Working for the Future: Female Factory Work and Child Health in Mexico*

David Atkin†

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Abstract

In this paper, I show that the women induced to work in export manufacturing by the opening of a new factory nearby have significantly taller children. This increased child height does not come through higher household income alone, with these women reporting stronger bargaining power within their households. Since women who choose to work in factories may be quite different from those who do not, I require an instrument that increases the probability of working in a factory but does not affect child height directly. Therefore, I instrument for whether a woman's first job was in export manufacturing with factory openings in her town at the legal employment age of 16. I use a LATE estimator to find that women induced to enter manufacturing by the opening of a factory have children who are over one standard deviation taller. This group of women, whose lives are altered by the factory, are exactly the group that concerns a local policymaker deciding industrial policy.

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†Department of Economics, Princeton University, Princeton, NJ 08544. E-mail: datkin@princeton.edu

1 Introduction

The last half century saw an enormous expansion in both female labor force participation and low-skill manufacturing around the developing world (Mammen and Paxson 2000). Industrialization may have been expected to increase labor demand primarily for men, who through a combination of cultural and physical reasons were initially favored in industrial jobs in most developing countries (Goldin 1994). However, in the developing world one of the most publicized aspects of industrialization has been the extensive employment of young unmarried women in low-skilled manufacturing, from the sweat shops of South East Asia, to the Maquiladoras of Mexico.

Many commentators, both inside and outside academia, have linked the globalization of production to an increase in demand for low-wage female workers in manufacturing. Export-oriented manufacturing industries such as textiles and low-skilled assembly work often have a preference for female labor. The explanations for this preference for female workers include women having greater agility, a higher tolerance for repetitive tasks, lower wages and being more docile and less likely to unionize (Fernández-Kelly 1983, Standing 1999, Fontana 2003).

Whilst there is much speculation on the effects of this kind of work on the effect of factory employment on female workers, there is very little rigorous evaluation. Human rights groups and anti-sweatshop activists often focus on the negative effects from arduous and unsafe working conditions, verbal and physical abuse at the workplace, social ostracization outside it and the regular violation of local labor laws. On the positive side the focus is on increased options of employment outside the household and the accompanying increases in a woman's ability to make her own choices, especially regarding marriage and fertility. Much of the academic work focuses on Bangladesh, where textile exports make up about 80% of total merchandise exports and these industries primarily employ young rural women who have very few other extra-household employment opportunities. Kabeer (2002) and Hewett and Amin (2000) provide evidence that working in textiles factories is associated with higher female status and better quality of life measures.

There is a far larger but unconnected literature detailing how female income and employment options affect household decision making by changing the bargaining power within a marriage. Strauss and Thomas (1995) and Behrman (1997) provide extensive surveys of this literature. Many papers find that women have stronger relative preference for expenditures on child goods than men do, and accordingly, increasing female income increases child education and health outcomes (Thomas 1997). Additionally, women have a relatively stronger preference than men for expenditures on female children (Duflo 2003).

Putting these two strands of literature together suggests that female manufacturing work may affect child outcomes as female incomes and work options are increased. Female manufacturing jobs may also lead to a more general increase in female empowerment if these jobs provide one of the few opportunities for women to work outside of the household in a formal employment environment. Surprisingly, no work has assessed these effects directly. Given the importance of early life health and education investments on later life outcomes for a child, the impact of female factory work on these investments is a vital component in assessing the welfare impact of the female manufacturing work that has become so prevalent in many developing countries, and is one of the most publicized elements of globalization discussions.

In this paper, I will show, in the context of Mexico, that female manufacturing employment opportunities lead to positive child health outcomes amongst the children of women who end up working in the manufacturing sector. Mexico makes a good case study as it saw a huge surge in female manufacturing in the last 20 years, closely associated with rises in export-oriented production that followed trade liberalization. Female labor force participation rose from 28% of the total labor force in 1985 to 40% in 2000 (Moreno-Fontes Chammartin 2004), with a particularly pronounced increase in the manufacturing sector. In 1985 there were 341,105 women in registered private manufacturing and this rose to 1,493,736 women by 2000, with those employed in export sectors rising from 200,635 to 966,722 women.[1] Of these jobs, Maquiladora employment alone accounted for 165,000 women in 1985 rising to a high of 685,000 female employees in 2000.

Accordingly, much of this growth came through export oriented manufacturing, since all Maquiladora jobs and many of the other new female manufacturing jobs were in factories producing merchandise for export. The time period 1986-2000 saw an explosion in export manufacturing in Mexico. Mexico had pursued an import substitution strategy until its entry into GATT in 1986. From this point onwards, there was a gradual dismantling of tariffs culminating with the signing of NAFTA in 1994 and its subsequent implementation. The Maquiladora plants were the most visible sign of this export expansion. Maquiladoras are a specific enterprise form that allows duty free importation of inputs, and while initially confined to the US-Mexican border, they were allowed to set up anywhere in Mexico by the 1980's.[2] These firms were primarily manufacturing electronics, transportation equipment and textiles to serve US markets. Maquiladoras are often foreign owned, employ more

[1] Authors calculation based on data from Mexican Social Security Institute (IMSS) which contains the complete universe of registered private firms. Export sectors are two digit industries where over half the output was exported in at least half the years between 1985 and 2000. The export and output data come from the Trade, Production and Protection 1976-2004 database (Nicita and Olarreaga 2007).

[2] While less than 10% of Maquiladora employment was in the interior (non border states) in 1990, this rose to 23% in 2000 as Maquiladoras begun to spread to poorer areas of the country in search of lower labor costs and in response to regional incentives.

women than men and have generally been characterized in negative terms as exploitative as in Fernández-Kelly (1983).

This paper will cover female manufacturing in general, as the data do not differentiate between Maquiladoras and other factories. However, these figures above show that much of the growth in Mexico's female manufacturing employment came in export-oriented manufacturing. Therefore, the time period 1985-2000 in Mexico provides an excellent opportunity to look at the effects of trade liberalization and globalization on the women who ended up working in manufacturing industries and the later results can be interpreted in this light.

Section 2 provides the theoretical background that will guide the empirics, section 3 looks at the econometric pitfalls and possible solutions and section 4 introduces the data. Section 5 empirically evaluates the hypothesis that women's manufacturing may affect child outcomes and tries to tease out some causal channels as well as performing some robustness checks. Section 6 concludes.

2 Theory

In order to better understand the pathways through which female manufacturing employment may affect child outcomes, I present a simple model. This model will also help clarify the various econometric assumptions necessary for the empirical evaluation.

The effect of manufacturing work for women on child investment decisions can be thought of as a three-stage game. In the first stage, a young woman reaches adulthood and chooses a career path. This career path depends on her skills and characteristics and the local job environment. In the second stage, she chooses a husband, gets married and has children. Her marriage choice will depend on her skills and characteristics again, and perhaps also her choice of job in the first stage. In the third stage, the couple bargain over how their combined income is spent. Fertility preferences are identical for all couples, although relaxing this will not alter the basic implications and will be discussed later.

An individual i has a lifetime utility of:

$$U_i = u_i(C_i, C_b, C_g).$$

Where C_i is personal consumption, C_b is the lifetime consumption of male children and C_g is the lifetime consumption of female children.

Income depends on your career choice, J_i, and some predetermined personal characteristics X_i:

$$Y_i = Y(J_i, X_i).$$

The sector of employment, J_i, is chosen from the set of attainable local jobs, Ω_i, at the time the woman reaches adulthood, and is irreversible in this simple model.[3] The elements of the set Ω_i may depend on X_i, and include remaining unemployed.

At stage 1, an individual chooses J_i to maximize U_i :

$$J_i = \arg\max_{J_i \in \Omega_i} u_i(C_i, C_b, C_g).$$

At stage 2, a marriage match is agreed upon. The marriage decision will be modelled by a matching function where the husband's lifetime income, characteristics and bargaining weight, γ, depend on the career choice, personal characteristics and the bargaining weight of the woman. Indexing husband variables by j and wife variables by i:

$$(Y_j, X_j, \gamma_j) = M(J_i, X_i, \gamma_i).$$

At stage 3, the amount of income devoted to child investment is chosen through a decision making process between husband and wife that results in Pareto efficient outcomes, termed the "collective" model by Chiappori (1992). The outcome will depend on relative incomes, outside options (V_i) and bargaining strength. I will follow Manser and Brown (1980), McElroy and Horney (1981) and much of the subsequent literature in modelling this process as a cooperative Nash Bargain:

$$\max_{C_i, C_j, C_b, C_g} (u_i(C_i, C_b, C_g) - V_i)^{\gamma_i} (u_j(C_j, C_b, C_g) - V_j)^{\gamma_j},$$
$$\text{s.t. } Y_i + Y_j = C_i + C_j + I_b + I_g.$$

Child lifetime consumption is a function of parental characteristics, investments made in male and female children, I_b, and, I_g, and the rate of return to that investment ρ. The parameter ρ may itself depend on individual characteristics and even job choice, as certain

[3] The woman may also change sector or leave the labor force later in life, but I will assume this is part of the chosen "career path" defined here. For now migration is ruled out, so only local job availability is relevant. I have not explicitly modelled the choice of education. However, J_i can be thought of as a choice of both a career and the training or education necessary to enter that career. In this context, income should be thought of as total income minus any investment costs.

jobs entail long hours and may make other child investments less effective:

$$
\begin{aligned}
C_g &= g(\rho_{gij}, I_g, X_i, X_j), \\
C_b &= b(\rho_{bij}, I_b, X_i, X_j), \\
\rho_{gij} &= p(X_i, J_i, X_j, J_j,), \\
\rho_{bij} &= q(X_i, J_i, X_j, J_j,).
\end{aligned}
$$

I will assume that adult consumption is enjoyed equally by the husband and the wife:

$$C_i = C_j.$$

The bargaining weights may depend on the career choice and personal characteristics as well as income:

$$\gamma_i = h(X_i, J_i, Y_i).$$

The outside options may also depend on personal income, sectoral choice and personal characteristics. Jobs which can be done outside of the household may particularly increase a female's options if she were to leave her husband:

$$V_i = v(X_i, J_i, Y_i).$$

Men, j, and women, i, may have different preferences. A common assumption is that women display greater inter-generational altruism then men, and within this, a relatively greater altruism for girls than for boys compared to men. Behrman (1997) surveys the evidence for this assumption in detail. In particular, I add the following structure to the utility function:

$$
\begin{aligned}
U_i &= u(C_i^{1-\alpha_i-\beta_i}, C_b^{\alpha_i}, C_g^{\beta_i}), \\
U_j &= u(C_j^{1-\alpha_j-\beta_j}, C_b^{\alpha_j}, C_g^{\beta_j}),
\end{aligned}
$$

$$\text{where } u_1 > 0, u_2 > 0, u_3 > 0,$$

$$
\begin{aligned}
\text{and } 1-\alpha_i-\beta_i &< 1-\alpha_j-\beta_j, \\
\frac{\alpha_i}{\beta_i} &< \frac{\alpha_j}{\beta_j}.
\end{aligned}
$$

The form of the utility function for men and women is identical, only differing in the relative weights assigned to the three types of consumption. Therefore, the more power a woman has in the decision making process, her preferences ensure that there will tend to be more child investment and the effect will be stronger for girls.

This model can be solved recursively, with individuals making sectoral choices anticipating the effect they will have on spouse choice and the subsequent bargain. Each woman makes a choice from a large pool of spouses, and so she can take the matching function, $(Y_j, X_j, \gamma_j) = M(J_i, X_i, \gamma_i)$, as given.

I am abstracting from the choice of the number of children. However, since C_g and C_b enter into the utility function directly, I can alternatively think of these as a child consumption aggregates, taking into account the quality-quantity trade off in having multiple children. Results will be unchanged if I explicitly modelled fertility choices, and the child consumption aggregates, C_g and C_b, that parents care about are increasing in the same arguments as for child consumption in the model above, in which the number of children is fixed. I will later provide supporting evidence that manufacturing employment does not seem to alter fertility.

In reality all the consumption outcomes above will have large amount of uncertainty associated with them at the time career and child investment decisions are made. In this paper, I will abstract from this uncertainty. Alternatively, I can think of all these decisions being made in expectation with the necessary independence assumptions when the functions are non linear.

The Nash Bargain results in the following reduced form for child lifetime consumption:

$$\begin{aligned} C_g &= g(\rho_{gij}, X_i, \gamma_i, Y_i, V_i, X_j, \gamma_j, Y_j), \\ C_b &= b(\rho_{bij}, X_i, \gamma_i, Y_i, V_i, X_j, \gamma_j, Y_j). \end{aligned}$$

All of these variables may be affected by the woman's choice of career, J_i. While functional forms could be chosen, and the model solved explicitly, this would add very little to the analysis. Instead I will draw the main implications out of the model that are relevant for guiding the empirical analysis, and answer the following question: How are child investment outcomes affected by women entering the manufacturing sector?

Initially, I will assume that the set of available careers, Ω_i, is sufficiently large such that there are jobs available, other than manufacturing jobs, that provide a very similar income to the manufacturing sector for the same set of skills and characteristics. Therefore, income affects from choosing the manufacturing sector can be ignored. Income effects will be dealt with subsequently when this assumption is relaxed.

In this case, female manufacturing work may be expected to increase child investment and particularly investment in girls through the following channels:

- By increasing a woman's fall back option, V_i, if manufacturing has a particular effect on the outside option, independent of income. Manufacturing work is highly impersonal and almost always done outside of the house in a formal environment. This may differ from other common alternatives, where a woman's income earning ability may be affected by leaving her husband (housework, domestic work for others, small retail). Therefore, manufacturing work may give a woman a higher payoff if she separates from her husband. Factory experience may also give women a greater freedom to migrate, in the expectation that a factory job can be easily found elsewhere.

- By directly changing a woman's bargaining weight, γ_i. Women working in a factory with male supervisors and a non-household environment may develop greater bargaining weight through experiences negotiating with men and talking with other women in the factory about their experiences. It is also possible that this greater bargaining power is accompanied by a willingness to disobey a husband and make child investments without his knowledge (e.g. take the child to get a medical checkup without discussing the decision with her husband), although this does not fit cleanly into a co-operative bargaining model. Note that changes in bargaining weight and the fallback option are clearly differentiable in the context of a Nash bargain, however empirically they will be impossible to disentangle from each other.

  Still abstracting from income channels, the female manufacturing work will have an ambiguous impact on child investment:

- By changing the choice of husband (and so his characteristics, income and bargaining power). It is quite possible that men who are matched with factory women have lower incomes, greater bargaining power or personal characteristics less conducive to child investment and so the effect could go either way.

- Education choices made when deciding on career choice may affect child investment directly. A mother may be better able to treat illnesses, as well as process and obtain health information if she is more educated. This channel works through the return on child investment, ρ_{gij} and ρ_{bij}, changing with J_i. The direction of the effect will depend on whether the availability of a manufacturing job encourages more or less education than the woman would otherwise have obtained.

- The mother may be at home less if she works in a job with long hours away from home. While bringing up the children and this may affect child investment efficacy, again working through ρ_{gij} and ρ_{bij}.

All of these effects do not require a woman to spend her whole career working in a factory as they are not occurring through income channels but are one off changes that could occur after a short period working in a factory at a formative age. The only exception is the last effect, which requires the woman to be working after having given birth, which will be related to the duration of her employment.

However, in reality the set Ω_i has a limited number of elements and not every woman has the option of working in every sector. There may not be any firms in that sector in their locality, the firms may not be hiring additional individuals with a given set of characteristics. In this scenario, a change in the set of available careers, Ω_i, may lead to different choices of career, J_i, and different incomes. For example, if a factory opens in a locality where there was none, formal manufacturing jobs are available to women for the first time. The set Ω_i grows larger and manufacturing is more likely to be chosen as a career by these women, and some women will earn significantly higher income from this job than from any other attainable alternative, given their characteristics. Female manufacturing work can now increase child investment, and particularly investment in girls, through three additional channels:

- By changing women's expectations about the future earning opportunities for women (the set of future careers for female children, Ω_g, a factor affecting the returns to investment in female children ρ_{gij}) or the perceived returns for her daughter, which increase ρ_{gij} directly. Both of these factors make female investment more worthwhile.

- By increasing income for the women who now have better options in the set Ω_i and choose manufacturing work. If child investment is a normal good, the amount invested will increase.

- By increasing income for the women who now have better options in Ω_i and choose manufacturing work. A woman's fallback option increases directly through higher income and so her bargaining power increases. What matters here is relative incomes within the household. For this channel to increase child investment, it is necessary that the different husband choice made by women in manufacturing does not fully negate the positive female income effect on relative bargaining power.

These last three channels are not specific to manufacturing, and apply for any sector where participation is positively correlated with the arrival of higher income opportunities in that sector. So for example, while domestic work is unlikely to fall into this category, the arrival of a large new government office requiring several hundred well paid unskilled women would. However, as will be discussed later, data issues preclude me from evaluating the other sectors in a believable manner. The channels particular to manufacturing involve direct changes in household bargaining if manufacturing jobs are substantially different in their nature to all other potential jobs a woman has available. Much sociological work suggests this is the case, as these industries are often the first to successfully break down gender labor norms (Wolf 1992).

The model also makes clear the potential pitfall in carrying out empirical work:

- Selection bias: If personal characteristics, X_i, are correlated with both sectoral choice and with parameters affecting child investment decisions, α_i, β_i, γ_i, Y_i and V_i, there will be omitted variable bias in the estimation of sectoral choice on child investment if some of the components of X_i are unobserved.

However, by finding an instrument for the career choice, J_i, the selection bias avoided. A valid instrument needs to be correlated with J_i yet uncorrelated with X_i. The model suggests a possible instrument. If the set Ω_i expands for reasons unrelated to personal characteristics, this will increase the chance of a woman choosing to work in that sector. So most obviously if a new factory opens employing several hundred women in a small town when a girl reaches adulthood, this will surely increase the probability of that woman choosing a career in manufacturing, as it increases the chance that Ω_i contains attainable manufacturing jobs. For this to be a valid instrumention strategy, the expansion of Ω_i must not be correlated with changes in individual characteristics in the locality. In effect I require an exclusion restriction; that new factory openings directly affect the chance of working in a factory, but affect the outcome of interest, child investment, only indirectly through the expansion of the set of available careers, Ω_i.

It is more difficult to identify similar effects from working in others sectors. While factory work by its nature is very lumpy (there is a large minimum efficient scale in many modern manufacturing techniques), the same is not true for most other working opportunities for women. This makes it difficult to find instruments that have a significant effect on the probability a woman enters that sector. Because the labor demand shocks are far less lumpy in other sectors, cohort variation in the sectoral choice of young women is not significantly related to annual changes in the employment environment, and so the instrumentation strategy is too weak.

A more subtle concern is that since these women are optimally choosing whether to work in manufacturing, the women who would not have worked in manufacturing had a factory not opened, but decide to work in a factory if one opens, may be unusual. In particular, women who anticipate they have unusually high personal benefits to working in a factory, such as large improvements in child outcomes, are the women most likely to be induced to switch sector by a new factory opening. However, in this situation, the heterogeneity of responses will not prevent me from addressing a key policy question: What is the impact of a new factory opening on the women affected by a factory opening in their local community. The next section will discuss the econometric assumptions that must be satisfied for the instrument to be valid, and for the coefficients obtained to be meaningful.

3 Econometric Interpretation and Instrumentation

Ultimately, my question of interest is how does female factory work for a young woman, i, change the human capital of her children, denoted by y_i, later in the woman's life. In the empirical work, height-for-age Z scores will be my main measure of y_i, child health investment in this case. A reduced form of the above model will be estimated with child outcomes regressed on a mother's sectoral decisions and some observable controls X (suppressing all individual, i, subscripts for now):

$$y = \delta Manuf + X\theta + \varepsilon. \tag{1}$$

Where $Manuf$ is a dummy variable taking the value 1 if a women started her career in manufacturing and the value 0 if she never worked or started her career in another sector. How this coefficient is interpreted requires some further exposition. Following Heckman and Vytlacil (2007b) closely, the problem can be set up as follows:

$$y = y_1 \cdot Manuf + y_0 \cdot (1 - Manuf).$$

I observe the child human capital outcome y, which is equal to y_1 if a woman works in manufacturing and y_0 if she does not. The problem is that for individual i, I can only observe one of y_1 and y_0:

$$y_1 = \mu_1 X + U_1,$$
$$y_0 = \mu_0 X + U_0.$$

Therefore

$$y = (\mu_1 X + U_1) \cdot Manuf + (\mu_0 X + U_0) \cdot (1 - Manuf),$$
$$y = (\mu_1 X - \mu_0 X + U_1 - U_0) \cdot Manuf + (\mu_0 X + U_0).$$

Here y can potentially depend on observable characteristics, X, as well as unobservables U_1 and U_0. U_1 and U_0 are likely to not correlated as individual unobserved characteristics may increase both y_1 and y_0. The above equation can be rewritten as:

$$y = (\overline{\delta} + \eta)Manuf + \mu_0 X + \varepsilon,$$

where $\overline{\delta} = \mu_1 X - \mu_0 X$, $\eta = U_1 - U_0$ and $\varepsilon = U_0$. The treatment effect, δ, is $y_1 - y_0$, which is the causal effect of a woman working in the manufacturing sector. There is heterogeneity in δ if $y_1 - y_0$ differs between individuals after controlling for observables. Following the theoretical section above, I can write the decision to move into the manufacturing sector as a simple version of the Roy (1951) model.

There is a latent variable, $Manuf^*$, which is the difference in a woman's utility between working in the best attainable manufacturing job and working in the sector that brings highest utility attainable from all the other sector choices. If there is no such attainable job in manufacturing, utility is at a lower bound from working in manufacturing. $Manuf^*$ is found by obtaining $J^* = \arg\max_{J \in \Omega^*} u(C^{1-\alpha-\beta}, C_b^{\alpha}, C_g^{\beta})$ after recursively solving the three stage game above, where Ω^* is the set of attainable sectors excluding manufacturing. I will assume that $u_{J=Manuf}(C^{1-\alpha-\beta}, C_b^{\alpha}, C_g^{\beta}) - \max_{J \in \Omega^*} u(C^{1-\alpha-\beta}, C_b^{\alpha}, C_g^{\beta})$ can be written as as $\mu_Q Q - V$, where Q is observable and V is not:

$$\begin{aligned} Manuf^* &= \mu_Q Q - V, \\ Manuf &= 1 \text{ if } Manuf^* \geq 0; Manuf = 0 \text{ otherwise.} \end{aligned}$$

I observe Q (which contains X as well as possible instruments Z) and X but not U_0, U_1 and V. These error terms need not be independent. From the model above, mothers should anticipate the positive effect on child outcomes and this will factor into their decision to choose to work in the manufacturing sector. Assuming linear separability as in a standard Roy model, I can rewrite this as a decision based on child investment outcomes (which enter utility through child consumption) and other observable factors, $\mu_M X$, as well as other unobservable factors, $-V_M$. These will include the income and husband choice comparisons between sectors implicit in the model, as well as other variables:

$$\begin{aligned} Manuf^* &= (y_1 - y_0) + \mu_M X + \mu_Z Z - V_M, \\ Manuf^* &= \overline{\delta} + \mu_M X + \mu_Z Z + \eta - V_M. \end{aligned}$$

This leads to selection into manufacturing based on unobserved individual characteristics, and running equation 1 will result in a biased estimator of δ since $Cov(Manuf, \varepsilon) = E[\varepsilon \mid$

$Manuf = 1] \Pr(Manuf = 1) \neq 0$ if ε and $V = \eta - V_M$ are correlated. This would be the case if, for example, we do not observe a woman's intrinsic drive, and this positively affects the human capital of her child, y_1 and y_0, as well as increases the relative benefits to working in manufacturing (U_0, U_1 and V all depend on intrinsic drive). Then $Cov(Manuf, \varepsilon) > 0$ and OLS will be biased upwards. Alternatively, we could imagine that only desperate women, who have suffered some unobserved negative shock (e.g. the death of a parent), are induced to work in manufacturing by a new factory opening. This could result in worse child health outcomes (e.g. if investment is less effective without parental help) and $Cov(Manuf, \varepsilon) < 0$. A priori, the direction of the bias is uncertain. Note, that even if V_M is uncorrelated with $\varepsilon = U_0$ there is still a problem as women are selecting into manufacturing partly based on the anticipated $(y_1 - y_0)$, which is a function of U_0, and so the decision to enter manufacturing will necessarily be correlated with ε.

In the case where δ does not vary between individuals, instrumental variables provide a solution to this bias caused by selection into treatment. As long as there is some variable that is in the set Z but not in the set X, I can instrument for $Manuf$ with that element of Z, providing $Cov(Z, Manuf) \neq 0$ and $Cov(Z, \varepsilon) = 0$. The instrumental variable regression will provide consistent estimates of δ conditioning on X. This strategy require a suitable instrument that has no direct effect on a child's human capital acquisition, y, but does affect the selection of mothers into manufacturing.

A measure of the attainable manufacturing jobs that is uncorrelated with (unobservable) individual characteristics, such as the opening of a new factory when a girl reaches adulthood, will likely satisfy the requirements for a suitable instrument. A new factory opening will increase the latent variable $Manuf^*$ by making it more likely that $u_{J=Manuf}(C^{1-\alpha-\beta}, C_b^\alpha, C_g^\beta)$ is non zero (e.g. that there is a manufacturing job that is attainable). The opening of a new factory may also increase the utility from working in manufacturing, $u_{J=Manuf}(C^{1-\alpha-\beta}, C_b^\alpha, C_g^\beta)$, if the new factory increases the chance of getting a job in manufacturing when there is job rationing and so uncertainty over whether a woman can get a job in the factory if she want it. Finally, a new factory will increase $u_{J=Manuf}(C^{1-\alpha-\beta}, C_b^\alpha, C_g^\beta)$ if the new factory offers manufacturing jobs that are preferred to existing manufacturing jobs (pay, conditions etc.) thereby raising $Manuf^*$.

It is plausible that a new factory opening in the year a woman enters the labor force, say at age 16, will not affect child outcomes directly, as these children are likely to be born many years later. While there may be effects on all children from a new factory having opened in the past (from pollution or accompanying infrastructure and services), these effects would not depend specifically on how old the mother was when the factory opened. I will separately try and identify the effects of factory openings on the entire age cohort in section 5.2 to show that these direct effects of factory openings are not driving the results.

Similarly, the decision of an entrepreneur to open a new factory in a locality will be related to the local skill distribution, the size of the local market and the local infrastructure. For endogenous factory placement to jeopardize the validity of the instrument, whatever is driving the timing of factory openings within a single municipality (changing population density for example), must also affect child health directly (by encouraging improved public health facilities). However, these effects would be common across all children of the same age group who will have equal exposure to any new facilities, rather than common across all children whose mothers are the same age.

Further difficulties arises in the case where δ varies between individuals. In this context, different women are affected differently by manufacturing work in ways that affect child outcomes. I can define $\overline{\delta} = \mu_1 X - \mu_0 X$ as the mean treatment effect and $\eta = U_1 - U_0$ as the unobserved heterogenous component.

$$y = \overline{\delta} Manuf + X\theta + (\varepsilon + \eta Manuf)$$

Now the problem is that I require $Cov(Z, \varepsilon + \eta Manuf) = Cov(Z, \eta Manuf) = 0$ for the instrument to be valid and the mean treatment effect, $\overline{\delta}$, to be identified:

$$Cov(Z, \eta Manuf) = E((Z - \overline{Z})\eta \mid Manuf = 1) \Pr(Manuf = 1) = 0$$
$$E((Z - \overline{Z})\eta \mid \overline{\delta} + \mu_M X + \mu_Z Z + \eta > V_M) \Pr(\overline{\delta} + \mu_M X + \mu_Z Z + \eta > V_M) = 0$$

In this situation, even if $Cov(Z, \eta) = 0$ (the instrument is uncorrelated with the heterogeneity in δ), $Cov(Z, \eta Manuf)$ will not be zero once we condition upon working in manufacturing as individuals with high unobserved heterogenous component, η, will be more likely to chose to enter manufacturing. This is the case of essential heterogeneity introduced in Heckman, Urzua, and Vytlacil (2006). Intuitively, an instrument that leads women to choose manufacturing but doesn't affect outcome y will not lead to unbiased estimates of $\overline{\delta}$ under heterogenous δ's. This is because women select into manufacturing partly based on the anticipated change in child investment outcomes, y. The women who select into manufacturing have a higher idiosyncratic gain in child investment outcomes from working in manufacturing, and the estimate of $\overline{\delta}$ will be biased upwards.

This mean treatment effect, $\overline{\delta}$, is the effect of working in a factory on an average women in society. Generally this is the parameter that economists aim to find. However, this is not necessarily the parameter that policymakers may find most relevant, and in this specific case the estimated parameter may actually be more useful to a policymaker.

The correct way to interpret the estimated value of δ is a Local Average Treatment Effect

(LATE) or more generally a Marginal Treatment Effect. Under a monotonicity assumption (that an increase in the instrument leads to a change in the probability of entering manufacturing that works in the same direction for all individuals) and the assumption that $Cov(Z, \delta) = 0$, the Local Average Treatment Effects approach (Imbens and Angrist 1994) is applicable. This interprets the coefficient from the IV regression as the average δ of the group who changed from the default (not working in the manufacturing sector) to working in manufacturing because of the instrument. This is $E(y_1 - y_0 \mid X = x, Manuf(z) = 1, Manuf(z') = 0)$, where z and z' are the values of the instrument in the simplest binary instrument case, $Manuf(z)$ is a random variable determining whether or not a woman enters manufacturing if the instrument takes the value z. The monotonicity assumption is then: $Manuf(z) \geq Manuf(z')$ or $Manuf(z) \leq Manuf(z')$ for all individuals. I derive this expression more fully in appendix 1. It is likely that the opening of a new factory is not going to make any women less likely to go into manufacturing, only more, so monotonicity is likely satisfied.[4] Whether the LATE estimate is of policy interest (or any interest at all) depends on the particular instrument.[5]

In the case of how female manufacturing work affects child investment outcomes, the LATE estimate corresponds to an important policy question. By using new factory openings as an instrument, I am finding the effect on child investment of female manufacturing work for those women who end up working in a factory, and would not have been working in manufacturing had the factory not arrived at the time that they reached employable age. When a local politician or a central planner making industrial policy is deciding what types of support to give to entrepreneurs hoping to build new factories or expand existing production, the policymaker wants to know the effect this will have on the local population affected. The effects on those who will end up working there is of primary interest, and especially those women who would not have worked in a factory otherwise. These are the new factory employment opportunities generated, often the primary motivation for attracting new factories to a locality.

Up until now the whole analysis has implicitly assumed that all the women live in the same locality. In reality, women live in many places and a panel structure will be used. Any locality level fixed effects will be swept out and the identification comes from variation within localities. This panel approach requires that the instrument must be strictly exogenous,

[4] It is possible that the heterogenous effects are related to the opening of the new factory itself. However, why the intrinsic variation in the effect of manufacturing on child outcomes across individuals would depend on a new factory opening is unclear.

[5] In the cases where these marginal returns generated by the instrument are at an uninteresting (and unknown) margin, the more general Marginal Treatment Effects approach in Heckman and Vytlacil (2007a) can be applied.

in the sense that it must be uncorrelated with all the error terms, past and future, in the locality. Subscripting individuals with i and locality with l, the requirement is that $Cov(Z_{il} - \overline{Z}_l, \varepsilon_{il} - \overline{\varepsilon}_{il}) = 0$, or that deviations in the instrument from the locality mean must be uncorrelated with deviations in the error from the locality mean error. This is a strong assumption and would be violated if a particularly good (in an unobserved way that affects child outcomes) cohort of women led an entrepreneur to open a factory in that locality (before or after those women enter the labor force). The more cohorts of data available, the smaller this bias will be if the decision to open a new factory is not contemporaneous with the large error deviation. Entrepreneurs responding to unobserved features of a specific female cohort does not seem a particular danger as cohort deviations in the unobserved attributes that will increase child investment decisions must be observed by the entrepreneur, and matter sufficiently to affect the entrepreneurs factory location decision. Mothers' education is the only strongly plausible candidate whose deviations might affect both child investment and manufacturing locational decisions and I will be able to control for this in the regressions as it is observed.

4 Data

The data come from two sources. All the individual level data come from the 2002 baseline round of the Mexican Family Life Survey (MxFLS). This survey is a very extensive panel study of 8,800 households collected by the Centro de Investigacion y Docencia Economicas and the Universidad Iberoamericana, and representative at the national level. There were 38,000 individual interviews conducted covering income, employment, consumption, health, access to services, anthropometrics, education and many other categories. For further details see Rubalcava and Teruel (2007) The data are particular suited to this study as they ask fairly detailed questions on employment history including the sector and timing of an individuals first job as well as their current job. This allows me to look at the effects of factory employment for women on outcomes that are only observable later in their life. Detailed anthropometric data was collected providing good measures of child health. Finally, all of these individuals can be matched to their current municipality, and migration histories are recorded.

The second data source is the Mexican Social Security Institute (IMSS), which has employment information on the complete universe of formal private sector establishments. This data come from the social security system into which all formal employees are required to enroll. The data are held securely at ITAM where the aggregations from firm to municipality level were carried out. I use the total employment data broken down by sex for a variety of industries at the municipality level. The data cover 1985 to 2000 and is the total number of employees on December 31st of that year. Kaplan, Gonzalez, and Robertson (2007) contains further details.

The two data sets are combined leaving 95 municipalities where MxFLS data was collected and IMSS data on employment opportunities are available. There are 136 municipalities in the MxFLS. However, some of these are inside metropolitan zones as defined by INEGI, and these municipalities have been merged. This leaves 112 municipalities. Of these municipalities, 13 are not in the IMSS data set as they had no formal employment or if they did it may have been registered in the neighboring municipality. That leaves 97 municipalities, although we will exclude Mexico City as it covers 7815 km^2 and so it is harder to believe that all the population can feasibly work in a new factory anywhere in the metropolitan zone. Monterrey and Guadalajara are also large cities and specifications will be run without them for robustness.

When I restrict the sample to women who filled in the adult survey (women over the age of 15 in 2002), just over 9500 women are in my sample. This sample is further reduced to about 2300 women who entered adulthood during the period 1985-2000 covered by the employment data, and who lived in their current municipality at age 12 and so would be sure to have been affected by municipality labor demand shocks when they were young.

4.1 Exploring the Data

Before tackling the impact of female factory work on child investment, I will explore the nature of female manufacturing in Mexico using the MxFLS data.

The MxFLS survey provides extensive employment data. Most usefully, the survey asks the sector of the respondents first job, and how old they were. Table 1 shows how various outcomes vary by sector of first employment. I restrict the sample to women who turned 16 between 1986 and 2000 and so are aged between 18 and 31 in 2002 as this is the range of years for which I have data on the employment environment. Finally I restrict the sample to women who were living in their current municipality at age 12, as ultimately I will be matching employment data to women at the municipality level, and the MxFLS data does not report the name of their municipality at age 12 if it was not the same as their current municipality. All these characteristics are weighted by survey weights representative at the national level, except where I report frequencies. I have 2,298 women in the sample.

Women who first worked in Agriculture, Manufacturing and Personal Services are less educated, start work younger, consume less and have lower household incomes and more children compared to other women who have worked. These women who first worked in

Agriculture, Manufacturing and Personal Services look fairly similar to women who have never worked. Some of these effects could be coming from different sectoral growth trends, but in the regressions include time controls. The large variation between professions strongly suggests that indeed personal characteristics, X_i, are correlated with initial sectoral choices.

Table 2 shows where women whose first job was in manufacturing currently work, and for women whose current job is in manufacturing, where their first job was. Not working, retail, food and transport and to a lesser degree personal services seem to be the main alternatives to manufacturing work for these women. Having a first job in manufacturing for most women seems to mean they are either still working in manufacturing or have left the labor force (78% of them).

So far very little has been said about migration. Table 1 shows that only about 70% of the women are living in the same municipality as they were living in at age 12. As ultimately I will be matching employment data to women at the municipality level, and the MxFLS data does not report the name of their municipality at age 12, it is important to see how different these non-migrants are. Table 3 shows how the characteristics differ between the two groups. Migrants seem to be slightly wealthier and more educated. The policy maker (especially if they are a local politician) may be more concerned about the effect of manufacturing on the children of current residents than the children of the women who migrate to the town if a factory opens. Therefore, omitting migrants may provide the correct parameter of interest for a local industrial policymaker. However, as I can only pick up the effects on the population that decided not to migrate, this is not the complete picture.

Finally, from current employment data it is possible to get an idea of the comparative earnings in these sectors. Table 4 shows the earnings in pesos both for the last month and the last year broken down by sector of employment. Manufacturing income is relatively high, larger than incomes of women in the Retail, Food and Transport sector whose employees are better educated. The hourly wage is relatively lower as manufacturing has long hours. However, at least the median hourly manufacturing wage (which will be a more reliable indicator in the presence of outliers) is still higher than agriculture, retail and personal services. Certainly, it seems believable that these manufacturing jobs are relatively good jobs for low-skilled women.

Table 1: Characteristics by Sector of First Job

Sector of First Job	Never Worked	Agri-culture	Construc. & Mining	Manufact-uring	Retail & Food	Profes. Serv.	Social & Govt.	Personal Serv.
Age	23.69	24.89	28.61	24.23	23.82	25.16	24.37	24.76
Age at first job		16.48	14.41	17.50	18.36	18.64	19.20	15.93
Age left school	14.79	13.82	13.80	14.77	16.85	18.12	19.22	13.77
Age at first marriage	18.38	18.70	19.00	18.93	19.03	20.35	22.29	18.04
Years worked	0.00	5.02	14.20	4.71	3.04	4.78	4.12	5.40
Consumption PC	12,491	9,553	22,785	12,067	15,911	16,906	24,649	10,253
Household Income PC	8,371	5,422	19,220	8,747	10,513	12,928	19,335	7,639
Same Mun. at age 12	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Currently works	0.00	0.61	1.00	0.62	0.66	0.68	0.74	0.52
Children	1.29	1.34	1.20	1.09	0.85	0.88	0.44	1.50
Attends school	0.22	0.03	0.00	0.07	0.15	0.16	0.34	0.10
Years of school (if fin.)	7.36	6.29	7.80	7.52	9.38	10.31	11.24	6.57
Ever married	0.60	0.58	0.60	0.57	0.49	0.45	0.30	0.62
Husband at home	0.89	0.62	1.00	0.75	0.80	0.82	0.83	0.81
Household size	5.24	5.73	3.20	5.77	4.94	4.88	4.78	5.73
Observations	860	116	2	263	430	232	120	275

Table 2: Transitions in and out of Manufacturing

Sector	Current Sector if First Job in Manufacturing	First Sector if Current Job in Manufacturing
No Work	90	
Agriculture	3	7
Construct., Mining, Elect.	1	1
Manufacturing	117	117
Retail, Food, Transport	31	41
Profesional Services	2	16
Social & Govt. Services	7	4
Personal Services	12	27
Total	263	213

Table 3: Differences Between Migrants and Non-Migrants

Sector of First Job	Migrated	Never Migrated
Age	24.88	24.12
Age at first job	18.07	17.77
Age left school	16.08	15.61
Age at first marriage	19.31	18.82
Years worked	5.21	4.25
Consumption PC	16,538	13,913
Household Income PC	12,678	9,821
Same Mun. at age 12	0.00	1.00
Currently works	0.43	0.40
Children	1.34	1.11
Attends school	0.12	0.17
Years of school (if fin.)	8.55	8.18
Ever married	0.67	0.54
Husband at home	0.88	0.83
Household size	4.76	5.26
Observations	960	2329

Table 4: Income by Sector (Pesos, 2002)

Current Sector	Statistic	Last Month's Income	Last Year's Income	Hourly Income (imputed)	Hours Worked (Week)	Weeks Worked (Last Year)	Years of School	Years of Work	Age	Obs.
Agriculture	Mean	1,346	7,182	15.06	36.45	35.04	6.12	5.51	23.4	23
	Median	1,200	3,840	5.15	40	48	6	6	23	
Construct.,	Mean	2,088	16,543	14.16	32.65	42.14	11.56	3.49	23.19	8
Mining, Elect.	Median	2,000	24,000	16.78	28	52	12	3	24	
Manufacturing	Mean	2,533	19,784	10.66	46.02	45.19	8.28	5.1	23.9	150
	Median	2,000	19,700	9.82	48	52	9	4	23	
Retail, Food,	Mean	2,419	19,125	15.7	44.46	45.44	9.68	4.72	23.95	171
Transport	Median	2,000	16,400	8.33	48	52	9	5	23	
Profesional	Mean	2,985	24,694	13.18	43.01	45.73	10.33	4.69	22.86	48
Services	Median	2,700	18,600	14.27	42	52	10	5	23	
Social & Govt.	Mean	3,695	36,312	37.25	33.67	47.59	12.52	4.97	26.15	105
Services	Median	3,000	28,080	17.39	35	52	12	4	27	
Personal	Mean	1,673	13,105	15.26	34.21	45.61	6.82	5.47	24.96	75
Services	Median	1,500	10,000	6.92	40	52	6	6	26	
Sample limited to women reporting a yearly wage, weeks and hours worked										

5 Empirical Evaluation

My basic approach is to look at the effects of working in a factory on child investment decisions. Going back to equation 1, the basic specification is:

$$hfaz_{i,l} = \alpha + \delta_1 Manuf_{i,l} + \beta_s Sex_{i,l} + \sum_{l=1}^{L} \beta_l d_l + \sum_{age=0}^{15} \beta_{age} d_{age} + \sum_{momage=0}^{100} \beta_{momage} d_{momage} + \sum_{l=1}^{L} \gamma_l (d_l \cdot momage_{i,l}) + X_{i,l}\theta + \varepsilon_{i,l}, \quad (2)$$

without differential effects on male and female children from working in manufacturing, where i subscripts children and l the L municipalities. With potentially differential effects on male and female children through $\delta_3 \neq 0$ the specification is:

$$hfaz_{i,l} = \alpha + \delta_2 Manuf_{i,l} + \delta_3 (Manuf_{i,l} \cdot Sex_{i,l}) + \beta_s Sex_{i,l} + \sum_{l=1}^{L} \beta_l d_l + \sum_{age=0}^{15} \beta_{age} d_{age} + \sum_{momage=0}^{100} \beta_{momage} d_{momage} + \sum_{l=1}^{L} \gamma_l (d_l \cdot momage_{i,l}) + X_{i,l}\theta + \varepsilon_{i,l}. \quad (3)$$

The measure for child investment is a health measure, the height-for-age Z scores, $hfaz_{i,l}$. A schooling measure would also be desirable. However, most of the children of these young mothers are not yet in school and so my sample size is too small to look at educational outcomes. Height-for-age Z scores have been calculated using the WHO child growth standards (WHO Multicentre Growth Reference Study Group 2007). Height for age is a good measure of child health investments as it is a cumulative measure of child nutrition and health insults up to that age, and is regularly used as an indicator of stunting. It is thought that early life (age 0 to 2) nutrition and disease exposure has a particularly large effect on adult lives. The impacts of early life nutrition (which height-for-age z scores are measuring) include earlier mortality, increased morbidity and negative labour market outcomes (Barker 1992, Case, Fertig, and Paxson 2005, Almond 2006, Case and Paxson 2006). While other anthropomorphic measures are available in the data (weight, and hemoglobin levels in the blood), height for age is the best available summary measure of child investment in nutrition and health. I also include a dummy variable to control for sex, $Sex_{i,l}$. The WHO norms originate from well nourished children around the world, and so accordingly the averages for my sample are below zero for both sexes. Female children are on average 0.58 standard deviations below the mean, and male children are 0.64 standard deviations below the mean.

$Manuf_{i,l}$ is a dummy variable indicating whether the child's mother worked in manufacturing as her first job. It is 1 if she did, and 0 if she never worked or worked in another sector for her first job. This matches most closely to the decision the mother is making in the theoretical model, and picks up some of the women who work in manufacturing until childbirth and then leave the labor force. Given the lack of a complete job history in the MxFLS, this is the only available indicator of the sector of employment of a woman when she was younger. It does however miss women who start working in manufacturing while young but not as their first job. If I use a reduced form approach, in which my instrument enters directly as an independent variable, some of these women missed in the IV regression should then be picked up as they would have been induced to start working in a factory when it opened even if they previously had a job. These results are presented in appendix 2, table 14, and results are similar.

Additionally, I include location fixed effects d_l (at the municipality level, or metropolitan zone if a conurbation contains several municipalities), age dummies d_{age}, mother's age dummies d_{momage} and mothers age trends at the municipality level γ_l, as well as other controls $X_{i,l}$. The linear mother's age trends serve as standard time trends in the first stage regression. I include them to control for the fact that if manufacturing was becoming an increasingly common job for women in a specific municipality throughout the period, $Manuf_{i,l}$ will be correlated with any variable that trends over time in that type of municipality. So most obviously, if fast growing municipalities saw more manufacturing opportunities over time, I may just be picking up effects of general rapid growth in the municipality through $Manuf_{i,l}$. In the second stage, the IV regression, the need for this mother's age trend is more obvious as it controls for cohort effects (for example if there is progressively better health education taught in schools). Identification of my parameters of interest, the $\delta's$, derives from deviations from the municipality trend of new factory openings, which is like a regression discontinuity coming from the lumpiness of factory openings.

I include three sets of additional control variables, $X_{i,l}$. Specification 1 includes no additional controls. This specification will not provide the best estimates of the $\delta's$ since relevant controls will tend to increase the precision of the estimates. In the case of OLS they are also useful in controlling for some otherwise omitted variables. Specification 2 accordingly adds controls that can be considered characteristics at age 16. These controls are adult height (a good measure of earlier life nutrition and income) and education capped at grade 9, which most girls should complete by age 15. Events after age 16 can plausibly affect both of these, but they are the observable characteristics most likely to be set by that age. These variables could be still be endogenous if there are omitted variables correlated both with child investment and mother's height and education at age 16. This is not such a problem as we are not actually interested in the coefficients on the controls, however they still may impart a bias on the coefficient on $Manuf_{i,l}$ and so it is useful to check that the results do not differ too greatly when there are no controls.

Finally, specification 3 also includes household per capita income in 2002 and the number of siblings. Both of these are plausibly affected by the decision to work in manufacturing, and so some of the effects of manufacturing may get loaded onto these variables. Therefore, this specification is not very useful for inference about the magnitude of the effect of working in manufacturing on child health. However, much of their variation in income and fertility will not be related to the sector of employment, and these variables serve as useful controls to show that there is an effect from sectoral choice beyond that working through income. My preferred specification is accordingly specification 2, but I will refer to specification 1 and 3 where appropriate.

Table 5: Basic OLS Regressions

Coefficient	Dependent Var: Height for Age Z Scores					
	OLS 1.1	OLS 1.2	OLS 2.1	OLS 2.2	OLS 3.1	OLS 3.2
Manuf (first job)	0.083	0.239	0.077	0.183	0.049	0.197
	0.67	1.52	0.72	1.3	0.44	1.34
Manuf*Sex		-0.323*		-0.217		-0.313*
		-1.84		-1.38		-1.89
Sex (1=male, 0=fem)	-0.141**	-0.107*	-0.172***	-0.149***	-0.145***	-0.111*
	-2.62	-1.9	-3.45	-2.87	-2.77	-1.98
Mothers Schooling			0.023	0.022	0.000	-0.002
(pre age 16)			1.05	1.02	-0.0046	-0.072
Mother's Height			0.0517***	0.0515***	0.0455***	0.0451***
(cm)			9.19	9.19	8.71	8.73
Hhold consumption					0.00645*	0.00642*
(per cap., 1000's Pesos)					1.75	1.74
Number of Siblings					-0.139***	-0.142***
					-3.59	-3.63
Observations	2068	2068	2025	2025	1960	1960
R-squared	0.3	0.3	0.35	0.35	0.37	0.37
Clusters	95	95	95	95	94	94

t-values under coefficients. All Regressions weighted by child survey weights and standard errors clustered at municipality level. All regressions include child age dummies, mothers age dummies, municipality fixed effects and municipality specific linear time trends in mothers age. * is significant at 10% level, ** at 5% and *** at 1%.

Table 5 shows the baseline regression results. There are around 2000 children whose mothers' turned 16 between 1986 and 2000. Most of these children are quite young, with the average age being just over 5 years old, and over 85% of the children under age 10. Sub-specification 1 shows the results of equation 2 (no sex specific effects). The sample size for the regressions including controls falls a little due to missing data issues. There seems to be no effect on child height from a mother working in manufacturing as her first job, with or without controls. Sub-specification 2 shows equation 3, which breaks the effect of manufacturing on child height for age down by sex. The theory section suggested there may be larger effects for females if mothers have a relatively higher regard for female child investment (and later life consumption for female children) compared to men, controlling for the regard for child investment. In this specification, some weak effects appear. Focusing on specification 2, female children are 0.18 standard deviations taller if their mothers worked in manufacturing. Male children are worse off relative to female children, and in total are 0.03 standard deviations smaller than boys whose mothers did not work in manufacturing (although none of these effects are statistically significant).

While these results are indicative that manufacturing work increases female health investments by a small amount, it is odd that male investments seem to have been slightly reduced if anything (although not significantly so). This is a possible outcome from the theoretical model (if for example the perceived returns to female child investment rose greatly, but returns to male investment did not, and total child investment remained fairly constant as incomes stagnated). However, most reasonable parameterizations would suggest both male and female investment increases with female manufacturing, but with female children gaining relatively more than male children.

5.1 Instrumenting for Selection Effects:

There are serious concerns, which have been discussed at length in section 3, regarding selection issues. Suppose that the type of women who decide to go into manufacturing are incredibly driven women who are willing to buck societal norms to work in a sector that was traditionally a masculine domain in Mexico. If these characteristics were also correlated with being good at making effective child investments, having a higher bargaining power in a relationship or the host of other places where personal characteristics can affect child investment outcomes, then I will be spuriously attributing these effects on child investment to female manufacturing work. This could produce the results above, but in fact manufacturing has no effect on child investment. Therefore, the instrument discussed in section 3 is required to control for this selection issue.

The exact instrument used for whether a woman's first job was in manufacturing is the number of new factory jobs in the formal manufacturing sector per working age female in the municipality created during the year the woman turns 16. The working age female population is defined as women aged 15 to 49 from the Mexican Census of 1990 and 2000 and the Conteo de Población y Vivienda 1995, with the other years interpolated.[6] New factory jobs in manufacturing are defined as positive employment changes in a firm classified as manufacturing by IMSS, where 50 or more women were hired in a single year for a new factory or total female employment increased by 50 or more women for existing factories that expanded. The results are robust to lowering the cutoff from 50 to 25. This variable is a measure of available employment opportunities in factories. By restricting the measure to only sizeable changes I obtain a lumpy measure of labor demand that is plausibly exogenous to the characteristics of individual cohorts of women.

The IMSS data contain all formal sector employment and many firms employ fewer than five women. The kind of mechanisms at play in the theoretical section are most relevant to factory work and so it is logical to exclude these very small workshops which may be more comparable to home employment. Including only new hires makes sense if the existing manufacturing jobs are generally filled (with a small amount of churn) and so large changes in the opportunity sets for female employment occur only with new factory openings or large factory expansions. Since I am trying to identify my parameters from year to year changes in this opportunity set within a municipality, such a pronounced measure makes sense. The measure is scaled by total working age female population as it is obvious that one factory opening in a city of 500,000 will have a much smaller effect on the probability of working in manufacturing than if the same factory opened in a town of 15,000. For equation 3, two variables need to be instrumented, $Manuf_{i,l}$ and $Manuf_{i,l} \cdot Sex_{i,l}$ and this is done by using the instrument above (inst1) and the same instrument interacted with child sex (inst2).

While the instrument only picks up formal sector hires, there are likely to be manufacturing spillovers into the informal sector. For example, if a formal textiles factory opens, it is quite likely the factory may employ some workers informally either directly or through contracting out. Therefore, my instrument may pick up more than formal sector employment. This slightly changes our LATE interpretation to covering women who were induced to work in any manufacturing industries by the opening of a new factory at age 16, who would not have worked in these sectors otherwise. The choice of age 16 is an obvious one as that is the legal age for starting manufacturing work in the formal sector.[7] While it is certain that

[6] All this data was obtained from the INEGI website at www.inegi.gob.mx.

[7] The minimum working age is 14 in Mexico, but rises to 16 in sectors considered hazardous, which includes many industrial sectors. Additionally children aged 14 and 15 require parental consent, a document confirming they are medically fit and can not work overtime or beyond 10pm. Accordingly, the minimum working age in the formal manufacturing sector is usually stated as 16.

some female employees are younger and have forged documents, Mexican factories do not have a particularly large problem with child labor.[8] Age 16 is also the age children start upper secondary school and so is a common age to drop out of school and start work. The assumption that this is the relevant age is supported by the first stage regressions which are shown in table 6. The sample is restricted to women who were living in their current municipality at age 12 and who started work at or after age 16 (as a woman who started work at age 9 'won't be more likely to have her first job in manufacturing if a factory opens when she is 16).

Table 6: Instrument First Stage

Coefficient	Dependent Var: Manuf (first job)		
	OLS 1	OLS 2	OLS 3
Age 16 New Hires/FemPop15_49	11.02***	11.33***	10.53***
	5.2	2.82	5.2
Age 15 New Hires/FemPop15_49		-4.470	
		-1.14	
Age 17 New Hires/FemPop15_49		1.491	
		0.23	
Age 18 New Hires/FemPop15_49		-4.639	
		-1.18	
Mothers Schooling		1.491	-0.006
(pre age 16)		0.23	-0.77
Mother's Height		-4.639	-0.002
(cm)		-1.18	-1.3
Observations	2074	1719	1795
R-squared	0.13	0.13	0.14
Clusters	96	96	95

t-values under coefficients. All Regressions weighted by adult survey weights and standard errors clustered at municipality level. All regressions include mothers age dummies, municipality fixed effects and municipality specific linear time trends in mothers age. OLS 1 includes only women who were 16 and older for first job, OLS 2 includes 15 and older. * significant at 10% level, ** at 5% and *** at 1%.

There is a very strong effect from age 16 new factory hires in table 6. For other ages, the effects are not significant, of smaller coefficient values and sometimes even negative. In OLS 3 I include controls for mother's height and education at age 16, and the results are unchanged. The coefficient size of 11 is a reasonable magnitude. If there were 100 new jobs and 100 unemployed women all age 16 wishing to enter employment in the town, and no one

[8] For example the Congressionally mandated "By the Sweat and Toil of Children" (US Department of Labor 1994) reports that "there is only limited evidence of the existence of child labor in maquilas currently producing goods for export although there have been past reports of child labor".

could move jobs, the coefficient should equal 1 if the jobs were suitably attractive. However, the working age female population picks up a much larger pool of women than those wishing to take a job who have not yet entered the labor force and women who are already working can fill the factory jobs, and all women may not want the jobs even if they are available. Therefore, if only one tenth of the working age female population would want such a job, the coefficient would be around 10. The reduced form regression of specification 2 is shown in appendix 2, table 14, and will be discussed later.

As a further check on the instrument, I look at the effect of a new factory opening on women who were not living in the municipality at age 12 or had already started working by the age of 16. These women's choice of first job should be unaffected by new factory openings at age 16 in their current municipality. Table 7 shows that this is indeed the case. When the sample is reduced to include only these 1530 women, the coefficient is tiny and the t-value is totally insignificant. This placebo test is evidence that the instrument is indeed picking up new job opportunities that are relevant to women's employment decisions.

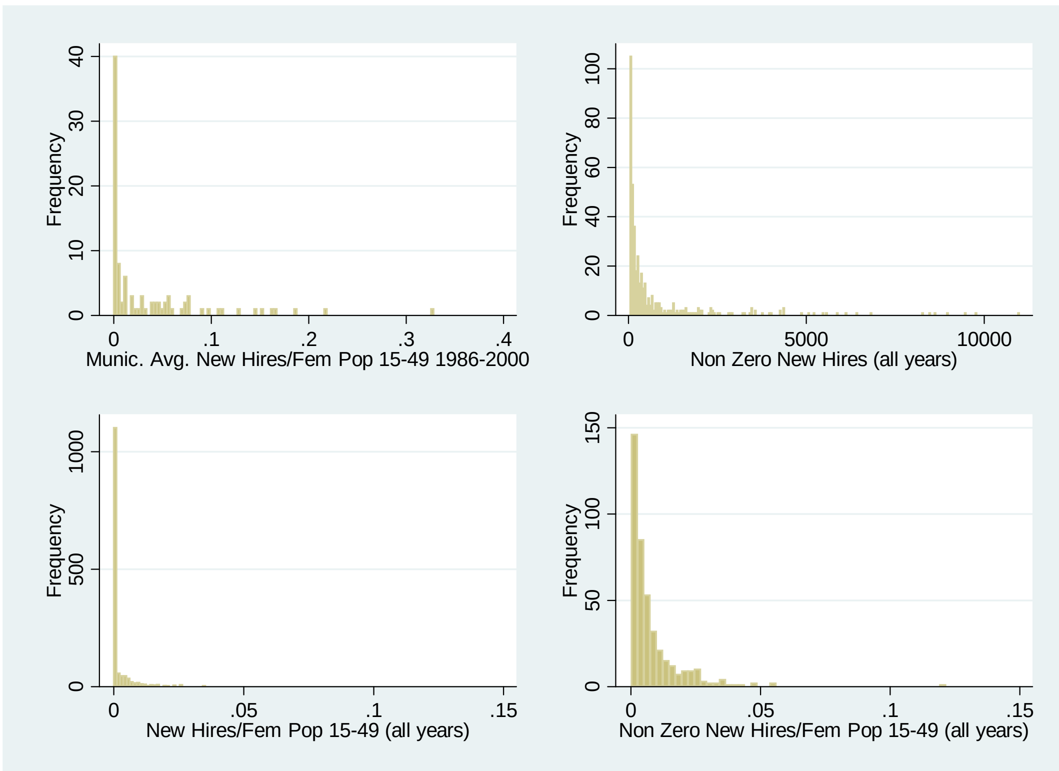
Table 7: Instrument First Stage (unaffected sample)

Coefficient	Dependent Var: Manuf (first job)
	OLS
Age 16 New Hires/FemPop15_49	0.804
	0.17
Observations	1530
R-squared	0.22
Clusters	95

t-values under coefficients. All Regressions weighted by adult survey weights and standard errors clustered at municipality level. All regressions include mothers age dummies, municipality fixed effects and municipality specific linear time trends in mothers age. Includes only women who were younger than 16 at first job or lived in another municipality at age 12. * is significant at 10% level, ** at 5% and *** at 1%.

Figure 1 shows the distribution of new hires across the 96 municipalities/metropolitan areas. The top left panel shows that 38 of the municipalities had no firms that increased employment by more than 50 women in a single year. The remaining 56 municipalities hired a total of 351,664 women in expansions of more than 50 jobs over the period 1986-2000. This number falls to 201,876 new hires if Guadalajara and Monterrey are excluded. However, in the robustness checks the analysis is carried out without these cities and the results do not change. The distribution is very much skewed towards zero, as in smaller municipalities new factories are not expected to open every year and this is exactly where the identification is

Figure 1: Distribution of New Hires



coming from. The top right panel shows the non-zero new hires (in absolute terms), the non zero new hires divided by female working age population are shown in the bottom right panel and the full sample of new hires divided by female working age population is shown in the bottom left panel.

Table 8 shows the results of the instrumented regressions using Limited Information Maximum Likelihood.[9] As the sample has now been reduced (only women who started work after age 15 are included), I have reproduced the OLS regressions with this smaller sample and the coefficients are very similar although the smaller sample size has made some of the t-statistics insignificant. The IV regressions show strong effects on child height of a woman having her first job in manufacturing. These children are between 1.18 and 1.75 standard deviations taller than children whose mothers did not have their first job in manufacturing. Remembering that Mexican children are over half a standard deviation on average below the WHO norms, this is still very sizeable.[10] There is still an interaction effect on sex, with female children between 1.92 and 2.61 standard deviations taller, and male children are now also significantly taller (at the 10% level except for specification 3) with heights between 0.67 and 1.22 standard deviations above those whose mothers first job was not in manufacturing. While the difference between boys and girls is no longer significant, given the consistent sign difference, and the large magnitude of the coefficient, it is likely there is a sex interaction effect with significance being obscured by the inflated standard errors coming from the use of an instrumental variable.

The preferred specification is specification 2, where the total effect is an increase in child height of about 1.4 standard deviations from a mother's first job being in manufacturing. In the IV context, the instrumented manufacturing variable is unlikely to be working through changes in adult height or schooling at age 16 as they are already determined, so the precision can be increased without risking falsely attributing some of manufacturing's effects to the controls (as is a worry in specification 3). Additionally, the inclusion of education assuages any fears that factories may be responding to educational differences between cohorts when deciding where to locate a factory and so violating the exogeneity assumption on the instrument. Later, for robustness, I will run the same formulation on total education to address potential correlations between future educational changes and factory location which could violate the strict exogeneity assumption if total education was omitted.

The robust first stage F stats are all quite large. The rule of thumb of Stock and Yogo

[9] Staiger and Stock (1997) show that Limited Information Maximum Likelihood is more robust to weak instrument problems than standard two stage procedures.

[10] To get a better idea of the magnitudes, the WHO norms for a girl aged 48 months is a mean height of 102.7cm and a standard deviation of 4.3cm. For boys at this age, their mean height is 103.3 with a standard deviation of 4.2cm (WHO Multicentre Growth Reference Study Group 2007).

(2002) is that the F-stat should be above 10 to avoid weak instrument problems with two stage least squares. While no tables are available for robust F-stats (which are necessary as I cluster standard errors at the municipality level), there does not seem to be a large problem with weak instruments as I am using Limited Information Maximum Likelihood anyway and the robust F-stats are lower than the non-robust F-stats. Only the instrument on manufacturing, when we include both manufacturing and manufacturing interacted with sex, falls below 10, and once the controls are included it climbs to 12.87. Given the similarity of the results both with and without controls, it is fairly safe to assume that these results are not being driven merely by weak instrumentation.

What do these results say about how the selection effects operate? Since the coefficients are larger than OLS rather than smaller, the story given at the beginning of this section is clearly not right. Instead, the selection seems to be working the other way. Women who are from particularly disadvantaged backgrounds, and have characteristics that have negative effects on child investment, household bargaining and the like seem more likely to sort into manufacturing jobs. This may be a more believable story as female factory work is generally seen as an undesirable job only chosen by more desperate women coming from poor and unskilled backgrounds. Before working in a factory, these women may have had lower intrinsic bargaining power and care relatively less about child investment and in particular investment in female children, as they come from more traditional and often rural backgrounds. These women may also have less resources at their disposal, making the returns to child investment lower (for example less help around the house, less able to optimally invest in child health due to lack of knowledge or education). All these factors would lead to OLS estimates being lower than the IV estimates, as I find.

These results are very large in magnitude and that warrants further discussion. As discussed in the econometric interpretation section, these estimates are local average treatment effects, and so they apply to the women who would not have entered manufacturing had a new factory not opened in the year they turned age 16. Presumably, the effects are much more muted for the women who would have gone into manufacturing anyway, and this group may be more sizeable. This is because, since women do care about child investment and will factor this in when deciding which sector to enter, some of the women who end up choosing manufacturing will be the women with particularly large heterogenous gains to child investment (including child height) from working in manufacturing, which the IV results support. However, as pointed out previously, the LATE group is not an irrelevant group. If a local policymaker is contemplating whether to authorize or encourage a new factory to open, they can expect there to be positive effects on child investment down the line which would not have occurred had they decided to not let the factory open in that municipality.

Table 8: Instrumented Regressions (Limited Information Maximum Likelihood)

IV (LIML)	Dependent Var: Height for Age Z Scores					
	IV 1.1	IV 1.2	IV 2.1	IV 2.2	IV 3.1	IV 3.2
Manuf (first job)	1.748***	2.617***	1.377***	2.120***	1.179***	1.925**
	2.72	2.9	2.64	2.7	3	2.5
Manuf*Sex		-1.397		-1.178		-1.256
		-1.35		-1.25		-1.23
Sex (1=male, 0=fem)	-0.161***	-0.038	-0.183***	-0.079	-0.143**	-0.028
	-2.82	-0.33	-3.36	-0.72	-2.5	-0.24
Mothers Schooling			0.026	0.024	0.007	0.003
(pre age 16)			0.98	0.86	0.31	0.11
Mother's Height			0.0498***	0.0486***	0.0425***	0.0408***
(cm)			7.82	7.16	6.81	6.22
Hhold consumption					0.0114***	0.0119***
(per cap., 1000's Pesos)					2.71	2.74
Number of Siblings					-0.110***	-0.122***
					-2.99	-2.87
1st Stage Robust F - inst1	15.17	8.072	19.44	9.934	21.94	12.87
1st Stage Robust F - inst2		59.59		55.08		25.79
Observations	1610	1610	1575	1575	1522	1522
R-squared	0.21	0.17	0.3	0.27	0.33	0.31
Clusters	95	95	95	95	94	94
OLS	**Dependent Var: Height for Age Z Scores**					
	OLS 1.1	OLS 1.2	OLS 2.1	OLS 2.2	OLS 3.1	OLS 3.2
Manuf (first job)	0.070	0.214	0.104	0.187	0.134	0.265
	0.44	1.04	0.77	1.05	1.05	1.48
Manuf*Sex		-0.292		-0.170		-0.267
		-1.22		-0.78		-1.15
Sex (1=male, 0=fem)	-0.175***	-0.150**	-0.189***	-0.174***	-0.148**	-0.124*
	-2.96	-2.35	-3.16	-2.77	-2.33	-1.78
Mothers Schooling			0.012	0.011	-0.004	-0.005
(pre age 16)			0.63	0.6	-0.23	-0.3
Mother's Height			0.0496***	0.0495***	0.0437***	0.0434***
(cm)			8.49	8.46	7.3	7.26
Hhold consumption					0.00765**	0.00766**
(per cap., 1000's Pesos)					2.16	2.17
Number of Siblings					-0.132***	-0.135***
					-3.32	-3.29
Observations	1610	1610	1575	1575	1522	1522
R-squared	0.31	0.32	0.36	0.36	0.37	0.37
Clusters	95	95	95	95	94	94

t-values under coefficients. All Regressions weighted by child survey weights and standard errors clustered at municipality level. All regressions include child age dummies, mothers age dummies, municipality fixed effects and municipality specific linear time trends in mothers age. Only children of women whose first job was at 16 or older included. * significant at 10% level, ** at 5% and *** at 1%.

5.2 Direct Effects of Factories on Child Health

One concern is that children may be directly affected by factories opening in their locality. Suppose a factory is particularly polluting, or it brings with it new services and infrastructure, then child health could be affected. These effects are quite separate from those coming from mothers working in factories, since they will affect an entire birth cohort in the same way. However, since older mothers tend to have older children, the results above may be confounded by these effects. Therefore, I run a regression similar to the one above, but with my new factory measure at key ages in a child's life as the explanatory variable.

$$hfaz_{i,l} = \alpha + \sum_{age=0}^{2} \delta_{age} \frac{NewHires_{i,age,l}}{Pop15\_49} + \sum_{l=1}^{L} \beta_l d_l + \sum_{age=0}^{15} \beta_{age} d_{age} + \sum_{momage=0}^{100} \beta_{momage} d_{momage} + X_{i,l}\theta + \varepsilon_{i,l}$$

I include controls for the age of the child, the child's sex and municipality fixed effects and use both total new hires per working age population as well as female new hires per working age population. I also include mother's education and height, although the results are unchanged if they are omitted. These results are shown in table 9. At none of the key ages (the first three years of life, which are the most important ages for nutrition and health insults) do new hires seem to significantly affect child height for age. These results also stand up to a more selective sample of mothers similar to the ones used in the main regressions. Therefore, the main results are unlikely to be contaminated by cohort effects on children. This suggests I am not overestimating the effects of female manufacturing on child height for age, which could be the case if all children suffered negative health shocks from pollution, and children whose mothers worked in the factory gained relative to other children. I am still ignoring any effects coming from father's employment but these would likely raise rather than lower child height. Unfortunately my instrumentation strategy is far weaker for the fathers, presumably as there were many more manufacturing jobs available to men so the opening of a new factory at age 16 does not have such a large effect on the probability of manufacturing employment.

5.3 Causal Channels

The theoretical model outlined a host of possible causal channels through which female factory work might affect child investment outcomes. Without multiple instruments for

Table 9: Effects on Child Age Cohorts

Age Cohort Effects	Height for Age Z Scores	
	Female Hires OLS	All Hires OLS
Age 0 New Hires/Pop15_49	-1.307	-2.484
	-0.23	-0.41
Age 1 New Hires/Pop15_49	4.910	1.846
	1.14	0.47
Age 2 New Hires/Pop15_49	-4.654	-5.100
	-0.96	-1.17
Sex (1=male, 0=fem)	0.008	0.007
	0.26	0.22
Mothers Schooling	0.0283***	0.0282***
(pre age 16)	4.79	4.79
Mother's Height	0.0556***	0.0556***
(cm)	18.7	18.7
Observations	6238	6238
R-squared	0.29	0.29
Clusters	95	95

t-values under coefficients. All Regressions weighted by child survey weights and standard errors clustered at municipality level. All regressions include child age dummies, mothers age dummies and municipality fixed effects. Only children under 15 included. * significant at 10% level, ** at 5% and *** at 1%.

each of these channels, it is impossible to clearly identify the exacts routes through which child height for age seems to be improving. However, it is possible to tease out the potential relevance of a variety of channels by seeing how female manufacturing work affects household bargaining, income, consumption etc.

The first insights come from specification 3, that contains controls for household consumption and the number of siblings. While it would be ideal to have some measure of permanent female income, household consumption per capita is perhaps the best measure the data allows. While current female personal income is available, most of the women in the sample are not working, and those that are may be doing very different jobs from the job they did before they had children. Household consumption is therefore may be a more reasonable measure of permanent income, although it is permanent family income. Since controlling for income does not cause the coefficient on manufacturing work to become insignificant, at least some of the effects of female manufacturing on child height are working through channels other than raising household income. Therefore, my results can not be fully explained by the fact that child investment is a normal good and income increases through greater manufacturing opportunities for women. Similarly, my results are unlikely to be only coming from increased woman's income raising her bargaining power. In the same way I can conclude the effects are not just working through fertility decisions, since the results hold up when I include sibling effects.

Further insights can be gleaned from running very similar regressions to those estimated previously, however now focussing on various other outcomes for mothers who work in manufacturing. Again age dummies, municipality dummies and municipality level age trends are included, and once more I instrument for $Manuf_{i,l}$ with new female hires per female working population at age 16, and include controls for education at age 16 and female height.

$$y_{i,l} = \alpha + \delta Manuf_{i,l} + \sum_{l=1}^{L} \beta_l d_l + \sum_{age=0}^{100} \beta_{momage} d_{momage} + \sum_{l=1}^{L} \gamma_l (d_l \cdot momage_{i,l}) + X_{i,l}\theta + \varepsilon_{i,l} \quad (4)$$

Tables 10 to 12 shows IV and OLS regressions for many variables that are useful in identifying the channels through which female factory work may affect child investments. Whilst some inferences regarding the effect of income were already drawn simply from comparing results both with and without the inclusion of income controls, I can investigate further by looking at how incomes are affected by the decision to work in manufacturing. Table 10 shows how entering manufacturing affects various income measures (in 2002 pesos). The household per capita income measure is the least reliable as it is based on a single household respondent reporting the earned income of all household members. The two consumption

Table 10: Consumption, Income and Work Channels

Consumption, Income and Work				
Dep Var: Coefficient	Household Consumption PC (2.5% tails dropped)		Household Consumption PC (2.5% tails winsorized)	
	IV	OLS	IV	OLS
Manuf (first job)	7046	-2125	4305	-1959
	1.63	-1.65	0.74	-1.15
Mothers Schooling	847.3***	801.9***	942.0***	906.9***
(pre age 16)	4.91	4.86	4.9	4.72
Mother's Height	218.5***	209.1***	264.0***	256.3***
(cm)	4.4	3.79	4.95	4.54
1st Stage Robust F	23.06		23.37	
Observations	1708	1708	1773	1773
R-squared	0.26	0.31	0.29	0.31
Clusters	95	95	95	95
Dep Var: Coefficient	Household Income PC (2.5% tails winsorized)		Mother Currently Works (1 = yes)	
	IV	OLS	IV	OLS
Manuf (first job)	-890	-1625*	0.542	0.266***
	-0.12	-1.78	0.92	5.69
Mothers Schooling	701.6***	697.5***	0.0407***	0.0392***
(pre age 16)	3.84	3.63	3.6	3.45
Mother's Height	171.7***	170.7**	0.002	0.002
(cm)	2.68	2.61	0.71	0.61
1st Stage Robust F	27.03		27.02	
Observations	1794	1794	1795	1795
R-squared	0.28	0.29	0.17	0.2
Clusters	95	95	95	95

t-values under coefficients. All Regressions weighted by adult survey weights and standard errors clustered at municipality level. All regressions include mothers age dummies, municipality fixed effects and municipality specific linear time trends in mothers age. Includes only women who were 16 and older for first job. * significant at 10% level, ** at 5% and *** at 1%.

measures are more reliable, based on an extensive consumption survey, however I have had to deal with the presence of improbable outliers by trimming or winsorizing at the 2.5% tails. The basic OLS regressions show that working in manufacturing for a first job is associated with living in a poorer household in 2002. This is not surprising given the selection effects I have previously found, with seemingly worse off women being drawn into manufacturing.

Reassuringly, there seems to be a positive income effect, or at least not a significant negative effect once manufacturing is instrumented for. Given that the average consumption for the sample is around 14,000 pesos a year, the coefficients are substantial even though insignificant. The theory section predicted that consumption should rise. The opening of a new factory increase their job choice set. A larger set would be unlikely to lower incomes for the group of women who only decide to enter manufacturing because of a new factory if they care about consumption, as the LATE group are the women induced to change their sector choice in anticipation of a higher future consumption path for themselves and their children. Therefore, the direct income channels through which working in manufacturing may affect child outcomes cannot be ruled out (either through child investment being a normal good or greater bargaining power if a woman's relative income has risen). However, this is not the complete explanation, as effects remain when income is included as a control in table 8.

Women whose first job was in manufacturing are more likely to be currently working from the OLS regression (which is not surprising given women who never had a first job, and so by definition are not working now, are included in the other group). While the IV estimate does rise, it is not significant. If the probability of currently working does increase, the efficacy of child investment should decrease if the mother is away from the house a lot, at least controlling for income.[11]

I can also say something about education, fertility and household composition decisions. Table 11 suggest that, at least for the LATE group picked up by the IV, women working in manufacturing do not seem to be any more likely to be married, to have more children or to get married at different ages. Household size seems to be the same as other households and these women seem to be no more likely to have a husband not living at home. This could be partly because the true effects are small and the IV has large standard errors associated with instrumentation. However, it would be foolish to read anything further into t-values of 0.5 and below. The OLS estimates suggest women in manufacturing are less likely to be married, although the effect is small. However, the OLS results do show that women whose first job was in manufacturing are significantly less likely to have a husband living at home. The latter factor (for which the IV estimate is almost identical but not significant) would suggest

[11] Controlling for income and household size (not shown) lowers the coefficient but only slightly to 0.496 (with a t-stat of 0.91).

Table 11: Education, Fertility and Household Composition Channels

Education, Fertility and Houshehold Composition				
Dep Var: Coefficient	Household Size		Children Born	
	IV	OLS	IV	OLS
Manuf (first job)	-0.539	0.681	0.222	-0.154
	-0.52	1.45	0.19	-1.52
Mothers Schooling (pre age 16)	-0.0928**	-0.0859**	-0.121***	-0.123***
	-2.47	-2.18	-4.87	-4.6
Mother's Height (cm)	-0.0201**	-0.0184*	-0.010	-0.011
	-2.07	-1.72	-1.51	-1.41
1st Stage Robust F	27.03		28.03	
Observations	1794	1794	1773	1773
R-squared	0.17	0.19	0.45	0.46
Clusters	95	95	95	95
Dep Var: Coefficient	Ever Married (1=yes)		Age at First Marriage	
	IV	OLS	IV	OLS
Manuf (first job)	0.0458	-0.0941**	0.008	0.552
	0.15	-2.27	0.0038	1.18
Mothers Schooling (pre age 16)	-0.0433***	-0.0441***	0.216***	0.223**
	-4.06	-3.97	2.73	2.57
Mother's Height (cm)	-0.003	-0.003	-0.007	-0.006
	-1.28	-1.27	-0.34	-0.26
1st Stage Robust F	27.02		18.91	
Observations	1795	1795	963	963
R-squared	0.34	0.34	0.35	0.35
Clusters	95	95	95	95
Dep Var: Coefficient	Husband Lives at Home (1=yes)		Total Years of Schooling (2002)	
	IV	OLS	IV	OLS
Manuf (first job)	-0.165	-0.165**	-2.240**	-0.894***
	-0.57	-2.52	-1.97	-5.12
Mothers Schooling (pre age 16)	0.000	0.000	1.265***	1.273***
	0.049	0.047	37.7	38.8
Mother's Height (cm)	0.000	0.000	0.009	0.011
	0.12	0.1	0.92	1
1st Stage Robust F	19.01		27.02	
Observations	969	969	1795	1795
R-squared	0.34	0.34	0.75	0.77
Clusters	95	95	95	95

t-values under coefficients. All Regressions weighted by adult survey weights and standard errors clustered at municipality level. All regressions include mothers age dummies, municipality fixed effects and municipality specific linear time trends in mothers age. Includes only women who were 16 and older for first job. * significant at 10% level, ** at 5% and *** at 1%.

that the children of women whose first job was in manufacturing would be worse off, if a father being present has a positive effect on child outcomes as most research suggests. Given that I find the opposite, this suggests other channels are at play. Unfortunately missing data and a small sample size make it impossible to look at the direct effects of manufacturing on husband education or income. In general, the fact that household composition and fertility decisions appear unchanged, casts doubt on any causal channels that work through husband choice or fertility choices.

Table 11 also shows the effect of manufacturing on total education (controlling for education up to grade 9). Both the OLS and the IV show sizeable reductions in total education from moving into manufacturing as a first job. Intuitively this is what would be expected, given that most manufacturing employment requires unskilled workers, and young women may well be pulled out of school if a good factory opportunity arises. Given that education has been shown in many studies to be positively correlated with child health, the fact these women are getting less education should reduce the efficacy of child investment and so lower child height for age. The fact the opposite happens is evidence that the education channel is not the channel driving my results.

In the theory section, I outlined several separate channels through which female factory employment may affect child height for age. From the available evidence, husband characteristics (abstracting from income), educational choices, fertility choices and time at home do not seem capable of explaining the positive effects on child height. Income is surely part of the story, but at least current household per capita consumption is not capable of explaining all or even most of the observed differences.

The relative lifetime income between husband and wife is another channel suggested by the theory. However, since most women with children don't currently work, lifetime income is impossible to calculate as there is no data on past income. However, a rise in relative income to women would almost certainly also increase total permanent income and hence consumption, so my results are still suggestive of other routes beyond this. This leaves the direct effects of manufacturing on changing bargaining weights and fallback options but not through income. Also the much larger effects on female children could be partly caused by factory work changing mother's expectations of future opportunities for women. While these routes are inevitably vague and perhaps require some additional qualitative analysis, I will present some suggestive evidence to support these propositions.

The MxFLS asks several questions on household bargaining. They ask household members who have a partner that lives in the same house about how the household makes decisions. I focus on decisions about child education, child health services and medicine and the strong expenditures for the house. Both husbands and wives are asked to list all household

Table 12: Household Bargaining Channels

Household Bargaining (Decisionmaker: 3= Wife, 2=Joint, 1=Husband)				
Coefficient	Education of Children			
	IV	OLS	IV	OLS
Manuf (first job)	1.519**	0.133	1.411***	0.135
	2.31	1.11	2.76	1.12
Mothers Schooling	0.010	0.005	0.003	-0.001
(pre age 16)	0.51	0.24	0.18	-0.027
Mother's Height	0.0003	-0.0019	-0.0016	-0.0031
(cm)	0.046	-0.26	-0.24	-0.43
Hhold consumption			0.005**	0.002
(per cap., 1000's Pesos)			2.45	0.68
Mother Works			-0.091	0.048
(1=yes)			-0.98	0.78
1st Stage Robust F	5.427		11.84	
Observations	718	718	708	708
R-squared		0.35		0.35
Clusters	95	95	94	94
Coefficient	Health of Children			
	IV	OLS	IV	OLS
Manuf (first job)	1.022**	0.157	1.194**	0.160
	2.15	0.99	2.38	0.92
Mothers Schooling	0.023	0.019	0.022	0.017
(pre age 16)	0.98	0.91	0.87	0.83
Mother's Height	0.0004	-0.0002	-0.0006	-0.0009
(cm)	0.063	-0.03	-0.094	-0.13
Hhold consumption			0.004	0.001
(per cap., 1000's Pesos)			1.58	0.26
Mother Works			-0.117	0.001
(1=yes)			-1.32	0.01
1st Stage Robust F	5.253		10.11	
Observations	726	726	716	716
R-squared	0.16	0.31	0.1	0.31
Clusters	95	95	94	94
Coefficient	Strong Expenditures			
	IV	OLS	IV	OLS
Manuf (first job)	1.990**	0.030	1.891***	0.024
	2.17	0.23	2.6	0.19
Mothers Schooling	0.013	0.000	0.007	-0.007
(pre age 16)	0.41	-0.029	0.25	-0.45
Mother's Height	0.0024	-0.0011	0.0009	-0.0017
(cm)	0.31	-0.16	0.11	-0.25
Hhold consumption			0.009	0.004
(per cap., 1000's Pesos)			1.61	1.15
Mother Works			-0.180	0.056
(1=yes)			-1.1	0.55
1st Stage Robust F	5.238		8.218	
Observations	778	778	768	768
R-squared		0.29		0.3
Clusters	95	95	94	94

t-values under coefficients. All Regressions weighted by adult survey weights and standard errors clustered at municipality level. All regressions include mothers age dummies, municipality fixed effects and municipality specific linear time trends in mothers age. Includes only women who were 16 and older for first job. * significant at 10% level, ** at 5% and *** at 1%.

members involved in the decision. I use the mother's responses and remove everyone who is not one of the two partners. I assign the code 3 if the woman makes the decision, 2 if the decision is made jointly and 1 if only the man makes the decision. The results are shown in table 12. For all three measures, the OLS shows small and positive coefficients, meaning that the woman's bargaining power is increased if her first job was in manufacturing. These effects are not significant. Once the instrument is used, the effects become much larger and significant for all three categories. The second specification adds controls for household consumption per capita in 2002 and whether the mother works in 2002 to address the relative income channel. The results are unchanged. The instruments are weaker (Robust F stats between 5.2 and 11.8) since the sample is much reduced as not all women answered the section and only women with a husband currently at home were asked these questions. However, this is suggestive evidence that female bargaining power increases with working in manufacturing.

5.4 Robustness Checks

For robustness I rerun the main regressions on child height for age with a sample that excludes Guadalajara and Monterrey. Since these are very large cities with lots of manufacturing, and the sample is weighted by survey weights, it is valuable to verify that these two locations are not driving my results Additionally all the municipalities where no factory employing 50 or more women opened or expanded between 1986 and 2000 are excluded. While these municipalities should not have much effect on my results (as identification of my key parameters was not coming from these municipalities anyway), they may be quite different types of municipalities with plausibly different parameters. For all of these robustness checks I use my preferred specification 2, with controls for education at age 16 and adult height. The results are unchanged and shown in the first panel of table 13 in appendix 2.

An obvious question is which sectors women who work in manufacturing would have entered had these jobs not been available. As shown in section 4, the main alternatives are not working, retail etc. and personal services. I therefore rerun the main specifications but limiting the sample to only women whose first job was in these sectors (or who never worked). These results are shown in the second panel of table 13 in appendix 2. Again there are no substantial differences, and so I can interpret the previous results as the effect of working in manufacturing compared to the likely alternatives of not working or working in either retail or personal services.

As another robustness check, reduced form OLS regressions are run. Instead of instrumenting for whether the first job was in manufacturing, I simply include a direct effect of new hires at age 16 on child height for age. This sample can include women who started work before age 16, so I run the regressions both with and without these women. The benefit is that despite not knowing the complete work history of all these women, some of the effects on child height can work through women who were already employed at 16 moving into manufacturing because a factory opened when they were 16. The results hold up and are contained in table 14 in appendix 2. Additional robustness checks were performed and are available on request, with the cutoff for new firms lowered to 25, and the ratio of new female hires to total formal sector female employment used as my instrument. In neither of these cases are the results substantially different.

6 Conclusions

Low-skill female manufacturing work has been an important facet of the global production chains that have become ubiquitous over the last few decades. Mexico is no exception, with enormous expansions in female manufacturing employment, especially in Maquiladoras, accompanying trade liberalization in the 1980's and 1990's. Many more countries less developed than Mexico will begin the process of attracting low skill assembly and textile work drawn by relatively low wages, and many of these jobs will taken by young women. Vietnam, for example, is currently seeing a rapid expansion in export-oriented manufacturing. The results in this paper suggest that there will be large increases in child investment, and in particular in child height, for the children of the women who are induced to work in these factories.

I find that women whose first job was in manufacturing, and were induced to enter that sector by the expansion of employment opportunities in manufacturing, have children who are 1.38 standard deviations taller than they would be otherwise. This effect is particularly strong for girls. While increased incomes through female manufacturing opportunities are almost certainly responsible for some of these gains, there also seems to be a direct positive effect on female bargaining power within their households attributable to female manufacturing work. These effects are important, as poor childhood nutrition is known to have serious effects on later life outcomes. While there may be pernicious effects on the lives of these women and their communities coming from the expansion of manufacturing in the developing world, these costs need to be weighed up against the full set of benefits. Given the enormous interest in the effects of globalization and the fact that local and national governments regularly pay millions of dollars in inducements for factories to locate in their constituencies, this evidence provides a vital input into the policy debate.

7 Appendix 1: Deriving LATE

Following Imbens and Angrist (1994) and Heckman and Vytlacil (2007b), the LATE interpretation can be derived as follows (conditioning throughout on X and assuming for now that Z only takes two values z and z'):

$$\begin{aligned} E(y \quad | \quad & Z = z) - E(y \mid Z = z') \\ = \quad & E((y_1 - y_0) \cdot Manuf + y_0 \mid Z = z) - E((y_1 - y_0) \cdot Manuf + y_0 \mid Z = z'). \end{aligned}$$

Assuming (U_1, U_0, V) are independent of Z, conditional on X, I can write the above equation as:

$$\begin{gathered} E(y \mid Z = z) - E(y \mid Z = z') = E((y_1 - y_0)(Manuf(z) - Manuf(z')), \\ = E((y_1 - y_0)(Manuf(z) - Manuf(z') = 1) \Pr(Manuf(z) - Manuf(z') = 1), \\ - E((y_1 - y_0)(Manuf(z) - Manuf(z') = -1) \Pr(Manuf(z) - Manuf(z') = -1). \end{gathered}$$

By monotonicity one of these probabilities is 0. For example in the case $Manuf(z) \geq Manuf(z') \, \forall i \implies \Pr(Manuf(z) - Manuf(z') = -1) = 0$. Also by monotonicity $\Pr(Manuf(z) - Manuf(z') = 1) = \Pr(Manuf = 1 \mid Z = z) - \Pr(Manuf = 1 \mid Z = z')$. Therefore

$$\frac{E(y \mid Z = z) - E(y \mid Z = z')}{\Pr(Manuf = 1 \mid Z = z) - \Pr(Manuf = 1 \mid Z = z')} = E((y_1 - y_0)(Manuf(z) - Manuf(z') = 1).$$

And this is simply the IV estimator since, with $p = \Pr(Z = z)$:

$$\begin{aligned} \delta^{IV} &= \frac{Cov(y, Z)}{Cov(Manuf, Z)} = \frac{E[y(Z - E[Z]]}{E[Manuf(Z - E[Z]]}, \\ E[y(Z - E[Z]] &= pE[y \mid Z = z](z - E[Z]) + (1 - p)E[y \mid Z = z'](z' - E[Z]), \\ z - E[Z] &= z - pz - (1 - p)z' = (1 - p)(z - z'), \\ z' - E[Z] &= z' - pz - (1 - p)z' = -p(z - z'), \\ E[y(Z - E[Z]] &= p(1 - p)(z - z')(E[y \mid Z = z] - E[y \mid Z = z']), \\ \delta^{IV} &= \frac{E[y \mid Z = z] - E[y \mid Z = z']}{E[Manuf \mid Z = z] - E[Manuf \mid Z = z']}. \end{aligned}$$

The key assumptions behind this derivation are:

1. (U_1, U_0, V) are independent of Z conditional on X.

2. Z is independent of $(y_1 - y_0)$ and y_0.

3. $Manuf(z) \geq Manuf(z')$ or $Manuf(z) \leq Manuf(z')$ for all individuals.

4. $E[Manuf \mid X, Z]$ is a nondegenerate function of Z given X.

8 Appendix 2: Robustness Tables:

Table 13: Robustness Checks: Alternative Samples

Guadalajara, Monterrey and Non-Manuf Munis. Removed				
Robustness Checks	Dependent Var: Height for Age Z Scores			
	IV	IV	OLS	OLS
Manuf (first job)	1.387***	2.351***	0.151	0.233
	2.77	3.14	0.96	1.13
Manuf*Sex		-1.421		-0.173
		-1.27		-0.79
Sex (1=male, 0=fem)	-0.203***	-0.052	-0.212***	-0.194**
	-3.01	-0.34	-3.1	-2.52
Mothers Schooling	0.050	0.050	0.024	0.023
(pre age 16)	1.33	1.2	0.97	0.94
Mother's Height	0.0500***	0.0478***	0.0512***	0.0510***
(cm)	7.83	6.37	8.31	8.2
1st Stage Robust F - inst1	17.42	8.765		
1st Stage Robust F - inst2		40.24		
Observations	1134	1134	1134	1134
R-squared	0.24	0.18	0.32	0.32
Clusters	56	56	56	56
Manuf=1 if First Job in Manuf, =0 if First Job in Competing Sectors Only				
Robustness Checks	Dependent Var: Height for Age Z Scores			
	IV	IV	OLS	OLS
Manuf (first job)	1.617***	2.550***	0.141	0.266
	2.62	3.74	0.99	1.45
Manuf*Sex		-1.484		-0.253
		-1.45		-1.11
Sex (1=male, 0=fem)	-0.194***	-0.046	-0.195***	-0.170**
	-3.12	-0.37	-2.99	-2.51
Mothers Schooling	0.024	0.021	0.011	0.010
(pre age 16)	0.8	0.65	0.5	0.46
Mother's Height	0.0477***	0.0465***	0.0485***	0.0483***
(cm)	7.62	7.27	7.86	7.91
1st Stage Robust F - inst1	13.82	7.186		
1st Stage Robust F - inst2		40.14		
Observations	1385	1385	1385	1385
R-squared	0.3	0.26	0.38	0.39
Clusters	95	95	95	95

t-values under coefficients. All Regressions weighted by child survey weights and standard errors clustered at municipality level. All regressions include child age dummies, mothers age dummies, municipality fixed effects and municipality specific linear time trends in mothers age. Only children of women whose first job was at 16 or older included. * significant at 10% level, ** at 5% and *** at 1%.

Table 14: Reduced Form Regressions

Reduced Form	Height for Age Z Scores	
	OLS	OLS
Age 16 New Hires/FemPop15_49	26.15**	36.99**
	2.33	2.1
Age 16 New Hires/FemPop15_49		-22.180
*Sex		-1.08
Sex (1=male, 0=fem)	-0.192***	-0.128
	-3.2	-1.52
Mothers Schooling	0.008	0.007
(pre age 16)	0.47	0.38
Mother's Height	0.0490***	0.0489***
(cm)	8.47	8.42
Observations	1575	1575
R-squared	0.36	0.36
Clusters	95	95

t-values under coefficients. All Regressions weighted by child survey weights and standard errors clustered at municipality level. All regressions include child age dummies, mothers age dummies, municipality fixed effects and municipality specific linear time trends in mothers age. Only children of women whose first job was at 16 or older included. * significant at 10% level, ** at 5% and *** at 1%.

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