
Are Children Really Inferior Goods?

Evidence from Displacement-Driven Income Shocks

Jason M. Lindo

ABSTRACT

This paper explores the causal link between income and fertility by analyzing women's fertility response to the large and permanent income shock generated by a husband's job displacement. I find that the shock reduces total fertility, suggesting that the causal effect of income on fertility is positive. A model that incorporates the time cost of children and assortative matching of spouses can simultaneously explain this result and the negative cross-sectional relationship. I also find that a husband's displacement accelerates childbearing, which is consistent with lifecycle models of fertility in which the incentive to delay is driven by expected earnings growth.

I. Introduction

Are children inferior goods? Economists have long wrestled with this question because cross-sectional evidence suggests that family size and income are negatively correlated: high income families tend to have few children and, analogously, high income countries tend to have low fertility rates. Further, time-series data frequently show that developing countries transition towards lower fertility rates as they industrialize. The negative relationship between income and fertility is not itself inconsistent with standard consumer choice models of household fertility. However, within that theoretical framework, it is inconsistent with the presumption that children are normal goods. Seminal papers by Becker (1960), Becker and Lewis (1973), and Willis (1973) reconcile this apparent conflict with economic theory by

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developing household models in which both child quantity and quality enter the household utility function and budget constraint. Others have tried to solve the puzzle with models that incorporate the time costs of children. These models, however, tend to be based on the assumption that the observed relationship between income and fertility is causal. In fact, little is known about what underlies this correlation.¹ This paper begins to fill this important gap in the literature by analyzing the fertility impact of the large and permanent income shock generated by a husband's job displacement.

This paper also aims to improve our understanding of the timing of fertility by estimating women's dynamic fertility response to the shock. Many theoretical models analyze fertility in a lifecycle framework because it provides another dimension along which households can adjust fertility in response to economic incentives. A household not only chooses how many children to have, but also when to have them. In these theoretical models, a credit-constrained household spaces out its children to balance the desire to have children early (so that the benefits can be enjoyed for more periods) with the desire to have children later when the husband's earnings are higher. Thus, these models predict that a flatter earnings trajectory will lead to accelerated fertility.² Analyzing the dynamic fertility response to a husband's job displacement, which typically has a permanent effect on earnings growth, provides a unique opportunity to test this prediction.

Across the world, influential fertility policies have been put into place based on the assumed relationship between fertility and economic well-being. In some developing countries, overpopulation is considered to be such a serious impediment to economic growth that laws restrict the number of children couples are allowed to have. China's one child per family policy highlights this end of the spectrum.³ Such laws are motivated by the belief that reducing population is a crucial step towards joining the rest of the world in prosperity. However, the efficacy of such policies is the source of much controversy—in part because little is known about the causal relationship between fertility and income.

On the other hand, with nearly one hundred countries below their replacement rates, there are many places where low fertility rates are of concern.⁴ While cultural factors are sometimes involved, the anxiety is usually driven by the idea that low fertility will result in an aging population in which support of the elderly places an undue burden on young workers. In response, many governments have implemented policies providing financial incentives for having children. Quebec, Canada led the way in 1988 by providing a "baby bonus" which paid up to nearly \$6,000 to families having a child. Despite the high costs of such programs, many countries have followed suit including Singapore (2001), Australia (2004), Italy (2005), and Poland (2006). Ulyanovsk, Russia has implemented what is perhaps the most original pronatalist policy, declaring September 12 the "Day of Conception." In addition to

1. Becker (1960) argues that knowledge about birth control might be a factor.

2. See Heckman and Willis (1975), Wolpin (1984), and Newman (1988).

3. Similarly in India, while there are no nationwide restrictions, some states have a two child per family policy for members of village councils and civil servants.

4. The 2008 CIA Factbook indicates 95 countries with total fertility rates below 2.1. The total fertility rate in the United States is just below the replacement rate.

getting the day off work, couples who “give birth to a patriot” nine months later on Russia’s Independence Day win prizes such as money, cars, and refrigerators (Stromova 2007).

Whether the concern is about high fertility rates or low fertility rates, it is important to understand how income can influence fertility both directly and indirectly. The results of this paper can be helpful in evaluating the consequences of policy interventions that alter family income. In the United States, for example, one of the main concerns with welfare policies is that families might use the additional income to have more children. The empirical evidence on this issue is mixed.⁵

Most studies that analyze the relationship between income and fertility make a weak case, if any, that their measure of income is exogenous with respect to their measure of fertility even though there are at least two reasons this is unlikely to be true. First, reverse causality is possible. For example, parents may adjust the amount they work or the amount they invest in human capital based on the number of children they plan to have.⁶ Second, there are likely to be unobservables that drive both income and fertility. For example, individuals with high discount rates may have low incomes and more unplanned children.

The ideal way to estimate the causal impact of family income on fertility would be via an experiment that randomly assigned individuals different levels of permanent income. The next best alternative might randomly add to or take away from couples’ incomes. While such experiments are unlikely to ever be conducted, this paper is motivated by the idea that husbands’ job displacements present a natural experiment of this type.⁷ It is well established that job losses result in large and permanent losses in earnings—the permanent nature of the shock has been documented using the same data set used in this study, the Panel Survey of Income Dynamics (as well as many other data sets).⁸ Prior studies have also shown that workers who will be displaced in the following year have low subjective probabilities

5. In separate reviews of the literature, Hoynes (1997) concludes that the effects are often insignificant or small in magnitude, while Moffitt (1998) concludes that welfare likely does impact fertility (although he explains that his conclusion is weakened by the lack of robustness).

6. Angrist and Evans (1998) provide the best evidence against this concern for husbands. Using the sex composition of the first two children as an instrument for having more than two children, they do not find a statistically significant impact on fathers’ earnings or employment. However, their estimates must be interpreted with caution as they only analyze the effect of having more than two children versus two children.

7. After the completion of an earlier draft of this paper, I discovered another work-in-progress (Amialchuk 2006) exploring the impact of husbands’ job losses on fertility. My paper differs from this paper, which finds delays in first and second births, in a number of important aspects. While Amialchuk (2006) uses a hazard model, I use a generalized difference-in-difference approach that allows me to control for fixed unobservable characteristics related to both fertility and the probability of having a displaced husband. I also do the analysis at the woman level rather than the couple level so that I can capture fertility effects that may be driven by marital separations following a job loss. I also omit women who were married prior to 1968 to reduce the probability that a husband’s first displacement is missed.

8. For studies using the PSID, see Rhum (1991), Stevens (1997), Stephens (2001), Stephens (2002), and Charles and Stephens (2004), among others. The earnings losses have also been documented using administrative data from Pennsylvania (Jacobson, LaLonde, and Sullivan 1993); the Displaced Worker Survey (de la Rica 1995); the Health and Retirement Study (Chan and Stevens 1999); the National Longitudinal Survey of Youth (Kletzer and Fairlie 2003); administrative data from Sweden (Eliason and Storrie 2006); and administrative data from Canada (Oreopoulos, Page, and Stevens 2008).

of job loss, which lends confidence to the notion that displacements are largely unanticipated.⁹

The main advantage of my research design is the transparent and plausibly exogenous source of variation in husbands' earnings. Like Jacobson, LaLonde, and Sullivan (1993) and Stevens (1997), I employ an individual fixed effects model to estimate the impact of a displacement so that the estimates will be unbiased even if there are fixed unobservable characteristics related to both displacements and the outcome of interest. In addition, I confirm that the results are robust to using a displaced sample that is limited to women whose husbands lost their jobs due to plant or business closures, since these types of displacements are more likely to be exogenous than the broader category which includes displacements resulting from husbands being laid off or fired.

Another important advantage of my research design is that the magnitude of the shock generated by a husband's job displacement is large. Generally, the income elasticity that has been implied by cross-sectional estimates is small. As a result, a large shock like that resulting from a husband's job displacement might be necessary to identify an effect.

I find that a shock to husbands' earnings impacts both short and long-run fertility. Consistent with dynamic models of fertility, I find that the shock accelerates the timing of fertility. Women are significantly more likely to have children in the years immediately following a husband's job displacement but significantly less likely to have children many years later. The immediate increase in fertility is outweighed by the subsequent decline, resulting in a net reduction in the number of children. This implies that the causal relationship between husbands' earnings and total fertility is positive.

While the most salient feature of a husband's displacement is clearly the large reduction in his earnings, an important caveat to my findings is that the estimated impact of a husband's job loss on fertility might be generated by aspects of the shock other than the loss of income. For example, a husband's job loss may impact fertility because of an impact on stress or women's work activity that is unrelated to the impact on income. However, researchers using panel data to study the impacts of unemployment on mental health have not found significant effects.¹⁰ And while it has been shown that husbands' displacements cause women to work more (Stephens 2002), it is reasonable to think that this effect is mostly driven by the desire to compensate for his lost earnings. Finally, while Charles and Stephens (2004) find that being laid off or fired increases the probability of divorce but that a job loss due to a plant or business closure does not affect divorce, I find evidence that both types of displacements lead to the fertility response described above. This indicates that the estimated response is not driven by displacements impact on marital dissolutions.

The remainder of this paper is organized as follows. Section II discusses household models of fertility. Section III reviews some of the related empirical literature.

9. Manski and Straub (2000) find a median subjective probability of 5 percent while Stephens (2004) finds a median of 20 percent.

10. See Björklund (1985) and Browning, Dano, and Heinesen (2006).

Section IV describes the data while Section V describes the empirical methodology. Section VI presents the main results. Section VII discusses the results in detail and Section VIII concludes.

II. Theoretical Considerations

In general, economists have used static models of the household to examine the determinants of completed fertility and dynamic models to examine the determinants of the timing of fertility. The following discussion, which focuses on the role of husbands' earnings in these models, is also separated along these lines. It draws from Becker (1991), Hotz, Klerman, and Willis (1997), and Jones, Schoonbroodt, and Tertilt (2008).

A. *Static Models of the Household Fertility Decision*

In a basic static model of the household, the relationship between income and child quantity can be thought of like the relationship between income and any other good. Thus, in such a model, the frequently observed negative relationship between income and fertility suggests that child quantity is an inferior good (assuming that the relationship is causal). Beginning with Becker's (1960) seminal work introducing the concept of child quality, theorists have expended a great deal of effort trying to reconcile the empirical relationship with the notion that children ought to be a normal good.

Becker and Lewis (1973) and Willis (1973) explain that the negative relationship can be generated by incorporating the interaction between child quantity and child quality into the household budget constraint. This interaction term captures the costs of child quality that must be spent on each child individually, such as college tuition. In this model, increasing either quantity or quality increases the shadow cost of the other. For example, sending children to college rather than high school makes additional children more expensive. Similarly, having an additional child makes it relatively expensive to send the children to college. The effects of income are not straightforward in this type of model because, through the direct impact on child quantity and child quality, income effects induce price effects. Consider a reduction in income under the assumptions that both child quantity and child quality are normal goods although the income elasticity is greater for quality than for quantity. Initially, the income loss will cause a "pure income effect" (holding shadow prices constant) that reduces both quantity and quality. However, the relatively large reduction in quality will cause the shadow price of child quantity to decrease and thus induce substitution of quantity for quality. As a result, the *observed* impact of the reduction in income may be positive even under the assumption that child quantity is a normal good.

Alternatively, one can generate a negative relationship between income and fertility by incorporating parental time into the production function for children, as in Mincer (1963), Becker (1965), and Willis (1973), among others. In such models, the cost of children includes a parent's wages since having a child necessitates time out of the labor market. Thus, a reduction in wages might simultaneously reduce earnings and increase fertility. While this approach is most commonly used to ex-

plain the negative relationship between women's wages and fertility, this type of model can also generate a negative relationship between husbands' earnings and fertility, even if the production of children does not require a husband's time. Specifically, assortative matching could produce high wage couples in which the husbands have high earnings and the wives have low fertility because of the high opportunity cost of their time.¹¹

B. Dynamic Models of the Household Fertility Decision

While static models of the household fertility decision have the advantage of relative simplicity, dynamic models are attractive because they can capture the effects of economic incentives that might affect the timing of fertility even if they do not affect completed fertility. The dynamic household optimization problem involves maximizing the stream of household utility by jointly choosing the wife's allocation of time, the consumption of market goods, and the timing of children. All else equal, the optimal choice generally entails the smoothing of consumption goods.

In these models, variation in the timing of children can be driven by lifecycle variation in household income if capital markets are imperfect.¹² On one hand, the household also has an incentive to have children early in the lifecycle so that they can be enjoyed for more periods. However, a credit-constrained household will have an incentive to postpone childbearing until late in the lifecycle when the husband's earnings are highest. As shown in Heckman and Willis (1975), Wolpin (1984), and Newman (1988), these opposing incentives can generate the spacing of children, as opposed to other models that predict that a household will have all of its children at the beginning or the end of its lifecycle. This type of model predicts that a flatter trajectory for the husband's earnings will reduce the incentive to delay childbearing and thus lead to accelerated fertility.

III. Previous Empirical Studies of the Income-Fertility Relationship

Most of the empirical literature analyzing the determinants of fertility has focused on the response to changes in the costs of having children rather than the response to changes in income.¹³ This is not surprising since many public policies generate variation in the costs associated with having a child. There exist far fewer

11. Instead of assuming assortative matching, the negative relationship between husbands' earnings and fertility can be generated by assuming the value of time in nonmarket activities is increasing in husbands' earnings. Specifically, in a model in which all consumption entails both time costs and money costs, an increase in money (husband's earnings) will cause substitution away from time-intensive consumption (children) towards money-intensive consumption.

12. Variation in the timing of children can also be generated by lifecycle variation in the costs of children.

13. For example, researchers have studied the fertility effects of the tax treatment of dependents (Whittington, Alm, and Peters 1990; Zhang, Quan, and Van Meerbergen 1994; Dickert-Conlin and Chandra 1999), the baby bonus programs described in the introduction (Duclos, Lefebvre, and Merrigan 2001; Milligan 2005; Gans and Leigh 2007), and maternal education (McCrary and Royer 2006; Black, Devereux, and Salvanes 2008).

empirical studies of the relationship between income and fertility because it is difficult to find exogenous variation in income. Even the best studies of this elusive relationship face challenges to identification.

Heckman and Walker (1990) estimate the relationship between male earnings and fertility using retrospective fertility data from Sweden. They find that higher male earnings are associated with shorter times to conception and increased total fertility. One disadvantage of the study is the lack of individual-level earnings data—the authors instead use cohort averages which could attenuate their estimated effects. In addition, their earnings measure may not be exogenous since fertility might affect aggregate wages through its effect on the labor supply. Merrigan and St-Pierre (1998) find similar results using the same methodology with data from Canada.

Schultz (1985) uses an instrumental variables strategy to try to isolate the causal relationship. He uses world prices of grain and butter as instruments for wages in his analysis of county-level fertility rates in Sweden from 1860–1910 and finds that increases in male wages increase fertility at younger ages but reduce fertility at older ages; he finds that these effects offset one another so that there is no impact on completed fertility. However, it is not clear that Schultz's instrument satisfies the usual exclusion restriction. For example, changes in the price of food may impact fertility because it changes the cost of feeding an additional family member.

Dehejia and Lleras-Muney (2004) explore how fertility responds to the business cycle. While the authors do not find significant effects, their point estimates indicate that mothers are less likely to have children during recessions. The research design, however, cannot distinguish between the effects of the potential mothers' unemployment and their husbands' unemployment. Given that theoretical models tend to associate a husband's employment with family income while women's employment is associated with both family income and the cost of having children, we are left wondering: are the authors' identifying the fertility impact of changes to income or changes to the cost of having children?¹⁴

This brief review of the literature reveals the key challenge facing the literature on the relationship between income and fertility: finding exogenous variation in family income. This paper seeks to address this concern by focusing on the fertility response to an income shock that I will argue is exogenous to other parental characteristics once mother fixed effects are included.

IV. Data

This paper uses data from the 1968–1997 waves of the Panel Survey of Income Dynamics (PSID) including its Childbirth and Adoption History Supplement (CAHS). The PSID is a longitudinal study that began as a nationally representative sample of households in 1968, with an additional oversample of low-income families. The survey has continued to follow these individuals and their children as they form new households. I use data from both original samples (and their split-offs) and use PSID weights. The CAHS includes retrospective fertility

14. Further, the effects could be driven by other aspects of local unemployment such as the increased risk of a job loss, health effects from production externalities, etc.

histories, with children's year and month of birth, for all individuals of childbearing age surveyed in the PSID in 1985 or later. The fertility variable I focus on is an indicator for whether or not a child is conceived in a given year, assuming that the pregnancy lasts nine months. I restrict the sample of women's observations to person-years between ages 14 and 42 after which no women in the sample have observed births.

My definition of displacement follows Stevens (1997) and others who have used the PSID to study the impacts of job loss. Displacements are identified based on the response to a question asking individuals who are not working, and those who began their current job within the last year, "what happened to your previous job?" Initially, I define an individual as displaced in the previous year if his last job ended due to a plant or business closing or due to being laid off or fired.¹⁵ I also analyze the effect focusing only displacements resulting from plant or business closures.

Topel (1990) explains that the survey might miss displacements since the question focuses on an individual's last job. That is, we might wrongly categorize an individual as not displaced if he has had and left another job after his displacement and before he is surveyed. Since this concern is likely greater for the years following 1997 when the PSID changed to a biennial format, data from 1999 and later are not used.

Unlike subsequent years, the 1968 survey asks those who began working for their current employer within the last *ten years* the reason they left their last job. This aspect of the survey has two consequences for my sample. First, I restrict the sample to women who married in 1968 or later. This restriction removes women who I cannot observe from the beginning of their marriage and reduces the probability that I miss a husband's first displacement.¹⁶ Second, since the timing of the displacement cannot be ascertained, women whose husbands report a displacement in 1968 are also excluded from the sample.

The displaced group is the sample of women whose husbands first experience a displacement between 1969 and 1997. Because my regressions include an indicator for two years prior to displacement, I limit the analysis sample to 1968–1995 since we do not know whether or not a displacement occurs two years into the future for 1996 and 1997 data. While a woman's husband may experience multiple displacements, I consider the year in which he first experiences a displacement the "displacement year." This is important because it has been shown that initial displacements predict future displacements (Stevens 1997). Thus, subsequent displacements should not be considered exogenous. For this reason, I also drop the handful of women whose husbands are displaced before their marriage. Once these sample restrictions have been made, the control group includes 2,594 women and the dis-

15. It is not actually clear from the survey whether the job loss occurs in the current or previous year. Since I assume that the displacement occurred in the previous year, the coefficient estimate on an indicator for t years following the displacement may partly capture the effect of being $t+1$ years following the displacement.

16. In results not shown in this paper, but available upon request, I find that this restriction improves the match between the treatments and controls. Further, these results show that the estimated impact of displacements on husbands' earnings is much smaller for women married prior to 1968. This finding suggests that, if this restriction is not made, the sample will include women whose husbands had prior job losses that are missed.

placement group includes 1,548 women. The more narrowly defined displacement group, which only includes women whose husbands' job losses were due to closures, includes 413 women.

V. Empirical Methodology

The analysis will be conducted in two parts. First, I will document that job displacements lead to permanent reductions in husbands' earnings. I will then estimate the dynamic fertility response to the shock.

A. Estimating the Impact of Displacements on Husbands' Earnings

Following Jacobson, LaLonde, and Sullivan (1993) and Stevens (1997), I estimate the impact of job displacements on husbands' earnings using the following regression equation:

$$(1) \quad \text{Earnings}_{it} = D_{it}\delta + X_{it}\beta + \alpha_t + \alpha_i + u_{it}$$

where Earnings_{it} is the husband's log earnings for individual i in year t , D_{it} is a vector of indicators indicating a husband's displacement in a future, current, or previous year, α_t are year fixed effects, α_i are individual fixed effects, and u_{it} is a random error term. X_{it} can include a variety of time-varying individual variables but is limited to a quadratic in age. In estimating this model, D_{it} includes indicators for two years prior to displacement, one year prior to displacement, the year of the displacement, and indicators for subsequent years following a displacement—the omitted category is three or more years prior to displacement and never having a husband displaced. Previous studies have shown that it is important to include indicators for years prior to displacement since earnings begin to fall below their expected levels prior to the actual event. The individual fixed effects control for permanent unobservable characteristics that may be related to both husbands' earnings and the probability of displacement. With the individual fixed effects, year fixed effects, and post-displacement indicator variables, this model is a generalized difference-in-difference model.¹⁷

B. Estimating the Impact of Displacements on Fertility

The primary fertility variable I analyze is an indicator for whether or not a child (who will be carried to birth) is conceived in a given year. The logit model is a natural choice for such a dependent variable. The following logit model is estimated:

$$(2) \quad \Pr [y_{it} = 1 \mid D_{it}, X_{it}, \gamma_t] = \frac{\exp(D_{it}\psi + X_{it}\beta + \gamma_t)}{1 + \exp(D_{it}\psi + X_{it}\beta + \gamma_t)}$$

where y_{it} is an indicator for whether or not individual i has a child in year t , D_{it} is defined as it is in Equation 1, and γ_t are age fixed effects. X_{it} can include a variety

17. This model is also used to estimate the impact on husbands' weeks worked, women's log earnings, and women's weeks worked.

of time-varying individual variables but is limited to a quadratic in year. The vector ψ traces the impact of the displacement over time.

Equation 2 will yield biased estimates of the impact of a husband's displacement on fertility if there are unobservable characteristics related to both fertility and the probability of having a displaced husband. For this reason, it is potentially important to follow the approach that is conventionally used to estimate the impact of a displacement on earnings and include individual fixed effects in the model, although doing so will reduce the precision of the estimates. Thus, the preferred model is:

$$(3) \quad \Pr[y_{it} = 1 \mid D_{it}, X_{it}, \gamma_t, \gamma_i] = \frac{\exp(D_{it}\psi + X_{it}\beta + \gamma_t + \gamma_i)}{1 + \exp(D_{it}\psi + X_{it}\beta + \gamma_t + \gamma_i)}$$

where γ_i are individual fixed effects and the rest of the notation is the same as in Equation 2.

In general, fixed effects estimation is not possible in binary panel models because of the incidental parameters problem—logit is an exception. The conditional logit model deals with the incidental parameters problem by conditioning on a sufficient statistic, the number of observed positive outcomes, rather than estimating individual-specific constant terms. There are a few features of the conditional logit model that should be noted. First, only individuals with variation in the dependent variable can be used. That is, individuals who are never observed having a child and individuals who are observed having a child in every period must be omitted from the analysis. As a result, the conditional logit model cannot capture the type of situation in which a woman planned to have children but never does due to her husband's displacement. Second, predicted probabilities and, hence, marginal effects cannot be estimated since the individual fixed effects are not actually estimated. Odds ratios are displayed instead. In addition to Equations 1 and 2, I estimate linear probability models with the same sets of regressors.

C. Estimating the Total Impact on Fertility

To estimate the total impact on fertility, I use the predicted probabilities based on the estimates from the models discussed in the previous section (when it is possible). First, I sum the post-displacement predicted probabilities for those who experience a husband's displacement. For each woman, this is her predicted number of additional children for the years following her husband's displacement. Using the same observations and parameter estimates, I sum the predicted probabilities with the displacement indicators "turned off" to obtain the counterfactual. Thus, for each woman who experiences a husband's displacement, we have the "true" predicted number of additional children and the counterfactual. The average difference provides an average treatment on the treated estimate of the impact on total fertility. I calculate a bootstrap estimate of the standard error to conduct inference.

VI. Results

A. Sample Statistics

Figure 1 compares the average log husbands' earnings for the control group to the earnings of the displaced group. The dashed line plots the age-husbands' earnings

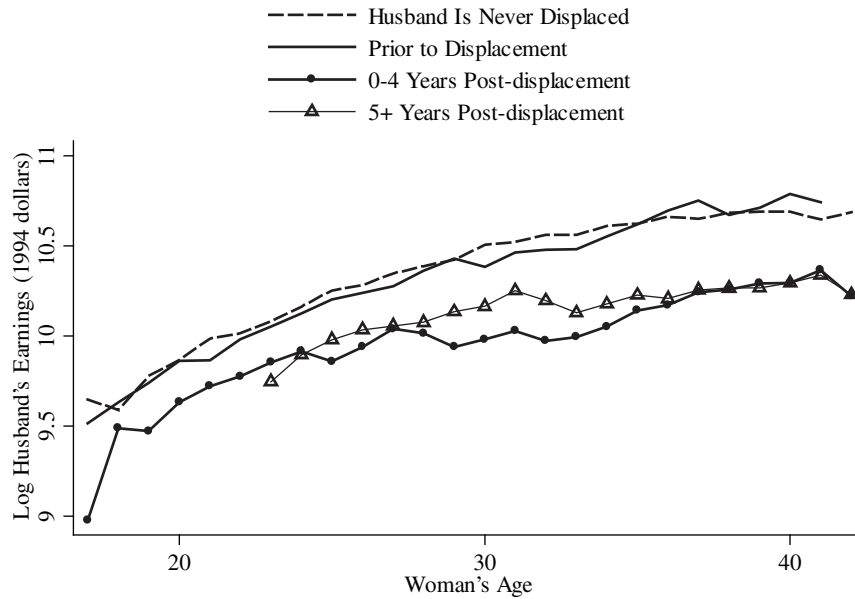
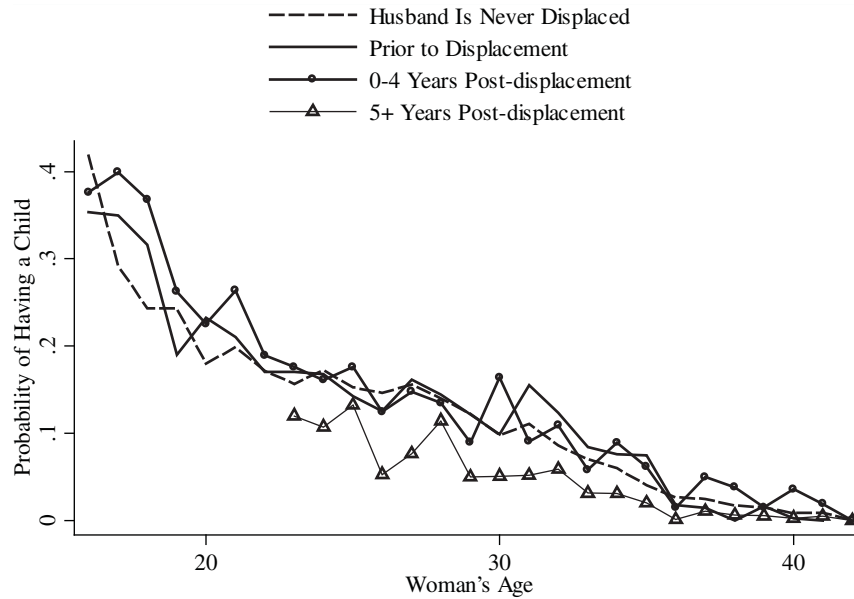


Figure 1
Age-husband's Earnings Profile by Husbands' Displacement Status

Notes: Data are from the 1968–1995 waves of the PSID, limited to ever married women. Averages calculated using less than 40 observations are not plotted. The profile for women 5+ years following a husband's displacement begins later than the others because there are few women who are younger than 18 when their husband is first displaced.

profile for the control group. The solid line plots the profile for the displaced group but only uses person-years prior to a husband's displacement—as such, it is the predisplacement profile of husbands' earnings. These two profiles are nearly identical which suggests that, at any given age, the average predisplacement husbands' earnings of the treated group are very close to the husbands' earnings of the control group. The figure also shows profiles for women using person-years zero to four years following the shock and five or more years following the shock. Both of these profiles lie below the first two, demonstrating the loss in husbands' earnings resulting from displacements. Further, the fact that the two post-displacement profiles are similar indicates the persistence of the shock.

Figure 2 presents similar profiles for the probability of having an additional child while Figure 3 presents profiles for the cumulative number of children at a given age. As with Figure 1, the treatment group's predisplacement profile matches the control group's profile closely in each of the figures. The positions of the two post-displacement profiles in Figure 2 suggest that a husband's displacement increases the probability of having a child in shortly after the event and reduces the probability later on. Similarly, Figure 3 also suggests this pattern as women who have experienced a husband's displacement tend to have relatively more children than the con-

**Figure 2**

Age-probability of Conception Profile by Husbands' Displacement Status

Notes: Same as Figure 1.

trols while the profiles converge as women near the end of their childbearing years. This suggests that the impact of an income shock is largely about timing as the controls “catch up” in later years.

Table 1 presents sample means. For time-varying variables, the means are calculated using all person-year observations for the control group and person-years three or more years prior to the husband's first displacement for the displaced group. This comparison is motivated by the idea that the predisplacement characteristics of women in the treatment group should be similar to the characteristics of women in the control group if husbands' displacements are exogenous. However, because this sample selection artificially reduces the average age of the displaced group, the means are not informative for time-varying variables. To remedy this issue, the third column calculates the estimated difference, with controls for age and year fixed effects for time-varying variables.

The racial makeup of the women and their husbands is roughly the same across groups. Approximately 90 percent of the sample is white and 10 percent is black. The average age at the time of first marriage is 22.2 for the control group which is 1.6 years later than the average for the displaced group. Both groups of women are about three years younger than their first husband.

As I discussed above, comparing the predisplacement characteristics of the displaced group to the characteristics of the control group requires controls for age and

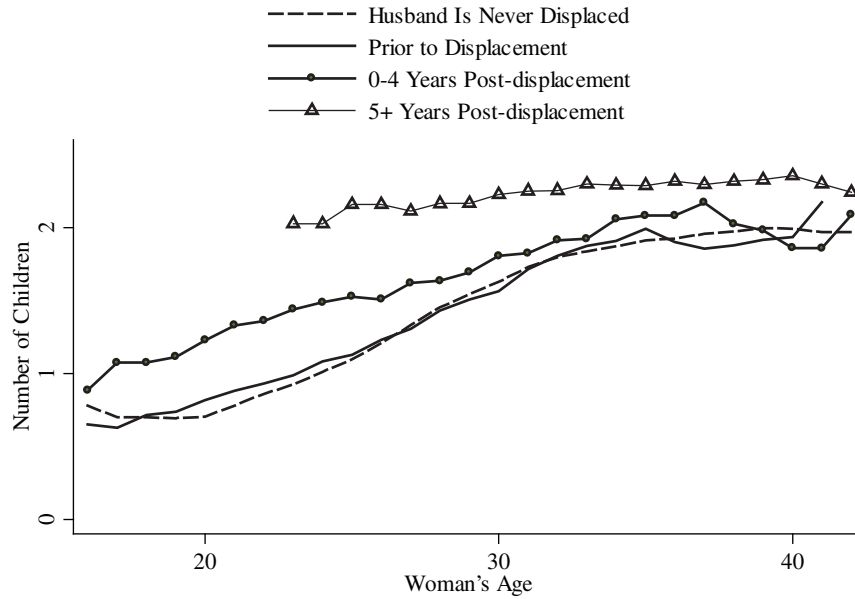


Figure 3
Age-children Profile by Husbands' Displacement Status

Notes: Same as Figure 1.

year. After making the appropriate adjustment, the differences in husbands' earnings, weeks worked, and hours worked per week are not statistically significant. Further, the women in the two groups have similar numbers of children and probabilities of having additional children. At the same time, there is evidence that the controls are more educated than women in the displaced group—they are eight percentage points more likely to have more than a high school education after adjusting for their age and year. Given the educational disparity, it is not surprising that control women's average adjusted earnings and family income are \$1,333 and \$2,929 greater than those in the displacement group, respectively. The differences in adjusted weeks worked per year, adjusted hours worked per week, and the adjusted probability of working are not statistically significant at the five percent level.

In summary, the treatments and controls are similar in many areas—most importantly, the women in the two groups are similar in their fertility and their husbands' labor market performance. On the other hand, the differences in educational attainment and age at marriage might be a concern since they could drive differences in fertility. However, my fertility analysis controls for individual fixed effects which will control for all fixed characteristics related to both fertility and the probability of having a displaced spouse.

Table 1
Women's Sample Means By Husbands' Displacement Status

	Never Displaced ^a	Displaced ^b	Adjusted Difference ^c	Standard Error
Husband's Earnings	32,715	25,632	692	(1,021)
Husband's Weeks Worked	43.8	41.9	0.3	(0.4)
Husband's Hours/Week Worked	44.2	42.4	0.5	(0.5)
Family Income	54,057	41,447	2,922	(1,184)
Husband's demographics				
Age	31.3	26.7	0.1	(0.2)
White	0.89	0.88	0.01	(0.01)
Black	0.09	0.11	-0.02	(0.01)
Own demographics				
Age	28.2	23.2	—	—
Age when first married	22.2	20.6	1.6	(0.2)
White	0.90	0.89	0.01	(0.01)
Black	0.08	0.10	-0.01	(0.01)
At least high school education	0.92	0.83	0.04	(0.02)
More than high school education	0.56	0.38	0.08	(0.03)
Own labor market variables				
Earnings	15,075	10,588	1,333	(606)
Weeks worked	29.4	25.2	1.9	(1.0)
Hours per week worked	27.2	24.8	1.3	(0.9)
Worked last year	0.69	0.68	0.03	(0.02)
Fertility in a given year				
Had an additional child	0.13	0.20	-0.01	(0.01)
Number of children	1.22	0.93	-0.05	(0.05)
Sample size	2,594	1,548		

Notes: Means and difference are weighted using the family weight in the last year with first husband. Money values are in 1994 dollars.

a. For time-varying variables, averages include all observations for women who never have a displaced spouse.

b. For time varying variables, averages use data for 3 or more years prior to displacement for women with a displaced spouse.

c. For time-varying variables, the difference is adjusted using age and year fixed effects. Standard errors are clustered on the individual.

B. Estimated Impact of Displacements on Husbands' Earnings

I focus on the effect of a husband's job loss on his earnings rather than family income because husbands' earnings are more likely to be exogenous to the impacts on fertility. For example, I do not want to capture income effects due to changes in

women's earnings if the change in women's earnings is driven by changes in fertility. Further, theoretical models often predict that different sources of income will have different impacts on fertility. Again, the most important distinction tends to be between women's earnings and other sources of income and, for this reason, empirical studies often focus on male earnings as the measure of family income.

Column 1 of Table 2 presents regression estimates of the effect of a displacement on husbands' log earnings.¹⁸ The estimates are based on a model with individual fixed effects, year fixed effects, and a quadratic in the husband's age in addition to the set of displacement indicators (Equation 1). Like previous studies, I find evidence that the immediate impact and the long-term impact are both large. The coefficient on the indicator for one year following the displacement suggests that earnings are reduced by 31.3 percent.¹⁹ The estimated impacts over the subsequent years suggest very little recovery. In fact, while not very precise, the estimated impact eight or more years following the displacement is as large as the estimated impact in the year following the displacement.²⁰

Like other studies, I also find that men's earnings begin to fall below their expected levels shortly before the displacement occurs. My estimates imply a 12.1 percent reduction in the year prior to the displacement. Given that many displaced workers initial jobs are in distressed firms, this finding is not surprising. While the displacement literature provides little evidence of the mechanism driving this result, potential explanations include wage stagnation, reduced overtime, and temporary layoffs.

To some extent, the fact that earnings fall below their expected levels before the job loss occurs raises a question about whether displacements might be a function of an individual's productivity. On the other hand, there is only weak evidence that they begin to fall below their expected levels two years prior to the job loss and, in results not shown, I have found no evidence that they begin to fall three years prior to the job loss. Further, the pattern is the same if I focus only on displacements that occur due to a plant or business closing, as in column 2. Specifically, women whose husbands' displacements are due to being laid off or fired are not used in this analysis and the estimates still indicate that earnings drop below their expected levels in the year prior to the displacement.

Finally, the estimated impacts on husbands' weeks worked, women's earnings, and women's weeks worked are displayed in Table A in the appendix. These estimates suggest that the long-term impact on husbands' earnings is largely driven by reduced wages since the long-term impact on his weeks worked is small. Like Stephens (2002), I find that women work more following a husband's displacement.

18. Note that, since the analysis is from the perspective of the woman, it does not track a husband's earnings if they are in different households and does include a new husband's earnings if the woman remarries.

19. The percentage effect on earnings is computed as $e^{\beta} - 1$.

20. The very large impact many years following the displacement might also suggest that those who do not experience a job loss may have steeper earnings trajectories than those who are displaced. In results not shown here, but available upon request, I have estimated the model with individual trends—while the estimated impact eight or more years following the displacement is smaller (-21.9 percent), it is not significantly different from the shown estimate. The p-value from the Hausman test is 0.361.

Table 2
Estimated Impact of Displacement on Husband's Log Earnings

	All Displacements (1)	Closures Only (2)
Displacement year - 2	-0.050* (0.029)	-0.094* (0.055)
Displacement year - 1	-0.129*** (0.037)	-0.187*** (0.069)
Displacement year	-0.322*** (0.041)	-0.336*** (0.085)
Displacement year + 1	-0.375*** (0.047)	-0.370*** (0.087)
Displacement year + 2-3	-0.277*** (0.044)	-0.273*** (0.081)
Displacement year + 4-5	-0.207*** (0.049)	-0.235*** (0.091)
Displacement year + 6-7	-0.275*** (0.052)	-0.275*** (0.098)
Displacement year + 8+	-0.380*** (0.054)	-0.409*** (0.089)
Individual observations	4,042	2,917

Notes: All regressions include individual fixed effects, year fixed effects, and a quadratic in husband's age. The omitted category is never having a displaced husband or three or more years prior to a husband's displacement.

Standard error estimates, clustered on the individual, are displayed in parentheses. Regressions are weighted using the family weight from the last year women are observed with their first husband.

* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

These results confirm that a husband's job displacement has a large permanent impact on his earnings and suggest that the reduction is driven mostly by reduced wages rather than reduced work activity. Because the income shock has such important impacts on a couple's labor market outcomes, it would not be surprising to find impacts on fertility decisions. These impacts are explored in the next section.

C. Estimated Impact of a Husband's Displacement on Fertility

Table 3 presents estimates of the impact of a husband's displacement on the probability of having a child. Columns 1 and 2 present odds ratios and marginal effects, respectively, based on a logit model (Equation 2) with age fixed effects and a quadratic in year in addition to the set of displacement indicators. Note that since odds ratios are e^ψ , where ψ is the estimated coefficient, an odds ratio of one indicates no effect, greater than one indicates increased odds, and less than one indicates reduced odds. Column 3 presents coefficients based on a linear probability model with the

Table 3
Estimated Impact of Any Husband's Displacements on the Probability of Having an Additional Child

<i>model:</i>	Logit Odds Ratios (1)	Logit Marginal Effects (2)	LPM Marginal Effects (3)	Logit Odds Ratios (4)	LPM Marginal Effects (5)
Displacement year - 2	1.160 (0.127)	0.011 (0.009)	0.019 (0.015)	1.103 (0.145)	0.012 (0.018)
Displacement year - 1	1.046 (0.105)	0.003 (0.007)	0.006 (0.013)	0.983 (0.124)	-0.005 (0.016)
Displacement year	1.299*** (0.116)	0.020*** (0.008)	0.036*** (0.013)	1.194 (0.146)	0.028* (0.017)
Displacement year + 1	1.167* (0.104)	0.011 (0.007)	0.020* (0.012)	1.125 (0.139)	0.013 (0.016)
Displacement year + 2-3	1.225*** (0.083)	0.015*** (0.005)	0.025*** (0.009)	1.279** (0.148)	0.024* (0.013)
Displacement year + 4-5	0.858* (0.073)	-0.010* (0.005)	-0.013* (0.008)	0.990 (0.135)	-0.014 (0.013)
Displacement year + 6-7	0.745*** (0.084)	-0.018*** (0.006)	-0.023*** (0.008)	0.918 (0.143)	-0.020 (0.013)
Displacement year + 8+	0.389*** (0.042)	-0.049*** (0.004)	-0.038*** (0.003)	0.570*** (0.094)	-0.023* (0.012)
Average Total Effect on the Treated	-0.145*** (0.040)	-0.145*** (0.040)	-0.149*** (0.040)	—	-0.098 (0.117)
Individual fixed effects	no	no	no	yes	yes
Individual observations	4,142	4,142	4,142	3,238	3,238

Notes: The dependent variable is an indicator for having a child in a given year. All columns include age fixed effects and a quadratic in the year. The omitted category is never having a displaced husband or three or more years prior to a husband's displacement. Standard error estimates, clustered on the individual, are displayed in parentheses. Regressions are weighted using the family weight from the last year women are observed with their first husband. The estimation of the average total effect is described in Section V.C. The odds ratios in columns 1 and 4 are e^{β} —an odds ratio of one indicates no effect, greater than one indicates increased odds, and less than one indicates reduced odds.

* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

same controls. Each set of estimates implies that the odds of having a child are significantly higher in the first four years following the job loss before the effect reverses and the odds are significantly reduced. That is, the negative shock to a husband's earnings accelerates the timing of children.

A possible concern with these estimates is that the estimated impact might arise as a result of unobservable characteristics that are related to both fertility and the probability of having a displaced husband. If such unobservable characteristics exist, then models that include individual fixed effects will provide unbiased estimates if they are fixed over time. Column 4 presents odds ratios based on an individual fixed effects logit model (Equation 3) while Column 5 presents coefficient estimates based on a linear probability model with individual fixed effects (in addition to the controls used in Columns 1–3). Like the estimates based on the models without individual fixed effects, these estimates suggest an increase in the odds of having children in the years immediately following the displacement and a subsequent reduction in the odds many years following the event. The magnitudes are slightly smaller and, not surprisingly, the standard error estimates are larger in these models due to both the individual fixed effects and the fact that individuals without variation in the dependent variable cannot be used in a fixed effects logit model.²¹ Nevertheless, the increased odds soon after the event and the reduced odds eight or more years following the event are statistically significant.

While the estimated dynamic fertility response does not itself tell us whether the net impact on fertility will be positive or negative, the estimated model can be used to estimate the net fertility impact with the approach described in Section VC. For a woman who has a husband displaced at some time, her predicted total (post-displacement) fertility can be obtained by adding up her predicted probabilities of having a child in each year following the displacement. Similarly, a counterfactual can be estimated by the same method after turning the displacement indicators off. The average difference between these estimates provides an estimate of the average impact on the total fertility of the treated.

These estimates, based on each of the models discussed above, are displayed in Table 4 beneath the estimates of the dynamic response. All of the estimates suggest that a husband's job loss has a net negative impact on fertility. The estimated effects based on the models without individual fixed effects are statistically significant, indicating a 0.14 reduction in the total number of children. While predicted probabilities cannot be obtained from the fixed effects logit model, the linear probability model with individual fixed effects indicates that a husband's displacement reduces total fertility by 0.10 children. However, the individual fixed effects greatly reduce the precision of the estimate and it is not statistically significant.

Table 4 presents estimates that focus on the effects of a husband's displacement resulting from a plant or business closure. While omitting women whose husbands lost their jobs due to being laid off or fired substantially reduces the sample of women experiencing a husband's displacement (from 1,548 to 413), the advantage of this approach is that job losses resulting from plant or business closures are more

21. Results without individual fixed effects based on this sample are very similar to the full sample results displayed in columns 1–3.

Table 4
Estimated Impact of a Husband's Displacement Due to a Closure on the Probability of Having an Additional Child

<i>model:</i>	Logit Odds Ratios (1)	Logit Marginal Effects (2)	LPM Marginal Effects (3)	Logit Odds Ratios (4)	LPM Marginal Effects (5)
Displacement year - 2	0.847 (0.184)	-0.011 (0.014)	-0.017 (0.022)	0.752 (0.197)	-0.037 (0.028)
Displacement year - 1	1.092 (0.203)	0.007 (0.014)	0.010 (0.022)	0.993 (0.232)	-0.002 (0.028)
Displacement year	1.150 (0.206)	0.011 (0.015)	0.016 (0.022)	1.043 (0.234)	0.006 (0.028)
Displacement year + 1	0.947 (0.181)	-0.004 (0.013)	-0.006 (0.020)	0.926 (0.236)	-0.013 (0.028)
Displacement year + 2-3	1.236* (0.150)	0.017 (0.010)	0.023 (0.014)	1.262 (0.255)	0.021 (0.022)
Displacement year + 4-5	0.992 (0.157)	-0.001 (0.011)	0.000 (0.015)	1.071 (0.239)	-0.007 (0.021)
Displacement year + 6-7	0.834 (0.167)	-0.012 (0.013)	-0.014 (0.015)	0.943 (0.249)	-0.020 (0.021)
Displacement year + 8+	0.443*** (0.083)	-0.044*** (0.007)	-0.030*** (0.005)	0.606* (0.156)	-0.020 (0.018)
Average total effect on the treated	-0.098 (0.061)	-0.098 (0.061)	-0.099 (0.061)	—	-0.104 (0.214)
Individual fixed effects	no	no	no	yes	yes
Individual observations	3,007	3,007	3,007	2,284	2,284

Notes: Same as Table 3.
 * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

plausibly exogenous than those resulting from being laid off or fired. These estimates provide less evidence of an increase in the odds of having a child in the year the job loss occurs but continue to indicate increased odds of having children 2–3 years following the event and reduced odds of having children six or more years after the event. Further, the estimates indicate that a husband's displacement reduces total fertility by approximately 0.10 children. While the estimates based on this sample are rarely statistically significant as a result of the small sample size, they lend confidence to the results based on the full sample which are similar but more precise.

To further explore the net effect of a job displacement on fertility, Table 5 uses the cumulative number of children born as of each given year as the dependent variable instead of an indicator for having a child in a given year. Here, the coefficients on the displacement indicators can be thought of as measuring the cumulative impact on the "stock" of children whereas the estimates presented in Table 4 measured the impact on the "flow." As with the prior estimates, age fixed effects and a quadratic in year are included in the models. Although the estimates that do not control for individual fixed effects (Columns 1 and 2) suggest that displacements lead to a long-term increase in the number of children, estimates controlling for individual fixed effects (Columns 3 and 4) are similar to the prior estimates.²² The positive coefficients on indicators for 2–3, 4–5, and 6–7 years following the displacement imply that the displacement causes an increase in the number of children soon after the event while the coefficient on the indicator for eight or more years following the event implies that the increase is only temporary.

It should be noted that the estimated coefficient on the indicator for eight or more years after a husband's job loss suggests that the long-term impact on fertility is smaller than the prior estimates (and positive when only closures are considered). However, this is due to the fact that this coefficient combines the cumulative impact as of 8–9 years after the event with the cumulative impact at later years. If the vector of displacement indicators instead includes indicators for 8–9 years following the event and 10 or more years following the event, the coefficient on 10 or more years following the event is negative for both samples.²³

VII. Discussion

These results suggest that negative income shocks have substantive impacts on the timing of fertility. The overall pattern, with increases in fertility in the years immediately following a husband's job loss and reductions many years later, is robust—the pattern is seen in both logit models and linear probability models and using both a broad definition of displacement and a definition that only includes plant or business closings.

This pattern of estimates is consistent with dynamic models of fertility. In these models, credit-constrained households have an incentive to delay childbearing until

22. As one would expect, the estimates that do not control for individual fixed effects mirror the graphical analysis of means in Figure 3.

23. Specifically, the coefficient is -0.125 (0.068) for the full sample and -0.044 (0.105) for the sample that only uses closures.

Table 5
Estimated Impact of a Husband's Displacement on the Cumulative Number of Children

	All Displacements (1)	Closures Only (2)	All Displacements (3)	Closures Only (4)
Displacement year - 2	0.062 (0.044)	0.007 (0.088)	-0.007 (0.033)	-0.019 (0.051)
Displacement year - 1	0.058 (0.041)	0.018 (0.079)	-0.007 (0.037)	0.007 (0.056)
Displacement year	0.082** (0.040)	0.075 (0.080)	-0.002 (0.042)	0.010 (0.061)
Displacement year + 1	0.169*** (0.040)	0.119 (0.081)	0.038 (0.044)	0.013 (0.064)
Displacement year + 2-3	0.313*** (0.041)	0.244*** (0.078)	0.107** (0.047)	0.083 (0.068)
Displacement year + 4-5	0.432*** (0.044)	0.386*** (0.084)	0.138*** (0.053)	0.140* (0.075)
Displacement year + 6-7	0.520*** (0.049)	0.504*** (0.092)	0.134** (0.058)	0.181** (0.087)
Displacement year + 8+	0.541*** (0.060)	0.493*** (0.106)	-0.048 (0.064)	0.011 (0.098)
Individual fixed effects	no	no	yes	yes
Individual observations	4,142	3,007	4,142	3,007

Notes: The dependent variable is the cumulative number of children the woman has had to date in a given year. All columns include age fixed effects and a quadratic in the year. The omitted category is never having a displaced husband or three or more years prior to a husband's displacement. Standard error estimates, clustered on the individual, are displayed in parentheses. Regressions are weighted using the family weight from the last year women are observed with their first husband.

* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

the husband's earnings are higher. Because job displacements reduce a husband's earnings growth, it then reduces the incentive to delay children. It is important to note that this theoretical prediction involves the slope of a husband's earnings trajectory rather than the absolute level of his earnings. A different type of shock that affected a husband's earnings in a different manner might have different effects.²⁴

Another possible explanation for the observed pattern involves the effect of displacements on husbands' time. While the reduction in work activity immediately following a job loss is not especially large, it might make for a relatively convenient

24. For this reason, my results are not necessarily inconsistent with Heckman and Walker (1990) and Merrigan and St-Pierre (1998) who find that husbands' earnings are negatively related to the time to conception.

Table 6
Estimated Cross-sectional Relationship Between Spouse's Earnings and the Number of Children

	(1)	(2)
Spouse's log earnings	-0.072*** (0.025)	-0.058** (0.025)
Own log earnings		-0.283*** (0.013)
Sample size	4,042	3,855
Average of dependent variable	1.57	1.45

Notes: Regressions include age and year fixed effects. Standard error estimates, clustered on the individual, are displayed in parentheses. Regressions are weighted using the family weight from the last year women are observed with their first husband.

* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

time for a woman to have a child. It is unfortunate that this hypothesis cannot be ruled out because, if true, the implications for economic theory are rather serious—as Hotz, Klerman, and Willis (1997) explain in an in-depth review of the literature, with all of the various features that have been integrated into lifecycle models of household fertility, none consider the time allocation decisions of fathers.

My estimated impact on *total fertility* is very similar to that of Heckman and Walker (1990), although it is not statistically significant when individual fixed effects are included. Heckman and Walker estimate that a 12 percent increase in male wages increases total fertility by 0.05 children and I find that a long-term 32 percent reduction in husbands' earnings is accompanied by a 0.10 reduction in total fertility.

My estimates provide an opportunity to assess of the degree to which the cross-sectional relationship between income and fertility mischaracterizes the causal relationship. Table 6 presents estimates of the cross-sectional relationship. Column 1 shows the coefficient from a regression of the current number of children on husbands' log earnings. Both age and year fixed effects are included in the model to isolate the cross-sectional variation in the longitudinal data. As is commonly seen in cross-sectional data, the relationship is negative. Column 2, which controls for women's log earnings, continues to indicate a negative relationship. Taking the standard approach of using husbands' earnings as the income measure, these estimates suggest that the income elasticity is between -0.04 and -0.05 .^{25,26}

In contrast, using the estimated long-term impact on husbands' earnings (-31.6 percent), the average total impact on the fertility of the treated (-0.10), and the average total observed fertility for the treated (2.06), I obtain an estimated income

25. Analysis focusing solely on women who have a displaced husband yields similar results.

26. As a comparison, Jones and Tertilt (2007) analyze how the income elasticity has changed over time using U.S. census data—the estimated elasticity for the youngest cohorts they analyze, which are consistent with the cohorts in this paper, is roughly -0.2 .

elasticity of 0.15. This estimate likely understates the true elasticity since the long-term impact on family income will be less than the impact on husbands' earnings (which are only a share of income).

This finding raises the question: if the causal relationship between husband's earnings and fertility is truly positive, why is the correlation negative? More specifically, what type of model is consistent with such phenomena? Extending the basic model to consider child quality is not sufficient since the causal relationship and the correlation are the same in such a model. A model that incorporates the time cost of children and assortative matching of husbands and wives, however, can solve the puzzle. In such a model, a high wage couple could have a high income but not have many children because of the high value of the wife's time. This would produce a negative correlation between husband's earnings and fertility in the cross-section but would not preclude a couple from having fewer (more) children in response to a reduction (increase) in income.²⁷

VIII. Conclusion

In this paper, I have examined the impact on fertility of a large negative shock to a husband's earnings. Consistent with dynamic models in which children produce a flow of benefits and credit markets are incomplete, I find that a reduction in a husband's earnings growth accelerates fertility. I also find evidence that a large negative income shock reduces completed fertility, although it is not statistically significant when individual fixed effects are included in the model. This estimate implies an income elasticity of 0.15 which stands in stark contrast to the negative correlation between fertility and husbands' earnings observed in cross-sectional data. While the quantity-quality model cannot itself explain these seemingly opposed relationships, a model with assortative matching of spouses that incorporates the time cost of children can.

Policy implications should depend on several factors. Clearly, whether or not it is desirable for job losses to reduce fertility depends on a given society's current and targeted fertility rates. Any policy designed to affect the link between fertility and displacement should also take into account the outcomes of the children born following a father's job loss. While the literature shows that children suffer long-term consequences of parental displacements, little is known about the effects on children who are born after a parent's job loss.²⁸ More research along these lines is needed.

More broadly, understanding the causal effect of income on fertility is important for any policy prescription that involves income shocks. The positive relationship implied by my estimates suggests that poverty relief programs could unintentionally promote fertility. Policy projections that ignore this fertility response will overstate the expected impact of financial assistance on living standards.

27. This type of explanation would predict a positive relationship between husband's earnings and fertility if women's wages are accurately measured and controlled for. As noted in Jones, Schoonbroodt, and Tertilt (2008), the empirical studies that measure this conditional correlation have found mixed results.

28. See Oreopoulos, Page, and Stevens (2008) and Page, Stevens, and Lindo (2009).

Appendix Table 1
Estimated Impact of a Husband's Displacement on Additional Labor Market Outcomes

	Husband's Weeks/Year Worked		Women's Log Earnings		Women's Weeks/Year Worked	
	All Displacements (1)	Closures Only (2)	All Displacements (3)	Closures Only (4)	All Displacements (5)	Closures Only (6)
Displacement year - 2	-0.974* (0.550)	-1.327 (0.857)	-0.022 (0.057)	-0.086 (0.097)	0.220 (0.931)	0.468 (1.705)
Displacement year - 1	-1.495** (0.587)	-0.502 (0.964)	0.026 (0.065)	-0.142 (0.116)	1.776* (0.954)	1.005 (1.642)
Displacement year	-5.749*** (0.617)	-5.005*** (1.091)	0.035 (0.061)	-0.049 (0.109)	1.166 (0.943)	-1.268 (1.687)
Displacement year + 1	-5.367*** (0.682)	-4.948*** (1.013)	0.026 (0.064)	-0.139 (0.117)	1.763* (1.003)	0.683 (1.719)
Displacement year + 2-3	-2.994*** (0.645)	-3.532*** (1.055)	0.050 (0.064)	-0.117 (0.114)	2.516*** (1.017)	0.446 (1.705)
Displacement year + 4-5	-1.339** (0.665)	-1.873 (1.165)	0.045 (0.072)	-0.025 (0.124)	2.895*** (1.143)	1.449 (1.941)
Displacement year + 6-7	-1.809*** (0.702)	-1.642 (1.156)	0.086 (0.081)	-0.049 (0.135)	3.186*** (1.238)	0.891 (2.138)
Displacement year + 8+	-2.096*** (0.761)	-3.172*** (1.149)	0.204** (0.085)	0.039 (0.136)	7.288*** (1.307)	4.139*** (2.054)
Individual Observations	4,141	3,006	3,980	2,870	4,141	3,006

Notes: All regressions include individual fixed effects, year fixed effects, and a quadratic in husband's age. The omitted category is never having a displaced husband or three or more years prior to a husband's displacement. Standard error estimates, clustered on the individual, are displayed in parentheses. Regressions are weighted using the family weight from the last year women are observed with their first husband.
 * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

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