The Effect of Fertility on Female Labor Supple in Africa: Estimating Treatment Effects with a Hazard of Treatment[1]

David Canning

Jocelyn E. Finlay

Harvard School of Public Health

Department of Global Health and Population

Harvard Center for Population and Development Studies

[1] This work is funded by the William and Flora Hewlett Foundation.

Abstract

Estimating the causal effect of fertility on female labor supply is difficult because the probability of having a child may depend on unobservables that also affect working. We show we can overcome this problem if the unobservables are continuous in time. We do this by constructing treatment and control groups of women that are balanced on both observables and unobservables. We use our approach to estimate the effect of fertility on female labor supply, and examine the heterogeneity of the relationship across sub-Saharan Africa and the Rest of the World, urban/rural living, and relative household wealth.

Introduction

The demographic transition and its associated decline in the number of children a woman has is a globally pervasive phenomenon. While the timing and pace of the demographic transition differ from country to country, its occurrence is a given. Understanding how the demographic transition comes about, the facilitation of the decline in fertility, and the social consequences of the fertility decline, have important health and social implications particularly for women of childbearing age, but also for families in their entirety.

In this paper, we explore the in detail how changes in fertility affect female labor force participation. Simple cross country correlations reveal that the relationship between fertility and female labor supply is not homogeneous around the globe. In sub-Saharan Africa women work a lot and they have more children compared to women in other parts of the world. Also within countries there is much heterogeneity in the female labor force participation response to fertility change depending on whether a women lives in an urban or rural area, or the relative wealth of the household. This heterogeneity calls for a deeper analysis, which we aim to do in this paper. Endogeneity of the relationship between fertility and female labor force participation adds to the complexity of the analysis. In this paper we aim to overcome this issue of endoegeneity to identify the causal effect of changes in fertility on female labor force participation decisions.

In a country level analysis Bloom, Canning, Fink and Finlay (2009) find that global averages indicate a decrease in fertility (as explained by a liberalization of abortion laws) of one child increases female labor force participation by 2 years. This finding provides us with an overview of the generalized relationship between fertility and female labor force participation. But if we take the analysis to the household level, and consider more deeply the low and middle income country context, this homogeneous relationship between fertility and female labor force participation is not as obvious. For women in low- and middle-income countries, where a woman works and who she works for, in addition to the binary choice of working or not working, can be affected by fertility decisions. Moreover, in the low-income country context, the income effect may dominate and an increase in fertility may in fact increase female labor force participation. An alternative hypothesis is that women in rural areas undertake work that can be more easily combined with childcare compared to her contemporaries in urban areas.

In this paper, we explore the complexity of the relationship between fertility and female labor supply by analyzing the effect of an extra child on the type of work a woman engages in. Who a woman works for and where she works affect greatly the flexibility of her work in terms of the degree to which she can engage in child care and work simultaneously. Self-employed and working at home potentially provides a greater degree of flexibility than working for someone else outside the home. An extra child may change the nature of her work – shifting from working outside the home for someone else to self-employment at home – or she may exit the workforce completely with the event of another child. We may expect that in rural areas, particularly in rural areas where agriculture is the dominant form of female employment, that women either change the nature of their work or continue working on the family farm in the face of an additional child in the family. Whereas in an urban setting, working outside the home is more likely and thus the binary choice of working or not defines the choice set more realistically for urban living women.

The relationship between fertility and female labor supply decision is not necessarily and inverse one. In low- and middle-income countries, not all households will have the freedom of choice over the matter of working or not. The woman in the family may have to work or her family will suffer starvation. Thus for some, there is no choice of labor supply when a child is born, and labor supply may actually have to increase to support the additional child in the family. Thus we may expect a stronger relationship between fertility and female labor supply in middle-income countries compared to low-income countries by virtue of there being a labor supply "decision" in the middle-income countries whereas in low-income countries there may be no such choice for many households. Moreover, we may expect the income effect to dominate in low-income countries, and for the poor women within those countries, and labor supply will increase as fertility increases.

In countries where women predominantly work in the agricultural sector and informal labor market the strict separation of time for childcare and labor supply may not be a realistic assumption. In some jobs, childcare and labor supply may be conducted jointly. We analyses a women's response to an increase in fertility on the type of work she does and the varying degrees of flexibility in her labor supply. Working at home or working for oneself present greater flexibility for joint allocation of time between working and childcare than working outside the home or working for a non-family member.

We expect that the relationship between the number of children and female labor force participation to be fundamentally different between sub-Saharan African countries and the Rest of the World, between urban and rural areas, and across women in households of differing relative wealth. Thus, we stratify across these three categories, and also the "deep" stratification of twelve permutations of SSA/ROW, Urban/Rural, and three education categories combined.

Fertility and female labor supply decision are simultaneous. Lower fertility frees up time for women to remain in the workforce; but also being in the workforce raises the opportunity cost of having children (Schultz 2009). The simultaneity of the fertility and female labor force decision has long been recognized (Cain and Dooley 1976), and an identification strategy is required to isolate the effect of fertility on female labor force participation. To identify the effect of a change in the number of children on female labor supply, we take account of the simultaneity in the fertility and labor supply decision.

We aim to identify the effect of a fertility change on female labor supply. When using observational data , as we do in this study, a common empirical approach is to employ the technique of instrumental variables. An instrumental variable is a variable that is correlated with the endogenous regressor, fertility in our case. The instrumental variable is also a variable that only affects the outcome, female labor force participation, through fertility. In Bloom, Canning, Fink and Finlay (2009) we applied abortion laws as the instrument for fertility in the female labor supply equation – showing that abortion laws affect fertility, but the only way abortion laws affect female labor force participation is through fertility. At the individual level, which we explore in this paper, the treatment effect of national policies can be weak in low- to middle-income countries. Moreover, within country heterogeneity can be difficult to identify.

There have been many studies that focus on the effect of fertility decline on female labor force participation within the United States using a variety of instrumental variables to identify a causal relationship. Rosenzweig and Wolpin (1980a; 1980b) use twins as an instrument for fertility, exploiting the fact that the arrival of an extra birth is random and independent of labor supply. They find that an extra child will reduce female labor force participation by 10 per cent.

Angrist and Evans (1996) use variation in US state abortion laws to identify the effect of fertility on schooling and labor force outcomes. Then in 1998 Angrist and Evans (1998) used twins as an instrument and compared this to results generated by an instrument based on sibling-sex composition. The latter instrument is derived from the observation that US parents with two children are more likely to have a third child if the first two children are of the same sex. Sex composition of the children is considered random and affects labor supply only through the change in fertility. Carrasco (2001) uses PSID data and sibling-sex composition to identify that each child less increases female labor force participation by 38 percent – an estimate nearly four times that of Rosenzweig and Wolpin (1980b). Kalist (2004) uses variation in abortion laws across states in the US as an instrument to find a 7 percent marginal effect of fertility on female labor force participation. Bailey (2006) uses US state variation in the laws associated with access to the contraceptive pill for young women as an instrument to find that childbearing reduces the probability of entry into the workforce for young women.

While the evidence for a positive effect of fertility decline on female labor force participation in the US has much support, there is a scattering of work on the causal effect of fertility on female labor force participation in a developing country context. Miller (2007) explores the effect of family planning programs on fertility in the developing world by examining the fertility response to the PROFAMILIA of Columbia. Miller (2007) finds that family planning explained 10 percent of the fertility decline in Colombia during the demographic transition. Rozenzweig and Zhang (2009) find that in the case of China's one child policy, that this population policy has had little effect in the development of its human capital which is the hypothesis of the quality-quantity trade off. In examining the relationship between fertility and female labor force participation in the developing world, Porter and King (2009) use the Demographic and Health Surveys to find that women have more children if they have twins in their first birth, or have the first two births are the same sex. They find that women who have a boy in their first or second birth are less likely to work in the developing world. Cruces and Galiani (2007) find in a sample of Latin American countries using sibling-sex composition as an instrument find an 8 percent marginal effect of fertility on female labor force participation. Using a sample from South American countries, Agüero and Marks (2008) use self-reported infertility as an instrument and find that there is no effect of fertility on female labor force participation in their sample.

In early attempts to identify the effect of fertility on female labor force participation, we also tried to adopt the instrumental variable approach using reproductive health laws (as in macro paper by Bloom, Canning, Fink and Finlay (2009)), twins, and sex selection. Each of these approaches yielded inconsistent or inconclusive results, in the most part due to the lack of orthogonality of the instrumental variable. The invalidity of the instrument became a particular issue when examining stratifications within the data: delving deeper into the data by looking at individual countries, by continent, by urban/rural, education, household wealth, and the array of permutations by combining these categories (eg. urban rich vs urban poor). Given that our aim is to explore the heterogeneous nature of the relationship between fertility and female labor supply, understanding the within-country dynamics of the causal relationship is key to our approach.

Abandoning the instrumental variable approach, we adopt a treatment/control approach akin to that used in a randomized control trial except we use the observational data provided by the Demographic and Health Surveys. We examine a select group of women who signal through their non-use of contraception (modern or traditional) that they would like to have an extra child[2]. The event of a pregnancy and birth within a given reference period means that some women, at random, have an infant by the time of survey. For other women who are also signaling their desire for a child through non-use of contraction, getting pregnant takes a few months longer and thus as at the time of interview they do not have an infant to care for but are pregnant. In our sample we take women who are between one and five months pregnant to limit any effects a later term pregnancy may have on a woman's decision, or ability, to work. We then compare the probability of a women working if she has a young child or not. The reference period is short enough to rule out sub-fecundity which may indicate the women who take longer to conceive are in some way different from those who have an infant by the time of survey. We also rule of infecundity as all women become pregnant in the reference period, but some have a birth before the date of interview, and some just after.

It is noted that female labor in a developing country context is difficult to quantify (Donahoe 1999), and in this paper we extend the literature on the causal effect of fertility on female labor supply on three significant fronts. First we apply an innovative treatment/control assignment

[2] Here we are assuming that the woman is making a choice over her reproductive life. As we include information regarding abstinence, post-partum amenorrhea, traditional and modern use of contraception the sub-sample of women is not just restricted to those who have access to modern contraception.

using the reproductive calendar in the Demographic and Health Surveys to identify the causal effect of fertility on female labor force participation. Second, we apply household level data from 50 low- and middle- income countries. To date, cross-country comparisons are done on macro level data, thus until now we have not been able to compare both within and across countries in the same study. Studies to date using individual level data tend to focus on one country, particularly the US, or a collection of two or three countries. The third significant contribution is the use of the household level data to generate a variable that pertains to work place flexibility. Rather than simply accounting for the binary choice of work or not work, we take account of the fact that some types of work will enable joint childcare and labor force participation. We incorporate a measure of workplace flexibility in our model and estimation to identify whether women work or not, and which type employment they engage in once they have a child.

In addition to these three contributions, we also take the literature further in exploring heterogeneity in the relationship between fertility and female labor supply by continent, urban/rural living, and by wealth tertile.

Theory: Estimating Treatment Effects with a hazard of treatment

Suppose treatment is not chosen but arrives with a hazard. However this hazard depends both on observable and unobservable covariates. Estimating the effect of treatment is difficult because even if we control for observables treatment may be correlated with unobservables that affect both treatment and outcomes. We show we can overcome this problem if the unobserved covariates are continuous in time. We select a group of people who receive treatment in a small time interval, the treatment group, and compare the outcome of interest during this time period with those in a control group who receive the treatment in the succeeding interval. We show that the probability of being in our constructed treatment versus control groups is exogenous and independent of the unobserved characteristics. This allows us to estimate the causal effect of treatment. As a check that our method works it is useful not to control for observable covariates that are continuous in time; these should be balanced automatically between our constructed treatment and control groups.

Consider a continuous time process where for person i

$$P(y_i(t)=1)=\Phi(f_i(t),x_i(t),z_i(t))$$

Where $y_i(t)$ is the outcome variable of interest (discrete in our case), $f_i(t)$ is the endogenous treatment variable taking the value of either zero or one. We assume that treatment is irreversible so that if a person is treated at time t there are treated at all later times. $z_i(t)$ are a set of covariates that are potentially discontinuous in time and $x_i(t)$ is a set of covariates that are continuous in time. The treatment variable and the covariates may be endogenous. We observe the process from time zero to time T . Let $t_i=\inf\{t:f_i(t)=1\}$ be the time at which individual i first receives the treatment.

The hazard rate $h_i(t)$ gives the treatment rate at time t given the individual has not been treated before t.

$$h_i(t)=\lim_{k\to 0}P[t\le t_i<t+k\,|\,t_i\ge t]/k$$

We assume the hazard function depends on a woman's characteristics and choices and is given by

$$h_i(t)=h(z_i(t),x_i(t))$$

The hazard function is potentially a function of both the discontinuous and continuous covariates. We do not include time directly as an explanatory variable but it is easy to add as a continuous covariate. A key assumption is that the hazard function is continuous.

We now impose the further condition that for all time in the interval $[0,t_i]$ we have $z_i(t)=z_0$. That is we drop form the sample anyone who has any value of the discontinuous covariates different from z_0 before receiving treatment . We now have a sample where prior to treatment all individuals have the same value of the discontinuous covariates. These are our eligible population for our study. The importance of this for what we do below is that given all women have the same value of the discontinuous covariates, the hazard function becomes continuous in time.

Now select a time t^*-k to $t^*+\alpha k$ which is our study period. Define a treatment group as those individuals in our sample who have $t_i \in [t^*-k,t)$. That is people who receive treatment in the interval $[t^*-k,t^*]$. Now take as our control group people who receive treatment in the interval $[t^*,t^*+\alpha k]$ for a positive constant α . Our new sample is people who are either in our constructed treatment or control groups; we discard other observations.

Now consider the probability of being in our treatment group relative to the probability of being in either the treatment or control groups for someone who has not received treatment before time t^*-k . This is given by

$$\frac{P[t^*-k \le t_i < t^* \mid t_i \ge t^*-k]}{P[t^*-k \le t_i < t^* \mid t_i \ge t^*-k] + P[t^* \le t_i < t^*+\alpha k \mid t_i \ge t^*][1-P[t^*-k \le t_i < t^* \mid t_i \ge t^*-k]]}$$

The top line is the probability of being in the treatment group conditional on being eligible at time t^*-k . The bottom line is this probability again plus the probability of being in the control group. This is the probability of not be in the treated group (one minus the probability of treatment before t^* time the probability of receiving treatment in the interval $t^*+\alpha k$

.

In general the probability of being in the treatment and control groups depends on the hazard rate and the unobservable characteristics of each woman. Now consider what happens to the probability of being in the treatment groups relative to the treatment and control groups we make the time interval for our study small.

$$\lim_{k\to 0}\frac{P[t^*-k \le t_i < t^* \mid t_i \ge t^*-k]}{P[t^*-k \le t_i < t^* \mid t_i \ge t^*-k] + P[t^* \le t_i < t^*+\alpha k \mid t_i \ge t^*][1-P[t^*-k \le t_i < t^* \mid t_i \ge t^*-k]]}$$

$$= \lim_{k\to 0}\frac{kh_i(t^*-k)}{kh_i(t^*-k)+\alpha kh_i(t^*)(1-kh_i(t^*-k))}$$

$$= \frac{1}{1+\alpha}$$

Where we use the definition of the hazard rate and the fact that for $z_i(t) = z_0$ the hazard function is continuous in time as well as applying L'hopital' s rule since both top and bottom of this expression tend to zero.

In general the probability of being in the constructed treatment and control groups will be different for different women. However if we make our study period short enough the probability of being in our treatment and control groups is independent of both observed and unobserved characteristics of the individuals that are continuous in time. This means that allocation of individuals to our treatment and control groups is random and independent of their characteristics or behavior. Let $I(F)$ be the set of N_F individuals in our constructed treatment group and let $I(C)$ be the set of N_C individuals in our constructed treatment group. We can estimate the causal effect of treatment on the group with $z_i(t) = z_0$ at all times prior to treatment as

$$\sum_{i \in I(F)} y_i(t^*) / N_F - \sum_{i \in I(C)} y_i(t^*) / N_c$$

since a time t^* the treated group have the treatment while the control group do not and the allocation of individuals between the treatment and control group is a random draw.

The approach has similarities with a randomized control trial. All the individuals in our "study" get the treatment. However some get it just before time t^* while some get it just after. While receiving treatment may not be random, among those who do get treatment, getting it just before or just after t^* is essentially a random draw.

The approach also has similarities with the regression discontinuity literature. In that literature treatment may be endogenous but is more likely just after the discontinuity point than before. Arguing that any unobserved covariates are continuous across this point we can estimate the causal effect based on the change in outcomes across the discontinuity. Our approach uses the idea that the covariates vary continuously. However we assume the treatment has a hazard rate that is continuous in time allowing us to construct treatment and control groups. Similar issues arise in the two approaches with regard to the length of the study window. In both cases the results depend on the window being very small so that continuity arguments can be used, but as we make the window smaller we lose sample size and so face a trade off in choosing the size of the window. As in the regression discontinuity literature, a longer window may be possible if we use a regression approach in which we control for unobserved continuous covariates by adding a continuous function of time.

Since allocation of individuals to our constructed treatment and control groups is random they the two groups should automatically be balanced in terms of the continuous covariates. While we cannot test this for unobservables we can check the balance of observable characteristics. For example the age, education level, and height are continuous in time and so should automatically be balanced between our treatment and control groups.

Data: Demographic and Health Surveys

The data that we use for this analysis comes from the Demographic and Health Surveys (DHS). The DHS are nationally (and sub-nationally) representative surveys that have information relating to population, health, reproductive health, and fertility histories. In its entirety the DHS cover more than 90 low- to middle-income countries, with some countries having multiple surveys between 1985 to the present. To date there are more than 260 surveys. For this study, however, we use a selection of these surveys as we apply the reproductive calendar which was only collected in a limited number of surveys.

The DHS aim to collect data that is comparable across countries and over time. To this end, Macro International, the administrators of the DHS, have developed a set of standard model questionnaires that are applied in each country.

To achieve national and sub-national representativeness, the DHS sample for the surveys based on a two-stage, stratified sample of households. In the first stage, enumeration areas in urban and rural areas are selected using systematic sampling with probability proportional to population size. Of those EAs selected in the first stage, a complete household listing is conducted to provide the sampling frame for the second stage. At the second stage of sample, within each EA 20-30 households are systematically sampled. There is a slight over sampling in anticipation of non-response.

In this paper, we use the reproductive calendar. The reproductive calendar provides month-to-month information regarding contraceptive use/non-use, pregnancies, births, and termination (induced and spontaneous are indistinguishable in the data). We use this detailed information to track the time to conception and first birth within a reference period. See Table 1 for the final list of countries used in this paper.

Table 1: Country List

sub-Saharan Africa	Rest of World
Ethiopia	Bolivia
Ghana	Colombia
Kenya	Dominican Republic
Madagascar	Egypt, Arab Rep.
Malawi	Guatemala
Namibia	Honduras
Nigeria	India
Sierra Leone	Indonesia
Swaziland	Jordan
Tanzania	Moldova
Uganda	Morocco
Zambia	Nepal
Zimbabwe	Nicaragua
	Peru
	Philippines
	Ukraine
	Vietnam

Variable Description

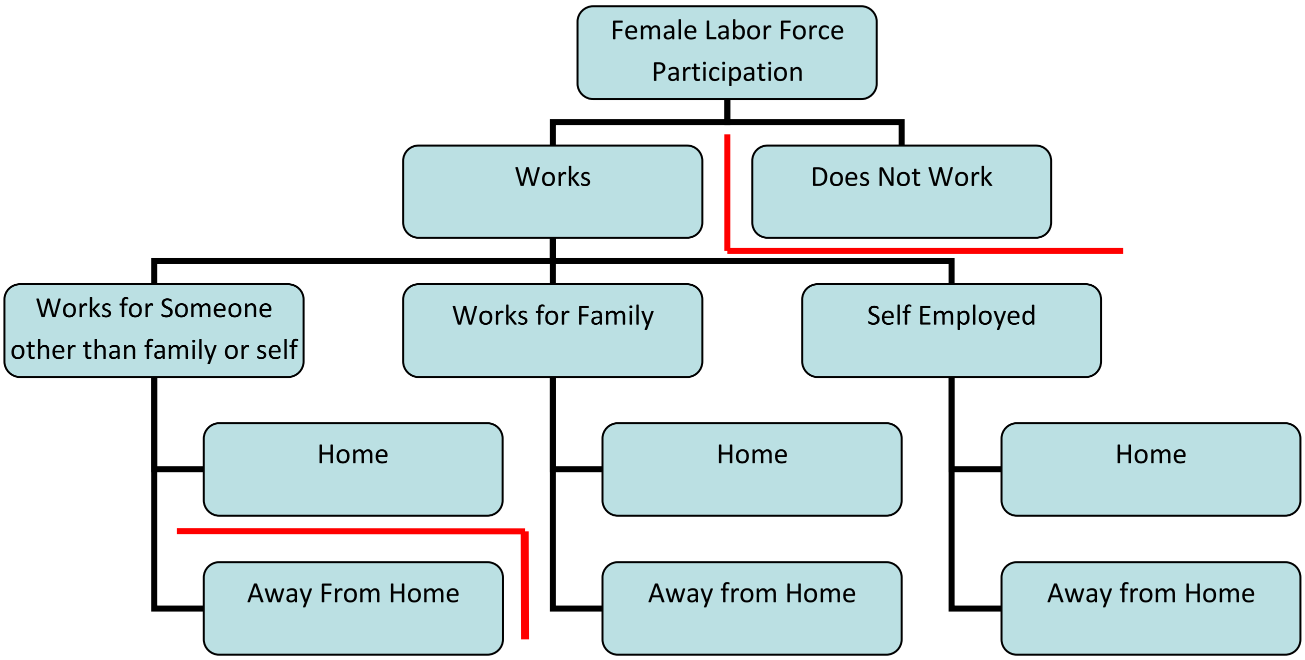
The DHS are recoded so that the variables take on the same name and description across all of the surveys. A bit of cleaning of the data are required however, to ensure than non-responses are excluded and that categories are grouped in the desired way.

In this paper we consider dependent variable that relate to whether a woman works or not, and they type of work she does.

We first consider the distinction between women who have worked in the past week or not. This is a dichotomous variable that takes the value of one if she worked in the past week and a value of zero if she reported to have not worked in the past week. We also consider the type of work a woman does. The variables relating to type of work refer to a reference group of women who have worked in the past year (not just the past week). To create the variables that capture the type of work a woman does, we generate three dichotomous variables. The first is "Does not work" and this takes a value of one if she does not work and a value of zero otherwise. The second is whether the woman has "moderate workplace attachment". This variable is designed to pick up women who are engaged in the informal labor market. This variable takes on a value of one if she works at home or if she works for family or is self-employed. The variable takes on a value of zero otherwise. The third variable is whether the woman has a "strong workplace attachment" and is designed to capture formal work. The variable takes on a value of one if the woman works for someone else other than family or herself away from the home. Figure 1 shows these splits in a diagram.

In this paper we stratify the results by geographic region, urban and rural living, and wealth tertiles. We take the United Nation's classifications of geographic region and group countries into sub-Saharan African and Rest of World. Table 1 details the list of countries that fall into each geographic grouping. The DHS report whether the interviewed woman lives in a house that is an urban or rural area (v025). We use these classifications to group women according to the urban or rural living status. We also stratify by wealth tertile. The DHS publish wealth quintiles and also the factor score for each individual household. Wealth quintiles reduce the sample size of each cell to too few, thus we use the factor score to rank each household within a country and then within each country group households into three groups: poorest; middle; and richest.

Figure 1: Classification of "Does not work", "moderate workplace attachment" and "strong workplace attachment" according to the DHS work categories.



Sample Design

In developing the sample for this study, we consider the Demographic and Health Surveys that have the module called the reproductive calendar. The reproductive calendar details information on a month-to-month basis going back up to 80 months prior to the January before the interview date. The information captured in the monthly history relates to contraceptive use (traditional, local, and modern), abstinence, sterilization, pregnancies, births and terminations (no distinction between spontaneous and induced). The data are presented as a string variable, with all of the 80 months blocked together. The code for "not using contraception" is "0", but for some countries, this is unfortunately coded as a blank. As the reproductive calendars are not of equal length for all women, the blanks close to the start of the reproductive calendar cannot be distinguished from non-existent data. Thus, a number of surveys that technically have the reproductive calendar are dropped due to indecipherable code.

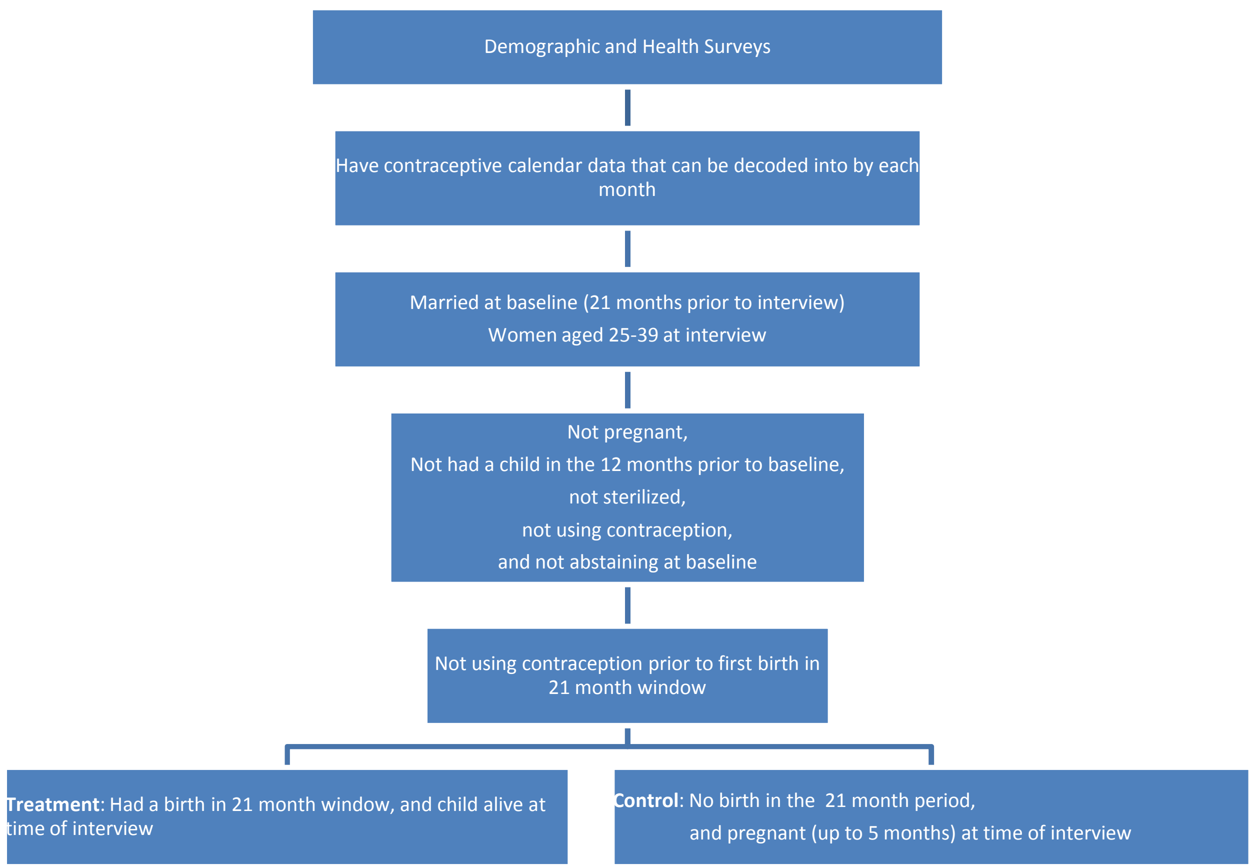
Those that are coded fully, with the "0" in place of the "not using contraception", are separated out into month specific variables. Thus one string variable becomes (up to) 80 variables with each of the 80 variables holding information for one month.

We start by constructing the sample group of women. These women, according to the reproductive calendar, were not using contraception at baseline. Moreover, they were not pregnant, were not sterilized and were not abstaining. We also exclude women who have had a birth in the year prior to the 21-month reference period to exclude women who are experiencing post-partum amenorrhea. For the baseline period, we choose 21 months. This is so that women have the chance to get pregnant, give birth to a child, and by the time of interview (at which time we have other demographic and socio-economic data correspondent to the female respondent) the child is up to one year old (21 months is the baseline month where women are not pregnant and not using contraction, 9 months of pregnancy and 11 months since the birth of the child). From the baseline month, we then consider women who do not use any form of contraception (none of modern contraception, traditional contraception, termination, abstinence, sterilization) up to their first pregnancy. For the treatment group this first pregnancy results in a successful birth within the 21 month window, thus the woman has a child to care for at the time of interview. For the control group she will also have a birth, and she is not using any form of contraception prior to the first pregnancy within the 21 month period, but it takes her longer to get pregnant than the treatment group women and at the time of interview she is at most five months pregnant (ie one to five months pregnant) and thus, as yet, does not have a new infant child to care for and is not heavily pregnant. Thus the women in the treatment and control groups are essentially the same in that they both signal through not using contraception of any form that they want a child and all get pregnant during the reference period. The control group, however, are slower to get pregnant and thus we observe at the time of interview that the treatment group will have an infant and the control group will not (although latter women will have one 4-8 months after the interview). While infecundity may be an issue if the reference period were say 80 months instead of 21 months, as the reference period narrows this becomes less of a concern.

We restrict the sample of women to those aged 25-39. While the Demographic and Health Surveys have information for women aged 15-49 (10-49 in some surveys), we limit the age to 25 on the lower end to avoid the interplay between education which may confound or dilute fertility effects on female labor supply. We also restrict the age range on the upper end to avoid any dilution from potential early retirement or age related fecundity effects. See Figure 2.

The aim of creating this select group of women for the treatment/control groups is that we are essentially taking a group of similar women (those who want to have a child), and some will randomly have an infant by the time of interview and some will have their child just after the interview. Importantly, the question regarding work is asked at the time of interview. Thus, we can deduce from the data whether a woman who had a child up to 12 months before the interview date is less likely to be working at the time of interview compared to similar women who have not yet had a child.

Figure 2: Sample deduction for the causal regression analysis



Estimation Strategy

The relationship between fertility and female labor supply is highly endogenous. The decision to work affects the decision on how many children to have, and the decision on having an extra child affects the decision to work. Increasing the number of children a woman has increases the amount of time she engages in child care and reduces the amount of time available for formal work (assuming childcare and work cannot be conducted simultaneously). In the reverse, a woman who wants to spend more time in work will choose to have fewer children as the opportunity cost of an extra child increases as her preference for work is stronger and her wage is potentially higher.

Thus, to identify how having an extra child affects the decision to work we need a method to isolate this effect and abstract from the influence of the effect of work on fertility.

To do this, we consider a select group of women who are signaling their desire for another child by not using any form of contraception at a certain baseline reference period. We consider women who could have a child aged 0-1 by the time of interview. Thus, we consider a reference period of 21 months. At the baseline, 21 months prior to interview, this group of women is not using any form of contraception. These women continue to not use contraception, thus signaling their desire for a child (modern methods, as well as traditional and abstinence are all considered forms of contraception) through to her first pregnancy in the reference period. All of these women become pregnant within the 21 months. The timing of this pregnancy is random – some women get pregnant earlier in the 21 month reference period than others. Those who have a pregnancy that ends in a birth will have a child between the ages of 0-1 by the time of interview. These women are the treatment group. Another group of women will get pregnant for the first time in the reference period just before the interview date, so that at the time of interview they are at most five months pregnant. At the time of interview, they do not have infant aged 0-1 but are pregnant, and these women are the control group. We estimate the probability of a woman working at the time of interview if she is "treated" or not.

$$\boldsymbol{FLS_{itc} = \beta_1 treatment + \varepsilon_{itc}}$$

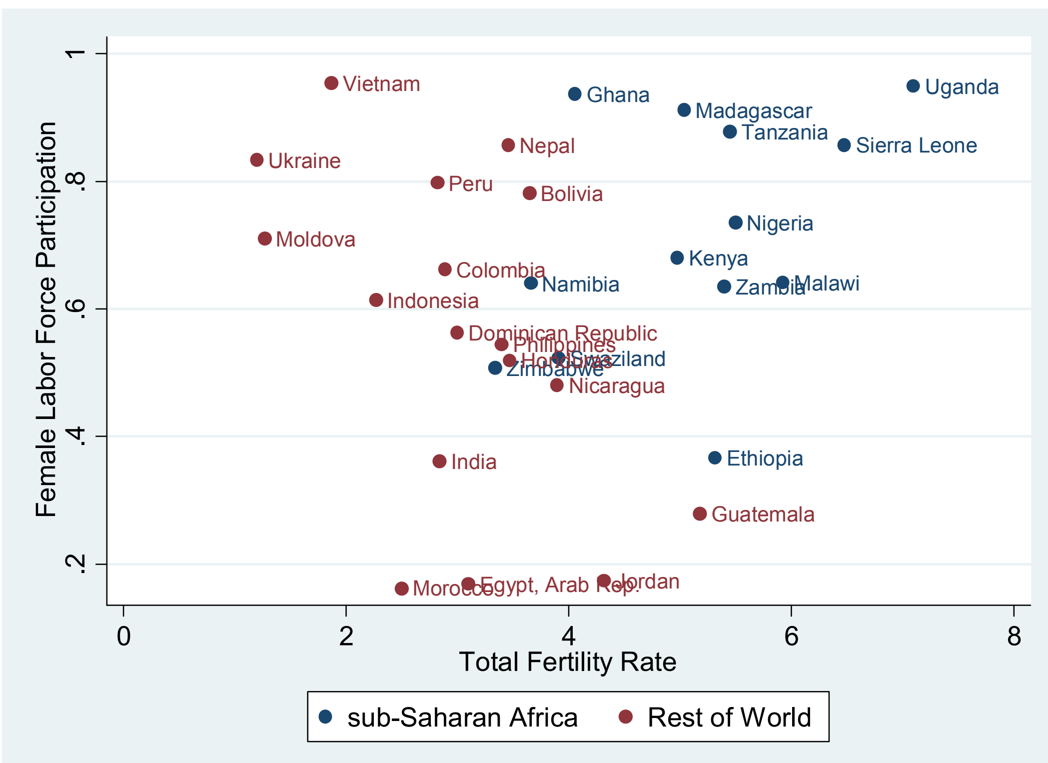
To ensure that the treatment and control women are similar, we estimate balance equations to test that they are statistically similar across a range of demographic, health and socio-economic factors.

Results

In this paper we focus on identifying the causal effect of fertility on female labor supply, and the heterogeneity of the effect of fertility on female labor supply across and within countries. We examine the pooled sample, regional split, urban and rural stratifications, and wealth tertile stratifications. We also stratify the regional groups by urban rural, and by wealth tertile respectively.

Cross Country Relationship

Figure 3: Cross Country Relationship between Fertility and Female Labor Supply



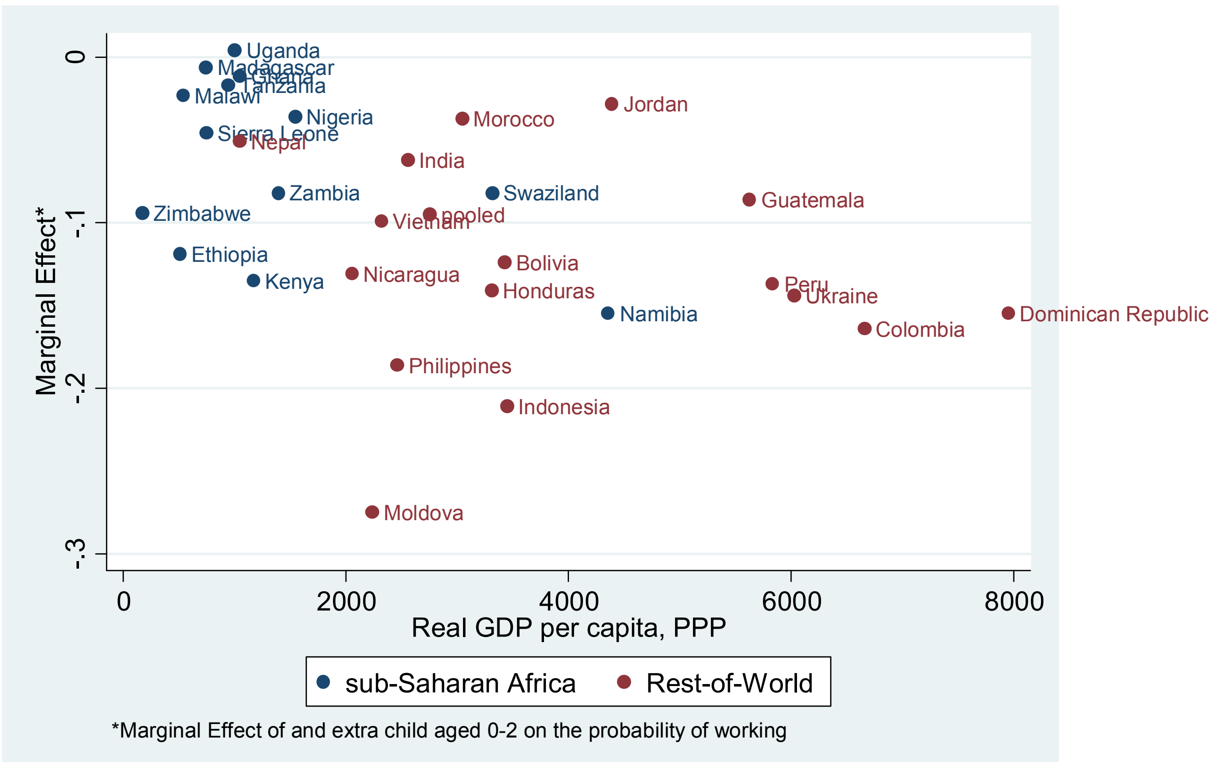
In considering the cross-country relationship between fertility and female labor force participation, we estimate the cross-country correlation of fertility as per the rates published in the World Development Indicators, and average number of women who worked in the past week in the latest survey year of our sample group of countries.

In Figure 3, we can see that the relationship between fertility and female labor supply varies around the globe. Across the sub-Saharan African countries we can see that in these countries women work a lot and have many children relative to countries in the Rest of the World. Also, there is evidence of a slight positive association between fertility and female labor force participation across the sub-Saharan African (SSA) Region. In the Rest of the World (ROW), however, it seems that there is a negative relationship between fertility and female labor force participation. The latter, the relationship across the "Rest of World" group of countries, augers well with the naïve prior that for all women in all countries as fertility increase, then labor force participation must decline as women are endowed with a fixed amount of time and with more children to look after she has less time to spend in the workplace. This plot, however, clearly shows that this narrative does not hold for countries within sub-Saharan Africa, and in fact if anything higher fertility is associated with higher labor force participation.

With sub-Saharan African countries standing out on their own in this relationship between fertility and female labor force participation in this paper we delve deeply into the nature of this relationship. Examining not only how much women work but also the type of work that women do. We examine the relationship within countries, and across sub-groups of women to try to decipher the characteristics of the woman and the nature of her work so that we can understand how in sub-Saharan Africa these women can work so much and yet still have so many children. In doing this we not only consider the correlation between fertility and female labor force participation, but also we identify the causal effect of fertility on female labor force participation.

OLS, unadjusted, association

Figure 4: Within Country Relationship between Fertility and Female Labor Supply. OLS, unadjusted coefficients. Outcome: worked (0/1). Graph of the regression coefficients detailing the effect of fertility on female labor force participation.



In Figure 4 we consider the within country relationship between fertility and female labor supply. The y-axis is the effect of increasing the number of children by one on the probability of working. In many sub-Saharan African countries we see that the associative effect of another child on the probability of a woman working is weak compared to the association in the Rest-of-World. In Kenya and Ethiopia the association is stronger despite the relatively low GDP per capita of these countries compared to Rest-of-World. In Kenya, without adjusting for any other socio-economic factors, an extra child is associated with approximately 13 percent less likelihood of working. In Moldove, the association is close to 30 percent.

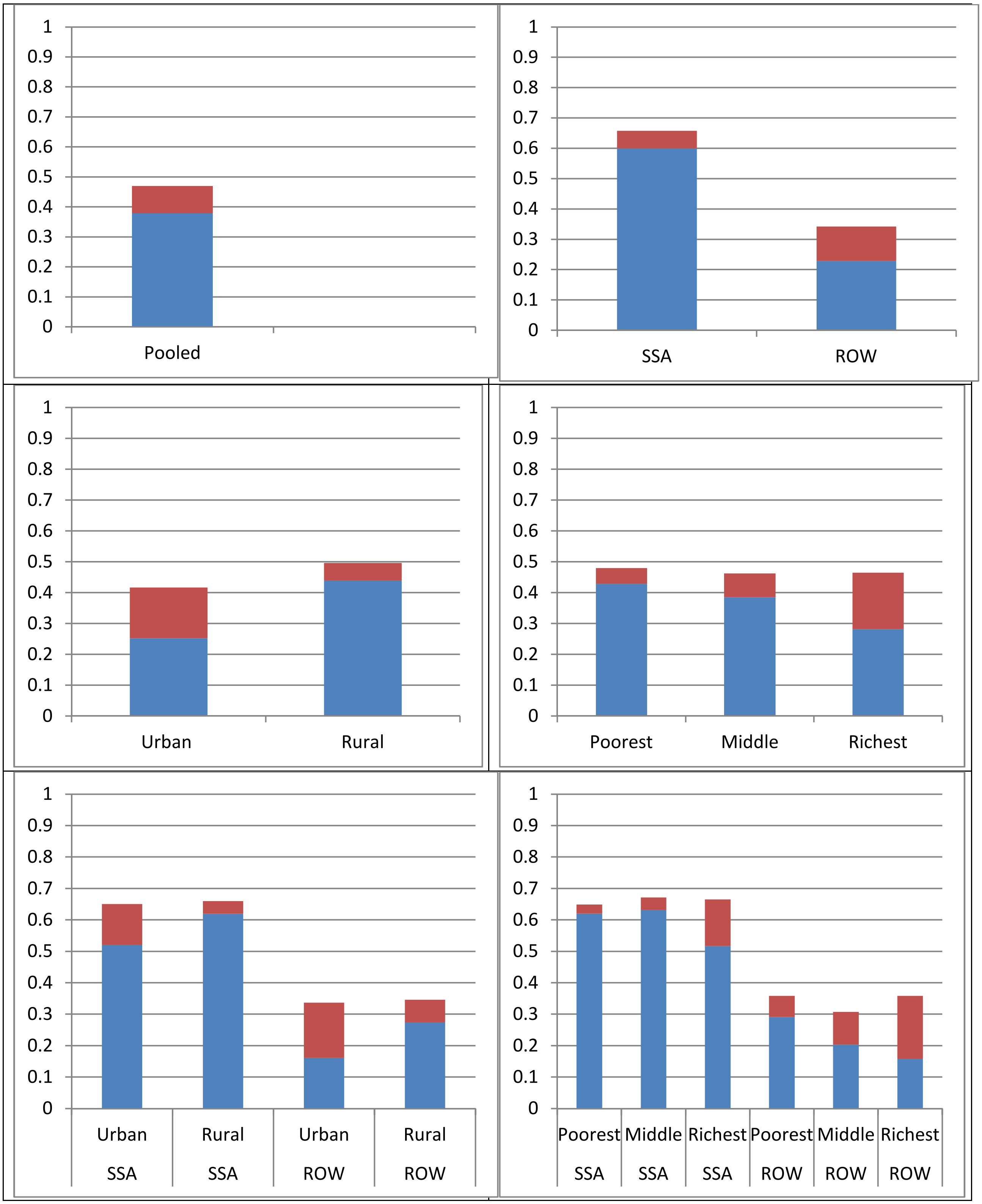
The Decision to Work or Not

Table 2 and Figure 5 provide a general snapshot of our sample of women. On average across all of the countries, 60.46% of the women reported to have worked in the past year. Women work more on average in sub-Saharan Africa (73.47%) than in the Rest of the World (54.93%). Female labor force participation is also higher in rural areas (61.76%) compared to urban areas (58.76%). Urban women are more likely to engage in strong workplace attachment than rural women, and if a woman lives in a rural area she is more likely to work (either in moderate or strong workplace attachment jobs) than a woman who lives in an urban area. These trends are evident in both the pooled, SSA and ROW samples. Although it is clear that SSA women (both urban and rural) work more than there contemporaries in ROW.

The likelihood of working is almost equal across wealth tertiles (close to 40%), but we also see that women in higher wealth tertiles are more likely to work in jobs of strong workplace attachment. These trends are evident in pooled, SSA and ROW samples, although it appears that middle wealth tertiles women in ROW work a little less in general than the poorest and richest women in ROW.

rate lace ment	Strong workplace attachment		Does not work	Moderate workplace attachment	Strong workplace attachment		Does not work	Moderate workplace attachment	Strong workplace attachment
1904	460								
6711	1613								
			ROW						
1235	120		1749	669	340				
4293	416		7140	2418	1197				
			Rural						
390	257		1650	1514	203				
1439	933		6152	5272	680				
			Middle				Richest		
910	113		801	630	138		580	352	202
3299	391		3176	2212	432		2295	1161	780
			SSA Rural						
218	51		596	1017	69				
735	188		1917	3558	228				
			ROW Rural						
172	206		1054	497	134				
704	745		4235	1714	452				
			SSA Middle				SSA Richest		
563	24		224	434	29		128	226	60
1973	94		807	1545	95		496	736	217
			ROW Middle				ROW Richest		
347	89		577	196	109		452	126	142
1326	297		2369	667	337		1799	425	563

Figure 5: Fraction of Women working in moderate or strong workplace attachment jobs



Treatment/control, causal

Balance

To ensure that the treatment and control groups are the same type of women, we estimate balance equations across a range of demographic, health and socio-economic characteristics.

In the balance equations we estimate the mean value of various outcomes, say respondent's education level in column (6) of Table 3. The constant represents the mean for the control group of women. As the education variable takes on values of 0 (no education), 1 (primary) and 2 (secondary) we can deduce that a mean of 0.615 indicates an average level of education for the control group somewhere between no education and completed primary. The treatment group has an education level slightly higher than this (0.04 points). This difference is statistically different from the control group mean, and indicates that the treatment/control groups are in fact statistically different in terms of their education attainment. To achieve balance in the treatment/control we should have no statistical difference between the treatment and control groups.

For brevity, we present stratified balance tables as indicators of significance or not to summarize the fraction of variables that are in balance for the chosen group. In the case of the SSA/Urban stratifications 60-90% are balanced (Table 8). In the case of the SSA/Wealth stratifications 67-100% are balanced in these groupings (Table 9).

Table 3: Balance equations. Pooled sample. Treatment/control, causal

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Country	Children Alive 1 to 5	Children Alive 5 to 15	SSA	Urban	Education	Husband's education	Husband's age	Age	Other women in HH	Wealth quintile	Height
Treatment	0.305	0.00757	0.0530**	-0.0309***	0.0198***	0.0400***	0.0423***	-0.570***	-0.233***	0.0110	-0.00804	-6.089
	(0.724)	(0.00977)	(0.0221)	(0.00818)	(0.00752)	(0.0125)	(0.0132)	(0.130)	(0.0665)	(0.0119)	(0.0234)	(14.17)
Constant (Control)	108.5***	0.567***	1.673***	0.431***	0.303***	0.615***	0.760***	37.18***	30.63***	0.341***	1.715***	1,618***
	(0.715)	(0.00894)	(0.0201)	(0.00842)	(0.00741)	(0.0117)	(0.0124)	(0.121)	(0.0595)	(0.0111)	(0.0227)	(12.91)
Observations	22,874	22,874	22,874	22,874	22,874	22,873	22,550	21,396	22,874	22,874	22,740	16,691
R-squared	0.000	0.000	0.000	0.001	0.000	0.000	0.000	0.001	0.001	0.000	0.000	0.000

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Indicator of Balance, Pooled Sample

	Pooled
Country	1
Children Alive 1 to 5	1
Children Alive 5 to 15	0
SSA	0
Urban	0
Education	0
Husband's education	0
Husband's age	0
Age	0
Other women in HH	1
Wealth quintile	1
Height	1
	42%

Table 5: Indicator of Balance, SSA/ROW

	SSA	ROW
Country	0	1
Children Alive 1 to 5	1	1
Children Alive 5 to 15	0	0
Urban	1	1
Education	1	0
Husband's education	0	1
Husband's age	1	0
Age	0	0
Other women in HH	1	1
Wealth quintile	1	1
Height	1	1
	64%	64%

Table 6: Indicator of Balance, Urban/Rural

	Urban	Rural
Country	1	1
Children Alive 1 to 5	1	0
Children Alive 5 to 15	1	0
SSA	0	0
Education	0	1
Husband's Education	0	0
Husband's Age	0	0
Age	0	0
Other Women in HH	1	1
Wealth Quintile	1	0
Height	1	1
	55%	36%

Table 7: Indicator of Balance, Wealth Tertiles

	Poorest	Middle	Richest
Children Alive 1 to 5	1	1	1
Children Alive 5 to 15	1	0	1
SSA	0	1	1
Urban	0	1	0
Education	0	1	0
Husband's Education	0	0	0
Husband's Age	0	0	0
Age	1	0	0
Other Women in HH	1	1	1
Height	0	0	1
	40%	50%	50%

Table 8: Indicator of Balance, SSA/Urban

	SSA		ROW	
	Urban	Rural	Urban	Rural
Country	1	0	1	1
Children Alive 1 to 5	1	1	1	0
Children Alive 5 to 15	1	0	1	0
Education	1	1	0	1
Husband's Education	1	0	0	1
Husband's Age	1	1	0	0
Age	1	0	0	1
Other Women in HH	1	1	1	1
Wealth Quintile	1	1	1	0
Height	0	1	1	1
	90%	60%	60%	60%

Table 9: Indicator of Balance, SSA/Wealth

	SSA			ROW		
	Poorest	Middle	Richest	Poorest	Middle	Richest
Children Alive 1 to 5	1	1	1	1	1	1
Children Alive 5 to 15	1	0	1	0	0	1
Urban	0	1	1	1	1	1
Education	0	1	1	1	1	0
Husband's Education	0	0	1	1	1	0
Husband's Age	1	1	1	0	0	1
Age	1	1	1	1	0	0
Other Women in HH	1	1	1	1	1	1
Height	1	0	1	0	1	1
	67%	67%	100%	67%	67%	67%

Type of Work

In this paper, we also explore the possibility that the type of work a woman does varies with fertility. Using data from the Demographic and Health Surveys, we create a variable indicating workplace attachment. We group women into three categories: women who do not work, women who work for someone else other than family or self and away from the home, and then thirdly all the states of the world in between. This is described above in the Variable Description section.

In Figure 5 we show the type of work women engage in by stratified groups. The most striking point of this figure is that women in sub-Saharan Africa work more than women in other continents, and that this work is primarily of "moderate workplace attachment". Thus women in sub-Saharan Africa tend to work in more flexible arrangements. While this is not necessarily beneficial for job security, it may bode well combining childcare and work. Unlike classic economic models that separate time discretely into leisure, work and child care, for women in sub-Saharan Africa who work in a situation of "moderate workplace attachment" it may be that work and childcare are carried out simultaneously. For example, if women work in the home they can also have their children there. Or if they work for family away from the home, the family may not strictly mandate that children cannot accompany the women to their workplace.

The following plots show a causal relationship between fertility and the type of work a women does.

Table 10: Pooled, Treatment/control, causal. Outcome: Type of Work

	(1)	(2)	(3)
	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment
Treatment	0.0233***	-0.0184**	-0.00491
	(0.00821)	(0.00806)	(0.00475)
Constant	0.511***	0.393***	0.0951***
	(0.00775)	(0.00769)	(0.00430)
Observations	22,731	22,731	22,731
R-squared	0.000	0.000	0.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In Table 10 we see that the birth of a child who is between 0-11 months by the time of interview is more likely not to be working (by 2.33%) and 1.84 percentage points are attributable to a decrease in moderate workplace attachment and close to 5 percentage points (although statistically insignificant) is attributable to a decrease in strong workplace attachment.

Table 11: sub-Saharan Africa vs Rest of World. Treatment/control, causal. Outcome: Type of Work.

	(1)	(2)	(3)	(4)	(5)	(6)
	SSA	SSA	SSA	ROW	ROW	ROW
	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment
Treatment	-0.00867	0.00805	0.000623	0.0297***	-0.0177*	-0.0120*
	(0.0120)	(0.0124)	(0.00580)	(0.0104)	(0.00927)	(0.00699)
Constant	0.349***	0.593***	0.0577***	0.634***	0.243***	0.123***
	(0.0116)	(0.0119)	(0.00522)	(0.00951)	(0.00855)	(0.00635)
Observations	9,218	9,218	9,218	13,513	13,513	13,513
R-squared	0.000	0.000	0.000	0.001	0.000	0.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In Table 11 we see that in SSA strong and moderate workplace attachment *increase* at the birth of a child (although statistically insignificant), and in ROW moderate workplace attachment decreases slightly more than strong workplace attachment on average.

Table 12: Urban vs Rural. Treatment/control, causal. Outcome: Type of Work

	(1)	(2)	(3)	(4)	(5)	(6)
	Urban	Urban	Urban	Rural	Rural	Rural
	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment
Treatment	0.0297**	-0.0163	-0.0134	0.0182*	-0.0141	-0.00411
	(0.0145)	(0.0128)	(0.0110)	(0.00992)	(0.00994)	(0.00463)
Constant	0.560***	0.265***	0.175***	0.490***	0.450***	0.0603***
	(0.0133)	(0.0121)	(0.0101)	(0.00945)	(0.00947)	(0.00419)
Observations	7,260	7,260	7,260	15,471	15,471	15,471
R-squared	0.001	0.000	0.000	0.000	0.000	0.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In Table 12 we see that urban women are more likely to not work when a child is born than women in rural areas. The flexibility of rural work may make it easier for women to stay in work relative to women in urban areas.

Table 13: By Wealth Tertile. Treatment/control, causal. Outcome: Type of Work

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Poorest	Poorest	Poorest	Middle	Middle	Middle	Richest	Richest	Richest
	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment
Treatment	0.00452	-0.00224	-0.00228	0.0381***	-0.0225*	-0.0156**	0.0166	-0.0233*	0.00674
	(0.0104)	(0.0102)	(0.00541)	(0.0124)	(0.0117)	(0.00765)	(0.0152)	(0.0135)	(0.0122)
Constant	0.319***	0.613***	0.0679***	0.433***	0.463***	0.104***	0.472***	0.352***	0.177***
	(0.0435)	(0.0466)	(0.0230)	(0.0534)	(0.0515)	(0.0296)	(0.0825)	(0.0802)	(0.0674)
Observations	9,840	9,840	9,840	7,389	7,389	7,389	5,370	5,370	5,370
R-squared	0.285	0.309	0.038	0.288	0.337	0.080	0.172	0.233	0.075
Survey FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In Table 13 we see that women in the poorest wealth tertile have no significant change in their propensity for work when they have another child. Women in the middle wealth tertile will decrease their labor force participation both in moderate and strong attachment jobs. Women in the richest tertile will decrease their moderate workplace attachment, but strong workplace attachment does not change significantly when a child is born.

Table 14: SSA/Urban. Treatment/control, causal. Outcome: Type of Work.

	(1)	(2)	(3)	(4)	(5)	(6)
	SSA Urban	SSA Urban	SSA Urban	SSA Rural	SSA Rural	SSA Rural
	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment
Treatment	0.0305	-0.0338	0.00328	-0.0182	0.0192	-0.00104
	(0.0261)	(0.0275)	(0.0189)	(0.0135)	(0.0139)	(0.00548)
Constant	0.326***	0.546***	0.128***	0.354***	0.605***	0.0410***
	(0.0236)	(0.0254)	(0.0173)	(0.0133)	(0.0135)	(0.00491)
Observations	1,833	1,833	1,833	7,385	7,385	7,385
R-squared	0.001	0.001	0.000	0.000	0.000	0.000

	(7)	(8)	(9)	(10)	(11)	(12)
	ROW Urban	ROW Urban	ROW Urban	ROW Rural	ROW Rural	ROW Rural
	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment
Treatment	0.0195	0.00139	-0.0209	0.0361***	-0.0272**	-0.00891
	(0.0164)	(0.0126)	(0.0134)	(0.0134)	(0.0127)	(0.00740)
Constant	0.648***	0.160***	0.192***	0.626***	0.295***	0.0795***
	(0.0148)	(0.0114)	(0.0122)	(0.0123)	(0.0117)	(0.00672)
Observations	5,427	5,427	5,427	8,086	8,086	8,086
R-squared	0.000	0.000	0.000	0.001	0.001	0.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In Table 14 we consider a deeper stratification of women in SSA in urban and rural areas, and then in ROW in urban and rural areas. Here we see that women in SSA in urban areas increase their strong workplace attachment (although statistically insignificant), and that this comes from women decreasing their moderate workplace attachment. Moreover, women in SSA in rural areas increase their moderate workplace attachment (although insignificant) when a child is born, and this comes slightly from women switching out of strong workplace attachment but for the most part the increase in moderate workplace attachment comes from women who were previously not working. Thus women are forced into the labor market at the birth of a child.

For women in ROW, we see that women reduce their strong workplace attachment in urban areas and increase their moderate workplace attachment, pointing to a trend in women changing the type of work they do when a child is born rather than exiting the workforce completely. In rural areas in ROW, moderate workplace attachment decreases significantly when a child is born.

Table 15: SSA/Wealth. Treatment/control, causal. Outcome: Type of Work.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	SSA Poorest	SSA Poorest	SSA Poorest	SSA Middle	SSA Middle	SSA Middle	SSA Richest	SSA Richest	SSA Richest
	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment
Treatment	-0.0175	0.0136	0.00384	0.0123	-0.0123	1.33e-05	0.0141	-0.0274	0.0133
	(0.0147)	(0.0151)	(0.00607)	(0.0185)	(0.0191)	(0.00827)	(0.0254)	(0.0260)	(0.0183)
Constant	0.680***	0.206***	0.114***	0.624***	0.319***	0.0577*	0.488***	0.376***	0.136**
	(0.0641)	(0.0551)	(0.0424)	(0.0719)	(0.0683)	(0.0341)	(0.0912)	(0.0855)	(0.0653)
Observations	4,089	4,089	4,089	3,134	3,134	3,134	1,863	1,863	1,863
R-squared	0.265	0.260	0.021	0.225	0.218	0.065	0.067	0.107	0.071
Survey FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	ROW Poorest	ROW Poorest	ROW Poorest	ROW Middle	ROW Middle	ROW Middle	ROW Richest	ROW Richest	ROW Richest
	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment	Does not work	Moderate Workplace Attachment	Strong Workplace Attachment
Treatment	0.0218	-0.0147	-0.00708	0.0581***	-0.0304**	-0.0277**	0.0180	-0.0211	0.00308
	(0.0146)	(0.0138)	(0.00841)	(0.0167)	(0.0145)	(0.0119)	(0.0190)	(0.0152)	(0.0160)
Constant	0.305***	0.623***	0.0717***	0.416***	0.470***	0.114***	0.471***	0.350***	0.179***
	(0.0442)	(0.0471)	(0.0235)	(0.0540)	(0.0520)	(0.0307)	(0.0829)	(0.0805)	(0.0679)
Observations	5,751	5,751	5,751	4,255	4,255	4,255	3,507	3,507	3,507
R-squared	0.190	0.197	0.035	0.149	0.144	0.068	0.109	0.099	0.072
Survey FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

In Table 15 we consider women within the three wealth tertiles within SSA and ROW. In SSA, we see that women in the poorest tertile decrease the probability of not working and increase their moderate workplace attachment at the birth of a child. This speaks to the potential income effect dominating the substitution effect, however, the results are statistically insignificant. The richest tertile women decrease their moderate workplace attachment and increase their strong workplace attachment at the birth of a child.

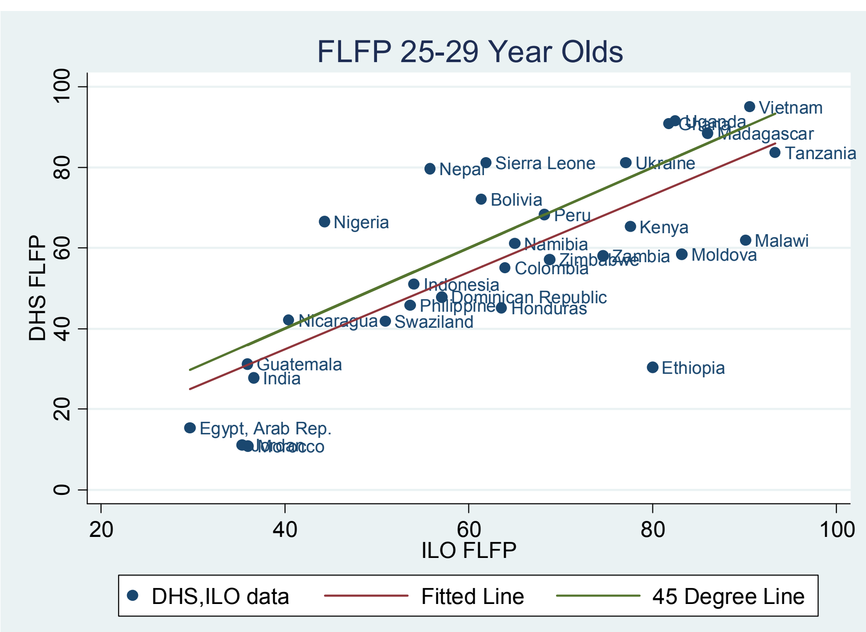
For women in ROW, those in the middle tertile see a decrease in moderate and strong workplace attachment in favor of not working.

Limitations

One of the limitations of this study is the use of the DHS to identify a woman's labor market participation. The DHS is not specifically designed as a labor force survey, and it has come under some criticism for the measurement error introduced into the labor force participation variable given the survey techniques (Langsten and Salem 2008). In Figure 6, we consider the measure of female labor supply of a group of women aged 25-29 and compare the average labor supply as per the latest DHS in our sample, and the corresponding year's measure of female labor force participation as published by the International Labor Organization. Consistent with Langsten and Salam (2008) we see that the DHS underestimate female labor force participation relative to the ILO published rate. If we take the ILO as a benchmark (an assumption that could easily be disputed), then we see from the scatter plot that not all DHSs underestimate female labor force participation. It seems the likes of Nigeria, Nepal, Bolivia and a few others female labor force participation is over estimated by the DHS. In general though, the fitted line lies just below the 45 degree line indicating that the DHS slightly underestimate female labor force participation.

In an earlier examination of the relationship between the DHS and ILO data, it was revealed that the DHS grossly underestimated female labor supply of the likes of Azerbaijan, Kazakhstan, Kyrgyzstan, and Uzbekistan. These four counties were thus dropped from the sample as the measure for female labor supply as per the DHS was considered too unreliable. There are a few countries that stray from the 45 degree line, such as Ethiopia, but we do not exclude them from the sample. Many countries, however, lie close to the 45 degree line.

Figure 6: Comparing Female Labor Force Participation Data from the International Labor Organization and the Demographic and Health Surveys



Conclusion

In this paper we have used a treatment/control assignment to a group of women signaling their desire for an extra child to identify the causal effect of fertility on female labor supply. Using observational data from the Demographic and Health Surveys, we apply information collected in the reproductive calendar to track women's reproductive behavior over a 21 month period.

Using this approach, as opposed to the usual instrumental variable approach, meant that we could identify the causal relationship between fertility and female labor supply not only for the pooled cross county analysis but also for intricate stratifications that enabled us to identify the heterogeneity around the world and within countries of the effect of fertility on female labor supply.

We find that for women in sub-Saharan Africa who live in rural areas increase their participation in the informal sector when they have an extra child. This increase goes against the typical prior that having a child takes time to care for and takes time away from the labor market, as it may be that women in these groups conduct childcare and work simultaneously. Her ability to increase work in the informal sector does not mean that she neglects her childcare duties that come with the birth of the new child. It may mean that for these women they can conduct childcare and work simultaneously. The child receives the care and food it needs throughout the day as the mother also works productively at her trade or on her farm as is likely for women in rural areas. For poor women in SSA, the income effect may dominate and an extra child means that the strain on household resources increases such that the woman must increase her work effort to support the new child. However, although the signs on the coefficients indicate this narrative, the results are statistically insignificant.

Truly understanding how women respond in their labor market behavior to an event of a new child brings light to the trials of a demanding life or the need for a dichotomous choice between working or not. It sheds light on the choices women make and can make, and how different policies can be introduced to help improve the welfare of women from all backgrounds in all parts of the world.

References

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

Angrist, J. and W. Evans (1998). "Children and Their Parents' Labor Supply: Evidence from Exogenous Variation in Family Size." American Economic Review **88**(3): 450-477.

Angrist, J. D. and W. N. Evans (1996). "Schooling and Labor Market Consequences of the 1970 State Abortion Reforms." NBER Working Paper **W5406**.

Bailey, M. J. (2006). "More power to the pill: The impact of contraceptive freedom on women's lifecycle labor supply." Quarterly Journal of Economics **121** (1): 289-320.

Bailey, M. J., B. Hershbein, et al. (2009). "The Opt-In Revolution: Contraception, Women's Labor Supply and the Gender Gap in Wages." mimeo.

Behrman, J. R. and B. L. Wolfe (1984). "Labor Force Participation and Earnings Determinants for Women in the Special Condistions of Developing Countries." Journal of Development Economics **15**: 259-288.

Bjorklund, A. (2006). "Does family policy affect fertility? Lessons from Sweden." Journal of Population Economics **19**: 3-24.

Bloom, D. E. and D. Canning (2003). "Contraception and the Celtic Tiger." Economic and Social Review **34**(3): 229-247.

Bloom, D. E., D. Canning, et al. (2008). "The Effect of Fertility on Economic Growth." mimeo.

Bloom, D. E., D. Canning, et al. (2009). " Fertility, Female Labor Force Participation, and the Demographic Dividend." Journal of Economic Growth **14**: 79-101.

Bloom, D. E., D. Canning, et al. (2003). "The Demographic Dividend: A New Perspective on the Economic Consequences of Population Change." Population Matters Monograph MR-1274, RAND, Santa Monica.

Brander, J. A. and S. Dowrick (1994). "The Role of Fertility and Population in Economic Growth." Journal of Population Economics **7**(1): 1-25.

Caceres-Delpiano, J. (2008). "Keeping the Best for Last: Impact of Fertility on Mother's Employment. Evidence from Developing Countries." Universidad Carlos III De Madrid Working Paper 08-69 **Economic Series (32)**.

Cain, G. C. and M. D. Dooley (1976). "Estimation of a Model of Labor Supply, Fertility, and Wages of Married Women." Journal of Political Economy **84**(4, Part 2): S179-S200.

Carrasco, R. (2001). "Binary Choice with Binary Endogenous Regressors in Panel Data: Estimating the Effect of Fertility on Female Labor Force Participation." Journal of Business and Economic Statistics **19**(4): 385.

Caucutt, E. M., N. Guner, et al. (2002). "Why Do Women Wait? Matching, Wage Inequality, and the Incentives for Fertility Delay." Review of Economic Dynamics(5): 815-855.

Chun, H. and J. Oh (2002). "An Instrumental Variable Estimation of the Effect of Fertility on Labor Force Participation of Married Women." Applied Economics Letters **9**: 631-634.

Contreras, D. and G. Plaza (2010). "Cultural Factors in Women's Labor Force Participation in Chile." Feminist Economics **16**(2): 27 - 46.

Cruces, G. and S. Galiani (2007). "Fertility and Female Labor Supply in Latin America: New Casual Evidence." Labor Economics **14**: 565-573.

Donahoe, D. A. (1999). "Women's Work in Developing Countries." Population and Development Review **25**(3): 543-576.

Goldin, C. (1995). The U-Shaped Female Labor Force Function in Economic Development and Economic History. Investment in Women's Human Capital and Economic Development. T. P. Schultz. Chicago, IL, University of Chicago Press: 61-90.

Goldin, C. and L. F. Katz (2002). "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions." Journal of Political Economy **110**(4): 730-770.

Kalist, D. E. (2004). "Abortion and Female Labor Force Participation: Evidence Prior to Roe v. Wade." Journal of Labor Research **25**(3).

Klepinger, D., S. Lundberg, et al. (1999). "How Does Adolescent Fertility Affect the Human Capital and Wages of Young Women? ." The Journal of Human Resources **32**(3): 421-448.

Langsten, R. and R. Salem (2008). "Two Approaches to Measure Women's Work in Developing Countries: A Comparison of Survey Data from Egypt." Population and Development Review **34**(2): 283-305.

Mammen, K. and C. Paxson (2000). "Women's Work and Economic Development." The Journal of Economic Perspectives **14**(4): 141-164.

Miller, G. (2007). "Contraception as Development? New Evidence from Family Planning in Columbia." NBER Working Paper **11704**.

Porter, M. and E. M. King (2009). "Fertility and Women's Labor Force Participation in Developing Countries." mimeo prepared for PAA 2009.

Rosenzweig, M. and K. I. Wolpin (1980b). "Life Cycle Labor Supply and Fertility: Causal Inferences from Household Models." The Journal of Political Economy **88**(2): 328-348.

Rosenzweig, M. R. and K. I. Wolpin (1980). "Testing the Quantity-Quality Fertility Model: The Use of Twins as a Natural Experiment." Econometrica **48**(1): 227-240.

Rosenzweig, M. R. and K. I. Wolpin (1980a). "Testing the Quantity-Quality Fertility Model: The Use of Twins as a Natural Experiment." Econometrica **48**(1): 227-240.

Rosenzweig, M. R. and J. Zhang (2009). "Do Population Control Policies Induce More Human Capital Investment? Twins, Birth Weight and China's "One-Child" Policy." Review of Economic Studies **76**(3): 1149-1174.

Schultz, T. P. (2009). "Population and Health Policies." Economic Growth Center Working Paper **974**.

Skirbekk, V. (2008). "Fertility Trends by Social Status." DEMOGRAPHIC RESEARCH **18**(5): 145-180.

Smith, J. P. and M. P. Ward (1985). "Time-Series Growth in the Female Labor Force." Journal of Labor Economics **3**(1): 59-90.

Vere, J. P. (2007). ""Having It all" No Longer: Fertility, Female Labor Supply, and the New Life Choices of Generation X." Demography **44**(4): 821-828.

Vere, J. P. (2008). "Dragon Children: Identifying the Causal Effect of the First Child on Female Labour Supply with the Chinese Lunar Calendar." Oxford Bulletin of Economics and Statistics **70**(3): 0305-9049.

Walle, E. v. d. and V. d. Luca (2006). "Birth prevention in the American and French fertility transitions: Contrasts in knowledge and practice." Population and Development Review **32**(3): 529-555.