The Wage Penalty for Motherhood in Developing Countries

Jorge M. Agüero[a/], Mindy Marks[b/], and Neha Raykar[c/]

May 2012

Abstract

Despite the growing size and importance of female employment worldwide, there have been limited efforts to explore the magnitude and causes of motherhood wage penalties in developing countries. Data from almost 130,000 women in 21 developing countries reveal a robust negative relationship between family size and female earnings. To address the endogeneity of family size, we instrument for the number of children using infecundity shocks and show that the negative relationship is causal. We find that for all women the negative impact of children diminishes as children age. For low-skilled mothers we find a differential impact by child's gender, with adolescent daughters *increasing* their mother's earnings relative to sons. Effort and selection into different types of jobs, occupations and work intensity fully explain the family gap for low-educated mothers; these variables account for two-thirds of the gap for women with secondary education or more.

Keywords: Female earnings, family size, family penalty, fertility, occupational sorting

JEL codes: J13, J22, J31.

[a/] Corresponding author: University of California, Riverside; Department of Economics, 4108 Sproul Hall; Riverside CA 92506; USA; jorge.aguero@ucr.edu; +1 951 8274108; fax: +1 951 8275685.
[b/] University of California, Riverside; Department of Economics, 4110 Sproul Hall; Riverside CA 92506; USA; mindy.marks@ucr.edu +1 951 8274164; fax: +1 951 8275685
[c/] Colgate Universty; Department of Economics; 229B Persson Hall; 13 Oak Drive, Hamilton, NY 13346, USA; nraykar@colgate.edu; +1 315 2286239.

1. Introduction

Women in developing countries now represent nearly 40 percent of the employed population. In 2009 there were 150 million more women in the labor force of low- and middle-income countries compared to 1999. This increase is not simply a byproduct of population growth. The annual growth rate in female labor force participation is 1.33 times higher than the average growth rate of the female population for the same period. As more women in developing countries enter the labor force, issues such as the gender gap in wages that once were labeled as irrelevant (Mammen and Paxson, 2000) are becoming an increasingly important topic (Duflo, 2011). International organizations now emphasize gender-based economic development policies by explicitly focusing on employment issues[1] and these changes in female employment patterns have triggered a growing literature exploring the gender wage gap in developing countries.[2]

Women are also working more outside of agriculture. Worldwide, the share of women in non-agriculture paid employment went from 35 percent in 1990 to nearly 40 percent in 2009, despite the effect of the 2008 financial crises (United Nations, 2011). This creates a significant change in the balance between work and family related activities. If non-agricultural jobs provide less flexibility in terms of work hours and childcare and are conducted away from home then the presence of children could affect

---

[1] The third Millennium Development Goal seeks to increase the share of female employment in non-agricultural sectors of the economy. Also, recent flagship reports by the World Bank (2012) and USAID (2012) identify the persistent gender gap in earnings as a key element of their focus on women's economic empowerment. The United Nations has created *UN Women*, its new entity for gender equality and the empowerment of women in July 2010 (UN Women, 2011) with an explicit focus on the gender gap in wages among other gender disparities.

[2] For example, Appleton, Hoddinott and Krishna (1999) and Fafchamps et al (2009) have studied the wage gender gaps in Africa; Psacharopoulos and Tzannatos (1992) and more recently Ñopo and Hoyos (2010) covered several Latin American countries. Horton (1996) collects several studies for Asian countries. Tzannatos (1998) and Ñopo, Daza and Ramos (2011) studied the gender gap in wages for several countries in all regions.

their mothers' productivity leading to large earning differences between women with and without children --a phenomenon known as the *family penalty*. Despite its importance, few papers have explored this area of research[3]. The goal of this paper is to fill this gap by studying the family gap in over 20 developing countries and address whether the observed gap is causal and if so what drives it.

In high-income countries, even after controlling for differences in productive characteristics, researchers typically find a family penalty of 10-15 percent for mothers compared to their childless counterparts (Waldfogel, 1998). A priori, it is not clear if the family penalty should be larger or smaller in the developing world. On one hand the recent expansion of female employment might not have been matched with an increase in the provision of formal childcare options and this may hinder a woman's ability to balance work and home responsibility (Attanasio and Vera-Hernandez, 2004). Additionally, since markets are less competitive there is more scope for taste-based discrimination in developing countries. This could be amplified with the presence of social norms that could limit the scope of jobs allowed to mothers (Goldin, 1995; Schultz, 1989). These characteristics would tend to suggest a larger family gap in developing countries. On the other hand, much of the production in the developing countries is in the informal sector which can be conducted from or close to home (World Bank, 2011). Additionally, multi-generational households are more common, which should ease the dual responsibility of wage earning and domestic work (Duflo, 2003 and Hamoudi and

[3] In a meta-analysis of the international gender gap, Weichselbaumer and Winter-Ebmer (2003) find that unexplained log wage differential is higher for studies that focus only on married women and significantly lower for studies that focus on single women. When they focus on the subsample of gender gap studies conducted in developing and former communist countries they find those studies, which did not include the presence of children as a control variable, see significantly larger unexplained gender wage gaps than those that do. In order words, the differential return to children can account for much of the gender gap in developing countries.

Thomas, 2011). Finally, if children are more likely to drop out of school at younger ages the family penalty might be smaller as older children, especially daughters, will be home to assist with domestic responsibilities.

The existing evidence about the size and causes of the motherhood penalties in low-income countries is mixed and scarce. Most papers focus on one single country and do not address the endogenously of having children[4]. Thus, our paper advances this literature in two key ways.

First, standardized and reliable earnings data are difficult to obtain across a large number of developing countries. We compiled and cross-validated a dataset on earnings for almost 130,000 women from 21 developing countries included in the Demographic and Health Surveys (DHS). To the best of our knowledge, this is the most comprehensive investigation of the impact of children on women's earnings in the developing world. Using the DHS, we document a sizable family penalty. The average woman in our sample earns $2.70 a day (in constant dollars of 2006). Each additional child is associated with a 48 cents decrease in daily earnings, which falls to 16 cents after conditioning on location, age, education and marital status. Since the average mother has 2.75 children,

[4] Piras and Ripani (2005) consider a sample of salaried women in Latin America (Bolivia, Ecuador, Peru and Brazil) between the ages of 14 and 45 who reside in urban areas. Their results are mixed. In some countries they find no relationship between children and log earnings, for Peru they find the expected negative relationship, in Brazil, mothers appear to earn more than non-mothers. Adair et al. (2002) find evidence of a negative impact of childbearing on women's cash earnings and work hours in the Philippines. Orbeta (2005) estimates the impact of children on labor force participation and earnings of parents in the Philippines. This paper finds a negative impact of children on women's labor force participation rates for the bottom three-income quintiles and a positive impact for the top two quintiles; in case of earnings, additional children have a regressive effect on women's earnings for the bottom two-income quintiles and a positive effect for the upper three quintiles. Olarte and Pena (2010) use Colombian data to find a wage gap of around nine percent and 18 percent for mothers with children aged five or less. Explaining the mechanism for this gap, they also show that motherhood increases the probability of being employed in the informal market but only for those who have young children.

our documented family penalty of six percent per child is similar in magnitude to estimates from more developed countries.[5]

Second, the relationship between family size and women's earnings is biased due to the endogeneity of family size. The negative relationship between family size and earnings may simply be a byproduct of unobserved heterogeneity. For instance, more able women may have fewer children and these women in turn have higher earning potentials. As reviewed by Schultz (2008), the related literature on the impact of family planning programs on women's outcomes is limited due to the lack of a credible identification strategy. Following Agüero and Marks (2008, 2011), in this paper, we focus on a source of variation in family size based on biological events. In particular, we use infertility/infecundity shocks as an instrument for family size to identify the causal effect of children on female earnings. We find no evidence that the OLS estimates are biased, and conclude that the negative relationship between fertility and mother's cash earnings is not being driven by negative selection into childbirth.

What accounts for the observed family penalty? A common explanation for the observed family penalty is that women typically face reproductive and productive roles that compete against each other. Children, especially very young children, imply greater constraints on women's productive economic activity; this results in less effort at work since mothers must devote time to child-rearing responsibilities and the reduced effort at work results in lowered earnings (Becker 1985). As children grow older the time their mothers allocate to child-rearing decreases, which in turn allows for a better balance between work and family responsibilities. We find support for this explanation. The

[5] Waldfogel (1997) finds, in the US, a wage penalty of 6 percent for mothers with one child and 13 percent for mothers with two or more children. Budig and England (2001) use more recent US data to find a wage penalty of 7 percent per child.

family penalty is largest when a woman's children are young and declines with children's age. Furthermore, a large development literature documents that the difference in gender roles becomes more pronounced as children get older with daughters, but not sons, contributing to household and child-rearing tasks as they age. We find support for this as well, especially for the low-educated subsample (i.e., women with complete primary education or less). There is a significant gender difference in the impact of older children on their mother's earnings with older daughters but not sons having a positive effect on their mother's earnings.

For all women, we show that mothers are less likely to work in the wage earning formal sector of the economy, mothers work less intensively, and sort into occupations where it is easier to balance the demands of work and family. We are able to explain the entire family gap for low-educated women and close to 2/3 of the gap for the higher educated. We conclude by discussing the possible explanations for the remaining family penalty in the high-educated sample.

2. Data and econometric model

A. Data and Sample Construction

In this paper we use cross-sectional data from the third round of the Demographic and Health Surveys collected between 1994 and 1999. The DHS are standardized nationally representative household surveys in developing countries. Women between the ages of 15 and 49 answered questions including information about their employment status, occupation, birth history, fertility preference and their socio-economic and marital status.

Relevant to our study, some DHS questionnaires contain a unique set of additional standardized questions in the employment module that collect detailed information on women's earnings and intensity of work (e.g., months per year, days per week, days in the last year). All respondents were asked if they are currently working or worked in the past 12 months. Respondents are also asked if they were paid in cash for their employment. Those who answer in the affirmative to both questions were asked the frequency of pay (hourly, daily, weekly, biweekly, monthly or annually) and their corresponding earnings. Thus, we have information on earnings of respondents who were currently working for cash at the time of the survey as well as earnings of those who were not currently working but who had worked for cash within the last twelve months.[6] We use the information about intensity of work and frequency of pay to impute daily earnings for all women in the sample based on their responses regarding the number of days worked in the last 12 months.[7] Earnings are expressed in constant 2006 US dollars by transforming them from local currency into US dollars (using the nominal exchange rates available from the International Financial Statistics of the International Monetary Fund) and then brought to 2006 prices using the US Consumer Price Index (from the Financial

[6] 91 percent of the women we classify as working are not currently working but reported having worked sometime in the 12 months prior to survey. Given the ambiguity in the timing of work for those who only report working in the last 12 months there is the potential for measurement error in the family size variable (since the family size information is recorded at the time of the survey). We have estimated all models where we only assign positive wages to those women that are currently working. The estimates mirror the findings of our main analysis. For instance, we find that each additional child reduces earnings by 14.6 cents whereas the main analysis suggests that each child reduces earning by 15.5 cents.

[7] We restrict our sample to observations that provide consistent and sufficient information about the number of days worked. Specifically, we exclude the few women who reported cash earnings but provided incomplete information about the number of days worked in the last 12 months. The DHS provides a daily wage variable but it was unreliable for some countries thus we constructed our own daily wage variable. When not reported, to compute daily earnings we assumed workers who reported hourly pay worked 8 hours a day, weekly pay worked 5.6 days a week (the sample average), monthly pay worked 30/7 weeks a month, and annual pay worked 250 days a year.

Statistics of the Federal Reserve Board).[8] To minimize measurement error, we also drop outliers whose real daily earnings belong to the lowest or highest percentile of the distribution.

We check for the validity of our standardized real daily earnings variable by comparing it to an alternative dataset on women's estimated earnings obtained from the World Economic Forum's Global Gender Gap Report 2008. The Report records, amongst other socio-economic indicators, estimated annual earned income of women in 2007 PPP US dollars for the countries included in our sample (with the exception of Central African Republic and Comoros).[9] We impute daily earnings of women for these countries assuming 250 working days a year for a comparable measure of real daily earnings. We then compare these earnings to our DHS measures of real daily earnings for female wage earners and find a highly positive correlation coefficient between data from the two sources. Appendix A presents, for each country, the average daily earnings from the DHS and the Global Gender Gap Report in the form of a scatter plot. From the pattern on the graph and the correlation coefficient of 0.82 (0.90 when Jordan is excluded) [10], we can conclude that the earnings data in the DHS surveys as well as our construction of the measure of real daily earnings are reliable for the purpose of our analysis.

[8] The cross-sectional nature of our survey implies that including country fixed effect variables in the regressions would control for PPP transformation since all earnings from the same country will be scaled up or down with PPP.

[9] The earnings figures reported in The Global Gender Gap Report 2008 are sourced by UNDP's *Human Development Report 2007/2008*. According to UNDP, because of the lack of gender-disaggregated income data, female and male earned income figures are crudely estimated on the basis of the data on the ratio of the female non-agricultural wage to the male non-agricultural wage, the female and male shares of the economically active population, the total female and male population and the GDP per capita in PPP US$ (Global Gender Gap Report 2008).

[10] All the results are robust to the exclusion of Jordan from the sample.

In addition to having detailed information on earnings, to be included in our analysis, a survey had to meet the following criteria: (1) to address endogeneity concerns as will be explained later, the survey had to include the questions that were used to identify infertile women and (2) the data had to be publicly available. Our final sample contains 22 surveys representing 21 countries.[11] Within this set of countries, we further exclude from the sample, women who are currently enrolled in school and women with missing labor force information. To be consistent with the literature, we restrict the sample to women between 20 and 44 at the time of the survey and exclude mothers with children over the age of 18 from the sample.[12] Daily earnings are set to zero for women who did not work for cash during the year, to avoid concerns regarding selection into working for cash. This approach is employed by other studies that use instrumental variables to correct for the endogeneity of family size (e.g., Angrist and Evans, 1998; Bronas and Grogger, 1994; and Jacobsen et al., 1999).[13]

Appendix B contains additional information about the 21 countries included in our sample. Our sample includes countries at various stages of economic development and wide variation in fertility rates and women's participation in the paid labor force and

[11] We omit Zimbabwe from our sample because of implausibly high real daily earnings.

[12] We have included women who are currently pregnant in the analysis. When we exclude pregnant women, who constitute 10% of the sample, from the analysis we continue to find that each additional child reduces mother's daily earnings by 16 cents.

[13] The alternative approach is to limit the sample to wage earners and use log wages as the dependent variable. This approach has been shown to underestimate the impact of children on earnings since mothers for whom children have the most detrimental impact on earnings are most likely to leave the labor market (Davies and Pierre, 2005 and Harkness and Waldfogel, 2003). When we estimate a model that conditions on age and country with log wage as the dependent variable, we find that each additional child is associated with a 10 percent reduction in earnings; this is similar to estimates of this nature in the existing literature that uses log wages. Additionally, Agüero and Marks (2011) document a small negative impact of motherhood on participation in the paid labor force in the developing world.

daily earnings. Note that the two countries with the lowest fertility rate (South Africa and Colombia) each have average daily earnings that are well above the sample average.

Our main sample contains 129,538 women. Summary statistics for our sample are shown in Table 1. The average woman in our sample is 30 years old with 2.2 children and one child under the age of six. 58 percent of our sample participated in the labor force last year and 44 percent of the women in our sample worked for pay. The average daily earnings for the women in our sample is \$2.70. Conditional on being in the paid labor force, the average women earned \$6.10 a day.

Eighteen percent of our sample is childless. Columns (2) and (3) contain summary statistics for non-mothers and mothers, respectively. A comparison of these columns suggests a family penalty. The average mother earns \$2.37 while her childless counterpart earns \$4.12 daily. However, non-mothers are more educated, far more likely to be unmarried, more likely to work year-round and more likely to reside in urban areas. There is also a difference in the occupational distribution between mothers and non-mothers. Mothers are over-represented in agricultural work and under-represented in office work (professional, managerial and clerical positions). In addition, mothers are less likely to work and more likely to be paid in-kind if they work. Conditional on being in the paid labor force, the average non-mother in our sample earns \$7.42 whereas the average mother earns only \$5.70 a day.

Columns (3) and (4) compare all mothers to mothers with young children. The demographic characteristics between these groups are very similar but mothers with young children earn significantly less (\$2.01 vs. \$2.37) and they are slightly more likely to work in agricultural jobs and less likely to have an office job. In addition, mothers with young children are less likely to work year-around.

Further evidence of a family gap is presented in Figure 1. The solid line represents average daily earnings for women with children of any age. Earnings clearly fall with family size. The average woman with one child earns \$2.85 whereas her counterpart with five children earns only \$1.30 a day. For women with young children, represented by the dashed line, the effects are more pronounced. Each additional young child (under the age of six) is associated with a sizable decrease in daily earnings. The first young child is associated with a \$1.17 decrease in earnings from a base of \$3.55, while the second young child is associated with an additional 93 cents decrease in daily earnings. Additionally, the average woman with younger children earns less than her counterpart with older children regardless of the number of children.[14]

B. Econometric model

For the sample described above, our general specification is given by equation (1):

$$\text{Earnings}_{ij} = \alpha_j + \Sigma_s \gamma_s \text{Age}_{ijs} + \beta K_{ij} + X_{ij}'\delta + e_{ij} \qquad (1)$$

[14] Note that non-cash earning women are included with earnings of zero. If we restrict Figure 1 to women who work for cash, a very similar pattern emerges. Earnings again fall with family size. For instance the average childless wage-earning woman earns \$6.85 a day whereas her working counterpart with 1 child earns \$6.26 and a mother with 4 children earns only \$4.24 a day. Each additional young child is also associated with a sizeable decrease in earnings. The first young child is associated with a \$1.20 decrease in earnings for wage-earning mothers from a base of \$6.82, while the second child is associated with an additional \$1.45 decrease in daily earnings.

where $Earnings_{ij}$ is daily earning for person i in country j in 2006 US dollars as described above. The key variable K_{ij} indicates the number of children living at home.[15] Thus, β is the parameter of interest. The most parsimonious specification (Model 1) includes only dummies for every single age in years denoted by $s=\{20,\ldots,44\}$ and country fixed effects (α_j) as they are clearly exogenous variables. Model 1, thus, assumes that vector $\delta=0$ in equation (1). The inclusion of country fixed effects allows us to control for the possibility that unobserved country-level characteristics are jointly correlated with the number of children a woman has and her earnings. For example, these unobservables could include differences in social norms or in attitudes toward women's employment outside the home across countries. Furthermore, while the all surveys employ a standardized questionnaire, minor changes in the survey design could also affect our results. Country fixed effects permit us to control for this possibility as well. All specifications, include robust standard errors clustered at sub-national levels (for example, departments, provinces, or districts depending on the country) and sample weights**.**

Model 2 expands Model 1 by including a set of variables commonly added in the literature studying the family penalty (vector X_{ij}). These are variables that may influence female earning potential and family size such as education, marital status, and four indicators for the size of the current place of residence. We lack data on work experience but include age times education interactions as a proxy for potential experience.

OLS estimates of β are likely to be biased due to unobserved variables in e_{ij}. The direction of bias is given by two elements: the relationship between the omitted variable and the outcome variable ($Earnings_{ij}$), and its relationship with the variable of interest

[15] As some countries have high child mortality rates, we re-estimate all the models in our paper using the number children ever born and obtain similar results.

(K_{ij}). We will use infertility to instrument for K_{ij} in equation (1) to address endogeneity concerns but first we present the OLS results in the next section.

3. Results

A. Documenting the Family Penalty

Table 2 presents the OLS estimates for equation (1). The estimates in column (1) are unconditional and suggest that each additional child is associated with a decrease in daily earnings of 47.7 cents. Column (2) adds age and country dummies as described in Model 1. The mothers in the sample are older than the non-mothers, thus, once we condition on age and country, the estimated coefficient increases to 55.8 cents. Given that the average women in our sample earns $2.70, this implies that each child is associated with a 21 percent reduction in earnings.

Clearly, education, marital status, and location in an urban area are important determinates of female earnings. As seen in column (3), these variables can account for much of the motherhood wage penalty; however, there is still a sizable negative relationship between children and earnings. Each child additional child is associated with a decrease in earnings 15.5 cents. For the average mother (with 2.75 children) this penalty translates into 43 cents (15.5 cents x 2.75) or about 16 percent of daily earnings ($0.427 / $2.70). This estimate is in line with the findings from developed countries. For instance, Harkness and Waldfogel (2003) show that the impact of three or more children on earnings varies from five percent to 30 percent for a sample of seven industrialized countries.

B. Selection into Motherhood

If women with high earnings potentials choose to have smaller (or no) families then naïve OLS estimates will be biased. Consider the case where female autonomy influences both family size and earnings. If autonomy correlates positively with the outcome variable (earnings) and negatively with the number of children; excluding this variable from equation (1) biases the OLS estimates downwards since part of the estimated effect of children on earnings can be attributed to lack of female autonomy.

Common approaches to address the bias induced by selection into motherhood include fixed effects (Anderson et al, 2003; Budig and England 2001; Korenman and Neumark, 1992; Lundberg and Rose; 2000; Waldfogel, 1997) and using plausibly exogenous variation in family size that arises from twinning (Bronars and Grogger, 1994; Jacobsen et al., 1999), or the sex composition of the first-born children (Angrist and Evans, 1998 and Cruces and Galiani, 2007). The limited studies in the developing world that focus on earnings and address the endogeneity of family size find mixed effects. Orbeta (2005) uses variation in family size caused by twinning in a sample of Philippine women and concludes that children are exogenous. Adair et al. (2002) also contains a sample of Philippine women. They use mother's fixed effects to account for selection and find a high level of endogeneity in models that estimate the effect of childbearing on women's earnings.

To address the selection into motherhood, we adopt the infertility/infecundity instrument first proposed in Agüero and Marks (2008) and recently used by Agüero and Marks (2011), Jensen (2012), Rondinelli and Zizza (2011) and Schott (2012). The key idea is that infecundity creates a "natural experiment" in which some women are biologically limited in the number of children they can have (either no children at all or no further children after parity n). The DHS datasets allow us to identify self-reported infertility in two ways. The first way is when women mentioned sub-fertility or infertility as their reason for not currently using contraceptives. The second way is when non-sterilized women responded as being unable to have more children when asked about their desire for future children. The infertility indicator is the union of these two measures. We will use infertility to instrument for K_{ij} in equation (1) to address the potential endogeneity concern.[16]

Of course, to serve as a valid instrument, infertility needs to be strongly correlated with family size. This is testable. In our sample, as we will show later in Table 3, infertile women have an average of 1.2 fewer children than their fertile counterparts. However, the main concern is about the exclusion restriction: infertility should only affect women's earning through its impact on family size. Any direct effect violates this assumption. The medical literature has shown that infertility increases with age (see Dunson, Baird, and Colombo, 2004, and Buck et al., 1997). Using data from the DHS we validate this claim. While the overall infertility rate is 4.7 percent, women between 20-24 years of age have a 1.3 percent rate while for those between 35-39 years the rate is 7.8. Finally, women between 40-44 years have a 16.3 percent infertility rate. Thus, in all our specifications we control for women's age.

However, the medical literature is not clear about what other variables affect infertility. Smoking and drinking tend to be included in the set of possible risk factors.

[16] See Agüero and Marks (2011) for more details. For example, the authors rule out the possibility that measurement errors (classical or not) in the self-reported infecundity variables might bias the results upwards. They also show that the presence of other household members does not affect the reporting of the infertility status when using DHS data.

Also, if lower socioeconomic background or poor health can explain infertility, then we could expect a direct effect of infertility on earnings as it will affect the accumulation of human capital (see Augood, Duckitt, and Templeton 1998; Grodstein, Goldman, and Cramer, 1994). Nonetheless, the vast majority of this evidence is dubious as it comes from couples (or women) recruited for prospective studies (Negro-Vilar, 1993). That is, couples experiencing fertility problems are recruited for a study and their observable characteristics are then correlated with their time to pregnancy. This type of study design has been shown to produce spurious associations (Juul, Keiding, and Tvede, 2000).

To investigate this issue further, we replicate the analysis done by Agüero and Marks (2011) and Jensen (2012) and test whether, in our sample, women's health and background characteristics are associated with their infertility status. In Appendix C we show that after conditioning on age, infertile women do not differ from their fertile counterparts along a wide set of characteristics such as age at first intercourse, month of birth, number of siblings, and birth order, among others. These results are consistent with the previous authors' findings that show that the onset of infertility is not associated with a large set of pre-determined characteristics. Jensen (2012), for example, finds that infertility is not associated with women's education or household expenditure. Infertility thus mimics an experiment in which nature assigns an upper bound for family size. The results of using infertility as an instrument for family size are shown in Table 3.

Column (1) from Panel A in Table 3 repeats the OLS estimate from Table 2, which suggests that each additional child decreases daily earnings by 15.5 cents. Column (2) contains the corresponding OLS estimate for the sub-sample for whom we can identify infertility.[17] The significant negative correlation between children and earnings is apparent in this subsample as well. For this slightly older subsample, each additional child is associated with an eleven cents decrease in daily earnings. Column (3) contains the estimates once we instrument for family size using our infertility indicator. As mentioned before, we have a very strong first stage; on average, infertility reduces family size by 1.2 children with a very high F-statistic. The 2SLS result suggests that the effect of children on earnings, using the variation in the number of children that comes through the infertility channel, is causal. The 2SLS estimate is larger in absolute value than the corresponding OLS, but one cannot reject the hypothesis that the two point estimates are the same (the reported Hausman test has a p-value of 0.297). Thus, there is no evidence of a downward bias in the OLS parameter and we conclude that selection into childbirth is not driving the negative relationship between fertility and mother's cash earnings.[18]

C. Child's Age and Gender

Next we explore the differential effect by age of the child on his mother's daily cash earnings. If the motherhood wage penalty reflects, in part, the effort associated with bearing and raising children, then the wage penalty should decline as the child ages (Becker, 1985). Thus, we modify equation (1) as follows:

$$\text{(2)} \qquad \text{Earnings}_{ij} = \alpha_j + \Sigma_s\gamma_s\text{Age}_{ijs} + \Sigma_a\beta_a\text{K}_{ija} + \text{X}_{ij}'\delta + e_{ij}$$

[17] We can only identify infertility for non-sterilized women who are not currently taking contraceptives. Thus we exclude from the sample sterilized women and those women who are currently taking contraceptives. Evidence presented in Agüero and Marks (2011) suggest that these sample restrictions do not dramatically alter the representative nature of our sample.

[18] The persistence of a motherhood wage penalty when unobserved heterogeneity is accounted for is consistent with the results from fixed effect models over long time horizons. Using US data, Budig and England (2001) and Waldfolgel (1997) estimate penalty with fixed effects that are almost identical to those estimated in a cross-section. Simonsen and Skipper (2012) compare the earning of twin sisters –one of whom is a mother- and finds no evidence of selection.

where now K_{ija} represents the number of children of age $a=\{0,1,\ldots,18\}$ the i-th woman in country j has. To be consistent with the models discussed above we use the full sample and restrict our focus to Model 2 (which includes controls for education, marital status, and current location in addition to controls for age and country).

In Figure 2 we show the estimated parameter β_a for a={0,18} as well as the 95 percent confidence intervals. The figure shows a clear pattern. Younger children have larger negative effects on their mother's earnings than older children. For example, having a one-year old child is associated with a reduction in earnings of 50 cents. This effect reduces by half if the woman has a child who is three years old. Furthermore, children aged 14 and above have no effect on their mothers' earnings.

These findings are confirmed by column (1) of Table 4. For ease of interpretation, we report the results of a regression where K_{ija} is aggregated by age groups as opposed to single ages. Following Anderson et al. (2003), we grouped children into developmental and schooling stages and formed the following age groups: under 3, 3 to 5, 6 to 10, 11 to 13 and 14 to 18. Clearly, infants have the largest negative impact on mother's earnings that lessens as the child ages. Each infant child reduces earnings by 39 cents while an additional toddler reduces earnings by 16 cents and an additional child of primary school-going age only reduces earnings around 14 cents. Children over the age of 13 have no impact on their mother's earnings. This relationship between child's age and the magnitude of the family penalty is consistent with the larger family gap literature. For instance Anderson et al. (2003), in a model with fixed effects and a rich set of control variables, finds that infants and toddlers impose the largest estimated wage penalty, but that the penalty persists as the child ages. Without fixed effects they find that older children (14-17 years old) do not impact wages. Jacobsen et al. (1999) find that the magnitude of the effect of a twin birth diminishes as the child ages and for one sample the impact on earned income is positive for the oldest age group (11 to 18).

Since we have only one infertility indicator, our instrumental variable approach described in section 3.B does not allow us to test the effect by separate age groups. However, we can explore whether the sizable negative impact of young children on mother's earnings is robust to endogeneity concerns. We redefine K_{ij} in equation (1) to be the number of children under the age of six. Columns (1) and (2) of Panel B in Table 3 show a sizeable negative impact of children under six on their mother's daily earnings for the full sample and the subsample for whom we can identify infertility, respectively. In column (3) we present the estimates once we instrument for the number of young children using infertility shocks. While the 2SLS estimate is less precisely estimated, it is significantly different from zero at the five percent level. This suggests that the observed negative effect is unlikely to be driven by unobserved heterogeneity. Panel C, in Table 3, considers even younger children (under three). Again, the OLS and 2SLS estimates show a negative effect of infants and toddlers on their mother's earnings. These results for young children confirm our earlier finding that the family penalty is unlikely to be driven by selection into motherhood. Comparing the estimates across the three panels further emphasizes that the penalty is larger for younger children compared to all children.

We now turn to the effects by gender (and age of the child). Equation (2) is modified by replacing $\Sigma_a\beta_a K_{ija}$ with $\Sigma_a\pi_a S_{ija} + \Sigma_a\theta_a D_{ija}$, where S_{ija} and D_{ija} refer to the number of sons and daughters of age a, respectively. A large body of the literature in developing countries documents that the difference in gender roles becomes more pronounced as children age, with daughters (but not sons) contributing to household tasks as they enter adolescence (see for example Ilahi, 2000; World Bank, 2001). Mueller (1984) examines time allocation and finds a newborn in the household increases the time his/her older sister would spend in nonmarket work but has no effect on the time allocation of an older brother. Pitt and Rosenzweig (1990) documents that 25 percent of girls aged 14 to 18 in households with infants report household care as their significant activity compared to only three percent for teenage boys. They also find that it is older daughters that care for their younger siblings when they take ill.

Column (2) of Table 4 shows the estimates of sons (π_a) while the estimates for daughters (θ_a) are shown in column (3). There are no differences in the effect of a child by gender when they are young (under six). We formally test this in column (4) where the p-value of the null hypothesis that coefficients for the number of sons and daughters is the same, by each age group, separately. For example, an additional son under the age of three is associated with a decline in earnings of 38.4 cents (column 2) and with 39.4 cents for an additional daughter (column 3) and we cannot reject the null hypothesis that these effects are the same (p-value=0.764).

However, the family penalty differs by the gender of the child, as children grow older. As seen in column (4) of Table 4, beyond the age of 10, there is a statistically significant difference in the impact of sons and daughters on daily earnings, with daughters having a more beneficial effect than sons. For children aged 11-13 years, earnings decline by 16.6 cents with an additional son but not with an additional daughter. Teenage sons have a neutral effect on their mother's earnings; while teenage daughters have a weakly positive effect on earnings.[19] This is consistent with older daughters (but not sons) substituting for their mother's time in time-intensive household and child rearing tasks.

D. Women's education and the family penalty

We next investigate if the magnitude of the family penalty and relationship by child age and gender varies with mother's education. If education impacts attitudes towards to the role of children in household production (by age or gender), or the investment in human capital of the next generation then we should expect the family penalty to differ across education levels. In Table 5 we test for this hypothesis by dividing the sample into women with "low" levels of education (i.e., complete primary education or less) and those with "high" levels (incomplete secondary or more) are re-estimate the family penalty by age and gender of the child for these two groups of women.

For young children the impacts are very similar across education levels. In both samples the family penalty is the largest for infants. For low-educated women, in column (1) of Panel A, an additional child under the age of three is associated with a reduction in 21 cents in earnings versus 77 cents for highly-educated women (Panel B, column 1). Note that in relative terms, these penalties correspond to, respectively, 17 and 16 percent of earnings for each additional child under three.

The differences emerge for older children. For highly-educated women the penalty persists for all age groups. Furthermore the magnitude of the penalty never varies by the gender of the child (Panel B, column 4). For the low-educated sample the penalty

[19] In Bolivia, Piras and Ripani (2005) find that having a girl between 13 and 18 has a positive effect on their mother's wages but boys in this age range have no impact.

disappears for children aged 11-13 and turns into a premium for teenage children. This premium is driven by the positive effect of older daughters on earnings. For low-educated women a teenage son (between 14 and 18) is associated with small increase in earnings (2 percent=0.24/1.22) that is not statistically different from zero (Panel A, column 2). However, as shown in column (3), having a teenage daughter provides a premium to her mother of 12 percent in earnings (=0.151/1.22). We can reject the null hypothesis that the effect of having a son the same as having a daughter for this age group. Thus, daughters bear a disproportionate burden of their mother's employment as they substitute for their low-educated mother's time at home.

These findings are consisting with the expected differential effects by women's education levels. Children of less-educated mothers tend to be more likely to drop out from school as teenagers and can therefore assist with household production (e.g. Behrman, Gaviria and Székely, 2001; Behrman et al, 2009; Agüero and Ramachandran, 2012). Additionally, the results suggest that gender biases in the time allocation of children in household production weaken with mothers' education.

E. The role of work intensity, occupation, and migration

Having established that a sizable family penalty exists, especially for very young children, in this section we investigate the underlying mechanisms that generate the family penalty by level of mother's education. Table 6 adds several controls to equation (2) to determine how much of the overall family gap can be attributed to differences in work type and intensity broadly defined. It is possible that mothers earn less than non-mothers because they sort into different types of work. As discussed by Goldin (1993) and Mammen and Paxson (2000), women in developing countries may regard informal sector work or non-participation in the labor force as more compatible with child rearing. In contrast, work in the wage-earning formal sector offers greater stability, and higher earnings, but generally longer hours and a work location typically away from home (Anker and Hein, 1986).

In Panel A of Table 6, column (1) replicates the results from Panel A of Table 5 - column (1) for comparison. Columns (2) and (3) of Table 6 investigate the role the type of job in explaining the family penalty for low-educated women in developing nations. In column (2), we add to the regression model a variable that denotes whether the woman works in a job for which she earns cash as opposed to not working or being paid in-kind. Non-cash-earning work is quite common: about 37 percent of the working mothers in our low-educated subsample report they are not paid in cash for their work. On the other hand, only 30 percent of working non-mothers report that they are not paid in cash for their work. As shown in the comparisons of columns (1) and (2), a significant part of the family penalty can be attributed to the fact that low-skilled mothers, especially those with very young children, sort out of the “formal” labor market. The difference in the likelihood of cash-earning employment alone can account for more than two thirds of the penalty for mothers with infant children and about half of the penalty for mothers of toddlers and primary school-aged children. Therefore, all the remaining columns in Table 6 will include an indicator denoting whether a woman was paid in cash or not, thus the relevant comparison is column (2).

Mothers may also sort into "mother-friendly" occupations. The theory of compensating wage differentials predicts that the features of these occupations that make them easier to combine with motherhood will in turn result in lower earnings (Anderson et al, 2003; Budig and England, 2001). For example, mothers may choose occupations that are located at or near the home (such as agriculture), that require less energy or that have parent-friendly characteristics, such as flexible hours and few demands for travel. In addition to the indicator for a cash-earning job, Column (3) adds 275 detailed occupational controls.[20] Mothers are over-represented in self-employed agriculture and under-represented in professional, technical, and managerial occupations as well as clerical and service occupations. Differences in occupations between low-skilled mothers and low-skilled non-mothers can account for part of the family penalty. The estimated coefficients in column (3) are about 25 percent smaller than those in column (2). For instance, after conditioning on occupation type, each additional toddler is associated with a marginally significant three cents decrease in daily earnings versus a five cents decrease without the occupation controls.

Column (4) includes the only measure of work intensity available in our sample: seasonality. The survey asks respondents if they work year-round, seasonally, or occasionally. Mothers are less likely to work year-round than non-mothers. Differences in work intensity account for a portion of the family penalty for low-skilled mothers with young children.

Finally we investigate if differences in migration can account for any of the family penalty. In our sample, migration is defined as a dummy that takes the value of one if a women's current place of residence is larger than her residence at birth. Women without children may be more mobile and more likely to move to urban areas for work. The estimated coefficients, shown in column (5), suggest that differential migration

[20] We have also included broader occupational categories and the overall results are very similar.

between low-educated mothers and non-mothers is not generating the family penalty. The point estimates are almost identical to the estimates in column (2), which do not include a migration control.

The final column in Table 6, Panel A, includes all of the above controls. We are able to explain the entire family penalty for low-skilled women. The effect of children between 3 to 13 are not statistically different from zero suggesting that low-skilled mothers with young children earn less because they are less likely to work in the formal market; work in different types of jobs and are less likely to work year-round. The remaining family gap -a family penalty for infants and a family premium for the oldest children- is consistent with the effort hypothesis. Very young children are time intensive while adolescent children can substitute for their mothers' time in household production. In results not shown, we use the number of children (instead of the number by child's age) as the variable of interest. After conditioning on the full set of controls, each additional child is no longer associated with a decrease in daily earnings, in stark contrast to the statistically significant five cents per child estimated without the job type and work intensity controls.

In Table 6, Panel B, we repeat this exercise for the subsample of women with at least some secondary education. As shown in the comparisons of columns (1) and (2) in Panel B, the difference in the likelihood of cash-earning work alone can account for more than two thirds of the penalty for mothers with infant children and about forty percent of the penalty for mothers of toddlers and primary school-aged children. However, even after conditioning on participation in the formal wage-paying labor market a sizable family penalty exists.

Like their low-skilled counterparts, occupation sorting can account for a significant share of the remaining family gap. The inclusion of detailed occupational controls (comparing column 3 to column 2) can explain around one-third of the remaining family gap. The exception is high-educated mothers with teenage children, occupational sorting can account for the entirety of the remaining family gap for high-skilled mothers with teenage children. Occupational sorting can explain a larger share of the family gap for high-skilled women than their low-skilled counterparts. This is not surprising as more than half of the working low-education women work in agriculture (the vast majority are self-employed) vs. about seven percent of the highly educated women. For highly educated women sales and professional/technical/management are the two largest occupations consisting of 53 percent of the workforce. We lack information on part-time work but would posit that the inclusion of such a variable would also account for part of the family penalty.

Column (4) includes our measure of work intensity. Differences in work intensity between mothers and non-mothers can account for little of the remaining family penalty for highly skilled mothers. For this sample, 80 percent of both mothers and non-mothers report working year-round. This is in contrast to the less educated subsample where 60 percent of mothers report working year-round while 2/3 of non-mother report working year round.

As shown in the final column of Table 6, Panel B, after we condition on working for cash, detailed occupation, seasonality and migration we are able to explain more than sixty percent of the family gap. In results not shown, that use the number of children as the dependent variable (as in equation 1), after conditioning on everything, each additional child is associated with a 16 cent decrease in daily earnings, in contrast to the 46 cents per child estimate without the job type and work intensity controls. However, a sizable family gap persists for high-skilled mothers. This magnitude of the family gap does not decrease with child age, which suggests that work effort alone cannot explain the family gap. It appears that many of the countries in our sample lack formal child care arrangements, do not provide transportation to and from school and have not adopted policy such as paid leave to care for sick children as such the challenges of balancing work and family may persist even for older children.

4. Conclusion

This paper provides the first global assessment of the family gap. We compile a unique collection of standardized household surveys with almost 130,000 women from 21 developing countries and document a sizable family penalty in women's earnings. The unconditional penalty indicates that women with children earn 48 cents less per day for each additional child compared to their childless counterparts. This corresponds to a 22 percent penalty for the average woman reporting positive earnings. The penalty reduces to 7 percent per child when conditioning on age, education, marital status, and size of the current location. An additional key contribution of this paper is to show that this wage penalty is indeed causal. Using infertility shocks as instruments for the number of children, we find no difference between the OLS and the 2SLS estimates.

We argue that the observed family penalty is driven by mothers being underrepresented in paid work, overrepresented in low-paying occupations and working at a lower intensity than their childless counterparts. Accounting for this sorting across types of work and occupation explains nearly 75 percent of the remaining family penalty. Whether this sorting is an optimal decision of women with children, the result of workplace policies (or conditions) that are incompatible with childbearing or the effect of discrimination in the labor market is still an open question.

The richness of our data allows us to show that the motherhood wage penalty is larger for younger children and is especially large for infants. These findings are confirmed in the 2SLS estimates. Furthermore, for the low-educated subsample only, our results suggest that gender matters when children become adolescents: older daughters have positive impact on their mother's earnings as opposed to the negative to neutral effect of older boys. This is consistent with a gender division in household tasks with older daughters, but not sons, assisting their mothers in time intensive household tasks. After accounting for sorting across occupations and sectors of the economy the remaining gap for low-educated women can be explained by effort in household production. This is not the case for the more educated subsample.

Why do we see a persisting family gap for school-age children for educated mothers and not for low-educated mothers? Poorly educated women can combine work and family responsibilities relatively easily because most work is conducted from or near home. Women with more education are far more likely to participate in the formal labor market and away from home. However, most developing countries lack institutions such as formal childcare, family medical leave and school buses, which facilitates the balance of work and family. As more women participate in the formal labor market demand for such institutions is likely to grow and the family gap for school age children should reduce. The evidence from the United States, with a higher share of highly educated women, shows that the family gap for school age is much smaller than that for younger children after conditioning on a full set of covariates. This highlights the need to focus on weakening the institutional constraints, which limit the balance between work and family, especially for women in the formal sector of the economy.

Given the dramatic decline in fertility in the developing world, our finding of a sizable family penalty highlights an additional channel of the demographic dividend. In particular, smaller family sizes translate into higher earnings and increased empowerment for women. According to our preferred estimate, the decline in the total fertility rate in the less developed regions of the world from 5.41 in the early seventies to the 2.75 in 2005-2010 (United Nations, World Population Prospects) translates to a gain of 41.2 cents per day or an increase of 15.3 percent relative to the mean daily earnings. Further gains in women's economic well-being are expected as the fertility rate in developing countries converges to the rate of developed countries.

5. References

Adair, Linda, David Guilkey, Eilene Bisgrove, and Socorro Gultiano. 2002. "Effect of childbearing on Filipino women's work hours and earnings" *Journal of Population Economics* 15:625-645.

Agüero, Jorge M. and Mindy Marks. 2008. "Motherhood and Female Labor Force Participation: Evidence from Infertility Shocks" *American Economic Review*, 98(2): 500-504.

Agüero, Jorge M. and Mindy Marks. 2011. "Motherhood and Female Labor Supply in the Developing World: Evidence from Infertility Shocks" *Journal of Human Resources,* 46(4): 800-826.

Agüero, Jorge M. and Maithili Ramachandran. 2012. "The Intergenerational Transmission of Education. Evidence from Zimbabwe." *Unpublished Manuscript*.

Anderson, Deborah, Melissa Binder and Kate Krause. 2003. "The Motherhood Wage Penalty Revisited: Experience, Heterogeneity, Work Effort and Work-Schedule Flexibility" *Industrial and Labor Relations Review*, 56(2): 273-294.

Angrist, Joshua and William Evans. 1998. "Children and their Parents' Labor Supply: Evidence from Exogenous Variation in Family Size" *American Economic Review,* 88(3): 450–577.

Anker, Richard and Catherine Hein. 1986. *Sex Inequalities in Urban Employment in the Third World*. London: MacMillan Press.

Appleton, Simon; John Hoddinott and Pramila Krishnan. 1999. "The Gender Wage Gap in Three African Countries, *Economic Development and Cultural Change*, 47(2), pages 289-312, January.

Ashraf, Nava; Karlan, Dean and Yin, Wesley, 2010. "Female Empowerment: Impact of a Commitment Savings Product in the Philippines," *World Development*, 38(3): 333-344, March.

Attanasio, Orazio, and Marcos Vera-Hernandez. 2004. "Medium- and Long-Run Effects of Nutrition and Child Care: Evaluation of a Community Nursery Programme in Rural Colombia." Working Paper Series, Institute for Fiscal Studies, London.

Augood, C., K. Duckitt, and A.A. Templeton. 1998. "Smoking and Female Infertility: a Systematic Review and Meta-analysis." *Human Reproduction* 13(6): 1532-1539.

Becker, Gary S, 1985. "Human Capital, Effort, and the Sexual Division of Labor," *Journal of Labor Economics*, 3(1): S33-58, January.

Behrman, Jere R.; Murphy, Alexis; Quisumbing, Agnes R. and Yount, Kathryn, 2009. "Are returns to mothers' human capital realized in the next generation?: The impact of mothers' intellectual human capital and long-run nutritional status on children's human capital in Guatemala," IFPRI discussion papers 850.

Behrman, Jere; Alejandro Gaviria and Miguel Székely, 2001. "Intergenerational Mobility in Latin America," *Economia*.

Bronas, Stephen and Jeff Grogger. 1994. "The Economic Consequences of Unwed Motherhood: Using Twin Births as a Natural Experiment" *American Economic Review,* 84(5): 1141-1156.

Buck, Germaine M., Lowell E. Sever, Ronald E. Batt, and Pauline Mendola. 1997. "Life-Style Factors and Female Infertility." *Epidemiology* 8(4): 435-441.

Budig, Paula and Michelle England. 2001. "The Wage Penalty for Motherhood" *American Sociological Review*, 66(2): 204-225.

Cochrane, Susan, Valerie Kozel, and Harold Alderman. 1990. "Household Consequences of High Fertility in Pakistan", *World Bank Discussion Papers*, 111, World Bank.

Cruces, Guillermo and Sebastian Galiani. 2007. "Fertility and female labor supply in Latin America: New causal evidence," *Labour Economics*, 14(3): 565-573, June.

Davies, Rhys and Gaelle Pierre. 2005. "The family gap in pay in Europe: A cross-country study" *Labour Economics,* 12, 469–486.

Duflo, Esther, 2003. "Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa," *World Bank Economic Review*, 17(1): 1-25, June.

Duflo, Esther, 2011. "Women's Empowerment and Economic Development" NBER Working Papers 17702, National Bureau of Economic Research.

Dunson, David B., Donna D. Baird, and Bernado Colombo. 2004. "Increased Infertility with Age in Men and Women." *Obstetrics and Gynecology* 103(1): 51-56.

Grodstein, Francine, Marlene B. Goldman, and Daniel W. Cramer. 1994. "Body Mass Index and Ovulatory Infertility." *Epidemiology* 5(2): 247-250.

Hamoudi, Amar and Duncan Thomas 2011. "Endogenous Coresidence and Program Incidence: Reexamining South Africa's old age pension." Mimeo.

Harkness, Susan and Jane Waldfogel 2003. "The Family Gap in Pay: Evidence from Seven Industrialized Countries", *Research in Labor Economics*, 22, 369-414.

Hausmann, Ricardo, Laura D. Tyson, and Saadia Zahidi (2008) *Global Gender Gap Report 2008*, World Economic Forum, Geneva, Switzerland, 2008.

Horton, S. ed. 1996. *Women and Industrialization in Asia*. London and New York: Routledge.

Ilahi, Nadeem. 2000. The Intra-household Allocation of Time and Tasks: What have we learnt from the empirical literature? Washington, D.C.: World Bank Policy Research Report on Gender and Development Working Paper Series No. 13.

International Labor Office. 2010. *Women in Labour Markets: Measuring Progress and Identifying Challenges*, March 2010, Geneva.

Jacobsen, Joyce, James Pearce III, and Joshua Rosenbloom. 1999. "The Effects of Childbearing on Married Women's Labor Supply and Earnings: Using Twin Births as a Natural Experiment" *The Journal of Human Resources*, 34(3): 449-474.

Jensen, Robert. 2012. “Another Mouth to Feed? The Effects of Fertility on Girls' Malnutrition” forthcoming, CESifo Economic Studies

Juul, Svend, Niels Keiding, and Mads Tvede. 2000. “Retrospectively Sampled Time-to-Pregnancy Data May Make Age-Decreasing Fecundity Look Increasing.” *Epidemiology* 11(6): 717-719.

Korenman, Sanders and David Neumark. 1992. “Marriage, Motherhood and Wages.” *Journal of Human Resources,* (27):233-255.

Lundberg Shelly and Elaina Rose. 2000. “Parenthood and the Earnings of Married Men and Women” *Labour Economics,* 7(6): 689-710.

Marcel Fafchamps & Mans Söderbom and Najy Benhassine, 2009. "Wage Gaps and Job Sorting in African Manufacturing," *Journal of African Economies*, vol. 18(5), pages 824-868.

Mueller, Eva. 1984. 'The value and allocation of time in rural Botswana," *Journal of Development Economics*, 15: 329-360.

Negro-Vilar, Andrés. 1993. “Stress and Other Environment Factors Affecting Fertility in Men and Women: Overview.” Environmental Health Perspectives 101(Supplement 2: Impact of the Environment on Reproductive Health): 59–64.

Ñopo, Hugo and Hoyos, Alejandro, 2010. "Evolution of Gender Wage Gaps in Latin America at the Turn of the Twentieth Century: An Addendum to New Century, Old Disparities" IZA Discussion Papers 5086, Institute for the Study of Labor (IZA).

Ñopo, Hugo; Daza, Nancy and Ramos, Johanna, 2011. "Gender Earnings Gaps in the World," IZA Discussion Papers 5736, Institute for the Study of Labor (IZA). ← Table 3A

Olarte, Laura and Ximena Pena. 2005. “*The Effect of Motherhood on Wages in Colombia*” Documento CEDE No. 2010-18.

Orbeta, Aniceto C. 2005. “Children and the Labor Force Participation and Earnings of Parents in the Philippines” *Philippine Journal of Development*, 32(1): 19-52

Piras, Claudia and Laura Ripani. 2005. “The Effects of Motherhood on Wages and Labor Force Paricipation: Evidence from Bolivia, Brazil, Ecuador and Peru” *Sustainable Development Department Technical Paper Series* WID-109

Pitt, Mark M., and Mark R. Rosenzweig. 1990. “Estimating the Intrahousehold Incidence of Illness: Child Health and Gender-Inequality in the Allocation of Time.” *International Economic Review*, 31(4): 969–80.

Ponczek, Vladimir, 2011. "Income and bargaining effects on education and health in Brazil," *Journal of Development Economics*, 94(2): 242-253, March.

Psacharopoulos, G. and Z. Tzannatos, eds., *Women’s Employment and Pay in Latin America; Overview and Methodology* (Washington, D.C.: World Bank, 1992).

Rondinelli, Concetta and Roberta Zizza. 2011. “(Non) persistent effects of fertility on female labour supply,” ISER Working Paper Series 2011-04, Institute for Social and Economic Research.

Schott, Whitney. 2012. “Does Motherhood Mean Less Livelihood? An Instrumental Variables Approach” Unpublished manuscript.

Schultz, Theodore P. 2008. “Population Policies, Fertility, Women's Human Capital, and Child Quality.” In Handbook of Development Economics, Volume Four, ed. T. Paul Schultz and John A. Strauss, 3249-3303. Amsterdam: Elsevier Science B.V.

Simonsen, Marianne and Lars Skipper, 2009. “The Family Gap Reconsidered: What Wombmates Reveal,” *Labour*, (19):102-112.

Tiefenthaler, Jill. 1997. “Fertility and Family Time Allocation in the Philippines” *Population and Development Review*, 23(2): 377-397.

Thomas, Duncan. 1990. "Intra-Household Resource Allocation: An Inferential Approach," *Journal of Human Resources*, 25(4): 635-664.

Tzannatos, Zafiris, 1998 “Women and labor market changes in the global economy: growth helps, inequalities hurt, and public policy matters,” Social Protection Discussion Papers 20051, The World Bank.

UN Women. 2011. *Annual Report 2010-2011*, New York, http://www.unwomen.org/wp-content/uploads/2011/06/UNwomen_AnnualReport_2010-2011_en.pdf. . Accessed on March 15, 2012.

United Nations, Department of Economics and Social Affairs, Population Division. 2007. *World Population Prospects: The 2006 Revision, Highlights*, Working Paper No. ESA/P/WP.202.

USAID. 2012. *Gender Equality and Female Empowerment*, USAID Policy, Washington DC, March. http://www.usaid.gov/our_work/policy_planning_and_learning/documents/GenderEqualiGenderEq.pdf. Accessed on March 15, 2012.

Waldfogel, Jane 1997. "The Effect of Children on Women's Wages." *American Sociological Review*, (62):209-217.

Waldfogel, Jane. 1998. “Understanding the “Family Gap” in Pay for Women with Children” *Journal of Economic Perspective*, 12(1) 137-156.

World Bank. 2001. *Engendering Development: Through Gender Equality in Rights, Resources, and Voice.* World Bank Policy Research Report 21776. New York: Oxford University Press.

Table 1: Descriptive Statistics

Variable	Full Sample (1)	Non-mothers (2)	Mothers (3)	Mothers with kids under 6 (4)
Number of children	2.23	-	2.75	2.86
	(1.78)		(1.72)	(1.64)
Number of children under 6	1.02	-	1.25	1.59
	(0.95)		(0.92)	(0.70)
Number of children desired[a]	3.83	3.32	3.95	4.05
	(2.48)	(2.24)	(2.56)	(2.57)
Worked in the last 12 months	0.58	0.66	0.56	0.54
	(0.49)	(0.47)	(0.50)	(0.50)
Office Work[b]	0.16	0.24	0.14	0.12
	(0.37)	(0.43)	(0.35)	(0.33)
Sales[b]	0.26	0.23	0.27	0.27
	(0.44)	(0.42)	(0.44)	(0.45)
Agricultural[b]	0.32	0.20	0.35	0.39
	(0.47)	(0.40)	(0.48)	(0.49)
Worked for cash	0.44	0.56	0.42	0.39
	(0.50)	(0.50)	(0.49)	(0.49)
Daily earnings	2.70	4.12	2.37	2.01
	(6.12)	(7.63)	(5.67)	(5.12)
Conditional daily earnings[c]	6.10	7.42	5.70	5.18
	(8.00)	(7.71)	(6.89)	(6.51)
Worked year-round	0.40	0.49	0.38	0.35
	(0.49)	(0.50)	(0.48)	(0.48)
Age	29.89	27.51	30.44	29.13
	(6.42)	(6.66)	(6.24)	(5.86)
Above primary education	0.41	0.55	0.38	0.36
	(0.49)	(0.50)	(0.48)	(0.48)
Currently married[d]	0.85	0.48	0.93	0.94
	(0.36)	(0.50)	(0.25)	(0.23)
Urban	0.50	0.61	0.48	0.45
	(0.50)	(0.49)	(0.50)	(0.50)
Observations	129,538	23,425	106,113	84,063

Notes: Sample weights are used. Standard deviation are in parenthesis.
a. Non-numeric answers are excluded.
b. Occupations are for the working sample only. Office work includes professional, technical, managerial and clerical positions. Agricultural work includes both self-employed and contractual agricultural work.
c. Conditional daily earnings are restricted to the sample with positive earnings.
d. Egypt, Jordan, and Nepal restrict the sample to currently married women.

Table 2: Documenting the Family Penalty

Dependent variable: Daily earnings	Unconditional (1)	Model 1 (2)	Model 2 (3)
Number of children	-0.477	-0.558	-0.155
	[0.094]***	[0.088]***	[0.023]***
Observations	129,538	129,538	129,538
R-squared	0.02	0.13	0.25

Notes: Robust standard errors (in brackets) are clustered at the sub-national level. * denotes significance at 10 percent; ** at 5 percent and *** significance at 1 percent. Model 1 includes women's age and survey fixed effects. Model 2 adds to Model 1 education, age and education interactions, marital status, and four indicators for the size of current location.

Table 3: Number of Children and Daily Earnings of Women

Dependent variable: Daily earnings	OLS Full Sample (1)	OLS with 2SLS Sample (2)	2SLS (3)
Panel A. Number of children (all ages)			
Number of children	-0.155	-0.106	-0.206
	[0.023]***	[0.015]***	[0.098]**
Observations	129,538	82,221	82,221
R-squared	0.25	0.28	
First stage			-1.199
			[0.050]***
F-statistic (1st stage)			581.7
Hausman (p-value)			0.297
Panel B. Number of children under six			
Under six	-0.280	-0.213	-0.382
	[0.040]***	[0.028]***	[0.181]**
Observations	129,538	82,221	82,221
R-squared	0.25	0.28	
First stage			-0.646
			[0.026]***
F-statistic (1st stage)			599.4
Hausman (p-value)			0.339
Panel C. Number of children under three			
Under three	-0.404	-0.313	-0.713
	[0.056]***	[0.044]***	[0.338]**
Observations	129,538	82,221	82,221
R-squared	0.25	0.28	
First stage			-0.347
			[0.014]***
F-statistic (1st stage)			586.2
Hausman (p-value)			0.230

Notes: Robust standard errors (in brackets) are clustered at the sub-national level. * denotes significance at 10 percent; ** at 5 percent and *** significance at 1 percent. The 2SLS instrument for the number of children uses the union of the infertility measures. The F-statistic refers to the first stage results. The Hausman p-value refers to the test where the null hypothesis equals the efficient and the consistent estimators.

Table 4: Effects of Children by Age and Gender

Age of child	All children	By gender		P-value testing: (2)=(3)
		Sons	Daughters	
	(1)	(2)	(3)	(4)
Under 3	-0.389	-0.384	-0.394	0.764
	[0.056]***	[0.058]***	[0.059]***	
3 to 5	-0.158	-0.165	-0.151	0.674
	[0.035]***	[0.037]***	[0.042]***	
6 to 10	-0.143	-0.115	-0.173	0.155
	[0.026]***	[0.029]***	[0.036]***	
11 to 13	-0.095	-0.166	-0.021	0.010
	[0.035]***	[0.045]***	[0.045]	
14 to 18	0.022	-0.043	0.098	0.076
	[0.045]	[0.055]	[0.065]	
Observations	129,538	129,538		
R-squared	0.26	0.26		

Notes: Robust standard errors (in brackets) are clustered at the sub-national level. Significance at 10 percent denoted by *, ** significant at 5 percent and *** significant at 1 percent. The p-value corresponds to an F-test for the equality of parameters across gender by age group. All regressions includes women's age, education, interaction between age and education, survey fixed effects, marital status, and four indicators for the size of current location.

Table 5: Effects of Children by Women's Education

Age of child	All children (1)	By gender: Sons (2)	By gender: Daughters (3)	P-value testing: (2)=(3) (4)
Panel A. Low-educated women (complete primary or less)				
Average daily earnings: US$1.22				
Under 3	-0.210	-0.208	-0.212	0.876
	[0.028]***	[0.033]***	[0.030]***	
3 to 5	-0.093	-0.097	-0.090	0.804
	[0.024]***	[0.025]***	[0.031]***	
6 to 10	-0.030	-0.025	-0.037	0.663
	[0.016]*	[0.022]	[0.021]*	
11 to 13	-0.002	-0.051	0.049	0.023
	[0.021]	[0.029]*	[0.031]	
14 to 18	0.081	0.024	0.151	0.014
	[0.030]***	[0.037]	[0.042]***	
Observations	79,526	79,526		
R-squared	0.12	0.12		
Panel B. High-educated women (incomplete secondary or more)				
Average daily earnings: US$ 4.86				
Under 3	-0.767	-0.763	-0.770	0.934
	[0.134]***	[0.134]***	[0.144]***	
3 to 5	-0.375	-0.380	-0.366	0.885
	[0.095]***	[0.104]***	[0.109]***	
6 to 10	-0.489	-0.418	-0.562	0.199
	[0.078]***	[0.075]***	[0.114]***	
11 to 13	-0.392	-0.525	-0.257	0.102
	[0.107]***	[0.134]***	[0.136]*	
14 to 18	-0.243	-0.316	-0.166	0.476
	[0.128]*	[0.172]*	[0.159]	
Observations	50,012	50,012		
R-squared	0.23	0.23		

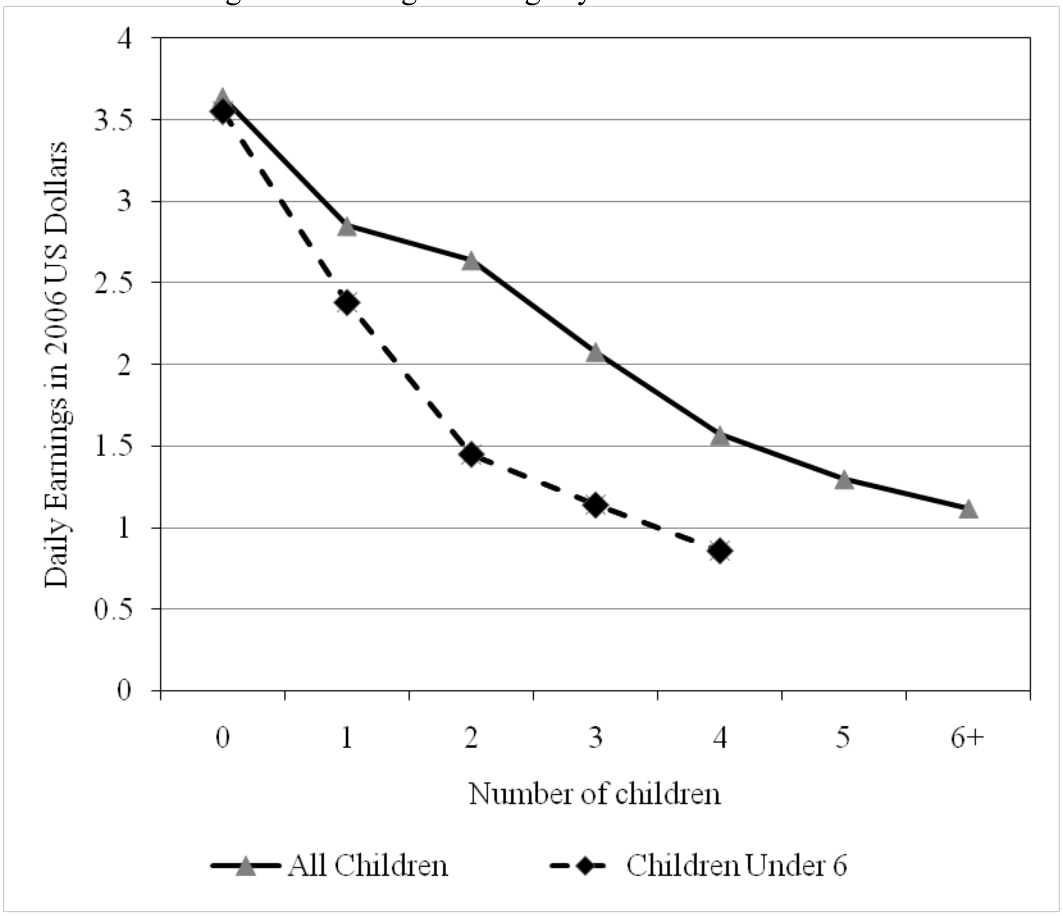
Notes: Robust standard errors (in brackets) are clustered at the sub-national level. Significance at 10 percent denoted by *, ** significant at 5 percent and *** significant at 1 percent. The p-value corresponds to an F-test for the equality of parameters across gender by age group. All regressions includes women's age, education, interaction between age and education, survey fixed effects, marital status, and four indicators for the size of current location.

Table 6: Explaining the Family Penalty

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Low educated women: completed primary or less (Mean wage=$1.15, N=79,526)						
Under 3	-0.210	-0.076	-0.058	-0.067	-0.076	-0.056
	[0.028]***	[0.021]***	[0.020]***	[0.021]***	[0.021]***	[0.019]***
3 to 5	-0.093	-0.046	-0.034	-0.040	-0.046	-0.033
	[0.024]***	[0.020]**	[0.019]*	[0.020]**	[0.020]**	[0.019]*
6 to 10	-0.030	-0.009	-0.006	-0.009	-0.009	-0.004
	[0.016]*	[0.014]	[0.012]	[0.014]	[0.014]	[0.012]
11 to 13	-0.002	-0.002	-0.014	0.002	-0.002	-0.011
	[0.021]	[0.018]	[0.018]	[0.018]	[0.018]	[0.018]
14 to 18	0.081	0.068	0.060	0.069	0.068	0.060
	[0.030]***	[0.026]***	[0.026]**	[0.026]***	[0.026]***	[0.026]**
Paid in Cash[a]	No	Yes	Yes	Yes	Yes	Yes
Occupation[b]	No	No	Yes	No	No	Yes
Seasonal[c]	No	No	No	Yes	No	Yes
Migrated[d]	No	No	No	No	Yes	Yes
R-squared	0.12	0.28	0.35	0.28	0.28	0.35
Panel B. High educated women: incomplete secondary or more (Mean wage=$4.57, N=50,012)						
Under 3	-0.767	-0.189	-0.154	-0.151	-0.189	-0.134
	[0.134]***	[0.071]***	[0.061]**	[0.068]**	[0.071]***	[0.058]**
3 to 5	-0.375	-0.204	-0.129	-0.184	-0.203	-0.118
	[0.095]***	[0.069]***	[0.059]**	[0.069]***	[0.069]***	[0.058]**
6 to 10	-0.489	-0.339	-0.219	-0.311	-0.339	-0.207
	[0.078]***	[0.069]***	[0.051]***	[0.064]***	[0.069]***	[0.049]***
11 to 13	-0.392	-0.344	-0.224	-0.322	-0.345	-0.214
	[0.107]***	[0.091]***	[0.080]***	[0.091]***	[0.091]***	[0.080]***
14 to 18	-0.243	-0.223	-0.060	-0.209	-0.222	-0.055
	[0.128]*	[0.112]**	[0.092]	[0.111]*	[0.111]**	[0.089]
Paid in Cash[a]	No	Yes	Yes	Yes	Yes	Yes
Occupation[b]	No	No	Yes	No	No	Yes
Seasonal[c]	No	No	No	Yes	No	Yes
Migrated[d]	No	No	No	No	Yes	Yes
R-squared	0.23	0.42	0.52	0.43	0.42	0.52

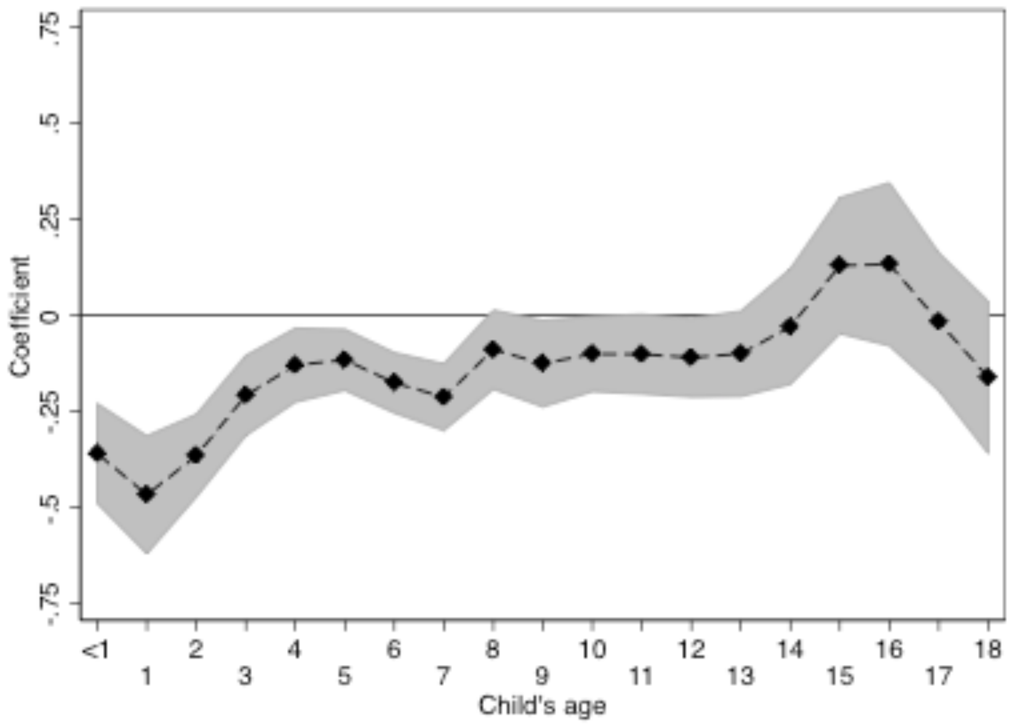
Notes: Robust standard errors (in brackets) are clustered at the sub-national level.* significant at 10 percent** significant at 5 percent; *** significant at 1 percent.
All regressions includes women's age, education, interaction between age and education, survey fixed effects, marital status, and four indicators for the size of current location.
a. Paid in Cash takes the value of 1 if the woman reported working for cash in the survey year.
b. Occupation denotes detailed occupational categories.
c. Seasonal refers to an indicator if the respondent has worked all year.
d. Migrated refers to an indicator if the woman's current residence is larger than her residence at birth to the model that includes paid in cash.

Figure 1: Average Earnings by Number of Children



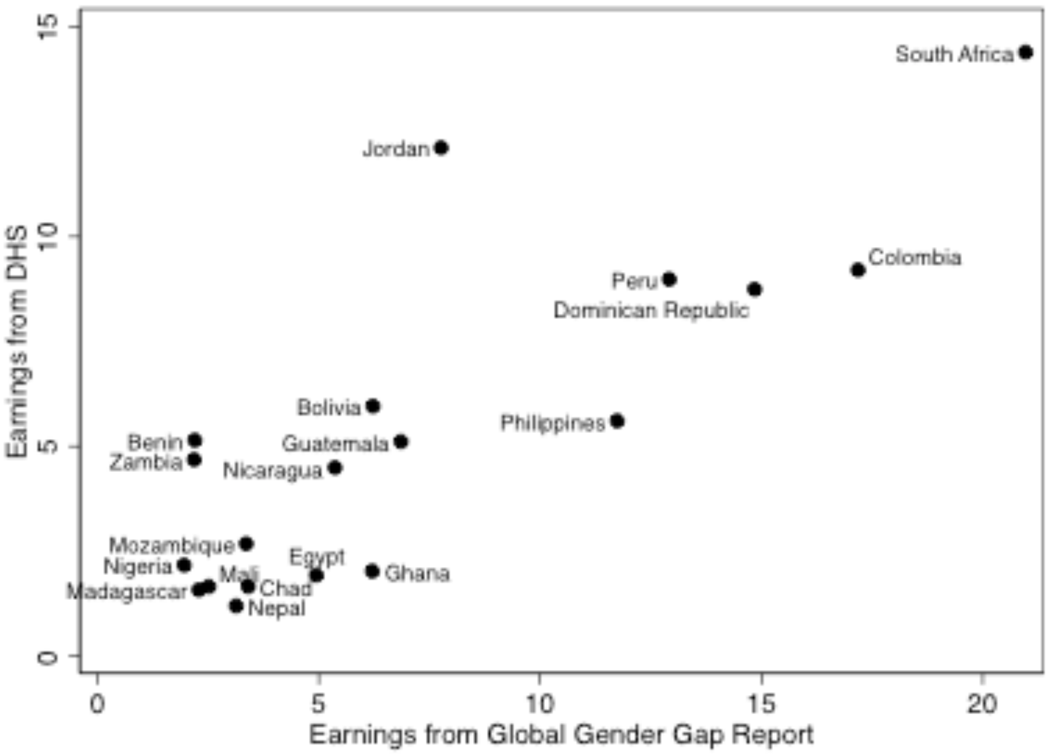
Notes: Each point corresponds to the average daily earnings of women with the given number of children. Non-cash earning women are included with earnings of zero. The solid line is for all children regardless of age. Women with 6 or more children were combined for the last data point. The dashed line is for women with varying numbers of young children (under age 6). Women with more than three young children were combined in the final data point.

Figure 2: Effect on Daily Earnings by Age of the Child



Notes: Coefficients represent the effect of the number of children by age of the child on female earnings. The regression includes the controls in Model 2. See Table 2 for details. The 95 percent confidence intervals are shown as shaded areas.

Appendix A. Validity of the DHS Earnings Data



Notes: Daily earnings from Global Gender Gap Report 2008 are expressed in 2007 PPP US$. These are imputed from the estimated annual earnings of women by assuming 250 working days. Daily earnings from the third phase of the DHS are expressed in 2006 US$.

Appendix B. Summary Statistics by Survey

Country (Abbreviation)	Survey year	Number of children	Worked for cash	Real Daily Earnings	Conditional Daily Earnings	Obs.
Benin (BJ)	1996	2.27	0.85	4.39	5.14	3,638
Bolivia (BO)	1994	2.46	0.50	2.58	5.20	4,915
Bolivia (BO)	1998	2.45	0.48	3.24	6.77	6,451
Central Afr. Rep. (CF)	1995	2.02	0.73	1.99	2.72	3,823
Chad (TD)	1997	2.61	0.46	0.75	1.65	3,871
Colombia (CO)	1995	1.78	0.59	5.38	9.19	6,549
Comoros (KM)	1996	2.29	0.33	1.38	4.23	1,659
Dom. Republic (DR)	1996	1.93	0.47	4.10	8.73	4,784
Egypt (EG)	1996	2.77	0.17	0.33	1.92	10,464
Ghana (GH)	1999	1.87	0.74	1.49	2.01	3,190
Guatemala (GU)	1999	2.79	0.31	1.56	5.11	3,586
Jordan (JO)	1997	3.37	0.14	1.66	12.12	4,057
Madagascar (MD)	1997	2.14	0.53	0.83	1.57	4,460
Mali (ML)	1996	2.55	0.40	0.66	1.65	6,450
Mozambique (MZ)	1997	2.09	0.12	0.31	2.67	5,514
Nepal (NP)	1997	2.53	0.10	0.12	1.18	6,052
Nicaragua (NC)	1998	2.42	0.41	1.84	4.48	7,708
Nigeria (NG)	1999	2.22	0.48	1.03	2.16	4,886
Peru (PE)	1996	2.20	0.50	4.52	8.98	17,379
Philippines (PH)	1998	2.13	0.54	3.01	5.59	8,446
South Africa (ZA)	1998	1.57	0.40	5.72	14.39	6,746
Zambia (ZM)	1997	2.21	0.45	2.09	4.68	4,910

Notes: Sample weights are used. Conditional daily earnings are restricted to the sample with positive earnings.

Appendix C. Correlates of Infertility

Women's Characteristics	Infertile θ_1	Fertile θ_2	Test θ_1-θ_2=0	N	Countries excluded due to lack of data
Age at first intercourse[a]	15.665	15.820	-0.155	65,034	EG, JO
	(0.166)	(0.080)	[-1.088]		
Month of birth	6.261	6.163	0.098	82,221	
	(0.115)	(0.082)	[1.241]		
Birth order	2.713	2.653	0.059	55,745	BO, CO, DR, EG, GH,
	(0.088)	(0.068)	[1.142]		GU, KM, NC
Number of siblings	5.567	5.506	0.061	57,674	BO, CO, DR, EG, GH,
	(0.160)	(0.126)	[0.603]		GU, KM, NC
Grew up in the countryside	0.468	0.526	-0.058***	82,221	
	(0.037)	(0.035)	[-3.563]		
Daughter preference[b]	0.164	0.149	0.015	70,029	KM, ZA
	(0.015)	(0.010)	[1.484]		
Total number of children desired[b]	3.682	4.098	-0.416***	74,533	
	(0.194)	(0.207)	[-4.263]		
	(0.034)	(0.032)	[3.559]		
Age at first marriage[c]	16.445	16.196	0.248	58,716	EG, JO, NP
	(0.231)	(0.112)	[1.21]		
Health visit in last 12 months	0.390	0.390	0.000	79,394	GH
	(0.023)	(0.017)	[-0.004]		
Miscarriage	0.076	0.093	-0.017	61,627	BJ, BO, KM, MZ, TD,ZM
	(0.012)	(0.009)	[-1.076]		

Notes: Robust standard errors in parenthesis and t-statistics in brackets. Significance at 10 percent denoted by *, ** significant at 5 percent and *** significant at 1 percent. The parameters θ_1 and θ_2 are estimated from regressions for each of the women's characteristics against an indicator of infertility (and 1-infertility) after controlling for dummies for age by single years and without an intercept. All estimates are for the subsample that answered questions regarding infertility.
a. Excludes inconsistent and don't know.
b. Non-numeric answers are excluded.
c. Married only.