)	(0.010)	(0.007)	(0.019)
metro area effects	N	N	Y	N	N	Y	N	N	Y
R-squared	0.00	0.20	0.31	0.03	0.09	0.27	0.09	0.24	0.44
N	1062	1062	1062	1062	1062	1062	1062	1062	1062

Notes: Data are from IMSS and ENEU baseline samples, collapsed to metro area/age group/firm-size category/sector level for 1990. The omitted category for age is 16-25, for firm size is 1-10 employees, and for sector is manufacturing. The wage gap (medians) is log median real daily take-home wage from the ENEU minus log median real daily post-tax reported wage from IMSS, calculated. Wage gap (means) is analogous, using mean in place of median. Excess mass is calculated as described in Section 2.5 and Figure 2.9. In calculating evasion measures, we pool ENEU data across quarters within year. *** 1%, ** 5%, * 10% level. See Section 2.4 and the Appendix for further details of data processing.

Table 2.5. Differential Effects of Pension Reform on Evasion, Men

	wage gap (medians) (1)	wage gap (means) (2)	excess mass (15^{th} perc.) (3)
1(age > 55)*1988	-0.050	0.069**	-0.019
	(0.037)	(0.031)	(0.020)
1(age > 55)*1989	-0.030	0.077**	-0.004
	(0.042)	(0.036)	(0.016)
1(age > 55)*1990	-0.038	0.089**	0.002
	(0.040)	(0.038)	(0.015)
1(age > 55)*1991	-0.047	0.069*	0.020
	(0.039)	(0.040)	(0.014)
1(age > 55)*1992	-0.069	0.016	-0.007
	(0.043)	(0.042)	(0.016)
1(age > 55)*1993	-0.067*	0.032	-0.014
	(0.040)	(0.038)	(0.017)
1(age > 55)*1994	-0.011	0.062*	-0.019
	(0.045)	(0.035)	(0.016)
1(age > 55)*1995	-0.106**	0.029	-0.017
	(0.045)	(0.031)	(0.017)
1(age > 55)*1996	0.019	0.087*	0.023
	(0.040)	(0.046)	(0.017)
1(age > 55)*1998	0.042	0.093***	0.023*
	(0.037)	(0.035)	(0.014)
1(age > 55)*1999	0.048	0.129***	0.035**
	(0.041)	(0.036)	(0.015)
1(age > 55)*2000	0.041	0.133***	0.034**
	(0.039)	(0.029)	(0.013)
1(age > 55)*2001	0.095**	0.181***	0.045***
	(0.047)	(0.039)	(0.015)
1(age > 55)*2002	0.137***	0.218***	0.032**
	(0.039)	(0.034)	(0.015)
1(age > 55)*2003	0.087**	0.204***	0.029*
	(0.040)	(0.035)	(0.015)
age group-metro area effects	Y	Y	Y
metro-year effects	Y	Y	Y
R-squared	0.92	0.89	0.96
N	1280	1280	1280

Notes: Data are from IMSS and ENEU baseline samples, collapsed to metro area/age group/year level. Wage gap (medians) is log median real daily net wage from ENEU minus log median post-tax daily wage from IMSS. Wage gap (means) is defined analogously, using means in place of medians. Excess mass is calculated as described in Section 2.5 and Figure 2.9. In calculating evasion measures, we pool ENEU data across quarters within year. *** 1%, ** 5%, * 10% level. See Section 2.4 and the Appendix for further details of data processing.

Table 2.6. Differential Effects of Pension Reform on Employment Gap, Men

	dep. var.: log(empl., ENEU) - log(empl., IMSS)	
	(1)	(2)
1(age > 55)*1988	-0.035	-0.035
	(0.100)	(0.090)
1(age > 55)*1989	0.040	0.040
	(0.104)	(0.087)
1(age > 55)*1990	0.065	0.065
	(0.097)	(0.091)
1(age > 55)*1991	0.100	0.100
	(0.109)	(0.098)
1(age > 55)*1992	0.044	0.044
	(0.100)	(0.083)
1(age > 55)*1993	0.090	0.090
	(0.092)	(0.076)
1(age > 55)*1994	-0.231**	-0.231***
	(0.101)	(0.082)
1(age > 55)*1995	0.017	0.017
	(0.108)	(0.093)
1(age > 55)*1996	0.003	0.003
	(0.102)	(0.092)
1(age > 55)*1998	0.042	0.042
	(0.104)	(0.092)
1(age > 55)*1999	0.027	0.027
	(0.106)	(0.096)
1(age > 55)*2000	-0.011	-0.011
	(0.094)	(0.084)
1(age > 55)*2001	0.009	0.009
	(0.105)	(0.098)
1(age > 55)*2002	0.087	0.087
	(0.103)	(0.089)
1(age > 55)*2003	0.033	0.033
	(0.091)	(0.080)
age group effects	Y	
age group-metro area effects	N	Y
metro-year effects	Y	Y
R-squared	0.55	0.68
N	1280	1280

Notes: Samples are IMSS and ENEU baseline samples, collapsed to metro area/age group/year level. *** 1%, ** 5%, * 10% level. See Section 2.4 and the Appendix for further details of data processing.

Table 2.7. Differential Effects of Pension Reform on ENEU Take-Home Wage, Men

	dep. var.: log daily net wage, ENEU				
	(1)	(2)	(3)	(4)	(5)
1(age > 55)*1988	-0.034	-0.058*	-0.055**	-0.059**	-0.060**
	(0.037)	(0.031)	(0.027)	(0.026)	(0.026)
1(age > 55)*1989	-0.010	-0.023	-0.041	-0.051*	-0.052*
	(0.040)	(0.034)	(0.030)	(0.030)	(0.030)
1(age > 55)*1990	-0.021	-0.040	-0.059**	-0.064**	-0.065**
	(0.040)	(0.034)	(0.029)	(0.029)	(0.029)
1(age > 55)*1991	-0.041	-0.038	-0.040	-0.050*	-0.052*
	(0.039)	(0.033)	(0.029)	(0.028)	(0.028)
1(age > 55)*1992	-0.040	-0.049	-0.054*	-0.061**	-0.059**
	(0.042)	(0.033)	(0.030)	(0.030)	(0.030)
1(age > 55)*1993	-0.068	-0.036	-0.063**	-0.067**	-0.070**
	(0.043)	(0.035)	(0.032)	(0.031)	(0.031)
1(age > 55)*1994	0.028	0.013	0.005	0.002	0.001
	(0.043)	(0.035)	(0.032)	(0.032)	(0.032)
1(age > 55)*1995	0.024	0.013	-0.010	-0.017	-0.022
	(0.043)	(0.036)	(0.032)	(0.032)	(0.031)
1(age > 55)*1996	0.056	0.045	0.033	0.021	0.018
	(0.043)	(0.035)	(0.031)	(0.030)	(0.030)
1(age > 55)*1998	0.037	0.009	-0.009	-0.016	-0.019
	(0.044)	(0.036)	(0.030)	(0.030)	(0.030)
1(age > 55)*1999	0.016	-0.043	-0.053*	-0.068**	-0.069**
	(0.040)	(0.032)	(0.028)	(0.028)	(0.028)
1(age > 55)*2000	-0.019	-0.077**	-0.065**	-0.072**	-0.072**
	(0.040)	(0.034)	(0.030)	(0.030)	(0.030)
1(age > 55)*2001	0.018	-0.057*	-0.055*	-0.061**	-0.062**
	(0.040)	(0.032)	(0.028)	(0.028)	(0.027)
1(age > 55)*2002	0.062	-0.031	-0.022	-0.026	-0.028
	(0.039)	(0.031)	(0.027)	(0.027)	(0.027)
1(age > 55)*2003	0.015	-0.038	-0.040	-0.049	-0.053*
	(0.044)	(0.037)	(0.033)	(0.032)	(0.032)
age group effects	Y	Y	Y	Y	Y
metro-year effects	Y	Y	Y	Y	Y
schooling effects	N	Y	Y	Y	Y
married indicator	N	Y	Y	Y	Y
occupation effects	N	N	Y	Y	Y
industry effects	N	N	N	Y	Y
firm-size effects	N	N	N	N	Y
R-squared	0.13	0.40	0.46	0.47	0.48
N	667566	667566	667566	667566	667566

Notes: Sample is ENEU baseline sample. Take-home wage is the post-payroll-tax net wage as reported on ENEU. Estimates use population sampling weights provided in ENEU dataset. Controls include sets of 9 schooling indicators, 22 occupation indicators, and/or 50 industry indicators, in addition to the sets of five age-group and firm-size indicators, in indicated columns; details of category definitions are in the Appendix. *** 1%, ** 5%, * 10% level.

2.10 Appendix: Additional Institutional Background

In this section, we provide additional details about IMSS, including information on the 1992 pension reform, the housing sub-account, and the pension simulation.

2.10.1 1992 Pension Reform

In an effort to restore financial stability to the IMSS system, the Mexican congress enacted a first attempt at pension reform in May 1992. This reform created a system of personal retirement accounts called the *Sistema de Ahorro para el Retiro* (SAR) to operate alongside the established PAYGO pension system. Employers contributed two percent of each worker's wage, depositing the contributions into a commercial bank of their own choosing. The commercial bank then transmitted the funds to the Mexican central bank. The central bank guaranteed a minimum two percent real return, and workers were supposed to receive a lump-sum payment at retirement equal to the accumulated balances. Several problems plagued the implementation of the SAR system, however. The scheme suffered from poor regulatory oversight and management. Commercial banks received low fees for administering the collection of employer contributions, weakening incentives for these banks to provide efficient record-keeping or enforce the mandatory contributions. In addition, workers were often unaware of the balances in their accounts or even of which bank held their accounts. This led to the creation of multiple accounts, especially for workers who changed employers; by 1997, over half of the 20 million accounts were duplicates (Grandolini and Cerda, 1998). As a result of these difficulties, the reform was widely considered a failure, with workers and employers viewing the reform as simply another payroll tax (Grandolini and Cerda, 1998; Aguila, 2011).

2.10.2 Housing Sub-Account

As an additional social-security benefit for workers, employers contribute five percent of a worker's wage, up to 10 times the applicable minimum wage, to the *Instituto del Fondo Nacional de la Vivienda de los Trabajadores* (INFONAVIT).[44] Employees can apply for housing loans through INFONAVIT. If approved, a worker can use the accumulated funds as a down payment on a house purchase; loan payments are subsequently deducted from employees' paychecks.

Prior to 1992, accumulated contributions in INFONAVIT not used toward housing were provided to the worker at the time of retirement. However, INFONAVIT only provided nominal accumulated contributions. Given the high inflation rates, the real value of nominal contributions was typically quite small. The 1992 IMSS reform sought to correct this problem by requiring that INFONAVIT provide workers with any unused contributions plus interest based on the operational surplus of the agency. In practice, however, INFONAVIT continued to suffer from several problems, including high rates of delinquency of loans, which limited the agency's ability to pay interest and resulted in a negative real rate of return (Grandolini and Cerda, 1998).

Following the 1997 reform, the INFONAVIT contributions are collected in a personal account managed by the same AFORE that manages an individual's retirement account (see the account statement in Figure 2.4). Workers who choose the PRA system at retirement receive the unused accumulated balances in their INFONAVIT account upon retirement. Transition workers that choose the PAYGO pension at retirement only receive the account balances that were accumulated between 1992 and 1997, thus forfeiting balances accumulated after 1997.

These changes in the housing account potentially made a substantial difference in the overall value of social security benefits. It is also worth noting that they tended to reinforce the differential change in incentives emphasized in the main text: the change in the administration of the housing

[44]Note that the INFONAVIT contribution is the component of contributions that uses the local minimum wage, as opposed to the minimum wage in Mexico City, to calculate the top-code.

accounts effectively made benefits more sensitive to reported wages, but only for those workers who in the end choose the PRA pension.

2.10.3 Pension Simulation

As described in the main text, we conduct a pension simulation, based on the simulation in Aguila (2011), in order to compare pension wealth at retirement under the PAYGO and the PRA systems. Here we provide additional details of the simulation.

For the PAYGO system, the monthly pension is calculated based on the benefit schedule in Table 2.A3, which was in effect from Jan. 1, 1991 to June 30, 1997. To calculate the final average wage (the average nominal wage in the five years before retirement), we assume a constant real wage and an annual inflation rate of 13.65 percent (the average annual inflation rate from 1988-2003, excluding the high-inflation years of 1988, 1989, 1995, and 1996). Under these assumptions, the final average wage is approximately 79 percent of the wage at retirement. Following Aguila (2011), we discount the value of the monthly pension benefits to the present assuming a discount rate of one percent.

Under the PRA regime, we calculate the total value of contributions over a worker's career, assuming a constant real wage.[45] We then determine the total value of accumulated wealth at retirement assuming a 8.59 percent annual return (the average return from 1998-2002, and the higher of two rates of return considered by Aguila (2011)).[46] We next calculate the schedule of monthly annuity payments equivalent in expected value to total pension wealth at retirement, assuming a life expectancy of 93 years for men — the life expectancy assumption used by IMSS, which includes an adjustment for expected survivor benefits paid out to widows or dependent

[45]For the PRA regime, we include housing account contributions, which assumes that workers do not use housing accounts for loans. Note that we do not include the housing account under the PAYGO system, as workers opting to take the PAYGO pension at retirement only receive housing account contributions accumulated between 1992 and 1997.

[46]These real interest rates are net of management fees charged by AFOREs.

children. As with the PAYGO pension, we discount the monthly pension assuming a one percent discount rate.

There are a number of thorny issues in inflation indexing in the simulation. Beginning in 1997, the minimum PRA pension was indexed to the Mexican Consumer Price Index (CPI) and the minimum PAYGO pension was indexed to the minimum wage in Mexico City. The real value of the minimum wage declined steadily over the 1997-2001 period, as it had been doing for many years (see Appendix Table 2.A4). The values of the minimum pensions under the two regimes thus diverged. In 2001, the Mexican congress passed a new law specifying that the CPI be used to index the PAYGO pension as well as the PRA pension and adjusting the PAYGO minimum pension to be equal to the PRA minimum pension. It is unclear what participants believed at the time of the reform about how the indexing would be carried out. Following Aguila (2011), we assume that participants expected a decline of 6.4 percent per year. At the same time, we assume that participants expected the minimum pension under the PAYGO regime to be the same as the minimum pension under the PRA regime, as the congress in fact subsequently legislated in 2001.

It is important to note that we have imposed a relatively optimistic assumption about future interest rates and a relatively pessimistic assumption about the decline in the real value of the minimum wage. Both tend to make the PRA pension appear relatively more attractive. We emphasize that the simulation is mainly to illustrate that younger workers are more likely to expect to opt for the PRA pension; we are not arguing that the pension reform made workers better off overall. Indeed, it seems clear from the simulation that workers with low wages who would have qualified for the minimum pension under the PAYGO system are generally worse off under the new regime.

In addition to simulating the pension wealth of workers who started working at age 25 and expect to continue working until age 60, but who are of different ages in 1997, as we present in

Table 2.1, we also simulate the pension wealth of a worker who entered the system on June 30, 1997, the day before the new personal retirement accounts (PRAs) went into effect. The results are presented in Table 2.A5. If the worker expects to contribute for at least 10 years (and thus be guaranteed the minimum pension), the basic message is the same as in the previous simulation: the PRA is more likely to dominate the PAYGO pension as one moves up and to the right in the table. A new worker earning below 200 pesos/day (approximately 80% of our sample) would need to contribute for at least 20 years, if not more, for the PRA pension to dominate the PAYGO pension.

2.11 Appendix: Theory

In this section, we develop in full the model that we have summarized in Section 2.3 of the main text.

2.11.1 Basic Set-up

Consider a setting with a competitive labor market populated by homogeneous workers and a continuum of heterogeneous, monopolistically competitive firms.[47] Let τ_f be the payroll tax statutorily imposed on the firm and τ_w the payroll tax statutorily imposed on workers; both are remitted by the firm and will enter similarly in our model.[48] Define $\tau = \tau_f + \tau_w$ and assume $0 < \tau < 1$.

Let w_r be the pre-tax wage reported by a firm to the government, and w_u the unreported wage, the wage paid to workers "under the table." The total wage paid by the firm is $w_f = w_r + w_u$. The net take-home wage received by workers is $w_{net} = w_u + (1 - \tau)w_r$. We assume in the theory

[47] It would be possible to incorporate different types of workers in our framework, along the lines, for instance, of Rothstein (2010). As long as evasion is worker-type-specific within the firm (i.e. as long as there is no internal equity constraint preventing firms from evading more for some groups of workers than for others) conceptually this exercise would be straightforward. In our view, however, it would complicate the exposition significantly with relatively little payoff in additional insight. We leave the development of a more general framework to future work.

[48] As pointed out by Slemrod (2008), in settings in which the costs of evasion by employers and employees differ, it is in general not irrelevant who remits the tax. But since in Mexico the statutory payroll taxes on both firms and workers are remitted by firms, in our setting there is no conceptually important distinction between them.

that w_r, w_u, and w_{net} are observable to workers.[49] Both the reported wage, w_r, and the net wage, w_{net}, correspond to quantities that are also in principle observable to the econometrician: w_r to the wages reported by firms in the administrative records of the social security agency, and w_{net} to the take-home pay reported by workers in the ENEU household survey. As mentioned in the main text, we do not observe w_{net} at the firm level, and hence cannot measure the unreported wage, $w_u = w_{net} - (1-\tau)w_r$, at the firm level, but we will be able to construct measures at the level of more aggregate cells.[50]

Future pension benefits depend on the reported wage, w_r. In the interests of simplicity, we impose an assumption of linearity on these benefits and let bw_r be the amortized per-period value of future pension benefits for each worker, where $b \geq 0$. We further assume that $b < \tau$, which corresponds to the Mexican institutional setting, where the tax payment includes contributions for health care as well as pension benefits. We refer to the wage inclusive of pension benefits received by each worker as the "effective" wage, w_e, where $w_e = w_{net} + bw_r = w_u + (1-(\tau - b))w_r$.

We assume that there is no stigma or other cost to workers of firms' under-reporting of their wages. Under this assumption, the effective wage is the wage relevant for workers' labor-supply decisions. We assume that the aggregate labor-supply function has constant elasticity:

$$L_{agg}^S = Bw_e^\rho \tag{A3}$$

where $\rho > 0$ and $B > 0$. This labor-supply function can be derived from maximization of a quasi-linear utility function for an individual choosing how many hours to devote to leisure versus wage

[49]In making the assumption that w_r, w_u, and w_{net} are all observable to the worker, we are following the main strand of the related theoretical literature (Yaniv, 1992; Kleven, Kreiner, and Saez, 2009), which presumes that employees collude in under-reporting. An alternative, plausible assumption would be that workers observe w_{net} costlessly but only observe w_r (and hence w_u) at a cost. The pension reform could then be modeled as reducing this cost, in addition to increasing the sensitivity of benefits to reported wages. Asymmetric information of this type would complicate the model considerably, and we leave the analysis of this case to future work.

[50]In the theory, we also abstract from the minimum wages in Mexico (of which there are three, depending on region).

work.[51] Note that we are not explicitly considering the extensive margin of labor supply or the lump-sum (i.e. independent of reported wage) benefits of participation in the social-security system. Such lump-sum benefits would affect employees' utility levels and participation decisions, but would not affect the evasion behavior that is the primary object of our analysis. An alternative approach to deriving the labor-supply elasticity (A3) would be to model individuals as choosing whether to supply labor to the formal sector (i.e. registered firms) or the informal sector (i.e. unregistered firms), as for instance in Marrufo (2001) or Galiani and Weinschelbaum (forthcoming). In this paper we focus on evasion within the formal sector, and leave the analysis of individuals' and firms' choices about whether to enter the formal sector to future work.

2.11.2 The Firm's Problem

We build on a standard model of heterogeneous firms under monopolistic competition, similar to Melitz (2003) but without international trade. Assuming a Dixit-Stiglitz (1977) representative consumer, the demand for each differentiated variety, ω, can be written:

$$x(\omega) = Ap(\omega)^{-\sigma} \tag{A6}$$

[51]Suppose that for each worker,

$$U(c, L) = u(c) - \beta L^{\frac{\rho+1}{\rho}} \tag{A4}$$

where c is consumption, L is hours supplied, and we assume $\beta > 0$, $\rho > 0$. Let the consumption good be the numeraire. The hours constraint is $L \leq \overline{L}$. As is standard in the literature on labor supply, we think of individuals as having "total income" $w_e\overline{L}$ and consuming leisure hours at a price w_e. Hence the budget constraint is:

$$c + (\overline{L} - L)w_e = w_e\overline{L} \tag{A5}$$

We assume that the hours constraint is not binding. Individual optimization then yields (A3), where $B = N\left(\frac{\rho}{\beta(\rho+1)}\right)^{\rho}$ and N is the number of workers in the workforce.

where $x(\omega)$ is the quantity consumed; $p(\omega)$ is the price; and σ is a parameter capturing the elasticity of substitution between varieties.[52] A captures the general level of demand, which individual firms treat as exogenous; in this partial-equilibrium framework, we abstract from the determinants of the level of demand and treat A as a parameter. We make the standard assumption that $\sigma > 1$. Firms are assumed to be heterogeneous in a productivity parameter, φ, with density $g(\varphi)$, with positive support over $[\varphi^{min}, \varphi^{max}]$ and zero support elsewhere.[53] There is assumed to be no cost of differentiation, and each firm differentiates and produces a distinct variety; hence φ also indexes varieties. There is a fixed cost of operation to be paid in each period, f. Each firm's production function is simply $x = \varphi L$, where L is labor input; this can be rewritten $L = \frac{x}{\varphi}$.

We assume that the cost of evasion is increasing in the unreported wage, w_u, and the output of the firm. In particular, we assume that the cost of evasion can be expressed in the multiplicatively separable form $xc(w_u)$, where $c(0) = 0$, $c'(w_u) > 0$, and $c''(w_u) > 0$.[54] As mentioned in the main text, there are a number of possible justifications for the assumption that costs of evasion are increasing in output. One is simply that auditors are more likely to audit larger firms because their operations are more visible, as suggested by Besley and Persson (2013, p. 66) — a conjecture that appears anecdotally to be relevant in Mexico.[55] Another is the argument of Kleven, Kreiner, and

[52]If the representative consumer has utility

$$U = \left[\int_{\omega \in \Omega} (x(\omega))^{\frac{\sigma-1}{\sigma}} \, d\omega \right]^{\frac{\sigma}{\sigma-1}} \tag{A7}$$

where Ω represents the set of all differentiated varieties available in the market, then optimization yields (A6) with $A = UP^{\sigma}$, where U is defined in (A7) and

$$P \equiv \left[\int_{\omega \in \Omega} (p(\omega))^{1-\sigma} \, d\omega \right]^{\frac{1}{1-\sigma}} \tag{A8}$$

[53]In Melitz (2003), firms do not know their productivity before paying a fixed cost to get a productivity draw. Here, as in Chaney (2008), we simply take the set of firms in business as given and abstract from entry and exit of firms. It would be straightforward to add the initial investment decision, but would not add substantively to our analysis.

[54]The assumption that the marginal cost of evasion incurs per unit of output is increasing is in the spirit of Slemrod (2001), who makes a similar assumption in the context of evasion by individuals.

[55]Also, as noted in footnote 26, employers with 300 or more employees have to submit an audit from a certified public accountant; this may be another reason why evasion is more costly for larger firms.

Saez (2009) that collusion in under-reporting is more difficult to sustain in larger firms.

The labor market is competitive, and firms are price-takers of the effective wage, w_e. The firm chooses the unreported wage, w_u; together w_e and w_u pin down w_r.[56] From the definitions of the wage variables above, the total wage paid by the firm is then $w_f = \frac{w_e-(\tau-b)w_u}{1-(\tau-b)}$. The firm also chooses the output price, p. Given the price, output, x, and hence labor demand, L, are pinned down by the firm-specific demand curve, (A6). Per-period profit for each firm can be written:

$$\pi(w_u, p; \varphi, w_e) = \left\{ p - \frac{w_e - (\tau - b)w_u}{\varphi(1 - (\tau - b))} - c(w_u) \right\} x - f \tag{A9}$$

The firm's problem is to choose w_u and p to maximize π.

The first order condition for the choice of w_u is:

$$c'(w_u) = \frac{\tau - b}{\varphi(1 - (\tau - b))} \tag{A10}$$

The left-hand side is the marginal cost of evasion and the right-hand side is the marginal benefit in the form of reduced tax payments, both per unit of output. Note that the solution to this equation, call it $w_u^*(\varphi)$, depends neither on the output price, p, nor on the market-determined effective wage, w_e. Note also that, given our assumptions on the $c(\cdot)$ function, in general we have that $w_u^*(\varphi) > 0$; we do not expect perfect compliance, even for highly productive (hence large in equilibrium) firms.

To derive an expression for labor demand, note first that the first order condition for price yields:

$$p^*(w_e, \varphi) = \left(\frac{\sigma}{\sigma - 1} \right) \left\{ \frac{w_e - (\tau - b)w_u^*(\varphi)}{\varphi(1 - (\tau - b))} + c(w_u^*(\varphi)) \right\} \tag{A11}$$

[56] We abstract from minimum wages, of which there are three in Mexico, depending on region. We consider the role of the minimum wages in the empirics below.

The term in brackets is simply marginal cost and, as usual with Dixit-Stiglitz demand, price is a fixed multiplicative mark-up over marginal cost. Given the optimal choices $w_u^*(\varphi)$ and $p^*(w_e, \varphi)$, the optimal output of the firm is given by:

$$x^*(w_e, \varphi) = Ap^*(w_e, \varphi)^{-\sigma} \tag{A12}$$

and the firm's labor demand is:

$$L^D(w_e, \varphi) = \frac{x^*(w_e, \varphi)}{\varphi} = \frac{Ap^*(w_e, \varphi)^{-\sigma}}{\varphi} \tag{A13}$$

Note that the firm's labor demand is decreasing in the effective wage, w_e, since price is increasing in w_e. Aggregate labor demand is the integral of firm-level labor demand (A13) over firms active in the market:

$$L^D_{agg}(w_e) = \int_{\varphi^{min}}^{\varphi^{max}} L^D(w_e, \varphi) g(\varphi) d\varphi \tag{A14}$$

Since we are assuming that the set of firms in the market stays fixed, and since each firm's labor demand is declining in the effective wage, w_e, we know that aggregate labor demand is also declining in w_e.

The equilibrium wage is the value of w_e that clears the labor market, i.e. that sets

$$L^S_{agg}(w_e) = L^D_{agg}(w_e) \tag{A15}$$

where aggregate labor supply, $L^S_{agg}(w_e)$, is given by (A3).

2.11.3 Evasion vs. Firm Size in Cross-Section

We now consider how the extent of evasion, as measured by the unreported wage, w_u, varies with firm size in cross-section, for a given effective wage w_e. Differentiating both sides of (A10) holding w_e fixed and rearranging, we have:

$$\frac{dw_u^*}{d\varphi} = -\frac{\tau - b}{\varphi^2 c''(w_u)(1-(\tau-b))} < 0 \tag{A16}$$

That is, evasion is decreasing in firm productivity.[57]

Firm output is unambiguously increasing in productivity. To see this, first note that price is decreasing in productivity; differentiating both sides of (A11) (again, holding w_e fixed) and using (A10), we have:

$$\frac{dp^*}{d\varphi} = -\left(\frac{\sigma}{\sigma-1}\right)\left\{\frac{w_e - (\tau-b)w_u^*(\varphi)}{\varphi^2(1-(\tau-b))}\right\} < 0 \tag{A17}$$

Prices are lower in higher-φ firms for the standard reason that labor costs are lower per unit of output and price is a fixed multiplicative mark-up over costs. Then from (A12):

$$\frac{dx^*}{d\varphi} = -\sigma A(p^*)^{-\sigma-1}\left(\frac{dp^*}{d\varphi}\right) > 0 \tag{A18}$$

Together (A16) and (A18) imply an unambiguously negative relationship between firm output and evasion.

The relationship between employment and productivity, and hence between employment and

[57] Given that the effective wage, w_e, is constant across firms, this immediately implies that the reported wage is increasing in productivity: using the fact that $w_r = (w_e - w_u)/(1-(\tau-b))$, we have:

$$\frac{dw_r^*}{d\varphi} = \frac{\tau - b}{\varphi^2 c''(w_u)(1-(\tau-b))^2} > 0$$

evasion, is more subtle. Differentiating (A13):

$$\frac{dL^D}{d\varphi} = -\frac{x^*(w_e,\varphi)}{\varphi^2} + \frac{1}{\varphi}\left(\frac{dx^*}{d\varphi}\right) \tag{A19}$$

Higher productivity leads firms to have greater output, which increases employment (the second term). But it also reduces the amount of labor required to produce a given level of output (the first term). Using (A11) and (A18)-(A19), it can be shown that labor demand will be increasing in productivity if and only if

$$\frac{c(w_u^*(\varphi))}{\sigma-1} < \frac{1}{\varphi}\left\{\frac{w_e-(\tau-b)w_u^*(\varphi)}{1-(\tau-b)}\right\} \tag{A20}$$

The term in brackets on the right-hand side is the total wage paid by the firm, w_f, and hence the right-hand side is labor cost per unit of output; $c(w_u^*(\varphi))$ is the cost of evasion per unit of output at the optimum. The condition thus requires that the equilibrium cost of evasion not be too large relative to labor costs. If enforcement were perfect, firms would set $w_u^*(\varphi) = 0$, condition (A20) would clearly be satisfied, and employment would be unambiguously increasing in productivity, as in Melitz (2003). But here the fact that the equilibrium cost of evasion per unit of output is positive dampens the responsiveness of output to productivity (since it raises prices and $(p^*)^{-\sigma-1}$ enters the expression for $\frac{dx^*}{d\varphi}$ in (A18)) and reduces the magnitude of the second term in (A19) relative to the first. In this context, it is theoretically possible that employment is declining in productivity. At the same time, previous work in Mexican data has found a positive correlation between employment and productivity (see Verhoogen (2008, Table A1)) and the positive correlation between size and productivity is robust across countries and datasets (see e.g. Foster, Haltiwanger, and Syverson (2008) for the U.S.). It seems clear that the empirically relevant case is the one in which (A20) holds. We will focus on this case hereafter. In this case, the extent of evasion is declining in employment, as it is in output.

2.11.4 Response of Evasion to Pension Reform

We now consider the response of evasion to the pension reform, which we model as an increase in the parameter b relating the reported wage to the amortized per-period pension benefits. Here we allow the equilibrium effective wage, w_e, to vary endogenously in response to the policy change. Differentiating (A10) with respect to b and rearranging:

$$\frac{dw_u^*}{db} = -\frac{1}{(1-(\tau-b))^2 \varphi c''(w_u^*(\varphi))} < 0 \tag{A21}$$

The unreported wage unambiguously decreases within a given firm. Note that $\frac{dw_u^*}{db}$ is firm-specific, but since the effect is negative for all values of φ, the aggregate effect will be negative for a given set of firms over time.[58]

It is worth emphasizing that the response of $w_u^*(\varphi)$ to the policy change here does not depend on the market-determined effective wage, w_e, or the incidence of the policy change on that wage. In this sense, the model suggests that it is not unreasonable to examine the effect of the policy change on evasion separately from the question of incidence, which is how we proceed in the empirical analysis.

[58] Given the discussion of cross-sectional patterns in Section 2.11.3, it is interesting to consider the heterogeneous effects of the pension reform by firm size. The sign of this cross-partial depends on the third derivative of the cost-of-evasion function. Formally,

$$\frac{d}{d\varphi}\left(\frac{dw_u^*}{db}\right) = \frac{\varphi c'''(w_u^*(\varphi))\frac{dw_u^*}{d\varphi} + c''(w_u^*(\varphi))}{\left[(1-(\tau-b))\varphi c''(w_u^*(\varphi))\right]^2} \tag{A22}$$

where $\frac{dw_u^*}{d\varphi}$ is given by (A16). Without imposing further structure on the cost of evasion, we do not have a clear prediction for a differential response by firm size.

2.11.5 Incidence of Pension Reform on Wages

Now consider the effect of the reform on the effective wage, w_e. Differentiating both sides of the labor market equilibrium condition (A15), and using the fact that the integrand and its derivatives are continuous to pass the derivative through the integral in (A14),[59] we have:

$$\frac{dw_e}{db} = \frac{\int_{\varphi^{min}}^{\varphi^{max}} [w_r^*(w_e, \varphi)] \frac{(p^*)^{-\sigma-1}}{\varphi^2} g(\varphi) d\varphi}{\frac{1-\tau+b}{\sigma A} \left(\frac{\sigma-1}{\sigma}\right) \rho B w_e^{\rho-1} + \int_{\varphi^{min}}^{\varphi^{max}} \frac{(p^*)^{-\sigma-1}}{\varphi^2} g(\varphi) d\varphi} \tag{A23}$$

where $w_r^*(w_e, \varphi)$ is the optimal reported wage corresponding to $w_u^*(\varphi)$ (i.e. $w_r^*(w_e, \varphi) = (w_e - w_u^*(\varphi))/(1 - \tau + b)$.)

The incidence depends in part on the labor-supply elasticity, ρ. In the limit as the labor supply elasticity becomes infinite (and hence B also becomes infinite), we have $\lim_{\rho \to \infty} \frac{dw_e}{db} = 0$. For finite positive values of ρ, we have $\frac{dw_e}{db} > 0$. That is, for a finite labor-supply elasticity, some of the increase in pension benefits will redound to workers. The upper bound on the incidence on workers is obtained as labor supply becomes perfectly inelastic:

$$\lim_{\rho \to 0} \frac{dw_e}{db} = \int_{\varphi^{min}}^{\varphi^{max}} \mu(\varphi) [w_r^*(w_e, \varphi)] g(\varphi) d\varphi \equiv \overline{w}_r^*(w_e) \tag{A24}$$

where

$$\mu(\varphi) = \frac{\left(\frac{(p^*)^{-\sigma-1}}{\varphi^2}\right)}{\int_{\varphi^{min}}^{\varphi^{max}} \left(\frac{(p^*)^{-\sigma-1}}{\varphi^2}\right) g(\varphi) d\varphi} \tag{A25}$$

That is, for a given set of active firms, the upper bound on $\frac{dw_e}{db}$ is equal to a weighted average of the equilibrium reported wages across firms, with weights $\mu(\varphi)$.

Now consider the effect of the pension reform of the (firm-specific) reported wage and net wage.

[59] See e.g. Bartle (1976, Theorem 31.7).

Formally, using (A22), and (A23), it can be shown that,

$$\frac{dw_r^*}{db} = \frac{1}{\varphi c''(w_u^*(\varphi))(1-\tau+b)^2} + \frac{1}{1-\tau+b}\left\{\frac{dw_e}{db} - w_r^*(w_e,\varphi)\right\} \tag{A26}$$

$$\frac{dw_{net}^*}{db} = -\frac{b}{\varphi c''(w_u^*(\varphi))(1-\tau+b)} + \frac{1-\tau}{1-\tau+b}\left\{\frac{dw_e}{db} - w_r^*(w_e,\varphi)\right\} \tag{A27}$$

In general, without imposing additional assumptions on the distribution of firm productivities and/or other parameters, the signs of the responses are ambiguous.

There is a special case in which the response of the net wage can be signed, namely when firms are homogeneous. In the limit as $\rho \to 0$, which yields the upper bound on $\frac{dw_{net}^*}{db}$,

$$\lim_{\rho\to 0}\frac{dw_{net}^*}{db} = -\frac{b}{\varphi c''(w_u^*(\varphi))(1-\tau+b)} + \frac{1-\tau}{1-\tau+b}\left\{\overline{w}_r^*(w_e) - w_r^*(w_e,\varphi)\right\} \tag{A28}$$

where $\overline{w}_r^*(w_e)$ is from (A24). When firms are homogeneous, the term in brackets on the right-hand side is zero and we have:

$$\frac{dw_{net}^*}{db} < -\frac{b}{\varphi c''(w_u^*(\varphi))(1-\tau+b)} < 0 \tag{A29}$$

In this case, we have a clear prediction that the net wage will fall in response to the pension reform.

When firms are heterogeneous, however, the term in brackets on the right-hand side of (A28) may be sufficiently positive that the upper bound is positive and hence the sign of $\frac{dw_{net}^*}{db}$ remains ambiguous. Intuitively, the strength of the reduction in $w_{net}^*(\varphi)$ depends on the firm's own $w_r^*(w_e,\varphi)$, while the strength of the increase in the equilibrium effective wage, w_e, depends on the aggregate increase in labor demand, which is related to the weighted-average reported wage, $\overline{w}_r^*$. For lower-productivity firms with relatively low w_r^*, the latter may dominate the former.[60]

For the reported wage, there is an additional reason for ambiguity in the sign of the incidence

[60] In principle, it is also possible to sign an average effect on $w_{net}^*(w_e,\varphi)$. Using the $\mu(\varphi)$ weights from (A25),

effect. On one hand, a higher b means that the government offers a greater benefit for a given w_r, which allows the firm to reduce its wage payment conditional on a given effective wage, w_e. On the other hand, the reform induces the firm to report a greater share of the total wage payment (i.e. raise w_r relative to w_u). The $\frac{dw_r^*}{db}$ term cannot be signed, even under the assumption of homogeneous firms.

Summarizing this subsection, we have no clear predictions for the incidence of the pension reform on the observable wage variables, w_{net} and w_r. At the same time, we emphasize again that the predictions for the response of evasion to the reform do not depend on the incidence.

2.12 Appendix: Data

2.12.1 IMSS Administrative 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, non-petroleum-sector employers and their workforces for 1985-2005.[61] (Public-sector workers and employees of the state-run oil company are covered by other insurance programs.) The IMSS data contain information on

define:

$$\overline{\frac{dw_{net}^*}{db}} = \int_{\varphi^{min}}^{\varphi^{max}} \mu(\varphi) \left[\frac{dw_{net}^*}{db}\right] g(\varphi) d\varphi \tag{A30}$$

Then the upper bound is:

$$\lim_{\rho \to 0} \overline{\frac{dw_{net}^*}{db}} = -\int_{\varphi^{min}}^{\varphi^{max}} \mu(\varphi) \left[\frac{b}{\varphi c''(w_u^*(\varphi))(1-\tau+b)}\right] g(\varphi) d\varphi < 0 \tag{A31}$$

and we can conclude that $\overline{\frac{dw_{net}^*}{db}} < 0$. The $\mu(\varphi)$ weights will not be observable, however, and it will not be feasible in our data to construct this average effect.

[61] Generally an employer identification number in the data refers to an establishment, rather than a firm. However, more than one establishment can be associated with a single identification number. We do not have access to information on which establishments belong to which firms, and throughout the paper we treat each employer with a separate identification number as a separate firm.

the daily wage of individuals. The wages are a measure of total compensation, called the *salario base de cotización*, which includes earnings and benefits, including payments made in cash, bonuses, commissions, room and board, overtime payments, and in-kind benefits.[62] 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 June 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 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* corresponding to permanent workers for which consistent, reliable wage figures are available.[63] (2) We require that an individual have a positive wage. (3) We treat workers in single-worker establishments as self-employed and exclude them.[64] (4) If two observations appear for the same individual, we select the standard *modalidad*[65] and the highest-wage observation within it. (5) We require that individuals be 16 years or older and 65 years or younger. (6) We drop observations with missing sex, industry, or location. (7) We drop workers outside of non-petroleum manufacturing, construction, retail, restaurants, and hotels, as explained in the main text. (8) We include workers employed by firms located in the original 16 metropolitan areas sampled in the ENEU (described below).

[62] In principle, the wage reports should include the *aguinaldo*, the holiday bonus that employers are required to pay each December, on a pro-rated basis throughout the year. IMSS has no means to enforce this requirement for wages above the minimum reportable wage, however, and it is possible that some establishments do not comply.

[63] In the internal classification system, we use *modalidades* 10, 13, and 17. 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.

[64] Including establishments with single workers yields similar results to those reported in the main text.

[65] The order of priority of *modalidades* we impose is 10, 13, 17. See footnote 63.

Over time, IMSS raised the maximum reported income in conjunction with the maximum taxable income. Initially, the maximum reported income was set at 10 times the minimum wage; this was increased to 18 times the minimum wage in 1993 and to 25 times the minimum wage in 1994. The lowest real value for the maximum reported income occurred in 1991; we impose a topcode equal this value to ensure comparability across years. Prior to 1991, IMSS allowed firms to report wages below the relevant minimum wage, and in other years a very small number of observations have wages below the minimum wage. We drop all observations with real daily wages below 30 pesos; observations with wages greater than 30 pesos but lower than the minimum wage are kept unchanged.

2.12.2 ENEU Household Data

In the *Encuesta Nacional de Empleo Urbano (ENEU)*, households are interviewed quarterly for five quarters, and then rotate out of the sample. The original ENEU sample focused on the 16 largest Mexican metropolitan areas.[66] Over time, the coverage of metropolitan areas expanded but we focus on the original 16 metropolitan areas in order to maintain a consistent sample for as many years as possible.[67]

Wages reported in the ENEU are based on the take-home (after-tax) pay reported by respondents, and include bonuses that workers receive on a regular basis (monthly or more frequently).[68]

[66] The 16 metropolitan areas are: Mexico City, Guadalajara, Monterrey, Puebla, Leon, Torreon, San Luis Potosi, Merida, Chihuahua, Tampico, Orizaba, Veracruz, Ciudad Juarez, Tijuana, Matamoros and Nuevo Laredo. Mexico also had a nationally representative survey that covered rural areas, the *Encuesta Nacional de Empleo (ENE)*, but until it was combined with the ENEU in 2000 it was carried out at less regular intervals: 1991, 1993, 1995, 1996, 1997, 1998, and 1999. Beginning in 2000, the combined ENEU/ENE survey was referred to as the *Encuesta Nacional de Empleo Trimestral (ENET)* and following a redesign in 2004 it has been known as the *Encuesta Nacional de Ocupación y Empleo (ENOE)*.

[67] The 16 metropolitan areas gradually expanded to include additional municipalities over our sample period. We include the new municipalities (and include establishments in the IMSS data in the new municipalities.)

[68] The wording of the relevant question (7a on 1994 survey) is: "En el trabajo principal de la semana pasada ... ¿[c]uánto ganó o en cuánto calcula sus ingresos? (Asegúrese de que la cantidad sea lo que la persona recibe efectivamente.)" Translation: "In the job you worked last week ... how much did you earn or how much do you calculate your income to be? (Assure yourself that the quantity is what the person actually received.)" The phrase in parentheses is an instruction to enumerators. The manual for enumerators goes into greater detail (p. 225): "Para los trabajadores a sueldo, marcará la opción que corresponda al periodo de pago y la cantidad que debe anotar es el

This measure differs from the wage measure in the IMSS administrative records in that it excludes workers' social security contributions and other taxes as well as the *aguinaldo*, the yearly holiday bonus employers are required to pay in December of each year.

The hourly wage figures were constructed as follows. (1) We recovered monthly wages for the job worked last week (as converted from hourly, daily, weekly or bi-weekly basis by INEGI enumerators). For a small number of workers, wages were reported in wage categories relative to the minimum wage; we calculated monthly wages for these workers using the midpoint of the wage categories.[69] Individuals who reported not working in the previous week or who were missing wage information 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. Workers with missing weekly hours were dropped. (3) The hourly wage was calculated as monthly wage divided by monthly hours. (4) The daily wage was calculated by multiplying the hourly wage by eight. (5) The wage was deflated to constant 2002 pesos using the main consumer price index (INPC) from *Banco de Mexico*, the Mexican central bank. (6) We imposed a topcode equal to the real value of the IMSS topcode in 1991 and dropped workers with a real daily wage below 30 pesos (approximately US$3).

The ENEU asks individuals about their main job as well as any secondary employment. We focus solely on the main job reported in the ENEU. We follow sample selection criteria similar to those we impose on the IMSS. Our baseline ENEU sample includes men ages 16-65 working

INGRESO NETO, lo que realmente recibió y pudo disponer la última vez que le pagaron en su trabajo, es decir, no debe anotar los descuentos hechos por impuestos, cuotas al seguro social, ... " Translation: "For salaried workers, mark the pay period and the quantity that should be noted is the NET INCOME, that which the respondent really received and could spend the last time he/she was paid, that is to say, you should not include taxes, contributions to social security ..."

[69] Prior to 1994, the categories were (for some wage w and the relevant minimum wage MW): $w < .25 * MW$; $.25 * MW \leq w < .5 * MW$; $.5 * MW < w \leq 1 * MW$; $1 * MW < w \leq 2 * MW$; $2 * MW < w \leq 3 * MW$; $3 * MW < w \leq 5 * MW$; $5 * MW < w \leq 10 * MW$; and $10 * MW < w$. In 1994, the survey combined the first three categories into a single "less than the minimum wage" category and added a category of greater than 20 times the minimum wage.

in non-petroleum manufacturing, construction, retail, restaurants, and hotels. We also drop self-employed workers, owners, and unpaid workers (retaining workers who receive a fixed wage, work for a commission, or work in a cooperative). The sample thus constructed is referred to as the "full ENEU sample" in Table 2.2. As explained in Section 2.4, in our ENEU baseline sample we focus on workers who report receiving IMSS through their main employment and who work full-time, defined as at least 35 hours in the last week.

When we include indicators for schooling, occupation, and industry in regressions using ENEU data only (i.e. Table 2.7), the sets of indicators are defined as follows. For schooling, we generate 9 categories: less than primary education (< 6 years), primary (6 years), some secondary (corresponds to junior high school, 7-8 years), secondary (9 years), some preparatory (corresponds to high school, 10-11 years), preparatory (12 years), some college (13-15 years), college (16 years), and more than college (> 16 years). The occupation definitions used by the ENEU changed over the study period. To maintain consistency across years, we generate the following 22 categories (the numbers in the parentheses correspond to occupation categories from the *Clasificación Mexicana de Ocupaciones (CMO)* 1996): professionals (11), technical and specialized personnel (12), educators (13), artists/entertainers/athletes (14), CEOs/directors (211), managers/administrators not including CEOs/directors (21, not including 211), agricultural workers (41), production supervisors (51), production workers (52), machine operators (53), production assistants (54), drivers and drivers' assistants (55), supervisors in administration/services (61), secretaries (620), administrative assistants, not including secretaries (62, not including 620), salespeople with fixed establishments (71), salespeople without fixed establishment *(ambulantes)* (72), bellhops, gardeners, janitors (812), providers of personal services, excluding bellhops, gardeners, janitors (81, not including 812), domestic workers (82), security guards/law enforcement/military (83), others (99).[70] For industry,

[70]The occupational categories from the CMO used by the ENEU from 1987-1994 are the following: professionals (11), technical and specialized personnel (12), educators (13), artists/entertainers/athletes (14), CEOs/directors

we include indicators at the two-digit level using the *Clasificación de Actividad Económica*, the industry classification used by the ENEU; there are 50 two-digit industries in the broad sectors we focus on.

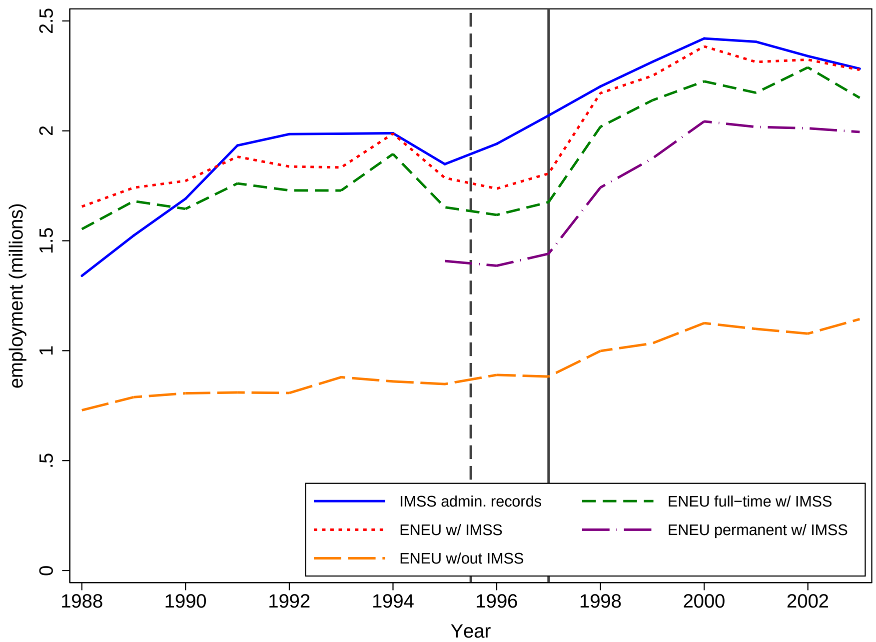
2.13 Appendix: Results for Women

Figures 2.A1-2.A6 and Tables 2.A6-2.A11 present results for women analogous to the results for men in the main text. They can be easily summarized. Figures 2.A2-2.A5 and Table 2.A8 indicate that the cross-sectional patterns we observe for men largely apply to women as well. Tables 2.A9-2.A11 indicate that the difference-in-difference results, by contrast, are not robust. As discussed in the text, there are a number of possible reasons for this. Women's labor force participation changed relatively rapidly over the study period and that many women receive IMSS benefits through their spouses, which provides an incentive to remain in the informal sector. In addition, because of relatively low labor force participation by older women, sample sizes in the ENEU household survey are often inadequate, especially when analyzing the data separately by metropolitan area (or metropolitan area, firm size and sector).

(220), managers/administrators not including CEOs/directors (21, 22, 31 not including 220), agricultural workers (41, 42, 431, 439, 526), production supervisors (51), production workers (520, 522, 523, 527), machine operators (521,524,525), production assistants (53, 529), drivers and drivers' assistants (430, 83), supervisors in administration/services (610), secretaries (611), administrative assistants, not including secretaries (612, 613, 614, 615, 616, 617, 618, 619), salespeople with fixed establishments (71), salespeople without fixed establishment *(ambulantes* (72), bellhops, gardeners, janitors (812), providers of personal services, excluding bellhops, gardeners, janitors (81, not including 812), domestic workers (82),security guards/law enforcement/military (84), others (99).

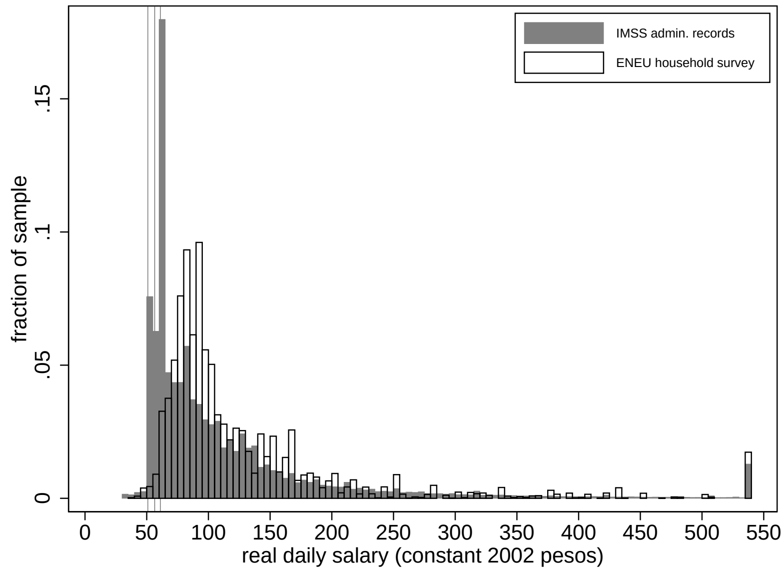
2.14 Appendix: Figures

Figure 2.A1. Employment, IMSS Admin. Records vs. ENEU household data, Women



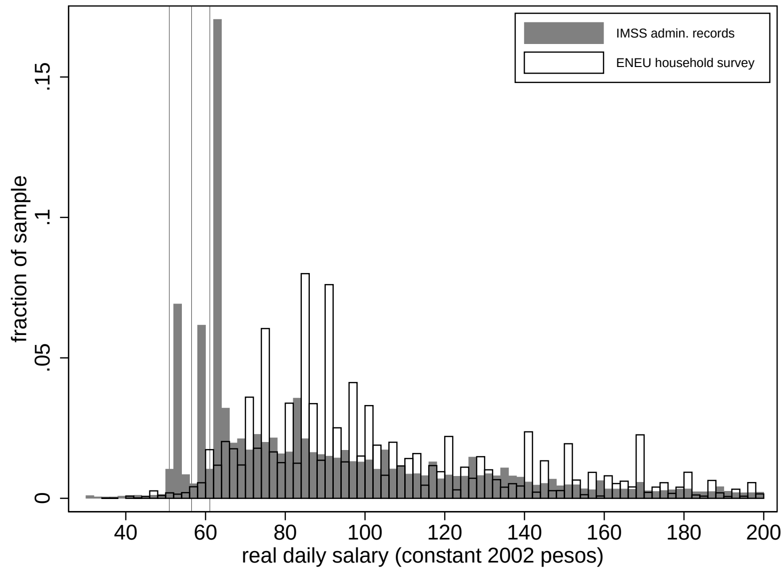
Notes: Samples are the same as those in Columns 1 and 3-6 of Table 2.A6; refer to that table for details. ENEU totals are calculated using sampling weights. The dashed vertical line indicates the date the pension reform was passed by Congress (Dec. 21, 1995); the solid vertical line indicates the date the reform took effect (July 1, 1997). Observations correspond to the second quarter of each year. See Section 2.4 and the Appendix for details of sample selection.

Figure 2.A2. Wage Histograms, Women, 1990



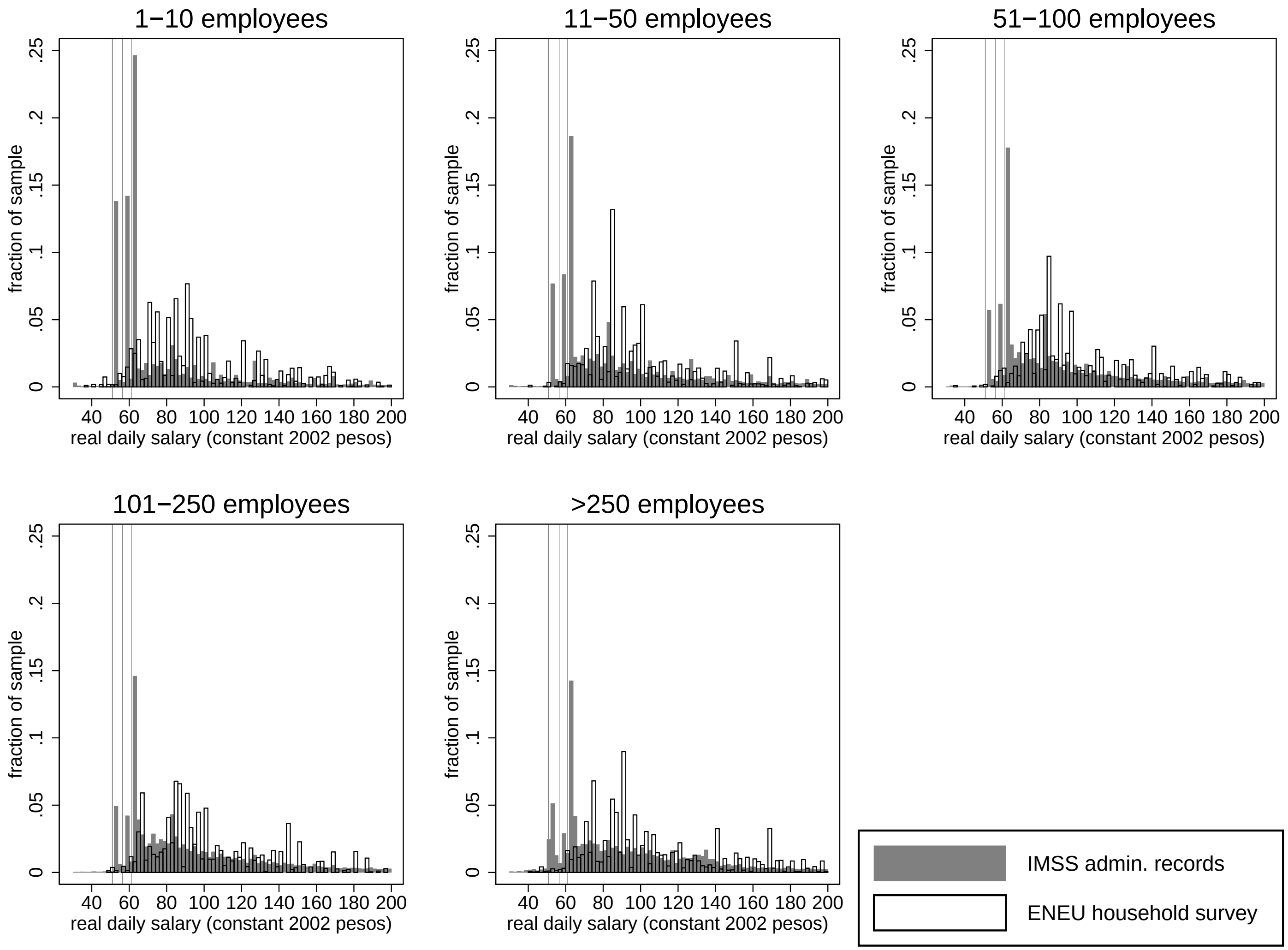
Notes: The ENEU wage is the real daily take-home wage reported to the ENEU household survey. The IMSS wage is the real daily *pre-tax* reported wage from the IMSS administrative records. Wages in 2002 pesos. Average 2002 exchange rate: 9.66 pesos/dollar. Samples are IMSS and ENEU baseline female samples. Data in IMSS sample are from second quarter of 1990, in ENEU sample from all four quarters of 1990 pooled. Vertical lines indicate minimum wages in the three minimum-wage zones in Mexico (A, B, C). Bins are 5 pesos wide. The rightmost bin captures all individuals with reported wages at or above the minimum IMSS topcode over the study period (from 1991). See Section 2.4 and the Appendix for further details.

Figure 2.A3. Wage Histograms, Women, 1990, Low Wage Levels



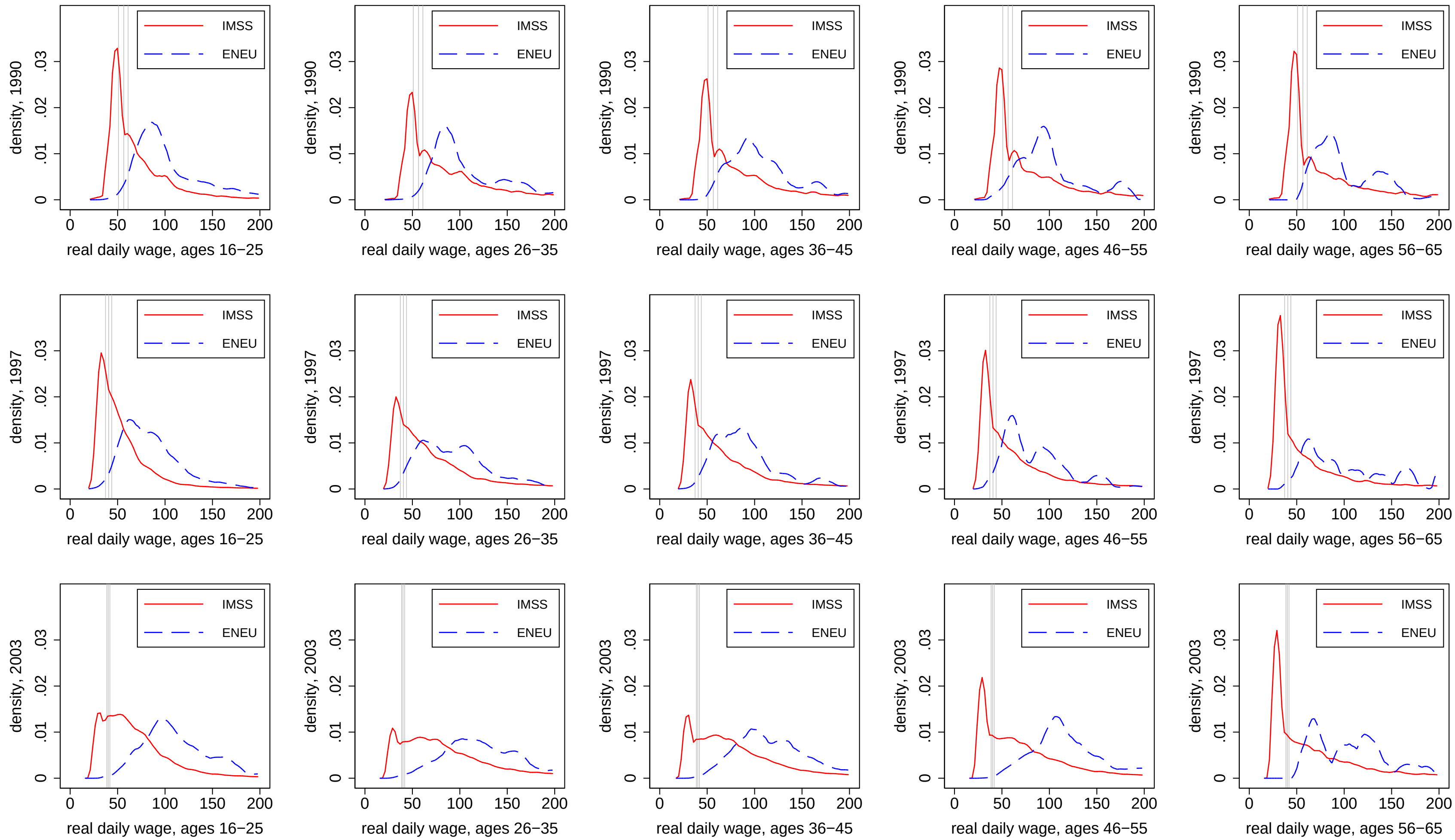
Notes: Histogram is similar to Figure 2.6 but only includes workers with wages less than 200 pesos/day (approx. $20/day) in constant 2002 pesos. Bins are 2 pesos wide.

Figure 2.A4. Wage Histograms by Firm Size, Women, 1990, Low Wage Levels



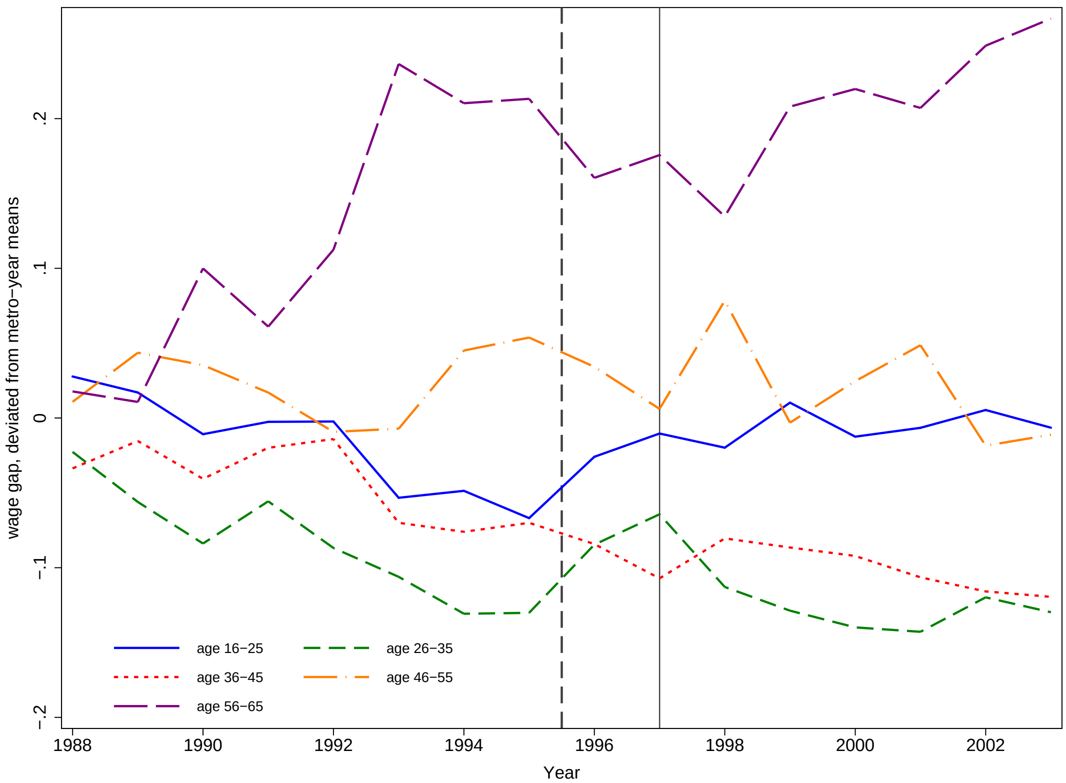
Notes: Histograms are similar to those in Figure 2.7. Vertical lines indicate minimum wages in the three minimum-wage zones in Mexico (A, B, C). Bins are 2 pesos wide. Average 2002 exchange rate: 9.66 pesos/dollar. See Section 2.4 and the Appendix for further details.

Figure 2.A5. Wage Densities by Age Group, 1990, 1997, 2003, Women



Notes: The wage variables are the real daily take-home wage from ENEU and real daily *post-tax* reported wage from IMSS. Densities are estimated using an Epanechnikov kernel with bandwidth 3 pesos for IMSS data and 6 pesos for ENEU data (using Stata kdensity command). Wages are in 2002 pesos. Average 2002 exchange rate: 9.66 pesos/dollar. Rows correspond to years 1990, 1997, 2003; columns to age groups 16-25, 26-35, 36-45, 46-55, 56-65. Samples are IMSS and ENEU baseline female samples, only including workers with wages less than 200 pesos/day. IMSS data are from second quarter; ENEU data are pooled across quarters within specified year. See Section 2.4 and the Appendix for further details.

Figure 2.A6. Wage Gaps (Medians) by Age Group, Women, Deviated from Metro-Year Means



Notes: Each wage gap is the difference between the log median net wage from the ENEU survey and the log median post-tax reported wage from the IMSS administrative records, using the ENEU and IMSS baseline female samples. To calculate deviated wage gaps, we calculate wage gaps separately by age group-year-metro area, regress them on a full set of metro area-year dummies, and average the residuals at the age-group level. The dashed vertical line indicates the date the pension reform was passed by Congress (Dec. 21, 1995); the solid vertical line indicates the date the reform took effect (July 1, 1997). See Section 2.4 and the Appendix for details of sample selection.

2.15 Appendix: Tables

Table 2.A1. IMSS Contribution Rates, Pensions and Child Care

	General pension fund				Personal retirement account				Childcare			
	E	W	G	TC	E	W	G	TC	E	W	G	TC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Jan 01 1973 - Jun 28 1986	3.750	1.500	0.750	10	0.000	0.000	0.000	10	1.000	0.000	0.000	10
Jun 29 1986 - Jan 04 1989	4.200	1.500	0.300	10	0.000	0.000	0.000	10	1.000	0.000	0.000	10
Jan 05 1989 - Dec 31 1990	4.200	1.500	0.300	10	0.000	0.000	0.000	10	1.000	0.000	0.000	10
Jan 01 1991 - Dec 31 1991	4.900	1.750	0.350	10	0.000	0.000	0.000	10	1.000	0.000	0.000	10
Jan 01 1992 - Apr 30 1992	5.040	1.800	0.350	10	0.000	0.000	0.000	10	1.000	0.000	0.000	10
May 01 1992 - Dec 31 1992	5.040	1.800	0.360	10	2.000	0.000	0.000	25	1.000	0.000	0.000	10
Jan 01 1993 - Jul 20 1993	5.180	1.850	0.370	10	2.000	0.000	0.000	25	1.000	0.000	0.000	10
Jul 21 1993 - Dec 31 1993	5.180	1.850	0.370	10	2.000	0.000	0.000	25	1.000	0.000	0.000	18
Jan 01 1994 - Dec 31 1994	5.670	2.025	0.405	10	2.000	0.000	0.000	25	1.000	0.000	0.000	25
Jan 01 1995 - Dec 31 1995	5.810	2.075	0.415	10	2.000	0.000	0.000	25	1.000	0.000	0.000	25
Jan 01 1996 - Jun 30 1997	5.950	2.125	0.425	10	2.000	0.000	0.000	25	1.000	0.000	0.000	25
Jul 01 1997 - Jun 30 1998	2.800	1.000	0.200	15	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 1998 - Jun 30 1999	2.800	1.000	0.200	16	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 1999 - Jun 30 2000	2.800	1.000	0.200	17	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 2000 - Jun 30 2001	2.800	1.000	0.200	18	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 2001 - Jun 30 2002	2.800	1.000	0.200	19	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 2002 - Jun 30 2003	2.800	1.000	0.200	20	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 2003 - Jun 30 2004	2.800	1.000	0.200	21	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 2004 - Jun 30 2005	2.800	1.000	0.200	22	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 2005 - Jun 30 2006	2.800	1.000	0.200	23	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 2006 - Jun 30 2007	2.800	1.000	0.200	24	5.150	1.125	0.225	25	1.000	0.000	0.000	25
Jul 01 2007 - .	2.800	1.000	0.200	25	5.150	1.125	0.225	25	1.000	0.000	0.000	25

Notes: Numbers represent contribution rates as a percentage of a worker's daily wage. E = employer contribution, W = worker contribution, G = government contribution, and TC = top-code, representing the maximum taxable income for the purposes of each calculation, as a multiple of the minimum wage in Mexico City. The "General pension fund" columns (columns 1-4) indicate contributions to the general IMSS pension fund; The "Personal retirement account" columns (columns 5-8) indicate contributions to individuals' personal retirement accounts. See Table 2.A2 for details on health care contributions.

Table 2.A2. IMSS Contribution Rates, Health Care

	Variable component				Fixed component				Additional for wage>3xMW			
	E	W	G	TC	E	W	G	TC	E	W	G	TC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Jan 01 1973 - Jun 28 1986	5.625	2.250	1.125	10	0.000	0.000	0.000	10	0.000	0.000	0.000	10
Jun 29 1986 - Jan 04 1989	6.300	2.250	0.450	10	0.000	0.000	0.000	10	0.000	0.000	0.000	10
Jan 05 1989 - Dec 31 1990	8.400	3.000	0.600	10	0.000	0.000	0.000	10	0.000	0.000	0.000	10
Jan 01 1991 - Dec 31 1991	8.400	3.000	0.600	10	0.000	0.000	0.000	10	0.000	0.000	0.000	10
Jan 01 1992 - Apr 30 1992	8.400	3.000	0.600	10	0.000	0.000	0.000	10	0.000	0.000	0.000	10
May 01 1992 - Dec 31 1992	8.400	3.000	0.600	10	0.000	0.000	0.000	10	0.000	0.000	0.000	10
Jan 01 1993 - Jul 20 1993	8.400	3.000	0.625	10	0.000	0.000	0.000	10	0.000	0.000	0.000	10
Jul 21 1993 - Dec 31 1993	8.750	3.125	0.625	18	0.000	0.000	0.000	18	0.000	0.000	0.000	18
Jan 01 1994 - Dec 31 1994	8.750	3.125	0.625	25	0.000	0.000	0.000	25	0.000	0.000	0.000	25
Jan 01 1995 - Dec 31 1995	8.750	3.125	0.625	25	0.000	0.000	0.000	25	0.000	0.000	0.000	25
Jan 01 1996 - Jun 30 1997	8.750	3.125	0.625	25	0.000	0.000	0.000	25	0.000	0.000	0.000	25
Jul 01 1997 - Jun 30 1998	0.700	0.250	0.050	25	13.900	0.000	13.900	25	6.000	2.000	0.000	25
Jul 01 1998 - Jun 30 1999	0.700	0.250	0.050	25	14.550	0.000	13.900	25	5.510	1.840	0.000	25
Jul 01 1999 - Jun 30 2000	0.700	0.250	0.050	25	15.200	0.000	13.900	25	5.020	1.680	0.000	25
Jul 01 2000 - Jun 30 2001	0.700	0.250	0.050	25	15.850	0.000	13.900	25	4.530	1.520	0.000	25
Jul 01 2001 - Jun 30 2002	0.700	0.250	0.050	25	16.500	0.000	13.900	25	4.040	1.360	0.000	25
Jul 01 2002 - Jun 30 2003	0.700	0.250	0.050	25	17.150	0.000	13.900	25	3.550	1.200	0.000	25
Jul 01 2003 - Jun 30 2004	0.700	0.250	0.050	25	17.800	0.000	13.900	25	3.060	1.040	0.000	25
Jul 01 2004 - Jun 30 2005	0.700	0.250	0.050	25	18.450	0.000	13.900	25	2.570	0.880	0.000	25
Jul 01 2005 - Jun 30 2006	0.700	0.250	0.050	25	19.100	0.000	13.900	25	2.080	0.720	0.000	25
Jul 01 2006 - Jun 30 2007	0.700	0.250	0.050	25	19.750	0.000	13.900	25	1.590	0.560	0.000	25
Jul 01 2007 - .	0.700	0.250	0.050	25	20.400	0.000	13.900	25	1.100	0.400	0.000	25

Notes: E = employer contribution, W = worker contribution, G = government contribution, and TC = top-code, representing the maximum taxable income for the purposes of each calculation, as a multiple of the minimum wage in Mexico City. The “Variable component” columns (columns 1-4) indicate contribution rates as a percentage of a worker’s daily wage. The “Fixed component” columns (columns 5-8) indicate fixed contribution rates for each worker, shown as a percentage of the minimum wage in Mexico City. The variable and fixed components (columns 1-8) must be paid for all workers. The “Additional for wage>3xMW” columns (columns 9-12) indicates additional contributions as a percentage of a worker’s daily wage exceeding three times the minimum wage in Mexico City, applicable only for workers whose daily wage exceeds that value. See Table 2.A1 for details on pension and child care contributions.

Table 2.A3. IMSS Pension Benefit Schedule, 1/1/1991 to 6/30/1997

Wage as multiple of min wage	Base replacement rate (%)	Extra benefit for each additional year (%)
1 or less	80.00	0.563
1.01 to 1.25	77.11	0.814
1.26 to 1.5	58.18	1.178
1.51 to 1.75	49.23	1.430
1.76 to 2	42.67	1.615
2.01 to 2.25	37.65	1.736
2.26 to 2.5	33.68	1.868
2.51 to 2.75	30.48	1.958
2.76 to 3	27.83	2.033
3.01 to 3.25	25.60	2.096
3.26 to 3.5	23.70	2.149
3.51 to 3.75	22.07	2.195
3.76 to 4	20.65	2.235
4.01 to 4.25	19.39	2.271
4.26 to 4.5	18.29	2.302
4.51 to 4.75	17.30	2.330
4.76 to 5	16.41	2.355
5.01 to 5.25	15.61	2.377
5.26 to 5.5	14.88	2.398
5.51 to 5.75	14.22	2.416
5.76 to 6	13.62	2.433
6.01 or higher	13.00	2.450

Notes: Base replacement rate is based on average nominal wage in the five years preceding retirement. Extra benefit is for each year of contribution exceeding the minimum contribution length of ten years.

Table 2.A4. Inflation Rate and Minimum Wage over Time

Year	Inflation rate	Real value of minimum wage
1983	112.50	118.11
1984	67.14	110.25
1985	53.43	110.08
1986	83.17	99.28
1987	126.73	77.61
1988	135.81	71.94
1989	17.58	66.07
1990	26.11	61.13
1991	23.10	58.62
1992	15.85	56.68
1993	9.87	55.23
1994	6.85	55.31
1995	37.72	48.13
1996	31.82	45.09
1997	20.35	43.85
1998	15.31	43.42
1999	17.39	42.19
2000	9.41	42.42
2001	6.57	42.38
2002	4.94	42.18
2003	4.27	41.90
2004	4.37	41.60
2005	4.33	41.25

Notes: Data from second quarter of each year. Real minimum wages are for daily wages in Mexico City (zone A), reported in constant 2002 pesos. Average 2002 exchange rate: 9.66 pesos/dollar.

Table 2.A5. Pension Wealth Simulation, Male Worker Entering System on June 30, 1997

		Real Daily Wage					
Years of Contributions	Plan	43	100	200	300	500	1079
35	PRA	398.6	*815.0*	*1626.2*	*2437.3*	*4059.7*	*8751.9*
	PAYGO	398.6	*398.6*	*603.8*	*890.2*	*1483.6*	*3200.1*
30	PRA	398.6	*523.4*	*1044.3*	*1565.3*	*2607.1*	*5620.5*
	PAYGO	398.6	*398.6*	*510.7*	*743.3*	*1238.9*	*2672.1*
25	PRA	398.6	398.6	*659.1*	*987.8*	*1645.3*	*3546.9*
	PAYGO	398.6	398.6	*406.9*	*579.5*	*965.8*	*2083.2*
20	PRA	87.9	202.4	*403.9*	*605.4*	*1008.4*	*2173.9*
	PAYGO	398.6	398.6	*398.6*	*449.6*	*749.3*	*1616.2*
15	PRA	51.1	117.8	235.0	352.2	*586.6*	*1264.7*
	PAYGO	398.6	398.6	398.6	398.6	*504.5*	*1088.2*
10	PRA	26.8	61.7	123.1	184.5	307.4	*662.6*
	PAYGO	398.6	398.6	398.6	398.6	398.6	*560.3*
5	PRA	*10.7*	*24.6*	*49.0*	*73.5*	*122.4*	*264.0*
	PAYGO	*0.0*	*0.0*	*0.0*	*0.0*	*0.0*	*0.0*

Notes: Values represent real present discounted value of the future stream of pension benefits in thousands of 2002 pesos under the pre-reform pay-as-you-go (PAYGO) and personal retirement account (PRA) systems, for a male worker who enters the system on June 30, 1997. 43 pesos is real daily minimum wage (in Mexico City) in 1997, 1079 pesos is the topcode we impose (corresponding to the lowest real value of IMSS topcode over study period.) See Section 2.2.3 and the Appendix for further details.

Table 2.A6. Comparison of IMSS Baseline Sample and Various ENEU Samples, Women

	IMSS baseline sample (1)	full ENEU sample (2)	ENEU w/ IMSS (3)	ENEU w/o IMSS (4)	ENEU permanent w/ IMSS (5)	ENEU full-time w/ IMSS (6)
A. 1990						
real avg. daily post-tax wage	88.29	133.55	136.91	124.84		128.57
	(0.08)	(2.16)	(2.65)	(3.59)		(2.50)
age	28.12	28.35	28.03	29.17		27.82
	(0.01)	(0.21)	(0.23)	(0.47)		(0.24)
fraction employed in ests >100 employees	0.55	0.45	0.54	0.21		0.54
	(0.00)	(0.01)	(0.01)	(0.02)		(0.01)
N (raw observations)	803579	6685	5126	1559		4745
N (population, using weights)	803579	1023858	738698	285160		677053
B. 2000						
real avg. daily post-tax wage	90.86	128.04	135.88	109.72	140.56	129.65
	(0.07)	(1.82)	(2.21)	(3.06)	(2.49)	(2.18)
age	30.44	30.34	29.85	31.50	30.17	29.71
	(0.01)	(0.18)	(0.19)	(0.40)	(0.21)	(0.20)
fraction employed in ests >100 employees	0.64	0.49	0.62	0.19	0.64	0.62
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N (raw observations)	1251832	9670	7227	2443	6305	6607
N (population, using weights)	1251832	1652164	1157184	494980	1001866	1056013

Notes: All columns focus on wage-earning female workers ages 16-65 in manufacturing, construction, and retail/hotel/restaurant sectors in 16 metropolitan areas from the original ENEU sample. Column 1 reports statistics for IMSS baseline female sample; Column 2 for full ENEU (household survey) sample (satisfying aforementioned criteria); Column 3 for employees in ENEU who report receiving IMSS benefit in current employment; Column 4 for employees in ENEU who report not receiving IMSS benefit; Column 5 for employees in ENEU who report receiving IMSS benefit and having a written contract of indefinite duration; and Column 6 for employees in ENEU who report receiving IMSS benefit and working at least 35 hours in previous week (the ENEU baseline female sample). Standard errors of means in parentheses. In IMSS data, the fraction in establishments with >100 employees variable refers to permanent employees. In the ENEU survey, the establishment-size question asks the total number of employees (without specifying permanent vs. temporary.) For further details, see Section 2.4 and the Appendix.

Table 2.A7. Age Composition by Firm Size Category, 1990, Women

	Age category (employment as % of row total)					employment as % of column total
	16-25	26-35	36-45	46-55	56-65	
A. IMSS						
1-10 employees	45.9	29.6	14.8	6.9	2.8	13.7
11-50 employees	49.0	29.2	14.0	5.8	2.0	20.9
51-100 employees	52.1	28.8	13.0	4.8	1.4	10.2
101-250 employees	55.0	28.3	11.9	3.8	1.0	14.0
> 250 employees	52.0	28.6	13.2	4.8	1.4	41.2
all firm sizes	50.9	28.9	13.4	5.2	1.6	
B. ENEU						
1-10 employees	51.6	28.0	12.7	6.1	1.6	12.2
11-50 employees	51.4	26.5	14.7	5.0	2.3	21.3
51-100 employees	56.0	24.1	15.3	4.0	0.5	11.8
101-250 employees	51.7	32.2	10.7	3.3	2.1	11.4
> 250 employees	52.7	28.7	13.5	3.9	1.2	43.4
all firm sizes	52.6	28.0	13.6	4.4	1.5	

Notes: Data are from IMSS and ENEU baseline female samples. Percentages are calculated based on employment (using sampling weights, in the case of the ENEU) in each cell. Panel B drops observations in ENEU baseline female sample that are missing the firm-size variable (which make up fewer than 1% of observations). For further details, see Section 2.4 and the Appendix.

Table 2.A8. Cross-Sectional Patterns of Evasion, 1990, Women

	wage gap (medians)			wage gap (means)			exc. mass (15th percentile)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
age 26-35	-0.017		-0.005	-0.098***		-0.087***	-0.111***		-0.105***
	(0.035)		(0.033)	(0.031)		(0.029)	(0.020)		(0.018)
age 36-45	0.003		0.029	-0.160***		-0.140***	-0.081***		-0.065***
	(0.038)		(0.036)	(0.037)		(0.034)	(0.020)		(0.018)
age 46-55	0.061		0.101**	-0.139***		-0.109**	-0.081***		-0.056**
	(0.049)		(0.047)	(0.044)		(0.043)	(0.024)		(0.022)
age 56-65	-0.002		0.033	-0.203***		-0.184**	-0.058*		-0.033
	(0.078)		(0.073)	(0.075)		(0.073)	(0.031)		(0.030)
11-50 employees		-0.198***	-0.234***		-0.100**	-0.130***		-0.100***	-0.118***
		(0.039)	(0.037)		(0.039)	(0.036)		(0.019)	(0.016)
51-100 employees		-0.246***	-0.281***		-0.166***	-0.200***		-0.140***	-0.158***
		(0.046)	(0.042)		(0.044)	(0.041)		(0.022)	(0.019)
101-250 employees		-0.236***	-0.256***		-0.118**	-0.140***		-0.118***	-0.131***
		(0.048)	(0.046)		(0.047)	(0.044)		(0.025)	(0.022)
> 250 employees		-0.193***	-0.203***		-0.065	-0.065*		-0.085***	-0.089***
		(0.040)	(0.040)		(0.039)	(0.038)		(0.021)	(0.021)
construction			0.146***			0.123**			0.045*
			(0.056)			(0.051)			(0.024)
retail/services			-0.137***			-0.108***			-0.127***
			(0.030)			(0.028)			(0.014)
constant	0.506***	0.689***	0.593***	0.463***	0.452***	0.499***	0.547***	0.572***	0.586***
	(0.019)	(0.025)	(0.058)	(0.017)	(0.026)	(0.055)	(0.013)	(0.013)	(0.026)
metro area effects	N	N	Y	N	N	Y	N	N	Y
R-squared	0.00	0.04	0.15	0.03	0.02	0.16	0.04	0.05	0.25
N	766	766	766	766	766	766	766	766	766

Notes: Data are from IMSS and ENEU baseline samples, collapsed to metro area/age group/firm-size category/sector level for 1990. The omitted category for age is 16-25, for firm size is 1-10 employees, and for sector is manufacturing. The wage gap (medians) is log median real daily take-home wage from the ENEU minus log median real daily post-tax reported wage from IMSS, calculated. Wage gap (means) is analogous, using mean in place of median. Excess mass is calculated as described in Section 2.5 and Figure 2.9. In calculating evasion measures, we pool ENEU data across quarters within year. *** 1%, ** 5%, * 10% level. See Section 2.4 and the Appendix for further details of data processing.

Table 2.A9. Differential Effects of Pension Reform on Evasion, Women

	wage gap (medians) (1)	wage gap (means) (2)	excess mass (15^{th} perc.) (3)
1(age > 55)*1988	-0.164	-0.067	-0.088
	(0.138)	(0.116)	(0.071)
1(age > 55)*1989	-0.206*	-0.109	-0.027
	(0.122)	(0.109)	(0.034)
1(age > 55)*1990	-0.095	0.010	-0.043
	(0.128)	(0.114)	(0.041)
1(age > 55)*1991	-0.143	-0.058	-0.114**
	(0.123)	(0.123)	(0.055)
1(age > 55)*1992	-0.079	-0.062	-0.027
	(0.131)	(0.115)	(0.036)
1(age > 55)*1993	0.076	0.087	0.009
	(0.146)	(0.128)	(0.034)
1(age > 55)*1994	0.043	0.079	0.015
	(0.141)	(0.116)	(0.032)
1(age > 55)*1995	0.047	0.076	0.035
	(0.148)	(0.131)	(0.039)
1(age > 55)*1996	-0.019	0.076	0.015
	(0.130)	(0.119)	(0.035)
1(age > 55)*1998	-0.051	-0.023	0.026
	(0.124)	(0.117)	(0.035)
1(age > 55)*1999	0.040	0.034	0.022
	(0.132)	(0.112)	(0.032)
1(age > 55)*2000	0.055	0.063	0.025
	(0.123)	(0.108)	(0.033)
1(age > 55)*2001	0.039	0.175	0.036
	(0.122)	(0.109)	(0.031)
1(age > 55)*2002	0.091	0.132	0.003
	(0.120)	(0.108)	(0.035)
1(age > 55)*2003	0.114	0.070	0.007
	(0.124)	(0.111)	(0.033)
age group-metro area effects	Y	Y	Y
metro-year effects	Y	Y	Y
R-squared	0.72	0.61	0.81
N	1279	1279	1279

Notes: Data are from IMSS and ENEU baseline samples, collapsed to metro area/age group/year level. Wage gap (medians) is log median real daily net wage from ENEU minus log median post-tax daily wage from IMSS. Wage gap (means) is defined analogously, using means in place of medians. Excess mass is calculated as described in Section 2.5 and Figure 2.9. In calculating evasion measures, we pool ENEU data across quarters within year. *** 1%, ** 5%, * 10% level. See Section 2.4 and the Appendix for further details of data processing.

Table 2.A10. Differential Effects of Pension Reform on Employment Gap, Women

	dep. var.: log(empl., ENEU) - log(empl., IMSS) (1)	(2)
1(age > 55)*1988	-0.181	-0.257
	(0.231)	(0.217)
1(age > 55)*1989	0.177	0.196
	(0.194)	(0.189)
1(age > 55)*1990	0.141	0.137
	(0.200)	(0.177)
1(age > 55)*1991	0.232	0.252
	(0.201)	(0.191)
1(age > 55)*1992	-0.185	-0.177
	(0.237)	(0.242)
1(age > 55)*1993	0.161	0.186
	(0.222)	(0.204)
1(age > 55)*1994	-0.021	-0.022
	(0.232)	(0.218)
1(age > 55)*1995	-0.316	-0.323
	(0.256)	(0.250)
1(age > 55)*1996	-0.092	-0.090
	(0.207)	(0.207)
1(age > 55)*1998	-0.117	-0.146
	(0.183)	(0.189)
1(age > 55)*1999	-0.292	-0.336
	(0.220)	(0.218)
1(age > 55)*2000	-0.158	-0.158
	(0.220)	(0.223)
1(age > 55)*2001	0.145	0.147
	(0.201)	(0.179)
1(age > 55)*2002	-0.009	-0.006
	(0.261)	(0.234)
1(age > 55)*2003	-0.267	-0.264
	(0.223)	(0.232)
age group effects	Y	
age group-metro area effects	N	Y
metro-year effects	Y	Y
R-squared	0.45	0.55
N	1258	1258

Notes: Samples are IMSS and ENEU baseline samples, collapsed to metro area/age group/year level. *** 1%, ** 5%, * 10% level. See Section 2.4 and the Appendix for further details of data processing.

Table 2.A11. Differential Effects of Pension Reform on ENEU Take-Home Wage, Women

	dep. var.: log daily net wage, ENEU				
	(1)	(2)	(3)	(4)	(5)
1(age > 55)*1988	-0.327***	-0.273***	-0.254***	-0.228***	-0.219***
	(0.107)	(0.094)	(0.081)	(0.079)	(0.078)
1(age > 55)*1989	-0.383***	-0.297***	-0.248***	-0.230***	-0.231***
	(0.098)	(0.085)	(0.076)	(0.072)	(0.071)
1(age > 55)*1990	-0.306***	-0.264***	-0.225***	-0.230***	-0.235***
	(0.106)	(0.087)	(0.079)	(0.075)	(0.074)
1(age > 55)*1991	-0.370***	-0.308***	-0.270***	-0.286***	-0.287***
	(0.099)	(0.083)	(0.075)	(0.072)	(0.072)
1(age > 55)*1992	-0.225**	-0.208**	-0.189**	-0.180**	-0.178**
	(0.115)	(0.101)	(0.087)	(0.085)	(0.085)
1(age > 55)*1993	-0.323***	-0.278***	-0.252***	-0.242***	-0.258***
	(0.103)	(0.093)	(0.089)	(0.088)	(0.086)
1(age > 55)*1994	-0.148	-0.100	-0.099	-0.102	-0.091
	(0.110)	(0.100)	(0.091)	(0.090)	(0.089)
1(age > 55)*1995	-0.163	-0.188**	-0.167**	-0.182**	-0.177**
	(0.108)	(0.089)	(0.081)	(0.079)	(0.078)
1(age > 55)*1996	0.055	0.028	0.036	0.024	0.025
	(0.122)	(0.100)	(0.090)	(0.087)	(0.086)
1(age > 55)*1998	-0.248**	-0.194**	-0.224***	-0.217***	-0.222***
	(0.114)	(0.096)	(0.084)	(0.081)	(0.080)
1(age > 55)*1999	-0.032	-0.022	-0.008	-0.005	-0.010
	(0.120)	(0.095)	(0.086)	(0.082)	(0.081)
1(age > 55)*2000	-0.274***	-0.210**	-0.187**	-0.190**	-0.187**
	(0.104)	(0.092)	(0.085)	(0.083)	(0.082)
1(age > 55)*2001	-0.177*	-0.137	-0.152*	-0.138*	-0.137*
	(0.105)	(0.088)	(0.078)	(0.076)	(0.074)
1(age > 55)*2002	-0.109	-0.144*	-0.137*	-0.136*	-0.135*
	(0.101)	(0.084)	(0.076)	(0.073)	(0.072)
1(age > 55)*2003	-0.202*	-0.242**	-0.212**	-0.222**	-0.219**
	(0.122)	(0.097)	(0.091)	(0.089)	(0.088)
age group effects	Y	Y	Y	Y	Y
metro-year effects	Y	Y	Y	Y	Y
schooling effects	N	Y	Y	Y	Y
married indicator	N	Y	Y	Y	Y
occupation effects	N	N	Y	Y	Y
industry effects	N	N	N	Y	Y
firm-size effects	N	N	N	N	Y
R-squared	0.11	0.39	0.47	0.48	0.48
N	309175	309175	309175	309175	309175

Notes: Sample is ENEU baseline sample. Take-home wage is the post-payroll-tax net wage as reported on ENEU. Estimates use population sampling weights provided in ENEU dataset. Controls include sets of 9 schooling indicators, 22 occupation indicators, and/or 50 industry indicators, in addition to the sets of five age-group and firm-size indicators, in indicated columns; details of category definitions are in the Appendix. *** 1%, ** 5%, * 10% level.

Chapter 3

Tariffs, Social Status, and Gender in India[1]

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Todd Kumler

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3.1 Introduction

Macroeconomic policies have microeconomic consequences as they alter prices, incomes, and the choice sets of individuals and firms. This paper focuses on one of the most important structural changes in recent history—the removal of international trade barriers. Specifically, in the context of India, we examine the implications of tariff cuts for fertility, sex-selection, and child mortality, and highlight the distributional effects of trade policy across gender and socioeconomic strata.

The 1991 trade liberalization of the Indian economy was externally imposed by the International Monetary Fund (IMF) in response to a severe balance of payments crisis, which we argue was an exogenous shock to industry-level tariffs in India. Moreover, the resulting changes in tariff- and non-tariff barriers (NTBs) were quite large in magnitude. In the manufacturing sector, the average tariff declined from 117 percent to 39 percent and the share of imports covered by NTBs fell from 82 percent to 17 percent between 1990-1991 and 1999-2000 (Gupta and Kumar (2008)). We exploit heterogeneity in the pre-reform industrial composition of Indian districts, combined with differences in tariff cuts by industry, to identify districts that were more or less exposed to trade liberalization.[3] We then estimate the effect of this differential exposure on parents' fertility decisions and children's health.

Using retrospective birth histories constructed from large-scale household surveys and an instrumental-variables strategy, first we show that the sex ratio at birth decreased and fertility as well as child mortality increased in districts more exposed to tariff cuts. However, these effects vary significantly across social strata. We find that tariff cuts improve the likelihood of birth and survival for girls in socially disadvantaged families. Births to lower-caste, uneducated, and less wealthy women are more likely to be female in districts that are relatively more exposed to tariff declines. Moreover, these daughters are significantly less likely to die within one, six, and twelve months of birth.

[3] Among several other papers, this identification strategy has been used by Hasan, Mitra, and Ural (2006-07), Topalova (2007), Edmonds, Pavcnik, and Topalova (2010b), Topalova (2010), and Gaddis and Pieters (2012).

However, mortality for girls born to upper-caste, more educated, and wealthier mothers *increases* significantly. These "high-status" women are also more likely to give birth to boys, but the sex ratio effects are not as strong as the mortality results. Fertility rises for low-status women and falls for high-status women.

The analysis of heterogeneity, in combination with the findings of prior literature on the effects of the Indian trade reform, allows us to distinguish between three potential mechanisms: (a) changes in household income, (b) changes in relative female bargaining power, and (c) altered future labor market returns from daughters relative to sons.[4] Topalova (2010) has shown that relative wages decreased in impacted industries and relative poverty increased in Indian districts more exposed to trade reform.[5] To the extent that income shocks are linked to investments in children's health[6] and parents' decisions about the number and the sex-composition of their children,[7] we expect tariff cuts to influence fertility and sex ratios. Additionally, structural adjustments resulting from trade liberalization may change the relative demand for female labor,[8] or the gender wage gap[9] and, thus, influence fertility through changes in relative female bargaining power. Similarly, if female fetuses are selectively aborted due to the lower economic value of daughters relative to sons, any effect of trade liberalization on the demand for female labor could also influence the sex ratio at birth (Qian (2008)).

Using data from the National Sample Survey (NSS), we show that tariff declines result in a significant increase in adult employment for lower-caste women and upper-caste men. On the other

[4] More open trade may also influence relative commodity prices in an economy and, hence, consumption levels (Porto (2007)). Changes in the amount and the type of food (and nutrients) consumed by the mother and her children due to differences in dietary preferences across districts could affect child health outcomes, in general, and infant mortality, in particular (Cutler, Deaton, and Lleras-Muney (2006)).

[5] Although, using state-level data, Hasan, Mitra, and Ural (2006-07) conclude that greater exposure to trade openness is not associated with slower reduction in poverty in rural India. For a more detailed discussion, we refer the reader to Topalova (2010).

[6] Strauss and Thomas (1998), Strauss and Thomas (2008), Case (2001), Case (2004), Paxson and Schady (2005)

[7] Edlund and Lee (2009), Almond, Li, and Zhang (2013)

[8] Katz and Murphy (1992), Kucera (2001), Kucera and Milberg (2000), Gaddis and Pieters (2012)

[9] Wood (1991), Black and Brainerd (2004)

hand, adult employment decreases for upper-caste women and lower-caste men, albeit insignificantly. These results suggest that differential changes in labor market returns by gender and social status are the most likely explanatory channel for our findings. Parents from socially disadvantaged groups are investing more in daughters in two ways. First, girls are more likely to be born, potentially due to reduced use of sex-selective abortions. Secondly, they take better care of the daughters since, conditional on being born, infant mortality for low-status girls decreases. Low-status boys do not benefit equally because their potential earning opportunities have not increased as much.[10]

We make a number of contributions. Most papers in the field of international economics have examined the effects of trade liberalization on industrial outcomes such as productivity, employment, and wages.[11] We highlight that trade policy can also have substantial implications for less obvious outcomes such as fertility and child mortality.[12] Moreover, we add to the extremely limited literature on the distributional effects of more open trade, especially along the gender dimension.[13] Despite methodological shortcomings, existing papers suggests that trade openness has not unambiguously benefited everyone (Goldberg and Pavcnik (2007a)). However, the identities of

[10] This may also be due to limited occupational mobility for low-status men as suggested by Munshi and Rosenzweig (2009)). It is also possible that low-status parents strategically choose not to over-invest in their sons so that they continue to participate in the traditional occupational networks of lower-caste men (Munshi and Rosenzweig (2006)).

[11] Tybout (2003), Trefler (2004), Hanson (2007), Goldberg and Pavcnik (2007b), Goldberg and Pavcnik (2007a), Harrison, McLaren, and McMillan (2011), Kovak (2013)

[12] In a similar vein, Edmonds, Pavcnik, and Topalova (2010b) and Kis-Katos and Sparrow (2011) examine the impact of trade policy on children's schooling and child labor in India and Indonesia, respectively.

[13] Since beginning work on this paper, we have become aware of another study, Chakraborty (2012), analyzing the impact of district-level exposure to trade liberalization on sex ratios in India. While she also follows Topalova (2010) to construct district-level tariff measures, our paper differs from Chakraborty (2012) in a number of ways. First, her outcome variable measures sex ratios among children still living in 1999. This measure will capture the impact of trade liberalization on the sex ratio at birth as well as its impact on male and female infant mortality. Our paper disentangles this impact by analyzing the sex ratio at birth and infant mortality separately. Second, she uses a difference-in-difference approach comparing the outcomes of children born between 1988 and 1996 (the treated group) to children born between 1980 and 1988 (the control group) in districts with different levels of exposure to trade liberalization, measured as the decline in district-level tariffs from 1990 to 1991. Our paper measures district-level tariff exposure for each year from 1987 to 1997, allowing us to directly link trade liberalization to each birth based on the year of birth. Finally, Chakraborty (2012) uses birth histories from the 1999 National Family Health Survey of India (NFHS) while we use the 2002-2004 District-Level Household Survey of India (DLHS). In addition to having a larger sample size, our dataset has the additional advantage of allowing us to compare births to the same mother at different points in time through a mother fixed effect approach. Ultimately, we believe that our empirical specifications and larger sample size allow us to better isolate the causal effect of trade liberalization on fertility outcomes.

"winners" and "losers" are not precisely known. Topalova (2010) finds that the slower decline in poverty caused by tariff cuts most affected the poorest sections of the Indian society. In addition, the decrease in education expenditure on children in response to the immediate change in family income affected girls more (Edmonds, Pavcnik, and Topalova (2010b)). Our results paint a slightly different picture. We find that adult female employment increases and mortality of girls relative to boys decreases in lower-caste families. The losers, however, are also female, albeit at the other end of the social hierarchy—from upper-caste, more educated, and relatively wealthy households. We show that (a) the labor market outcomes for men and women from different social strata have changed differentially, and (b) households are responding to this altered economic landscape via fertility and investments in children's human capital. Interestingly, we find that the results of Edmonds, Pavcnik, and Topalova (2010b) exhibit substantial heterogeneity by caste: in districts more exposed to tariff cuts, the decrease in schooling they find is driven by upper-caste children; children from scheduled caste households are, in fact, *more* likely to be in school and *less* likely to work. These distributional effects can play an important role in exacerbating or combating historical socio-economic inequalities and, hence, it is crucial to take them into account while estimating the total costs and benefits of trade policies.

Second, our paper adds to the somewhat scarce literature on the determinants of discrimination against female children in several developing societies. While overall fertility and child mortality have decreased, the sex ratio at birth has sharply increased in many Asian countries, including India. A "preference" for sons relative to daughters is widely believed to be the underlying cause of prenatal and postnatal discrimination against girls, but credible explanations for this difference in valuation are limited, especially in the Indian context.[14] Our finding that girls fare better (worse) in terms of mortality rates precisely in groups where the relative earning potential of women has

[14] Carranza (2012) is an exception.

improved (worsened) is consistent with the findings of Qian (2008) in the context of China, and emphasizes the economic roots of seemingly subjective discrimination.

Third, our fertility results underscore the fertility-sex ratio trade-off present in countries with a stronger valuation of sons (Anukriti (2013)). If son preference persists over time,[15] parents may substitute less than perfectly between sons and daughters despite an increase in the relative benefit from girls. They may continue to have the desired number of sons but decrease prenatal (as well as postnatal) discrimination against girls, and hence, fertility may increase as the sex ratio at birth decreases, consistent with our results for low-status families. On the flip side, a decrease in the relative returns from girls, as we observe for high-status families, may enhance the use of sex-selective abortions, causing fertility to fall and the sex ratio at birth to rise.

Lastly, on the technical side, our ability to control for state-specific time trends and mother fixed-effects in the regression analysis makes our identification more robust than previous literature on the effects of tariff reforms on household and individual outcomes.

In Section 3.2, we provide a summary of the Indian trade reform. In Section 3.3, we outline our empirical methodology and describe the data. Section 3.4 presents the regression estimates, Section 3.5 discusses the underlying mechanisms, and Section 3.6 conducts some robustness checks. Section 3.7 provides a brief discussion and Section 3.8 concludes the paper.

3.2 India's Trade Liberalization

We analyze the effect of trade liberalization on household fertility decisions and children's health in the context of India's 1991 trade reform. Faced with a balance of payments crisis in August 1991, the Indian government embarked on several major economic reforms as conditions of an International Monetary Fund (IMF) bailout. Included among these requirements was a unilateral

[15]Abrevaya (2009), Almond and Edlund (2008), Almond, Edlund, and Milligan (2011), and Dubuc and Coleman (2007) find evidence for persistence in son preference among Asian immigrants to the United States, Canada, England, and Wales.

reduction in the overall level and the dispersion of import tariffs as well as the removal of non-tariff barriers (NTBs), such as import licensing.

The period after the IMF bailout, therefore, marks a sharp break in Indian trade policy. The maximum tariff fell immediately from 400 percent to 150 percent, with later revisions bringing the maximum tariff to approximately 45 percent by 1997 (Hasan, Mitra, and Ural (2006-07)). Meanwhile, the average tariff fell from 80 percent in 1990 to 37 percent in 1996 and the standard deviation of tariffs declined by 50 percent (Topalova (2010)). NTBs also fell, with the proportion of goods subject to quantitative restrictions receding from 87 percent in 1987 to 45 percent by 1994 (Topalova (2010)).

In addition to the sharp decline in trade protection, the 1991 episode possesses several important features that are valuable for our analysis. Since the policy reform was imposed as part of the IMF bailout, the tariff cuts were largely unanticipated by firms and households in India. As other commentators have observed, the removal of trade barriers was implemented swiftly as a form of "shock-therapy" and was not part of any pre-existing development plan (Bhagwati (1993), Goyal (1996)). It is, therefore, unlikely that our results are driven by any adjustments in fertility in anticipation of these reforms.

The quick initiation of the liberalization episode also reduces concerns about industries with greater political influence or higher productivity shaping the structure of the tariff reform in a way that would undermine our empirical strategy (described in detail in the following section). Topalova (2007) finds that industry-level tariff changes are uncorrelated with several proxies of an industry's political influence prior to the Indian reform, such as the number of employees, proportion of skilled workers, and industrial concentration. Previous studies also find no correlation between an industry's future tariffs and its productivity before 1991 or productivity growth during 1989-1997 (Topalova (2004)). Finally, tariff changes through 1997 were spelled out in India's Eighth Five

Year plan (1992-1997), suggesting little room for manipulation of tariffs based on political economy concerns during this time period.

It must be noted that, like Edmonds, Pavcnik, and Topalova (2010b), we ignore changes in NTBs, primarily due to data availability issues. Thus, our results measure the effect of only one important dimension of the trade reform, i.e. the tariff cuts. The exclusion of NTBs is potentially harmful for our empirical strategy if the trends in NTBs were in the opposite direction as compared to tariffs. But as mentioned by Edmonds, Pavcnik, and Topalova (2010b), there is a positive correlation in tariffs and NTBs during our sample period. Thus, our results are biased only to the extent that some of the effects we assign to tariff cuts may have instead been caused by the removal of NTBs.[16]

3.3 Empirical Strategy

3.3.1 Measuring Exposure to Tariff Reduction

The impact of trade liberalization on a developing economy, such as India, can be felt through many channels. The availability of cheaper imported final goods can be welfare-improving for consumers, while the reduction in tariffs on intermediate inputs can increase firm productivity. Although a decrease in consumer prices could certainly influence fertility behavior, this effect will be common across all households in India. On the other hand, an increase in supply of cheaper imported products that compete with domestic goods can reduce employment and wages at domestic firms. Like many other papers in the literature, our measure of tariff protection emphasizes this latter effect of trade openness on employment.

National tariff protection varies across industries and over time in India. Moreover, there is

[16] Edmonds, Pavcnik, and Topalova (2010b) also argue that despite incomplete removal of NTBs by 1997, the volume of imports increased in response to tariffs cuts suggesting that the latter were a significant and important part of the reform.

substantial heterogeneity in the industrial composition of Indian districts prior to 1991. Therefore, depending on their industrial composition of employment at the time of reform, some Indian districts experienced relatively larger reductions in trade protection than others. Following Topalova (2010) and others, our identification strategy relies on this comparison to estimate the causal effect of tariff reform.

Specifically, we interact the national nominal ad-valorem tariff faced by industry i in year t , $tariff_{it}$, with the share of employment in industry i and district d in 1991, $empshare_{id}^{1991}$, to construct a measure of tariff for district d in year t:

$$tariff_{dt} = \sum_i empshare_{id}^{1991} \times tariff_{it} \tag{1}$$

Since the employment shares are based on a district's industrial composition *before* the initiation of trade liberalization, our tariff measure is free of any endogenous changes in employment composition that take place due to the removal of tariff barriers.

Even though tariff cuts took place across a wide range of industries, certain industries, such as cereals and oilseeds production, were non-traded, i.e. only the government was allowed to be an importer of goods in these industries.[17] Consequently, $tariff_{dt}$ assigns a zero tariff to the non-traded industries for the entire time period. This implies that districts with higher levels of employment in the non-traded sector in 1991 will mechanically have lower $tariff_{dt}$ (Hasan, Mitra, and Ural (2006-07)). Since a large proportion of non-traded employment is in the cereal and oilseeds sectors, and workers in these industries tend to be poor rural farmers, this introduces a negative correlation between poverty and $tariff_{dt}$.

Previous studies have addressed this concern by constructing a second measure of district tariffs that only depends on employment in traded industries (Hasan, Mitra, and Ural (2006-07), Topalova

[17] Other non-traded industries during our sample period were services, trade, transportation, and construction.

(2007), Topalova (2010)). We follow the literature and create this measure as follows, where emp_{id}^{1991} is the employment in a traded industry i and district d in 1991:

$$tradedtariff_{dt} = \frac{\sum_{i \in traded} emp_{id}^{1991} \times tariff_{it}}{\sum_{i \in traded} emp_{id}^{1991}} \tag{2}$$

The only difference between the two measures of tariff protection in (1) and (2) is that the latter excludes employment in non-traded industries while constructing weights for industry-level tariffs. The traded tariff measure is, therefore, independent of the proportion of workers in the non-traded sector and is uncorrelated with initial poverty levels within a district.

3.3.2 Data

We use data from the second round of the District-Level Household Survey (DLHS-2) of India. The DLHS-2 surveyed 507,000 currently-married women (aged 15 - 44 years) from 620,000 households in 593 districts during March 2002 - October 2004. This survey includes a complete retrospective birth history for every woman interviewed, containing information on the month and the year of child's birth, birth order, age of the mother at birth, and the age at which the child died, if the child is deceased.

We focus our analysis on rural areas within Indian districts. Topalova (2010) finds an insignificant relationship between tariff protection and poverty in urban areas of Indian districts, which she attributes to pre-existing trends in poverty and the presence of other reforms in addition to trade liberalization that impacted urban areas. Due to concerns of simultaneous reforms and pre-existing trends in urban areas, we focus on rural areas only.

Since we focus on district as the geographical unit of interest, ideally we would like to know the district in which a birth takes place. However, DLHS-2 only includes district of residence identifiers at the time of survey. As a result, we assume that all births to a woman take place in her district of residence at the time of survey. This implicitly assumes that mothers do not migrate to a different district after initiating child-bearing. This is a reasonable assumption in this context since inter-district migration in India is low and mostly consists of women relocating as a result of marriage. In addition, this assumption is problematic only if the measurement error induced by it varies, systematically, with our measures of district-level tariff protection. Later, in Section 3.6 we show that the tariff cuts had no significant effect on the gender and caste composition of in-migrants from other districts.

We restrict our sample to the 1987-1997 time period. There are two reasons for this. First, 1987 is the earliest year for which we have tariff data. Second, the tariff changes during 1992-1997 were spelled out in India's Eighth Five Year Plan, so they are unlikely to have been influenced by political economy decisions. After 1997, however, industry-level tariffs are negatively correlated with the industry's current productivity (Topalova (2004)), suggesting that these latter changes may be endogenous to an industry's performance. For this reason, we only focus on years up to 1997. Figure 3.1 shows the evolution of nominal national industry-level ad-valorem tariff during 1987-1997. Average tariff fell from about 95 percent in 1987 to about 30 percent in 1997.

We impose three additional sample-selection criteria. First, we only include births for whom the mother's age at birth was between 13 and 40. Second, we exclude birth parities of 11 or higher. We use these restrictions due to the small number of births to women outside of the 13-40 age range and the small number of births with parity above 10. However, our results are not sensitive to the exclusion of these observations. The DLHS questionnaires were also administered to women who were visiting the household at the time of the survey. Since there is no information on their permanent district of residence, we exclude them from our analysis. Our final sample comprises 464,916 births to 269,661 women in 408 districts.

The district-level tariff data comes directly from Topalova (2010). Industry- and district-wise employment data comes from the 1991 Census of India while tariff data at the six-digit level was collected by Petia Topalova from the Indian Ministry of Finance publications. The rainfall data used later comes from the annual district-level precipitation time series created by Ram Fishman using Indian Meteorological Department database. Lastly, we use Rounds 43 (1987-88) and 55 (1999-00) of the Employment-Unemployment survey of the National Sample Survey (NSS) of India to examine the effects of tariff cuts on children's schooling and labor force participation, adult employment, and in-migration by caste and gender.

3.3.3 Regression Framework

The question of interest in this paper is how the removal of tariff barriers influences households' fertility decisions and children's health outcomes. In particular, we investigate whether reductions in tariff protection faced by a woman (based on her district of residence) impact the probability that she gives birth in a year, the sex ratio of these births, and their mortality rates. Our regression framework is similar to Edmonds, Pavcnik, and Topalova (2010b) and Topalova (2010) and compares women (births) in districts that were more or less exposed to tariff cuts.

We start by reshaping the retrospective birth data to create a woman-year panel and construct a dummy variable, $birth_{mdt}$, that equals one if a woman m in district d gives birth in year t, and is otherwise zero. Then, we estimate the following base specification using ordinary least-squares (OLS):

$$birth_{mdt} = \beta_0 + \beta_1 tariff_{dt} + \beta_2 X_{mdt} + \gamma_d + \tau_t + \delta_d t + \epsilon_{mdt} \tag{3}$$

The main regressor of interest, $tariff_{dt}$, represents the level of tariff protection assigned to a women based on her district of residence. Although the variation in $tariff_{dt}$ occurs at the district-level, we also control for a vector of individual covariates, X_{mdt},that may impact the outcome variables, including indicators for a woman's age in year t, the number of previous births, the household's caste[18] and religion.[19] Inclusion of district fixed-effects, γ_d, controls for time-invariant differences across districts while year fixed-effects, τ_t, control for any India-wide shocks that may influence our outcomes. The inclusion of year fixed-effects also highlights that our empirical strategy *does not* estimate the overall effect of trade liberalization on fertility, sex ratios at birth, or mortality, since any economy-wide impact on consumer prices or productivity will be captured by the year-effects. Since our data spans all years between 1987 and 1997, we also include linear district-specific time trends in our regressions.

The sex ratio and mortality regressions are run using the retrospective panel of *births*. The base OLS specification is similar to (3):

$$y_{imdt} = \beta_0 + \beta_1 tariff_{dt} + \beta_2 X_{imdt} + \gamma_d + \tau_t + \delta_d t + \epsilon_{imdt} \tag{4}$$

where i indexes a child born to mother m in district d and year t. For mortality regressions, the outcome is an indicator variable for whether a child dies before Q months of birth, where we allow Q to equal one, six, or twelve months.[20] For the sex ratio regressions, the outcome is an indicator variable that equals one if the child is male, and zero otherwise. The remaining controls are the same as in (3).

Since a large majority (89 percent) of women in our sample report giving birth to more than one child during the time period we study, we also run specifications with mother fixed-effects:

$$birth_{mdt} = \beta_0 + \beta_1 tariff_{dt} + \beta_2 X_{mdt} + \tau_t + \phi_m + \delta_d t + \epsilon_{mdt} \tag{5}$$

$$y_{imdt} = \beta_0 + \beta_1 tariff_{dt} + \beta_2 X_{imdt} + \tau_t + \phi_m + \delta_d t + \epsilon_{imdt} \tag{6}$$

[18] Caste categories are scheduled caste (SC), scheduled tribe (ST), other backward caste (OBC), and general caste.
[19] Religion categories are Hindu, Muslim, Sikh, Christian, and Others.
[20] Infant mortality is defined as death before age 1, while child mortality usually refers to death before age 5.

where γ_m represents the mother fixed-effect and controls for all unobserved, time-invariant heterogeneity across women that could influence her fertility decisions. X_{mdt} and X_{imdt} now include just the indicators for number of previous births and woman's age in year t. By including mother fixed-effects, we are essentially comparing the birth outcomes for the same woman under different levels of tariff protection in her district. Our ability to control for district-specific time trends and mother fixed-effects makes our identification more robust than previous literature. A positive (negative) β_1 implies that tariff decline is associated with a decrease (increase) in the outcome of interest, *relative to the national trend.*

The coefficient β_1 is identified under the assumption that changes in our tariff measure are uncorrelated with district-specific, unobserved time-varying shocks (or mother-specific, unobserved time-varying shocks in (5)-(6)) that influence fertility, sex ratios, and infant mortality. Since we interact a district's pre-reform industrial composition with national changes in industry tariffs to construct $tariff_{dt}$, any source of bias would have to be correlated with both pre-reform industrial composition and national tariff changes by industry. Like Topalova (2010), Edmonds, Pavcnik, and Topalova (2010b) and others, we assume that this is not the case. Nevertheless, we test the validity of this assumption by checking that our results are robust to the inclusion of other observable district-specific, time-varying shocks, such as rainfall shocks.

For our sex ratio regressions, t refers to the year of conception, instead of the year of birth. Since an ultrasound test, followed by an induced abortion, are believed to be the primary channel through which parents exercise control over the sex of their births in India during our sample period (Bhalotra and Cochrane (2010)), and these technologies are most effective and safe during the first or second trimesters of birth (Epner, Jonas, and Seckinger (1998)), district-level tariff protection during the year of conception is more relevant for explaining the effect of trade reform on sex ratios at birth. We define the year of conception as the year nine months prior to the month of birth, thereby implicitly assuming that no birth is premature.

One concern is that $tariff_{dt}$ may be correlated with the pre-reform size of a district's non-traded sector and, hence, correlated with its initial level of poverty. If this is the case, OLS estimates in specifications (3)-(6) will be biased. We deal with this issue by using traded tariff, $tradedtariff_{dt}$ as an instrument for $tariff_{dt}$. Figure 3.2 plots both these measures for our sample period. Since non-traded industries are automatically assigned a zero tariff for all years, the average tariff measure is, by construction, substantially lower than the average traded tariff measure. While $tradedtariff_{dt}$ declined from about 88 percent to 31 percent, $tariff_{dt}$ decreased from about 7 percent to 2 percent during 1987-1997. There is a significant correlation between the two measures (first-stage regression estimates presented later) and they both exhibit a sharp downward trend.[21] Moreover, $tradedtariff_{dt}$ is independent of the baseline proportion of workers in the non-traded sector and therefore, uncorrelated with initial poverty levels within a district. This validates the use of traded tariff as an instrument.

Next, we look at the time trends in fertility, the sex ratio at birth, and infant mortality in India during our study period. The total fertility rate declined from 4.1 in 1987 to 3.3 in 1997 (Figure 3.3). The male-female sex ratio in the 0-6 age group has been rising rapidly (Figure 3.4), especially since the 1980s. Increased availability of technology for sex-selection combined with declining fertility and a strong preference for sons are widely believed to be the causes for this growing sex-imbalance in the child population. According to DLHS-2 data, under-5 mortality in rural India fell from 127 deaths per 1000 live births in 1987 to 95 deaths per 1000 live births in 1997. As Figure 3.5 shows, infant mortality has been declining over time. Mortality before age 1 is much higher than mortality during ages 1-4. Mortality for girls is larger during ages 1-4. It is important to keep in mind that our identification strategy does not estimate the causal impact of tariff reductions in explaining

[21] The only exception is an increase in tariffs from 1992 to 1993. Due to measurement error concerns, we also run regressions that exclude the data for 1993. Our results remain the same and are available upon request.

these aggregate trends. Instead, we estimate the effect of tariff reductions on *deviations from the trend*. Table 3.1 provides a description of the socio-economic characteristics of our sample.

3.4 Results

3.4.1 Fertility

We begin by looking at the effect of changes in district-level tariff exposure in a year on the probability that a woman gives birth in that year.[22] Column (1) in Table 3.2 presents the baseline results controlling for district and year fixed-effects. In Column (2) we also control for mother's years of schooling, indicators for mother's age in that year, her number of previous births, and household's caste and religion. In Column (3) we add district-specific linear time trends. Finally, Columns (4) and (5) also control for mother fixed-effects. In all specifications, robust standard errors are clustered at the district level and district-level sampling weights are used.[23]

The OLS results in Panel A indicate a positive and significant relationship between our district-level tariff measure and the probability that a woman gives birth across all columns. These positive coefficients suggest that women in districts more exposed to trade liberalization (i.e. a relative decline in our tariff measure) witnessed a relative *decrease* in fertility. However, for reasons previously described, changes in the tariff measure utilized in Panel A may be negatively correlated with a district's initial poverty level. If women in initially poorer districts also experience relatively smaller declines in fertility over our time period for reasons unrelated to trade liberalization, OLS will overestimate the causal effect of tariff protection on fertility. We, therefore, instrument for our tariff protection measure using traded tariff protection, which is uncorrelated with the size of the non-traded sector, as previously argued. Panel B of Table 3.2 shows the first-stage regression of a

[22] Throughout this paper, we use the term fertility to indicate probability of birth in a given year. A higher probability of birth does not necessarily imply higher completed fertility. It is possible that our results capture changes in the timing of births rather than changes in overall fertility levels.

[23] The unweighted regressions yield very similar results, which are available upon request.

district's tariff measure on a district's traded tariff protection. In all specifications, traded tariff has a significant and strong first-stage impact on district tariff protection, indicating that traded tariff is a strong instrument for district tariff.[24]

When we use traded tariff as an instrument (Panels C and D of Table 3.2), district tariff protection in a year has a *negative* and largely significant effect on the probability that a woman gives birth in that year. The fact that our coefficient of interest changes sign when instrumenting for district tariff protection suggests that including non-traded industries in the tariff measure introduces a significant upward bias, likely due to the correlation between initial poverty and changes in the tariff measure. The reduced form coefficient of traded tariff is also negative throughout and significant (except in Column (4)).

The IV coefficients indicate that the Indian trade reform had a substantial effect on fertility – a woman living in a district that experienced the average decline in tariff protection of 7 percentage points was between 0.6 percentage points (Panel C, Column 1) and 1.7 percentage points (Panel C, Column 5) more likely to give birth in a given year.[25]

3.4.2 Sex Ratio at Birth

Having established that a reduction in a district's relative tariff exposure leads to an increased probability of birth in rural India, we turn our attention to the sex-composition of these births. The sex ratio at birth (SRB) deviates from the natural SRB if female fetuses are terminated more frequently than male fetuses due to less prenatal care or sex-selective abortions. In India, prenatal sex-determination is illegal, but widely prevalent, leading to a large number of female fetuses being aborted. Bhalotra and Cochrane (2010) estimate that approximately 480,000 sex-selective abortions took place in India annually during 1995 - 2005. Trade liberalization can affect the SRB by (a)

[24]The first stage F-statistic is large in all specifications.

[25]Table A3.1 in the Appendix presents the IV results for urban areas and we find a similar increase in the likelihood of birth in response to tariff cuts. Coefficients are negative and significant across all five specifications.

changing the demand for sex-selective abortions due to changes in the relative demand for sons, (b) changing the demand for sex-selective abortions due to changes in parents' ability to afford prenatal sex determination and abortion resulting from changes in income, (c) through income shocks which impact fetal viability differentially based on the sex of the fetus (Trivers and Willard (1973)),[26] or (d) through greater access to sex-determination technology *via* imports of ultrasound machines, for example.[27]

Sex-determination can be effectively performed through an ultrasound test around 12 weeks of gestation or through *amniocentesis* around 8-9 weeks of gestation. If a mother has an induced abortion, it is likely to take place during the first or second trimester of pregnancy. This suggests that the relevant tariff variable to examine the effect of trade liberalization on the sex of a birth is not the tariff at the time of birth, but the tariff during the first two trimesters of pregnancy. Therefore, we use tariff in the district of birth in the year of conception as the explanatory variable for all sex ratio regressions.

Using the retrospective panel of births, Table 3.3 presents the results from OLS and IV regressions of an indicator for male birth on district-level tariff during the year of conception.[28] The OLS coefficients in Panel A show that a child born in a district with a relative decline in tariff protection during the year of conception is *less likely to be a boy*; but the effect is not significant at conventional levels. Panels B, C, and D present the IV regression estimates. The first stage coefficients of traded tariff are positive and highly significant throughout. The IV and reduced form coefficients of district tariff in the year of conception are always positive, but only significant when we include the mother fixed-effects in Columns (4) and (5).[29] For a district with the average

[26]The Trivers-Willard hypothesis suggests that negative shocks to the fetal environment make births less likely to be male.

[27]Changes in prenatal sex-determination technology, however, are likely to impact the entire country, or at least all districts within a state similarly. Since our measure of tariff protection varies at the district level, this channel is unlikely to explain our results.

[28]Each cell indicates a separate regression. As before, all regressions use district-level sampling weights and robust standard errors are clustered at the district level.

[29]However, the coefficients for non-mother fixed-effects specifications are also significant when we use alternate

decline in tariffs of 7 percentage points, Column (4) in Panel C suggests that the likelihood of a male birth decreases by 2 percentage points. Thus, the reduction in trade protection seems to have caused some relative improvements in the probability of a female birth in rural Indian districts more exposed to tariff declines.[30]

The fact that we find significant results when we control for mother fixed-effects and not without them highlights the importance of time-invariant unobserved heterogeneity in factors that influence decisions about sex-selection. Apart from the monetary cost of prenatal sex-detection and sex-selective abortion, unobserved subjective son preference is likely to be an important factor in parents' decisions about sex-selection. Although in specifications without mother fixed-effects we control for some observable socio-economic characteristics that are likely to be correlated with son preference, for example religion, it is possible that they do not fully capture the unobserved heterogeneity across women. In Section 3.5.1, we present evidence for heterogeneity in the effects on the sex ratio at birth across socio-economic groups.

3.4.3 Infant Mortality

Next, we examine the effect of tariff decline on infant mortality. Following the same format as before, Table 3.4 presents results from OLS (Panel A) and IV regressions (Panels B and C) of indicators for whether a child dies within one, six, or twelve months of birth on district-level tariff protection. Across all specifications, the coefficient estimates are negative, indicating that a larger decline in tariff protection within a district is associated with a relative increase in infant mortality within one, six, as well as twelve months of birth. However, our OLS and IV estimates lose significance at conventional levels when we add district-specific linear time trends to the regressions (except for

levels of clustering, e.g. district-year. Here, we report results with more conservative standard errors, which in our case are obtained from clustering at the district level.

[30]Table A3.1 in the Appendix shows that the effects on SRB in urban areas were insignificant even for the mother fixed-effects specification. Moreover, the coefficients are of the opposite sign as compared to the rural results.

mortality within 12 months).

The magnitude of coefficient estimates increases as we change our outcome variable from mortality within one month of birth to mortality within six months to twelve months of birth. The fact that we find significant results on mortality within the first month of birth for some specifications suggests that trade liberalization also influences parents' ability to invest in the health of a child while in-utero.[31] However, the increase in the magnitude of the coefficients as we look at mortality within six and twelve months implies that trade liberalization prevents families from making the necessary investments in a child's health to prevent infant death even after birth.[32]

Moreover, the estimated effects are economically significant. For example, our coefficient estimate of -0.118 in Column (4), Panel C3 of Table 3.4 indicates that, relative to other districts, a district that experienced the average decline in tariff protection of 7 percentage points witnessed an increase in infant mortality within twelve months of birth of 0.8 percentage points – about a 9 percent increase with respect to the baseline (1987) mortality within a year of birth in all districts (9 percent).

In Table 3.5, we interact the tariff measure with a dummy for the child being male to test if the effect on mortality differs by child's sex. Previous research on the Trivers-Willard Hypothesis indicates that male children are less likely to survive relative to females in harsher environments (Almond and Edlund (2007)). If a decline in tariff protection increases poverty and decreases health investments in pregnant women or newborn children, we might expect trade liberalization to have a greater effect on mortality rates of male children. The main effect of our tariff measure suggests that there is a significant increase in mortality within twelve months of birth for girls.[33]

[31] Investments in health while in-utero are also likely to be affected by the tariff in the year of conception. In order to examine this channel, we also run regressions using tariff in the year nine months prior to the year of birth as the explanatory variable. The tariff coefficients are negative but not always significant and smaller in magnitude in comparison to the coefficients in Table 3.4.

[32] We find no significant effect on any mortality outcome for urban areas in Table A3.1 in the Appendix.

[33] Similar results are obtained for mortality within one and six months of birth and are available upon request.

The coefficient of the interaction term is positive everywhere, suggesting a smaller effect on boys. However, the interaction term is significant only in Columns (4) and (5) that include mother fixed-effects. According to the coefficients in Column (4), in a district that experienced the average decline in tariff protection of 7 percentage points, girls witnessed an increase in infant mortality within twelve months of birth of 1.6 percentage points as opposed to boys for whom mortality within twelve months increased by a much lower 0.1 percentage points. These effects are consistent with prior evidence on postnatal neglect and discrimination in care against girls in India. To the extent that parents are able to exercise their preference for male children at the prenatal stage through sex-selection, an increase in sex-selective abortions can lead to a decline in relative female mortality (Lin, Liu, and Qian (2010)). But if poverty makes sex-selection less affordable, then we may expect the pattern we observe: a lower sex ratio at birth, but higher relative female mortality. As we show in the next section, there is substantial heterogeneity in the effects on infant mortality across socio-economic groups.

3.5 Mechanisms

So far, our results suggest that relative declines in tariffs in rural Indian districts lead to a significant increase in the probability of birth and the likelihood of these births being female, although the latter effect is significant only for the mother fixed-effects specification. Moreover, the likelihood that a child dies within one, six or twelve months of birth significantly increases. Next, we explore the mechanisms underlying these results.

We attempt to distinguish between three potential channels: 1) poverty, 2) female bargaining power, and 3) relative returns from daughters. Unfortunately, our data does not contain information on household income, consumption expenditure, wages, or mother's labor force participation status, making it difficult to directly test for the aforementioned causal channels. As a second best approach, we examine heterogeneity in effects by the socio-economic characteristics of mothers to provide suggestive evidence about the causal mechanisms underlying our main results. This approach is based on the premise that the three channels mentioned above should affect our three outcome variables differently, thus helping us deduce the underlying mechanisms. Moreover, it is also possible that mechanisms differ across socio-economic groups. Before we proceed to the regression results, we first discuss the expected effect on our outcome variables through each of these three channels.

Topalova (2010) shows that districts more exposed to trade liberalization witnessed a relative increase in poverty. Households that suffer a negative income shock due to tariff cuts may be less able to afford modern birth control methods and sex-selective abortions, causing an increase in births, especially female births.[34] In addition, the supply of free or subsidized contraception may decline if government finances decline as a result of trade liberalization. Moreover, if poverty impacts the probability that a child survives to adulthood or the likelihood that a child is male, households may choose to increase fertility if decisions about the number and the sex-composition of children are jointly made. Poverty can also lead to increases in infant mortality if families reduce investments in infant health as a result of a decline in income. Furthermore, if the additional girls born as a result of the increase in poverty and the resulting inability to plan fertility are viewed as "unwanted," we would expect the increase in infant mortality to be higher for daughters relative to sons. Thus, if the relative increase in poverty from trade reform is the underlying channel, we are likely to observe an increase in fertility, a decrease in the sex ratio at birth, and an increase in infant mortality, more so for girls. On the other hand, if poverty decreases as a result of trade reform, we expect the opposite effects, i.e. lower fertility, a higher sex ratio at birth, and lower infant mortality rates.

[34] Bhalotra and Cochrane (2010) show that wealthier families in India are more likely to practice sex-selection.

Aguayo-Tellez, Airola, and Juhn (2010) show that a NAFTA-related decrease in tariffs increased intra-household bargaining power of women in Mexico. They believe this is due to two reasons. First, technology-upgrading by firms in response to trade liberalization makes physically demanding skills less important in blue-collar jobs. As a result, the relative wage and employment of women improves in blue-collar occupations, as shown by Juhn, Ujhelyi, and Villegas-Sanchez (2012). Second, trade reform leads to growth which is concentrated in initially female-intensive industries, and thus benefits women in these industries relatively more if male and female labor are imperfect substitutes. If intra-household bargaining is the primary channel through which trade reform affects fertility and infant mortality, then we expect to see lower fertility due to higher opportunity cost of childbearing (Chiappori, Fortin, and Lacroix (2002), Rosenzweig and Wolpin (1980)), and lower mortality due to higher relative income of mothers. However, it is not clear which direction the sex ratio at birth would change in. For a given degree of son preference, a higher opportunity cost of "unwanted" children for working mothers might cause greater sex-selection and, thus, result in higher sex ratios at birth. Women in the labor force may also have a lower search cost of accessing prenatal sex-determination and abortion.

Lastly, Munshi and Rosenzweig (2006) find that the new economic opportunities resulting from globalization are mainly benefitting lower-caste girls in India. Despite increases in returns to non-traditional white-collar occupations, lower-caste parents continue to educate their sons in local language schools (that lead to traditional blue-collar jobs) in order to continue benefitting from caste-based networks. However, historically, lower-caste girls have not participated in these caste-based occupational networks due to low labor market participation, and are, hence, not constrained by them. As a result, lower-caste parents continue to channel boys into traditional occupations despite higher returns in more modern jobs, but their daughters benefit as a result of these improved employment opportunities. In a similar vein, Jensen and Miller (2011) show that parents in rural

India strategically try to prevent sons from migrating to urban areas to take advantage of better income opportunities because they want them to work on the farm. They find large gains in education for girls but not much for boys in response to greater employment opportunities in urban areas. They conclude that these results are driven by changes in returns (to parents) from sons and daughters. In our context, an increase in the relative demand for daughters, due to a relative increase in returns to parents from girls, would imply that lower-caste parents should now be more likely to give birth to daughters, who might also experience a *decrease* in mortality. In other words, the increase in female births in this scenario is driven by more "wanted" girls, unlike the poverty channel where more "unwanted" girls are born due to reduced affordability of sex-selection or lower opportunity cost of children. If the decrease in female mortality is sufficiently large, we will also observe an overall decrease in mortality across all births. The impact on the likelihood of birth depends on the extent to which parents substitute between sons and daughters in the short- and the long-run.

The following table summarizes the predictions discussed above. So far, our main findings appear consistent with the increased poverty channel, although the sex ratio at birth results are weak. Armed with these predictions, we now turn to analyzing heterogeneity in our effects across three dimensions – household's caste, mother's education level, and household's wealth index – to distinguish between these potential mechanisms.

	Expected effect on:		
Channel	Birth	Sex Ratio at Birth	Mortality
↑ Poverty	+	–	+
↑ Female bargaining power	–	Unclear	–
↑ Relative returns to daughters	Unclear	–	–

3.5.1 Heterogeneous Effects

We begin our examination of heterogeneity with a household's **caste**. We divide our sample into four categories - scheduled caste (SC), scheduled tribe (ST), other backward caste (OBC), and general caste - and interact our tariff variable with indicators for these categories. Table 3.6 presents these results for the birth dummy, male birth dummy, and mortality within twelve months. General caste is the omitted category. Panel A shows that the main effect of tariff in the year of birth is positive (insignificant in Column (3)) while the interaction terms for SC, ST, and OBC mothers are negative, highly significant, and larger in magnitude than the main effect. This implies that lower-caste women experience a significantly *larger increase* in the probability of birth relative to upper-caste mothers. Thus, our overall findings for fertility seem to be driven mainly by higher fertility for lower-caste mothers. There are no significant differences in our sex ratio results across caste groups, however.

The fertility effects are consistent with either the poverty or the returns channel working for the lower-caste households. Scheduled and other backward castes have historically been more economically and socially disadvantaged in India. A relative increase in poverty levels is, therefore, likely to affect them more strongly than upper-caste households. But, as the mortality results show, births to general caste mothers are significantly more likely to die due to tariff cuts while births to lower-caste mothers have significantly lower likelihood of death. The interaction coefficients for SC mothers are larger in magnitude, significant, and of the opposite sign as compared to the main effect. This pattern is inconsistent with the increased poverty channel.[35] Moreover, when we separate the effect on mortality by child's sex in Table 3.7, we find that the lower mortality results for SCs and OBCs are completely driven by girls. Upper-caste girls, on the other hand, experience

[35] Another possibility is that lower-caste households actually *benefit* from tariff cuts and experience a relative decrease in poverty which lowers mortality. But in that case, we would not expect to see their fertility increase, which is what we find in Panel A.

a rise in mortality.

In Table 3.8 we repeat the same exercise by mother's **education** level. We divide women into three categories - uneducated women, those with 1-5 years of schooling, and women with more than 5 years of schooling. Panel A shows that there is no effect on the likelihood of birth for mothers with more than primary education. But, births *increased* significantly and the probability that these births are male *decreased* significantly for uneducated mothers. For mortality within a year of birth, we observe a similar pattern as our caste results. For uneducated mothers, there is a relative decrease in mortality, whereas more educated mothers experience an increase. When we split the mortality results by child's sex in Table 3.9, we find a pattern somewhat similar to Table 3.7. Although both boys and girls born to uneducated mothers experience a relative decrease in mortality whereas those born to mothers with more than primary education experience a relative increase in mortality, the effects are larger for girls. To the extent that lower-caste women are likely to have a lower educational attainment, these results together highlight the possibility of gains for girls born to lower-status parents from tariff reductions through a relative increase in returns on the labor market.

To further explore the mechanisms, we next examine how our effects vary by the **wealth** index of the household. DLHS combines information on ownership of durables, type of toilet facility, cooking fuel, housing, source of lighting, and drinking water to calculate a standard of living score for each household. On the basis of these scores, households are divided into three categories: low, medium, and high standard of living (SLI) households. Ideally, we would like to know the household wealth score for each year in our sample period. But, unfortunately, since we create our birth and woman panels retrospectively from a single cross-section, we know a household's wealth category only at the time of survey. To the extent that trade reform affects standards of living, the wealth index variable is not exogenous. However, if most households move within a wealth category, and not across categories (e.g. a high-SLI family becoming low-SLI) due to tariff reduction, this comparison is still informative. With these caveats in mind, Panel A in Table 3.10 shows that the higher fertility and the decreased likelihood of a birth being male are mainly driven by low-SLI families. In fact, medium- and high-SLI families experience a significant decrease in the likelihood of birth. Unlike the weak effects in Table 3.3, there is a significant decrease in the sex ratio at birth for relatively poorer, low-SLI families and a slightly significant increase for the high-SLI households in response to the tariff decline. The magnitude of this effect is remarkably similar across all specifications and suggests that in districts with an average relative decline in tariff of 7 percentage points, the sex ratio at birth decreases by 1.5 percentage points in low-SLI households. Yet again, the mortality results suggest that the effects for low-SLI women are potentially driven by the returns channel. Mortality *decreases* for births to low-SLI women relative to high-SLI families. For latter, the main effect is an increase in mortality. When we split the mortality results by child's sex in Table 3.11, we find that both boys and girls born to high-SLI mothers experience a relative increase in mortality whereas those born to low-SLI mothers experience a significant decrease in mortality relatively. But unlike the caste results in Table 3.7, the difference between the results for boys and girls is not remarkable.

3.5.2 Supporting Evidence

Next, we utilize data from NSS to shed further light on potential mechanisms. If a relative change in the returns from children on the labor market is an underlying channel for our previous findings, we also expect differential changes in educational investments by socio-economic status in response to tariff cuts. To test this, we follow Edmonds, Pavcnik, and Topalova (2010b) and estimate the effect of tariff cuts on schooling and child labor outcomes of 10-14 year old children in rural Indian districts using the 43rd (1987-88) and the 55th (1999-00) rounds of the NSS Employment-Unemployment survey. Edmonds, Pavcnik, and Topalova (2010b) find that schooling increased and child labor declined at a slower rate in districts that were relatively more exposed to tariff cuts. We extend their analysis and examine heterogeneity in these outcomes by the caste of the household.[36] Since the 43rd round does not distinguish between OBC and general castes, we divide the sample into three categories - SC, ST, and the rest. Table 3.12 presents the IV estimates for the effects of tariff reform on an indicator for the child being in school (*School*) and five categories of work. $Work$ is an indicator for a child's principal activity status being work-related[37], irrespective of her school attendance status. *Work only* indicates that the child's principal activity is work and she does not attend school. *Market work* implies that the child works as a regular salaried/ wage employee, a casual wage laborer, in a household enterprise (farm or non-farm), or as a beggar. *Domestic work* indicates that the child's principal activity is domestic work. Lastly, $Idle$ refers to children who neither work nor attend school.

Table 3.12 shows that the estimates presented in Edmonds, Pavcnik, and Topalova (2010b) exhibit substantial heterogeneity by caste and are consistent with our previous findings. Panel A[38] shows that SC children in districts relatively more exposed to tariff reform are significantly more likely to be in school, relative to children from OBC and general castes. The overall effect on schooling for SC children is also positive. In addition, Columns (2) - (4) show that SC children are less likely to work, both relative to the excluded category as well as overall, as tariffs decrease. We also observe a significant decrease in the likelihood of an SC child being $Idle$ in response to the reform. On the other hand, general and OBC children are significantly less likely to be in school when tariffs decrease relatively in their districts. However, the effects on work categories for these non-SC and non-ST children are not significant. These results are consistent with tariff declines

[36] The NSS does not provide information on mother's education or the household wealth status.

[37] This includes the following categories of work: regular salaried/ wage employee, casual wage laborer, begging, work in a household enterprise, and domestic work.

[38] In Table 3.12 Panel A, we do not control for the sex of the child since it is also affected by the tariff cuts. However, the results remain the same when we control for child sex.

resulting in a relative increase in the returns from lower-caste children, relative to general and OBC children. However, further disaggregation of the sample by child sex in Panels B and C does not reveal differential effects for SC boys and girls.

In addition, we assess the relative returns channel by examining the effect of tariff cuts on adult employment in a manner similar to Table 3.4 in Edmonds, Pavcnik, and Topalova (2010b). These results are presented in Table 3.13. The outcome variable is the number of days worked in the last year. Column (1) shows that tariff declines are associated with a significant decrease in the number of days worked for SC and ST men relative to non-SC and non-ST men. Overall, the latter group experiences a significant increase in the number of days spent in wage work in response to tariff cuts, but the total effect for SC men is not large. On the other hand, in Column (2), there is a significant increase in the number of days worked for SC and ST women, relative to other women as well as overall, as tariffs decrease. For non-SC and non-ST women we observe a decrease in employment, albeit the coefficient is not significant. These results are consistent with trade liberalization increasing (decreasing) the returns for lower-caste (upper-caste) females, relative to males, in turn causing lower-caste (upper-caste) parents to give birth to more (fewer) girls and investing more (less) in them.

3.6 Robustness

Although prior literature on Indian trade liberalization (Topalova (2010), Edmonds, Pavcnik, and Topalova (2010b)) shows that there is no significant factor mobility in response to tariff cuts, we explicitly check for endogenous sorting by gender and caste. We cannot use DLHS data for this exercise since it only reports a woman's district of residence at the time of survey. Consequently, we use NSS data to estimate the effect of tariff cuts on the caste- and gender-composition of a district's in-migrants[39] using the same empirical strategy as in Section 3.5.2. These instrumental-variable estimates are reported in Table 3.14. There is no significant change in the share of female in-migrants and low-caste female in-migrants in a district due to the tariff reform (Rows 1 and 2). The same is also true for the share of short-run migrants (i.e. those who have moved within the past ten years) in Rows 3 and 4 of Table 3.14.

Although we include district-specific linear time trends in all regressions, a potential concern with our identification strategy is the presence of other time-varying district-specific omitted variables. Since our tariff exposure index varies at the district-year level, we cannot include district-year fixed-effects to prevent this omitted variable bias. Instead, we re-estimate all specifications by controlling for district-level annual rainfall shocks as a further check of robustness. Annual fluctuations in rainfall are an important determinant of economic outcomes in agriculture-dependent developing countries, such as India.[40] We define rainfall shock as an indicator variable that is equal to one if the annual rainfall in a district deviates by more than 30 percent from its historic annual mean precipitation, and zero otherwise. The point estimates on tariff measures in all specifications remain consistent with our previous results (with similar signs, magnitudes, and significance). These results are available upon request.

3.7 Discussion

To sum up, we see substantial differences in how trade liberalization has affected women and children across social strata. Broadly, we find that low socioeconomic status women experience an increase in fertility which is driven by more female births. Since we also observe a relative decrease in mortality for their daughters, we interpret this as a higher demand for daughters by low-status families. The same does not hold for high-status women. There is some evidence that they have

[39]In-migrants are individuals whose district of residence at the time of survey is different from their last place of usual residence (i.e. a place where the person has stayed continuously for a period of six months or more).

[40]Paxson (1992), Rosenzweig and Wolpin (1993), Townsend (1994), Jayachandran (2006).

fewer children, driven by fewer girls, and we find strong evidence that girls born to high-status mothers fare worse in terms of higher mortality. Mortality rates for boys, however, do not seem to be significantly affected by the trade reform, irrespective of their parents' socio-economic status.

Thus, there appears to be a strong gender component to the effects of trade liberalization. If tariff reforms have improved earning opportunities for women in blue-collar occupations, as recent literature suggests, we would expect the gains to be derived by girls born to low-status families. We find some suggestive evidence that low-caste women work more and low-caste men work less relative to upper-caste women and men when tariffs decreases. To the extent that upper-caste and more educated women in India are less likely to participate in blue-collar occupations, we do not expect girls in high-status families to benefit from the new labor market opportunities in relatively blue-collar jobs as much as low-status women. Moreover, if returns from more skilled jobs have increased mainly for men in India, high-status families will prefer to have more sons. As supporting evidence, we find that there is an increase in the number of days worked for non-SC and non-ST men in districts experiencing a relative decrease in tariffs.

The effects of trade reform on fertility and child health outcomes that we find do not suggest that there has been an increase in the relative bargaining power for women. Lastly, the apparent increase in valuation of girls in low-status families and decrease in high-status families in response to trade reform can also be due to differential returns from children on the marriage market instead of or in addition to returns on the labor market (Edlund and Lee (2009)).

3.8 Conclusions

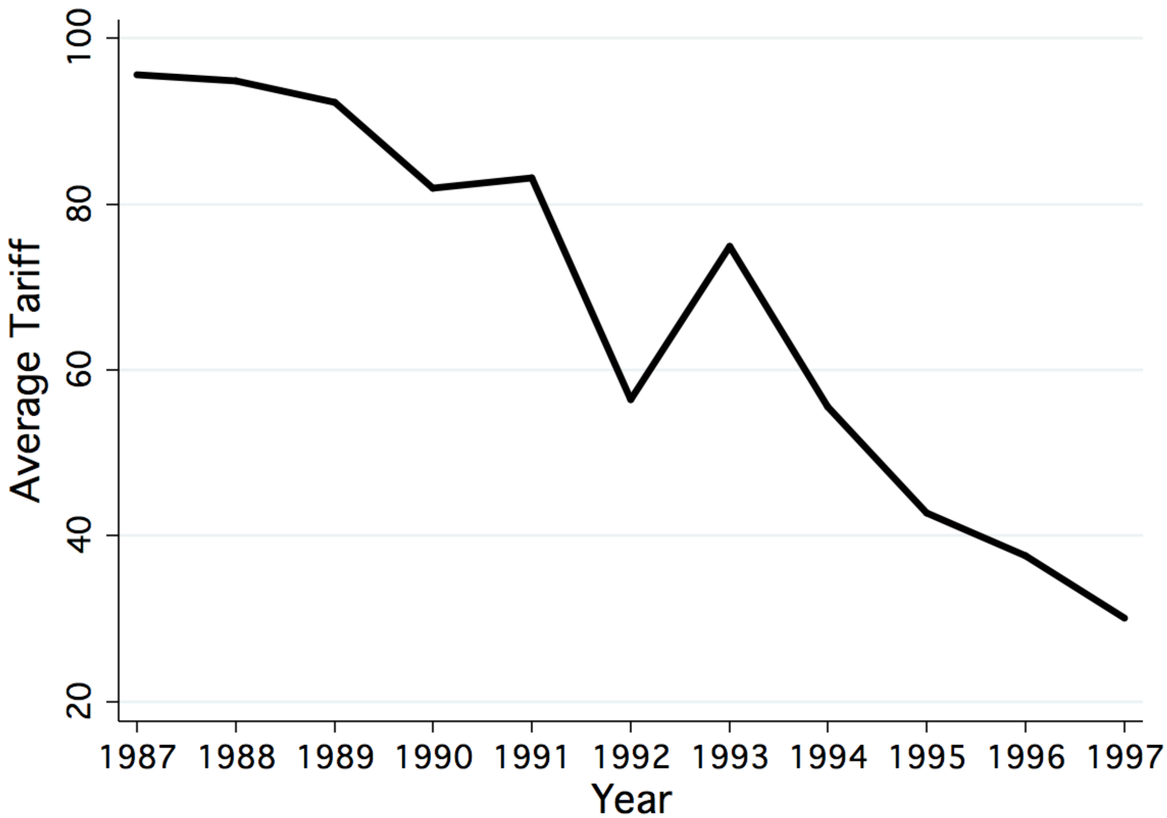
This paper analyzes whether India's trade liberalization, beginning in 1991, affected fertility, infant mortality, and sex ratios at birth. To identify the impact of this trade policy shock, we compare rural districts more exposed to tariff cuts to rural districts less exposed to tariff cuts. Previous research using a similar empirical strategy finds that districts subject to greater reductions in tariffs experience slower declines in poverty as well as slower increases in school enrollment ((Topalova (2010), Edmonds, Pavcnik, and Topalova (2010b)). We find that lower-caste, less educated, and less wealthy women in districts with a higher relative trade reform exposure are more likely to give birth and these births are more likely to be female. Moreover, infant mortality (within one, six, and twelve months of birth) decreases for these girls. In contrast, girls born to upper-caste, more educated, and wealthier mothers experience relatively higher mortality. They are also less likely to be born.

It is important to emphasize that these results do not suggest that more open trade leads to overall increases or decreases in fertility, sex ratios, or infant mortality.[41] However, our results do confirm that removal of trade barriers has important distributional consequences along these dimensions, especially for girls. Data limitations prevent us from conducting a more rigorous analysis of the exact channels through which tariff cuts affect individuals' fertility decisions and investments in children, but we highlight the potential role played by differential returns from children that vary by socio-economic status and child sex.

[41] We note again that this paper only examines the effect of tariff reductions and ignores the removal of NTBs.

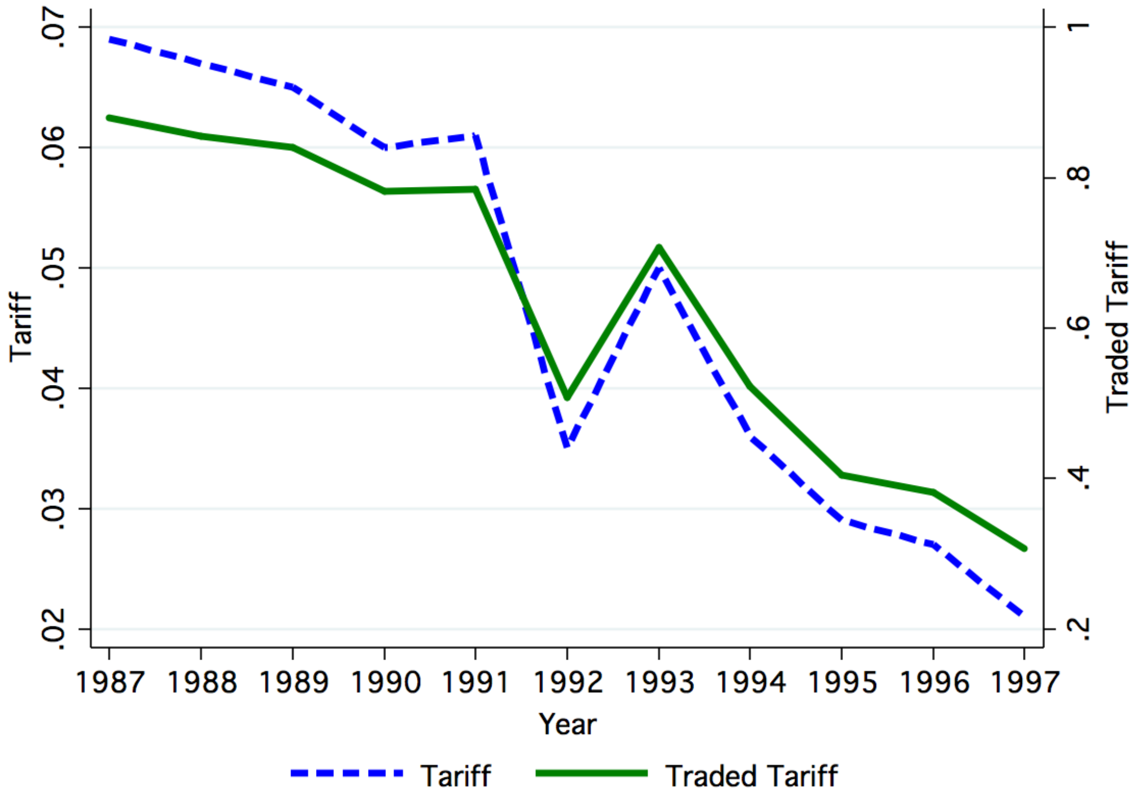
3.9 Figures

Figure 3.1. Average Industry-level Tariff



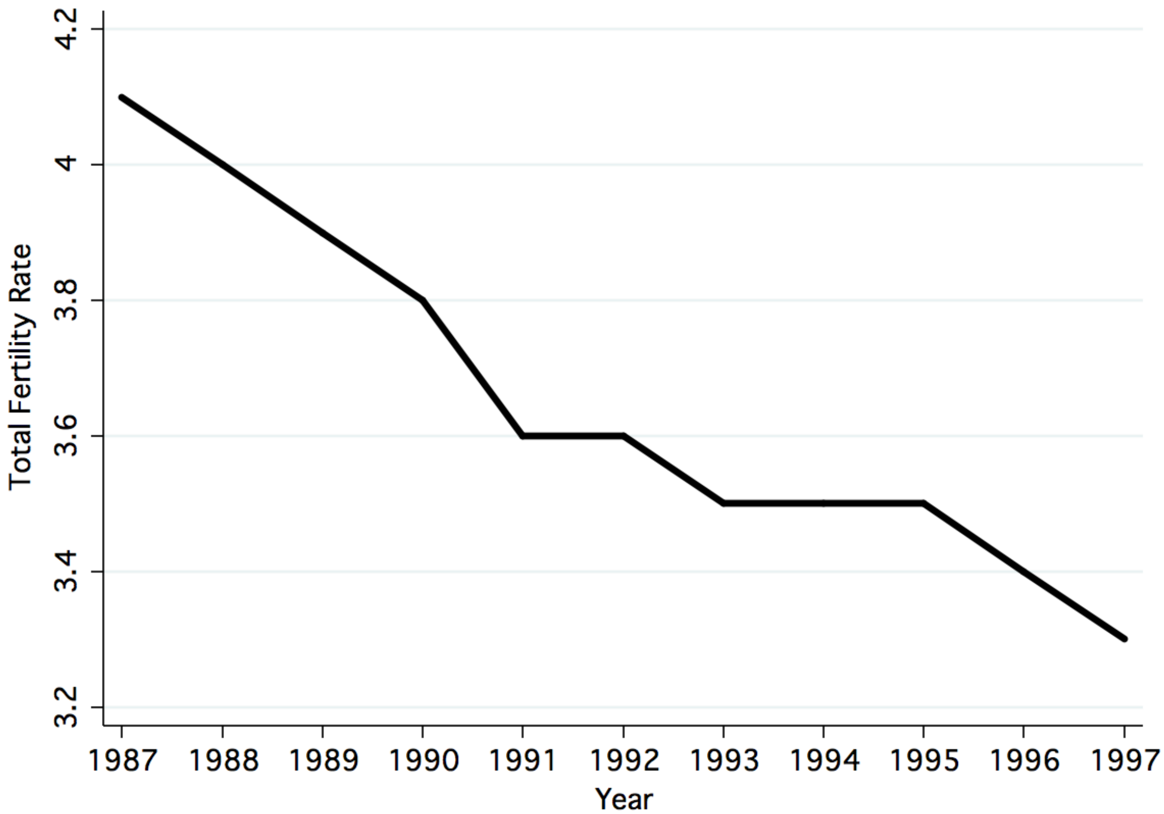
Notes: This figure plots the yearly averages of nominal, national, industry-level ad-valorem tariffs using data provided by Petia Topalova.

Figure 3.2. Average District-level Tariff and Traded Tariff, by Year



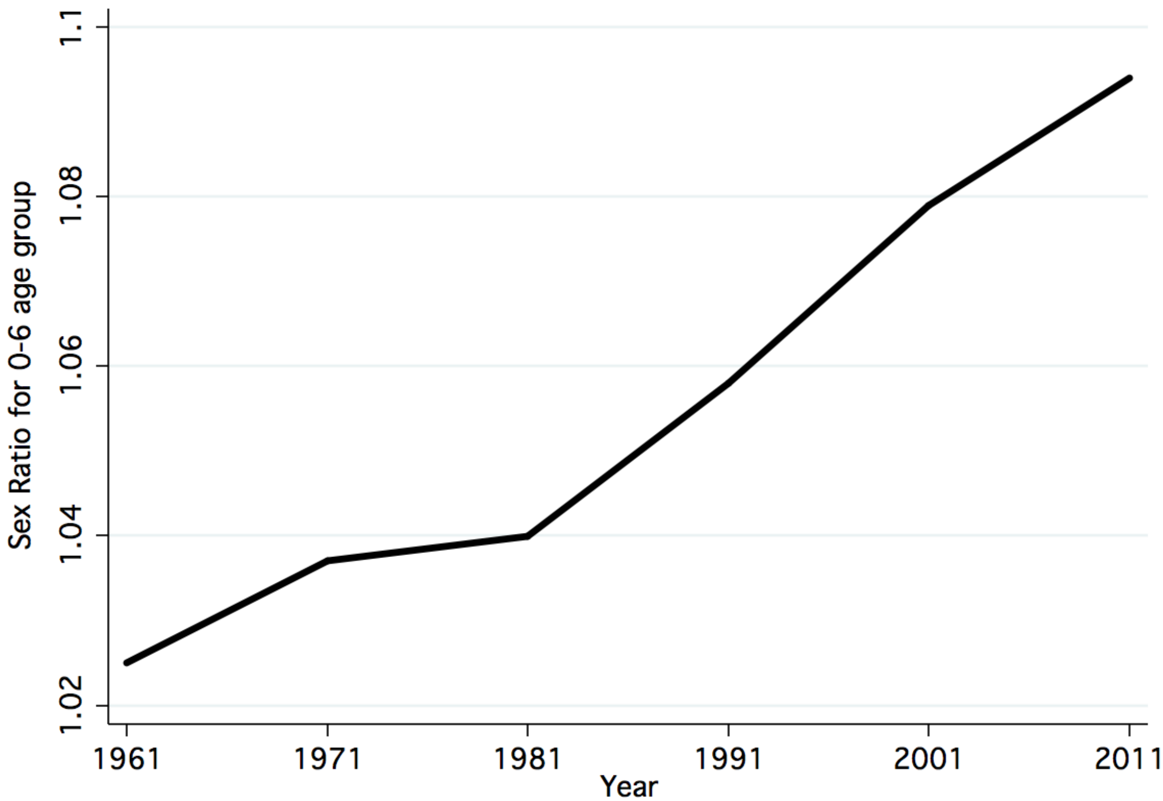
Notes: This figure plots the yearly averages of the district-level tariff and traded tariff measures used in this paper. District-year data on both measures was provided by Petia Topalova. Tariff is constructed as the district-specific employment weighted sum of industry-specific national tariffs. Traded tariff is constructed in a similar way, but only uses employment in traded sectors within a district. District-level employment shares in 1991 are used as weights. More details are available in Section 3.3.1.

Figure 3.3. Total Fertility Rate in India, by Year



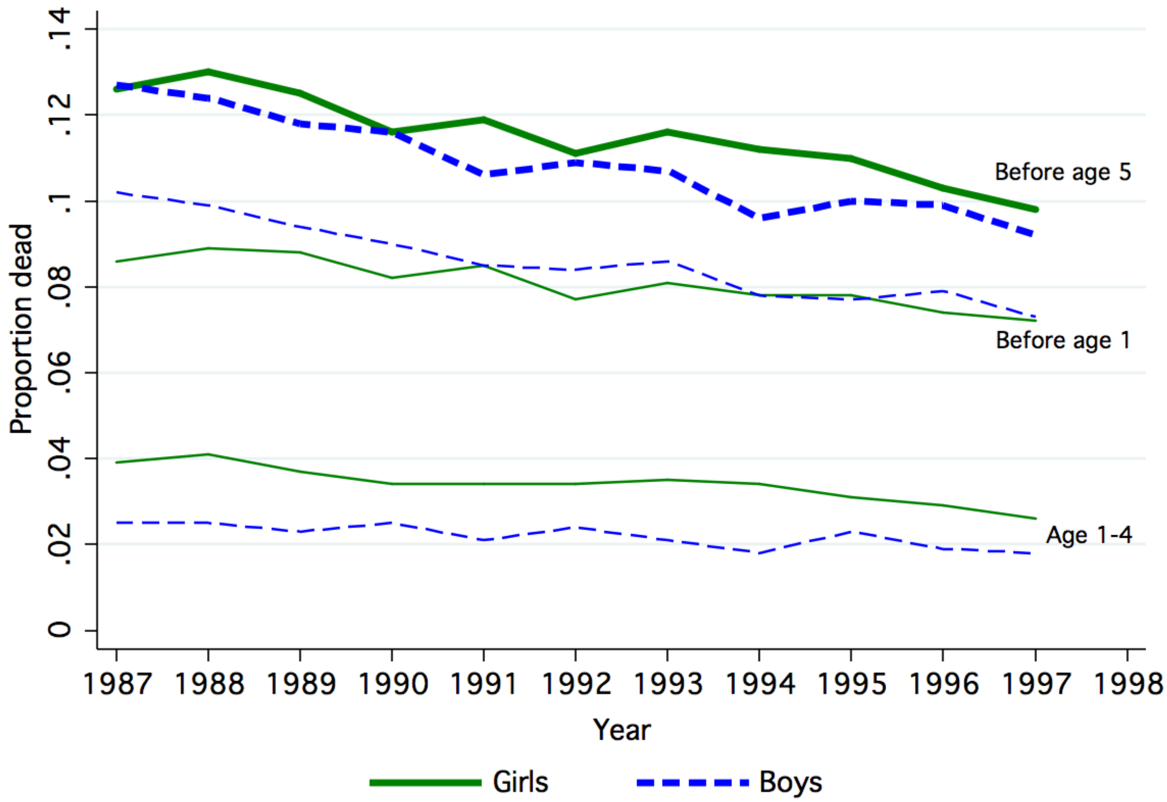
Notes: Data from Ministry of Health and Family Welfare, Govt. of India (*accessed from Indiastat*

Figure 3.4. Child Sex Ratio (0-6) in India, by Year



Notes: Data from Census of India

Figure 3.5. Infant Mortality in Rural India, by Year of Birth and Sex



Notes: This figure plots the average proportion of children who died before age 1, during ages 1-4 and before age 5, by year of birth and sex. All-India sample weights used. Data source is DLHS (2002-04).

3.10 Tables

Table 3.1. Summary Statistics for the Rural Sample, 1987 and 1997

Variable	1987	1997
	A. Panel of Births	
Birth is male	0.517	0.521
Parity of birth	2.34	2.96
Mother's age at birth	21.04	23.28
Mother's years of schooling	1.90	2.51
Hindu	0.78	0.77
Muslim	0.09	0.10
Sikh	0.03	0.02
Christian	0.07	0.08
Scheduled Caste	0.18	0.19
Scheduled Tribe	0.19	0.22
Other Backward Caste	0.38	0.38
Died within one month of birth	0.06	0.05
Died within 6 months of birth	0.08	0.06
Died within 12 months of birth	0.09	0.07
Low HH Wealth Index	0.60	0.67
Medium HH Wealth Index	0.30	0.25
High HH Wealth Index	0.10	0.08
N(births)	31,356	48,755
	B. Panel of Women	
Birth	0.22	0.18
N(women)	139,478	269,347
N(districts)	408	408

Notes: This table presents summary statistics for the earliest (1987) and the latest (1997) years included in our rural sample. All regressions include every year during 1987-1997.

Table 3.2. The Effect of Tariff Reduction on the Probability of Birth

	(1)	(2)	(3)	(4)	(5)
		A. OLS			
Tariff in YOB	0.129***	0.104***	0.069**	0.581***	0.478***
	[0.031]	[0.030]	[0.030]	[0.109]	[0.104]
		B. First Stage			
Traded Tariff in YOB	0.217***	0.217***	0.212***	0.212***	0.208***
	[0.031]	[0.031]	[0.030]	[0.030]	[0.029]
F-stat	48.16	48.19	49.14	49.86	50.92
		C. IV			
Tariff in YOB	-0.081	-0.118**	-0.163***	-0.108	-0.244*
	[0.054]	[0.055]	[0.061]	[0.118]	[0.130]
		D. Reduced Form			
Traded Tariff in YOB	-0.018*	-0.026***	-0.035***	-0.023	-0.051**
	[0.010]	[0.010]	[0.010]	[0.025]	[0.025]
N	1,857,834	1,857,834	1,857,834	1,857,834	1,857,834
District FE	x	x	x		
Year FE	x	x	x	x	x
Covariates		x	x	x	x
District-specific linear trends			x		x
Mother FE				x	x

Notes: YOB stands for the year of birth. Each cell constitutes a separate regression. Columns (2) - (5) include indicators for mother's age at birth and number of previous births. Columns (2) and (3) also include mother's years of schooling and household's religion and caste dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.3. The Effect of Tariff Reduction on Probability that a Birth is Male

	(1)	(2)	(3)	(4)	(5)
		A. OLS			
Tariff in YOC	0.078	0.080	0.084	0.035	0.055
	[0.050]	[0.050]	[0.053]	[0.101]	[0.106]
		B. First Stage			
Traded Tariff in YOC	0.201***	0.201***	0.196***	0.182***	0.178***
	[0.031]	[0.031]	[0.030]	[0.028]	[0.027]
F-stat	42.45	42.47	43.02	42.44	42.34
		C. IV			
Tariff in YOC	0.097	0.097	0.107	0.314***	0.356***
	[0.066]	[0.066]	[0.070]	[0.113]	[0.127]
		D. Reduced Form			
Traded Tariff in YOC	0.019	0.020	0.021	0.057**	0.063**
	[0.014]	[0.014]	[0.015]	[0.025]	[0.028]
N	449,065	449,065	449,065	449,065	449,065
District FE	x	x	x		
Year FE	x	x	x	x	x
Covariates		x	x	x	x
District-specific linear trends			x		x
Mother FE				x	x

Notes: YOC stands for the year of conception - defined as the year nine months prior to the month of birth. Each cell constitutes a separate regression. Columns (2) - (5) include indicators for mother's age at birth and number of previous births. Columns (2) and (3) also include mother's years of schooling and household's religion and caste dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.4. The Effect of Tariff Reduction on Infant Mortality

	(1)	(2)	(3)	(4)	(5)
		Mortality in 1 month			
		A1. OLS			
Tariff in YOB	-0.035**	-0.041**	-0.025	-0.023	-0.013
	[0.016]	[0.016]	[0.017]	[0.039]	[0.041]
		B1. Reduced Form			
Traded Tariff in YOB	-0.009	-0.011*	-0.007	-0.015	-0.010
	[0.006]	[0.006]	[0.006]	[0.011]	[0.011]
		C1. IV			
Tariff in YOB	-0.045	-0.052*	-0.036	-0.080*	-0.058
	[0.028]	[0.029]	[0.030]	[0.047]	[0.047]
		Mortality in 6 months			
		A2. OLS			
Tariff in YOB	-0.046**	-0.054***	-0.031	-0.036	-0.018
	[0.018]	[0.019]	[0.019]	[0.043]	[0.044]
		B2. Reduced Form			
Traded Tariff in YOB	-0.011*	-0.013*	-0.008	-0.020	-0.014
	[0.007]	[0.007]	[0.007]	[0.012]	[0.013]
		C2. IV			
Tariff in YOB	-0.055*	-0.064*	-0.040	-0.106**	-0.080
	[0.033]	[0.033]	[0.035]	[0.054]	[0.055]
		Mortality in 12 months			
		A3. OLS			
Tariff in YOB	-0.052**	-0.063***	-0.038*	-0.041	-0.019
	[0.020]	[0.021]	[0.022]	[0.046]	[0.046]
		B3. Reduced Form			
Traded Tariff in YOB	-0.014*	-0.016**	-0.010	-0.022	-0.016
	[0.007]	[0.008]	[0.008]	[0.013]	[0.013]
		C3. IV			
Tariff in YOB	-0.067*	-0.076**	-0.051	-0.118**	-0.091
	[0.036]	[0.037]	[0.038]	[0.058]	[0.058]
		First Stage			
Traded Tariff in YOB	0.207***	0.207***	0.201***	0.185***	0.180***
	[0.032]	[0.032]	[0.030]	[0.028]	[0.027]
F-stat	42.85	42.88	43.88	43.14	43.44
N	473,430	473,430	473,430	473,430	473,430
District FE	x	x	x		
Year FE	x	x	x	x	x
Covariates		x	x	x	x
District-specific linear trends			x		x
Mother FE				x	x

Notes: YOB stands for the year of birth. Each cell constitutes a separate regression. All regressions include indicators for mother's age at birth and number of previous births. Columns (2) - (5) include indicators for mother's age at birth and number of previous births. Columns (2) and (3) also include mother's years of schooling and household's religion and caste dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.5. IV Estimates for Infant Mortality: By Child's Gender

	(1)	(2)	(3)	(4)	(5)
	A. Mortality in 12 months				
Tariff in YOB * Boy	0.071	0.063	0.065	0.212***	0.214***
	[0.046]	[0.046]	[0.046]	[0.066]	[0.067]
Tariff in YOB	-0.104**	-0.109**	-0.085*	-0.226***	-0.200***
	[0.044]	[0.044]	[0.045]	[0.068]	[0.068]
N	473,430	473,430	473,430	473,430	473,430
District FE	x	x	x		
Year FE	x	x	x	x	x
Covariates		x	x	x	x
District-specific linear trends			x		x
Mother FE				x	x

Notes: YOB stands for the year of birth. Each column constitutes a separate regression. Columns (2) - (5) include indicators for mother's age at birth and number of previous births. Columns (2) and (3) also include mother's years of schooling and household's religion and caste dummies. The main effect of *Boy* is included in all specifications, but not reported. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.6. IV Estimates: By Caste

	(1)	(2)	(3)
	A. Birth=1		
Tariff in YOB * SC	-0.449***	-0.362***	-0.326***
	[0.096]	[0.085]	[0.082]
Tariff in YOB * ST	-0.730***	-0.731***	-0.793***
	[0.147]	[0.142]	[0.151]
Tariff in YOB * OBC	-0.250***	-0.211***	-0.145**
	[0.077]	[0.069]	[0.063]
Tariff in YOB	0.191**	0.126*	0.059
	[0.078]	[0.072]	[0.075]
N	1,857,834	1,857,834	1,857,834
	B. Boy=1		
Tariff in YOC * SC	0.031	0.035	0.010
	[0.138]	[0.138]	[0.138]
Tariff in YOC * ST	0.083	0.081	0.123
	[0.154]	[0.154]	[0.158]
Tariff in YOC * OBC	-0.036	-0.034	-0.061
	[0.113]	[0.113]	[0.112]
Tariff in YOC	0.093	0.092	0.110
	[0.097]	[0.097]	[0.101]
N	449,065	449,065	449,065
	C. Mortality in 12 months		
Tariff in YOB * SC	0.248***	0.247***	0.207***
	[0.081]	[0.080]	[0.079]
Tariff in YOB * ST	-0.127	-0.131	-0.073
	[0.085]	[0.086]	[0.079]
Tariff in YOB * OBC	0.138**	0.136**	0.085
	[0.058]	[0.058]	[0.058]
Tariff in YOB	-0.136***	-0.142***	-0.103*
	[0.052]	[0.052]	[0.053]
N	473,430	473,430	473,430
District FE	x	x	x
Year FE	x	x	x
Covariates		x	x
District-specific linear trends			x

Notes: YOB stands for the year of birth. YOC stands for the year of conception. General caste households are the excluded group. The main effects of SC, ST, and OBC are included in all regressions but not reported. Columns (2)-(3) include indicators for mother's age at birth and number of previous births. Column (2) also includes mother's years of schooling and household's religion dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.7. IV Estimates for Infant Mortality: By Caste and Child's Sex

	Girls	Boys
	A. Mortality in 12 months	
Tariff in YOB * SC	0.334***	0.097
	[0.110]	[0.100]
Tariff in YOB * ST	-0.059	-0.074
	[0.111]	[0.103]
Tariff in YOB * OBC	0.216***	-0.028
	[0.078]	[0.078]
Tariff in YOB	-0.167**	-0.050
	[0.069]	[0.072]
N	227,881	245,549
District FE	x	x
Year FE	x	x
Covariates	x	x
District-specific linear trends	x	x

Notes: YOB stands for the year of birth. General caste households are the excluded group. The main effects of SC, ST, OBC are included in all regressions but not reported. All regressions include indicators for mother's age at birth, number of previous births, mother's years of schooling and household's religion dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.8. IV Estimates: By Mother's Education

	(1)	(2)	(3)
	A. Birth=1		
Tariff in YOB * Uneducated	-0.138**	-0.396***	-0.338***
	[0.067]	[0.069]	[0.067]
Tariff in YOB * 1-5 years	0.309***	0.102	0.109*
	[0.081]	[0.065]	[0.065]
Tariff in YOB	-0.080	0.060	0.002
	[0.060]	[0.053]	[0.056]
N	1,354,769	1,354,769	1,354,769
	B. Boy=1		
Tariff in YOC * Uneducated	0.305**	0.292**	0.267*
	[0.143]	[0.142]	[0.142]
Tariff in YOC * 1-5 years	-0.152	-0.161	-0.159
	[0.188]	[0.187]	[0.187]
Tariff in YOC	-0.091	-0.082	-0.066
	[0.125]	[0.124]	[0.129]
N	277,601	277,601	277,601
	C. Mortality in 12 months		
Tariff in YOB * Uneducated	0.207***	0.258***	0.214***
	[0.066]	[0.067]	[0.064]
Tariff in YOB * 1-5 years	-0.068	-0.044	-0.052
	[0.071]	[0.071]	[0.071]
Tariff in YOB	-0.173***	-0.216***	-0.174***
	[0.056]	[0.058]	[0.059]
N	290,653	290,653	290,653
District FE	x	x	x
Year FE	x	x	x
Covariates		x	x
District-specific linear trends			x

Notes: YOB stands for the year of birth. YOC stands for the year of conception. Women with more than 5 years of education are the excluded group. The sample is restricted to women above age 20 at the time of survey. The main effects of education groups are included in all regressions but not reported. Columns (2)-(3) include indicators for mother's age at birth and number of previous births. Column (2) also include household's caste and religion dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.9. IV Estimates for Infant Mortality: By Mother's Education and Child's Sex

	Girls	Boys
	A. Mortality in 12 months	
Tariff in YOB * Uneducated	0.292***	0.140*
	[0.084]	[0.078]
Tariff in YOB * 1-5 years	-0.063	-0.050
	[0.109]	[0.102]
Tariff in YOB	-0.177**	-0.164**
	[0.076]	[0.075]
N	139,491	151,162
District FE	x	x
Year FE	x	x
Covariates	x	x
District-specific linear trends	x	x

Notes: YOB stands for the year of birth. Women with more than 5 years of education are the excluded group. The sample is restricted to women above age 20 at the time of survey. The main effects of education groups are included in all regressions but not reported. All regressions include indicators for mother's age at birth, number of previous births, and household's caste and religion dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.10. IV Estimates: By Household Wealth Index

	(1)	(2)	(3)
	A. Birth=1		
Tariff in YOB * Low SLI	-0.835***	-0.768***	-0.710***
	[0.097]	[0.090]	[0.085]
Tariff in YOB * High SLI	0.066	0.014	0.005
	[0.051]	[0.046]	[0.046]
Tariff in YOB	0.366***	0.301***	0.235***
	[0.054]	[0.052]	[0.054]
N	1,857,834	1,857,834	1,857,834
	B. Boy=1		
Tariff in YOC * Low SLI	0.239**	0.234**	0.226**
	[0.110]	[0.111]	[0.112]
Tariff in YOC * High SLI	-0.247	-0.245	-0.249*
	[0.150]	[0.150]	[0.150]
Tariff in YOC	-0.022	-0.018	-0.009
	[0.097]	[0.097]	[0.102]
N	449,065	449,065	449,065
	C. Mortality in 12 months		
Tariff in YOB * Low SLI	0.201***	0.217***	0.178***
	[0.054]	[0.054]	[0.052]
Tariff in YOB * High SLI	0.078	0.046	0.054
	[0.063]	[0.063]	[0.063]
Tariff in YOB	-0.197***	-0.212***	-0.169***
	[0.045]	[0.046]	[0.046]
N	473,430	473,430	473,430
District FE	x	x	x
Year FE	x	x	x
Covariates		x	x
District-specific linear trends			x

Notes: YOB stands for the year of birth. YOC stands for the year of conception. Medium SLI households are the excluded group. The main effects of High SLI and Low SLI are included in all regressions but not reported. Columns (2)-(3) include indicators for mother's age at birth and number of previous births. Column (2) also includes mother's years of schooling and household's religion and caste dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.11. IV Estimates for Infant Mortality: By Household Wealth Index and Child's Sex

	Girls	Boys
	A. Mortality in 12 months	
Tariff in YOB * Low SLI	0.178***	0.174**
	[0.068]	[0.071]
Tariff in YOB * High SLI	-0.090	0.023
	[0.084]	[0.082]
Tariff in YOB	-0.160**	-0.173***
	[0.063]	[0.062]
N	227,881	245,549
District FE	x	x
Year FE	x	x
Covariates	x	x
District-specific linear trends	x	x

Notes: YOB stands for the year of birth. Medium-SLI households are the excluded group. The main effects of High-SLI and Low-SLI are included in all regressions but not reported. All regressions include indicators for mother's age at birth, number of previous births, years of schooling, and household's caste and religion dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

Table 3.12. IV Estimates: Children's Schooling and Labor Force Participation by Caste

	School (1)	Work (2)	Work only (3)	Market work (4)	Domestic work (5)	Idle (6)
			A. All			
Tariff * SC	-1.027***	0.491***	0.517***	0.485***	0.006	0.510***
	[0.219]	[0.174]	[0.175]	[0.131]	[0.115]	[0.180]
Tariff * ST	-0.003	-0.302	-0.293	-0.009	-0.293**	0.296
	[0.240]	[0.217]	[0.215]	[0.174]	[0.143]	[0.199]
Tariff	0.456**	-0.093	-0.102	0.016	-0.109	-0.354*
	[0.187]	[0.134]	[0.134]	[0.126]	[0.099]	[0.184]
N	95,488	95,514	95,488	95,514	95,514	95,488
			B. Boys			
Tariff * SC	-1.173***	0.753***	0.778***	0.667***	0.086	0.395*
	[0.273]	[0.197]	[0.197]	[0.184]	[0.069]	[0.222]
Tariff * ST	-0.339	-0.09	-0.077	-0.146	0.057	0.416*
	[0.297]	[0.216]	[0.214]	[0.205]	[0.079]	[0.236]
Tariff	0.439**	-0.158	-0.129	-0.085	-0.073	-0.310*
	[0.201]	[0.150]	[0.147]	[0.143]	[0.057]	[0.167]
N	51,153	51,170	51,153	51,170	51,170	51,153
			C. Girls			
Tariff * SC	-1.007***	0.331	0.359	0.266*	0.065	0.649***
	[0.270]	[0.245]	[0.249]	[0.161]	[0.218]	[0.223]
Tariff * ST	0.395	-0.580*	-0.573*	0.183	-0.762***	0.178
	[0.316]	[0.319]	[0.320]	[0.242]	[0.288]	[0.246]
Tariff	0.479	-0.006	-0.056	0.136	-0.142	-0.424
	[0.298]	[0.199]	[0.198]	[0.157]	[0.192]	[0.272]
N	44,335	44,344	44,335	44,344	44,344	44,335

Notes: These estimates are based on the 43rd and the 55th rounds of NSS. Robust standard errors are in brackets and have been clustered at the district level. The main effects of SC and ST are included in all regressions but not reported. The IV regression specification is the same as in Table 3.5 of Edmonds, Pavcnik, and Topalova (2010b). Differences in sample sizes across columns are due to missing data. *** 1%, **5%, *10%.

Table 3.13. IV Estimates: Adult Employment by Caste

	Men	Women
	A. Days worked in the last year	
Tariff * SC	329.303***	-736.672***
	[57.392]	[87.187]
Tariff * ST	201.177**	-1257.041***
	[81.187]	[210.937]
Tariff	-334.291***	246.735
	[86.667]	[187.515]
N	99,781	112,568

Notes: These estimates are based on the 43rd and the 55th rounds of NSS. Robust standard errors are in brackets and have been clustered at the district level. The main effects of SC and ST are included in all regressions but not reported. Sample is restricted to adult men and women in the 25-50 age-group. Other controls in this regression are the same as in Table 3.4 of Edmonds, Pavcnik, and Topalova (2010b). *** 1%, **5%, *10%.

Table 3.14. IV Estimates: Migration from Other Districts by Caste and Gender

Dependent variable - Share in the district population of:	Coefficient of Tarrif
1. Female in-migrants	0.091
	[0.082]
2. Low-caste female in-migrants	0.043
	[0.030]
3. Female in-migrants who have moved within the last ten years	0.041
	[0.035]
4. Low-caste female in-migrants who have moved within the last ten years	0.017
	[0.015]
N	722

Notes: These estimates are based on the 43rd and the 55th rounds of NSS. Each coefficient is from a different regression. Robust standard errors are in brackets and have been clustered at the district level. Low-caste refers to SC and ST women. In-migrants refers to individuals whose place of enumeration is in a district different from their last place of usual residence. Regressions are weighted by the number of households in a district and control for district and year fixed effects and initial district conditions that are interacted with the post-reform indicator. *** 1%, **5%, *10%.

3.11 Appendix: Tables

Table 3.A1. IV Estimates: Urban India

	(1)	(2)	(3)	(4)	(5)
		Birth = 1			
		A1. First Stage			
Traded Tariff in YOB	0.317***	0.317***	0.325***	0.315***	0.323***
	[0.030]	[0.030]	[0.029]	[0.029]	[0.028]
F-stat	108.05	108.3	126.43	118.72	135.13
		B1. IV			
Tariff in YOB	-0.100**	-0.141***	-0.143***	-0.230*	-0.307***
	[0.049]	[0.047]	[0.045]	[0.127]	[0.115]
N	895,134	895,134	895,134	895,134	895,134
		Boy = 1			
		A2. First Stage			
Traded Tariff in YOC	0.292***	0.292***	0.302***	0.279***	0.288***
	[0.028]	[0.028]	[0.027]	[0.022]	[0.024]
F-stat	111.27	111.56	122.24	158.02	149.55
		B2. IV			
Tariff in YOC	-0.040	-0.033	0.050	-0.251	-0.238
	[0.110]	[0.110]	[0.109]	[0.155]	[0.151]
N	186,953	186,953	186,953	186,953	186,953
		Infant Mortality			
		C1. Mortality in 1 month			
Tariff in YOB	-0.012	-0.009	0.012	-0.013	0.017
	[0.034]	[0.034]	[0.033]	[0.052]	[0.051]
		C2. Mortality in 6 months			
Tariff in YOB	-0.030	-0.027	-0.009	-0.025	-0.001
	[0.035]	[0.035]	[0.035]	[0.055]	[0.055]
		C3. Mortality in 12 months			
Tariff in YOB	-0.033	-0.030	-0.002	-0.038	-0.001
	[0.038]	[0.039]	[0.038]	[0.059]	[0.057]
		C4. First Stage			
Traded Tariff in YOB	0.300***	0.300***	0.308***	0.283***	0.290***
	[0.029]	[0.029]	[0.028]	[0.023]	[0.023]
F-stat	105.37	105.63	121.76	149.57	152.44
N	198,400	198,400	198,400	198,400	198,400
District FE	x	x	x		
Year FE	x	x	x	x	x
Covariates		x	x	x	x
District-specific linear trends			x		x
Mother FE				x	x

Notes: YOB stands for the year of birth. YOC stands for the year of conception. Each cell constitutes a separate regression. Columns (2) - (5) include indicators for mother's age at birth and number of previous births. Columns (2) and (3) also include mother's years of schooling and household's religion and caste dummies. Robust standard errors are in brackets and have been clustered at the district level. All regressions use district-level sampling weights. *** 1%, **5%, *10%.

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