Forthcoming in the *Journal of Health Economics*

Parental Job Loss and Infant Health

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June 2011

Abstract

This paper is the first to explore the extent to which the health effects of job displacement extend to the children of displaced workers. Using detailed work and fertility histories from the Panel Study of Income Dynamics, estimates are identified by comparing the outcomes of children born after a displacement to the outcomes of those born before. This analysis reveals that husbands' job losses have significant negative effects on infant health. They reduce birth weights by approximately four and a half percent with suggestive evidence that the effect is concentrated on the lower half of the birth weight distribution.

JEL Classification: I10, J13, J63

Keywords: infant health, job loss, displacement, unemployment

*I thank Alan Barreca, Alfredo Burlando, Colin Cameron, Nicole Fortin, Hilary Hoynes, Doug Miller, Marianne Page, Heather Royer, Thomas Lemieux, Larry Singell, Mark Stater, Ann Huff Stevens, Joe Stone, Nick Sly, and Glen Waddell along with seminar participants at the UC-Davis and the University of British Columbia and conference participants at the 2010 Economic Demography Workshop, the 2010 NBER Children's Program Meetings, The Economics of Family Policy Conference at Bergen, the 2010 EALE/SOLE World Conference, and the 2010 Western Economic Association Annual Meetings for helpful comments at various stages of this paper. Contact: Department of Economics, 1285 University of Oregon, Eugene, OR 97403; jlindo@uoregon.edu.

1 Introduction

Although a number of recent papers consider the health effects of job displacement, this literature has focused almost exclusively on the health effects for displaced workers themselves. For example, Eliason and Storrie (2009), Rege, Telle, and Votruba (2009), and Sullivan and von Wachter (2009) find that being displaced increases mortality using data from Sweden, Norway, and Pennsylvania, respectively, whereas Martikainen, Maki, and Jantti (2007) find no effect using data from Finland. Researchers have also analyzed the mental health effects of own displacements, finding mixed results.[1] Salm (2009) provides a notable exception in this literature, considering the short-run health effects of displacements for older workers *and their spouses*, finding no effect for either group. This paper is the first to explore the extent to which the health effects extend to the children of displaced workers. Specifically, I estimate the impact of parents' job displacements on birth weights. To deal with the possibility that job displacements might not be exogenous to infant health, the most flexible estimates are based on models with mother fixed effects so that the estimated effects are driven primarily by a comparison of children born after a displacement to their siblings born before.

While not usually focusing on health, a number of papers have demonstrated that job displacements have important consequences for the entire family. For example, Stephens (2002) shows that women work more following a husbands' job loss to compensate for his lost earnings; Charles and Stephens (2004) show that getting fired increases the probability of divorce; and Lindo (2010) shows that husbands' displacements affect fertility. Moreover, Oreopoulos, Page, and Stevens (2008), Page, Stevens, and Lindo (2009), Stevens and Schaller (forthcoming), and Rege, Telle, and Vortruba (forthcoming) have demonstrated that there are important consequences for children who are in the household when a parent is displaced.

[1]For example, Browning, Dano, and Heinesen (2006) find no effect with data from Denmark and Kuhn, Lalive, and Zweimueller (2009) find harmful effects with data from Austria.

This paper, however, is the first to consider the impacts on children born following a parent's job loss.

This work is also closely related to Dehejia and Lleras-Muney (2004) who show that birth weights improve during recessions. While they present evidence suggesting that both selection into motherhood and improvements in health-related behaviors play a role, like other analyses of the health effects of local unemployment rates, the identification strategy cannot disentangle the effects of own job displacements from other aspects of recessions. Recent research suggests that this distinction is crucial. Specifically, Sullivan and von Wachter (2009) find that own job displacements increase mortality for U.S. workers despite the fact that mortality tends to improve during recessions (Ruhm 2000).

This paper can also be thought of as providing a window into the relationship between socioeconomic status and health. In general, measures of socioeconomic status are positively related with measures of health. Figure 1 demonstrates that birth weights, the measure of infant health I focus on in this paper, are no exception.[2] Of course, it is difficult to ascertain extent to which differences in socioeconomic status *cause* differences in infant health outcomes because there may be characteristics that lead individuals to have both lower socioeconomic status and to have children with poorer health. As argued in Oreopoulos, Page, and Stevens (2008), Page, Stevens, and Lindo (2009), Sullivan and von Wachter (2009), and Lindo (2010), we can learn about the causal effect of income on various outcomes by considering the effects of job displacements which provide a plausibly exogenous shock to household income after controlling for individual fixed effects. As such, this paper offers insight into the causal link between family income and infant health.[3]

[2]A large literature demonstrates that birth weights are a good proxy for infant health. Almond, Chay, and Lee (2005), Black, Devereux, and Salvanes (2007), and Royer (2009) show that birth weight is associated with important short-run outcomes including infant mortality and hospital costs. Further, Behrman and Rosenzweig (2004), Black, Devereux, and Salvanes (2007), Johnson and Schoeni (forthcoming), and Oreopoulos, Stabile, Walld, and Roos (2008) show that birth weights are associated with a wide variety of important long-term outcomes such as IQ, education, earnings and adult health.

[3]This paper complements Currie and Cole (1993) who consider the effect of AFDC, Lindahl (2005) who

Using data from the Panel Study of Income Dynamics (PSID) which has detailed information on both employment histories and fertility histories, I find that a husband's displacement reduces the birth weight of subsequent children by approximately four and a half percent, or five ounces. In addition, there is suggestive evidence of economically meaningful effects in the lower end of the birth weight distribution.[4]

The main limitations of this work are twofold. First, the sample size that one can draw on from the PSID is small. As a result, there is not enough power to rely solely on job displacements due to plant and business closures. A majority of analysis instead follows the convention of other papers that have used PSID to consider the effects of job displacement and focuses on a broader category of involuntary separations that includes individuals who have been laid off or fired (e.g., Stevens 1997). With that said, the main results, based on models with mother fixed effects, tend to be statistically significant and robust to the inclusion of a number of control variables. In addition, while imprecise, the estimates are similar when focusing on job losses due to plant and business closures. Second, it is important to recognize that the endogeneity of fertility has important implications for interpretation. In particular, we might like to observe potential outcomes for families who do not have further children after a job loss. Since we do not, the analysis is appropriately characterized as estimating the effect of job loss on infant health *for families who continue having children after a job loss*. Further, as discussed in greater detail in Section 3, the inclusion of mother

analyzes the health effects of monetary lottery prizes in Sweden, Burlando (2011) who considers the effect an income shock generated by a prolonged blackout in Tanzania, and Almond, Hoynes, and Schanzenbach (2011) who consider the effects of food stamps which they argue are equivalent to a cash transfer.

[4]While it is possible to conduct a similar analysis of women's job displacements, in results not shown in this paper, I have found that women's work activity tends to increase substantially above expected levels prior to displacements. This is not too surprising since many women are have weak ties to the labor market and labor market participation is a prerequisite for displacement. Regardless, this finding suggests that women's job displacements are not exogenous and that any analysis of their effects will be challenged to separate the effects of displacement from the effects of unobservables leading to increased work activity. Although the sample sizes in the PSID are not well suited to this approach, a natural solution to this problem would be to focus on a sample of high-tenure women. Because of their strong attachment to the labor market, this type of issue does not present itself in the analysis of men's work activity.

fixed effects will control for most types of selection bias that fertility effects might introduce.

The rest of this paper is organized as follows. Sections 2 and 3 describe the data and empirical strategy. Section 4 presents the results of the empirical analysis while Section 5 discusses the results. Section 6 concludes.

2 Data

This paper uses data from the 1968–1997 waves of the PSID to identify parents' job displacements and the PSID's Childbirth and Adoption History Supplement (CAHS) to measure children's outcomes.[5] 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 each of the original samples (and their split-offs) and use PSID weights.[6] The CAHS includes retrospective fertility histories, with children's year and month of birth, for all individuals of childbearing age surveyed in the PSID in 1985 or later. Most importantly, the data include birth weights in ounces for children born in 1985 and later.[7]

In the appendix, Table A1 shows the distribution of birth weights for the children in the sample. One potential concern with the PSID as a source of birth weight information is that it is reported by parents and, thus, subject to recall error. While misreporting cannot be ruled out, it is reassuring that the sample distribution of birth weights is similar to the nationwide distribution in 1990, the median year of birth for the analysis sample.

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

[5]Because the data are similar, the discussion in this section has substantial overlap with Lindo (2010).

[6]In particular, throughout the analysis, all estimates use children's sample weights in the last year they are observed in the PSID.

[7]The PSID also has retrospective data on whether or not children born before 1985 were low birth weight. The results shown in this paper do not use this data so that the sample is consistent throughout; however, estimated impacts that make use of this data are very similar to the presented results.

question asking individuals who are not working, and those who began their current job within the last year, "what happened to your previous job?" Throughout most of the analysis, 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.[8] Since it is not clear from the survey whether the job loss occurs in the current or previous year, I assume that the displacement occurred in the previous year.

Topel (1990) explains that the survey might miss displacements since the survey question focuses on the last job. That is, we might incorrectly 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 following 1997 are not used. [9] Finally, while one might experience multiple displacements, I consider the year of the first displacement the "displacement year." This is important because it has been shown that initial displacements predict future displacements and, thus, subsequent displacements should not be considered exogenous (Stevens 1997).

Since the PSID began tracking job changes for heads of households beginning with the 1968 survey and the sample of mothers are those having children in 1985 and later, we can potentially observe work histories for many years before a child's birth. This is important to help ensure that children are not incorrectly classified as "not treated" if a displacement occurred several years prior to a child's birth. With clean identification in mind, the analysis sample that I consider consists of mothers married in 1968 or later. In addition, only children born after a mother was first married are considered so that the indicator for being born after a husband's displacement is not necessarily zero.[10]

[8] As a robustness check, I test for different effects across the two categories of involuntary job losses.

[9] Another feature of the data usually mentioned in papers using the PSID to consider the effects of displacement is that, unlike subsequent years, the 1968 survey only asks those who began working for their current employer in the last ten years their reason for leaving the last job. As such, those who report a displacement in 1968 tend excluded from analyses since the timing of their displacements cannot be ascertained. Due to the primary years under consideration in this paper, this restriction is benign.

[10] That is, these children are not included in the analysis because it is impossible for them to be "treated."

To summarize, the 1968–1997 waves of the PSID are used to identify job losses occurring 1968–1996 and the CAHS is used to measure the outcomes of children born 1985–1996. Because the links between women, their children, and their spouses is a critical component of this exercise, it may be worth mentioning how these links are made for the following analysis. The CAHS data has identifiers that are used to match the children to their mothers.[11] Women are linked to husbands on the basis of their relationship code in each given year. Women who are cohabiting with a male partner are considered married.[12] As a result of this approach, it will not necessarily be the case that each displacement event for a given child will correspond to a father's job loss. The job losses will always correspond to those experienced by a mother's partner while they are living together.

3 Empirical Strategy

As a starting point, I estimate a simple model that compares the birth weights of children born following a husband's first displacement to children for whom no such event has taken place. The regression equation is given by:

$$y_{sma} = D_{sma}\delta + X_{sma}\beta + \alpha_a + u_{sma} \tag{1}$$

where y_{sma} is a birth outcome for child s of mother m at age a, D_{sma} is an indicator variable equal to one if the child is born in the same year the mother has a displaced husband or any year afterwards, X_{sma} is a vector of covariates, α_a are age fixed effects, and u_{sma} is a random error term. The parameter δ is the estimated impact of a husband's displacement.

In doing so, I ensure that all children in the analysis sample are "at risk" of being born following a displacement.

[11] Adopted children are not used in any part of the analysis.

[12] While this approach can be motivated for philosophical reasons, it is also motivated by practical concerns when using the PSID because the distinction between married and cohabiting is not made throughout many years of the survey.

The estimated impact based on equation (1) will only be valid if husbands' displacements are exogenous to birth outcomes. Since this is unlikely, my preferred estimates are based on alternative approaches. First, I estimate the effect as in equation (1) but control for whether the mother *ever* experiences a husband's job displacement. This will lead to unbiased estimates if there are not systematic differences between the types of mothers who have children before displacements and the types who have children after displacements. My second approach relaxes this assumption by controlling for systematic differences across mothers in a more flexible manner. In particular, this approach uses a model that includes mother fixed effects to control for fixed characteristics of mothers related to both children's birth weights and the probability of having a displaced husband. The resulting regression equation is as follows:

$$y_{sma} = D_{sma}\delta + X_{sma}\beta + \alpha_a + \alpha_m + u_{sma} \tag{2}$$

where all of the notation is the same as in equation (1) and α_m are mother fixed effects. Using this model, the estimated effect of a husband's displacement is identified by the comparison of *siblings* born before versus those born after a displacement. Mothers who do not ever have a displaced husband, or only have children before or only after a husband's displacement, are included in the analysis to help identify the other parameters. I also estimate models that allow for heterogeneous effects over time, across gender, and across education groups.

The inclusion of mother fixed effects will control for most sources of bias, including those that might arise as a result of effects on fertility.[13] For example, one might be concerned that estimated effects will be biased towards finding negative effects if: (1) husbands' displacements are more likely for women who tend to have lower birth weight children; (2) husbands' displacements increase the probability of having children for women who tend

[13]Lindo (2010) shows that husbands' job displacements do affect fertility, also using data from the PSID. The analysis reveals significant impacts on the timing of fertility, causing women to be more likely to have children in the short-run but to be less likely to have children in the long-run. The estimated effect on net fertility ranges from -0.15 to -0.10 children.

to have lower birth weight children; or (3) husbands' displacements reduce the probability of having children for women who tend to have healthier children. However, all of these concerns are addressed by the inclusion of mother fixed effects which control for a woman's baseline tendency to have low birth weight children in addition to all other fixed unobservable characteristics. In addition, the results that follow show the estimated effect for children who are born in the year the husband loses his job. Because most of these children will have been conceived prior to the job loss, this is a sample for whom we can be especially confident that selection bias is not of great concern.

With mother fixed effects in the model, in order for the identifying assumption to be violated, it would have to be the case that older siblings born before a job displacement do not serve as a valid counterfactual for children born afterwards. While not impossible, it is rather difficult to come up with stories that would result in such a violation. One example, however, is the idea that women who experience husbands' displacements might have different "birth weight trends." If they have flatter positive trends, it would bias the estimates towards finding negative effects. For this reason, I also present estimates that control for education group trends and industry trends as controls. In addition, I demonstrate that children's birth weights do not diverge from their expected levels in the years immediately prior to a husband's displacement.

This model is very similar to models that have been used to estimate the impact of displacements on labor market outcomes. That is, it is a difference-in-difference model that controls for individual fixed effects and time fixed effects. An important aspect that distinguishes this analysis from the analysis of labor market outcomes is that post-displacement birth outcomes cannot be measured for all women. As such, if older siblings born before a parent's job displacement provide a good counterfactual for those born after, then the estimation strategy will provide an unbiased estimate of the effect of job loss on infant health *for families who continue having children after a job loss.* The analysis will *not* capture

how families who cease having children after a job loss would have been affected if they had continued to have children.

It is also worth noting that this estimation strategy might understate the harmful effects on infant health by neglecting to consider impacts on fetal deaths. However, given that fetal deaths are quite rare, approximately seven per thousand births during my sample window, the magnitude of this bias is likely small.[14]

4 Results

4.1 Summary Statistics

Table 1 presents summary statistics for the sample of children. The first three columns separate the children into those who are born to a mother who never experiences a husband's displacement, children born before their mother has a displaced husband, and children born following the displacement of a mother's husband. One noteworthy aspect of this comparison is the relative sample sizes. In particular, a large share (one third) of the sample of children are born to mothers who experience a husband's displacement at some point in time. Further, among children born to mothers experiencing a husband's displacement, over three times as many children are born following a displacement as are born before a displacement.

It is also important to note important differences in the characteristics of the mothers who experience a husband's displacement and those who never experience a husband's displacement. On average, those who experience a husband's displacement are less educated, marry at a younger age, and begin to have children at a younger age. These differences serve as motivation for my empirical approach that controls for whether a mother ever experiences a husband's displacement in order to control for systematic differences across women who experience displacements and those who do not. However, the fact that mothers having chil-

[14] See MacDorman and Kirmeyer (2009) for fetal death statistics.

dren after a displacement have lower levels of education than mothers having children before a displacement suggests that this approach may not be sufficient for addressing selection bias. This observation highlights the potential importance of controlling for mother fixed effects in order to obtain unbiased estimates of the effect of displacement.

It is also notable that children born to women never experiencing a husband's displacement appear less healthy than children born to women who do experience a husband's displacement. At first glance, this suggests that a husband's displacements might improve birth weights but it could also suggest that husbands' displacements are more likely for women who tend to have higher birth weight children (despite the fact these women tend to have lower levels of education and income). Turning to the means in columns 2 and 3, we see that children born after a job loss have birth weights approximately five ounces lower than children born before a job loss, which suggests that there are negative consequences when we begin to address selection by focusing solely on children born to women who experience husband's displacements. At the same time, this comparison may also be biased by differences in the composition of mothers.

The next three columns of Table 1 show sample means for the sample of children with at least one sibling in the sample, as only these children will contribute to the estimates based on the mother fixed effects models. This sample restriction appears to have little effect on the group means, with the exception of reducing the difference in education between children born to mothers experiencing a displacement and children born to mothers never experiencing a displacement. In this restricted sample, we again see that children born following a displacement have lower birth weights than those born before.

The final two columns show the sample means for the subsample of children born to mothers who have at least one child before a husband's displacement and at least one child after a husband's displacement. As one would expect, this sample restriction reduces the sample size substantially while further balancing the characteristics of the children across

the treatment and control groups. The means continue to reveal that children born before a displacement have higher birth weight than those born after a displacement. The next section provides a more formal analysis that allows for statistical inference.

4.2 Impacts of Husbands' Displacements on Infant Health

Table 2 presents regression-based estimates of the effect of a husband's job displacement on children's birth weights. All of the estimates control for year of birth and the mother's age at the time of the birth.[15] The controls for mothers' ages are potentially important because older women are more likely to have had a displaced husband while maternal age is also related to birth weight.

Echoing the summary statistics shown in the preceding section, the estimate in the Column 1 indicates that children born following a husband's displacement have roughly similar birth weights, on average, to children who are not born following a husband's displacement. However, the estimate in Column 2, which controls for whether a mother *ever* experiences a husband's job displacement, exploiting variation in the timing of displacements relative to the timing of births, indicates that we should not conclude that job loss does not affect infant health. This estimate, which is significant at the five-percent level, suggests that displacement reduces birth weights by 3.9 percent when we control for systematic differences across mothers who experience husbands' displacements and those who do not.[16] The estimate in Column 2, however, may be biased towards zero because children born following a displacement will tend to have higher birth orders and birth order tends to be positively

[15]Mother's age at birth is always controlled for using unrestricted fixed effects. Unrestricted fixed effects control for year of birth in models that do not include mother fixed effects. In models with mother fixed effects, I control for year of birth using a third-order polynomial because it is not possible to simultaneously identify year fixed effects in addition to mother fixed effects and age fixed effects.

[16]In order to explore what lies behind the apparent omitted variable bias present in Column 1, I have explored many alternative models that control for mothers' observable characteristics such as education, race, and lagged income. Including these controls does not produce strong negative estimates comparable to the Column 2 estimate that controls for all fixed observable and unobservable characteristics of mothers.

related to infant health outcomes.[17] For this reason, Column 3 adds birth order fixed effects to the model along with an indicator variable for gender.[18] As anticipated, the estimated effect is somewhat larger in magnitude with the inclusion of these controls, suggesting that a husband's displacement reduces subsequent children's birth weights by 4.1 percent on average. This estimate is also statistically significant at the five percent level. The final estimate based on this approach, shown in Column 4, limits the sample to children with at least one additional sibling in the sample for comparability with the mother fixed effects estimates which only use variation across siblings. This estimate is somewhat larger in magnitude, possibly because the effects might be greater for children in larger families, and again provides evidence that parental job loss has harmful effects on infant health.

Column 5 presents results from the analysis based on the mother fixed effects approach in which children's older siblings who were born before a displacement are used as the counterfactual for children born after a displacement. The point estimate, which is significant at the five-percent level, suggests that a husband's displacement reduces subsequent children's birth weights by 4.7 percent on average. Columns 6 and 7 attempt to address the concern that the estimated effects might be biased if women experiencing husband's job displacements have different "birth weight trends" from those who do not. Because of limitations of the data, Column 6 begins by only including four parameters corresponding to separate trends for women with less than a high school education, those with exactly a high school education, those with some college, and those with a college degree. The point estimate is largely unchanged and remains significant at the five-percent level. Column 7 goes a step further, also including trends for the initial industry each woman's husband is observed in

[17] In particular, second and higher-parity births tend to have better outcomes than first births.

[18] Inherent in this approach is the assumption that the change in birth weights observed across birth order for the control children provides a good counterfactual for the change that would have been observed for children born after a displacement if the displacement had not occurred. Also note that it is not clear that sex should be included as a control since it might be considered an outcome variable. Results are nearly identical when this control variable is omitted.

(adding approximately thirty additional parameters to the model). Although this estimate is only significant at the ten-percent level, this change in statistical significance is due solely to an increase in the standard error estimate as the point estimate is actually larger in magnitude than the estimate in Column 6.

Table 3 presents estimated effects on the conventional measure of low birth weight, or the probability that a child has a birth weight of less than 88 ounces (2500 grams), using a linear probability model. While the estimates that control for systematic differences across mothers experiencing husbands' displacements and those who do not provide suggestive evidence impacts on low birth weight, there is not sufficient power to reject zero.

Although the definition of low birth weight considered in Table 3 is standard, it is rather arbitrary. Further, we might be interested in knowing the impact on the full range of the birth weight distribution. One way of doing so is to estimate the impact on the probability that a child is less than Z ounces for all possible Z. These estimates, based on the mother fixed effects model with the full set of controls, are presented in Figure 2 which summarizes the distributional impact. Again, while the power of these estimates is limited, they collectively suggest that the impact is concentrated primarily below the median birth weight (120 ounces).[19]

4.3 Tests for Robustness and Heterogeneity

In this section, I allow the effects of husbands' job displacements to vary with the timing of births, the type of displacement, child gender, and mother's education. All of the estimates are based on my most flexible empirical model, controlling for mother fixed effects, child-specific characteristics, education-group trends, and industry trends. Column 1 of Table 4 interacts the indicator for being being born following a husband's displacement with the

[19]It is also important to note the economic significance implied by the estimated effects at the very low end of the birth weight distribution. Because the baseline probabilities are so small in that region, the percentage point increases implied by these estimates constitute a substantial effect in percentage terms.

timing of the birth. Specifically, the regression includes indicators for being born in the two years prior to a husband's displacement, an indicator for being born in the same year that a husband is displaced, an indicator being born one to four years after a displacement, and an indicator for being born five or more years following a displacement. The omitted category includes children born three or more years before a displacement or being born to a mother who never has a displaced husband.

Two aspects of these results support the validity of the research design. First, the estimated coefficient on the indicator for being born in the two years prior to a husband's displacement is approximately zero. This finding provides evidence against the possibility that changes in households' unobservables simultaneously drive husbands' displacements and reduced birth weights.[20] For example, if family turmoil led to husband's job displacements and poorer infant health, we would probably expect the health effects to manifest themselves prior to the husband's job loss. Second, the harmful effects of displacement are evident immediately, i.e., for children born in the year of displacement. This is encouraging evidence that the effect is not driven by selection since these children were likely conceived prior to the job loss.

The set of indicators for being born following a displacement reveal that both children born immediately following a husband's displacement and those born many years later suffer negative consequences of the displacement. The fact that children born many years following the event are affected should not be surprising given that it is well established that job losses have permanent impacts on income. Although it is tempting to interpret the differences across the three point estimates, caution is necessary for two reasons. First, the timing of children is likely endogenous to the magnitude of the income shock. As such, the effect might be greatest for children born 5 or more years after the displacement because families hurt

[20]It is worth noting that it would not be completely unexpected if there was an effect preceding the actual event. The displacement literature consistently finds that individuals' earnings begin to deteriorate prior to displacements taking place.

most by the job loss delay childbearing the longest. Second, and perhaps more importantly, the estimates are far from being statistically distinguishable from one another.

Column 2 of Table 4 interacts the indicator for being born following a displacement with the type of displacement. Specifically, this model allows the effect of displacements to be different for job losses due to plant and business closures and for job losses due to a husband being laid off or fired. Both point estimates are negative, indicating birth weight reductions of 6.4 and 4.2 percent, respectively. We cannot reject that they are the same at conventional significance levels.[21]

Given the small sample size afforded by the PSID, another important check is to verify that outliers do not drive the main results. In order to address this concern, I have estimated the effect of displacement after dropping sibling groups at the extremes of the data, i.e., those sibling groups with the greatest post-displacement reductions in birth weight and those with the greatest post-displacement increase in birth weight. These restrictions have almost no impact on the estimates.[22]

To the extent to which a child's health at birth can be influenced by behavior during pregnancy, it is possible that a husband's job displacement might have different consequences for male and female children. In particular, parents expecting boys might exert more effort to mitigate the negative effects of displacement if there is a preference for boys. Column 3 of Table 4 explores the extent to which there are heterogeneous effects across genders. The point estimates suggest that there are harmful effects for children of both genders. The estimated average effect for females is indeed larger than the estimated effect for male children (6.1 percent and significant at the five percent level versus 3.2 percent and insignificant at

[21]In the appendix, I present a full set of estimates focusing only on job losses due to plant and business closures. This is accomplished by omitting mothers for whom their husband's initial displacement is due to being laid off or fired. The estimates are broadly consistent with the main estimates although standard error estimates are fairly large.

[22]For example, whereas the estimates across columns 5 through 7 in Table 2 are -0.047, -0.045, and -0.048, dropping the five sibling groups with the greatest birth weight changes (positive and negative) leads to estimates of -0.051, -0.049 and -0.050.

conventional levels) which is consistent with a preference for boys, but the estimates are not significantly different from one another (p-value = 0.237).

Column 4 of Table 4 allows the estimated effects to vary with mothers' levels of education. In particular, the indicator for being born after a displacement is interacted with an indicator variable taking a one if the mother has a high school education or less and it is also interacted with an indicator variable taking a one if the mother has more than a high school education. While both point estimates are negative, they suggest that the impact may be greater for women with higher levels of education. However, the difference between the estimated impacts is not statistically significant (p-value = 0.247).

4.4 Potential Mechanisms

Table 5 explores the differences in circumstances at birth for children born following a father's displacement relative to their siblings born before. These results are again based on the the most flexible approach used to estimate the effects on birth weights—in addition to an indicator variable taking a one if a child is born following a displacement, the model includes the full set of controls for mother fixed effects, mother's age, year, child gender, birth order, education-group trends, and industry trends.

Panel A considers the average effect among all children. Consistent with a vast literature on the earnings losses resulting from job displacement, Column 1 shows that husbands' earnings are approximately twenty-two percent lower in the year prior to birth for children born following a displacement while Column 2 shows that the effect on family income is thirteen percent.[23] Columns 3 and 4 reveal that there is little effect on men's work activity—this is also consistent with the displacement literature which tends to find that work activity recovers quickly while wages do not.

[23]The percentage effects are computed as $e^{\delta} - 1$. For estimated effects on husbands' earnings over time using a similar sample from the PSID, see Lindo (2010).

Because the standard errors tend to be large, the results in columns 5 through 10 are best cast as providing suggestive evidence of the mechanisms at work in order to provide hypotheses that merit consideration for future research. Columns 5 and 6 suggest that women work more actively in the year prior to their child's birth following a husband's job displacement, with a greater response on the extensive margin.[24] This finding suggests that work-induced stress may be a potential factor linking husband's displacements to reduced birth weights.[25] The estimate in Column 7 suggests that husband's displacements reduce pre-birth food expenditures by 20 percent. Given overwhelming evidence on the importance of maternal nutrition (Bitler and Currie 2005; Almond and Mazumder forthcoming; and Hoynes, Page, and Stevens 2011), this offers a plausible explanation for the impact on birth weights. Finally, columns 8 through 9 show small imprecise effects on mother's marital status and self-reported health in the year prior to their child's birth while Column 10 shows a similar effect on the probability that a child was born after a short inter-pregnancy interval, defined as less than six months between pregnancies.[26]

With the same caveats, Panel B interacts the indicator for a child being born after displacement with mother's education in order to shed light on the estimates from the previous section, which suggest that there might be greater impacts on the birth weight of children born to more highly educated mothers. While Column 1 suggests that the impact on husbands' pre-birth earnings is similar for children born to mothers with less than or equal to a high school education and mothers with more education, Column 2 suggests that the impact on pre-birth family income is greater for women with more than a high school education. The

[24]This "added worker effect" has been shown previously in Stephens (2002) and Lindo (2010).

[25]Aizer, Stroud, and Buka (2009) speak to this issue, using within family variation to show that maternal stress is a significant predictor of child outcomes. They do not find significant impacts on birth weights, however. In contrast, Eskenazi et al. (2007) and Camacho (2008) both find that prenatal stress reduces birth weights.

[26]The indicator variable for being born after a short inter-pregnancy interval will necessarily equal zero for all first-born children. In results not shown but available upon request, the estimated effect on the probability that a child is born to a divorced mother is also close to zero.

point estimates imply that the effect on pre-birth family income is minimal for women with lower education (negative 3.4 percent) whereas it is substantial for women with higher levels of education (negative 18.2 percent), although the estimates are not significantly different from one another (p-value = 0.287). Columns 5 and 6 help to explain this difference, as a husband's displacement has a significant positive impact on work activity in the year prior to birth for women with lower levels of education. In contrast, the point estimates reveal no evidence of an impact on pre-birth work activity for women with more than a high school education. Finally, Column 7 suggests that the impact on pre-birth food expenditures is greater for those with less than a high school education although one cannot know whether this implies a greater impact on their nutrition.

Collectively, the set of estimates presented in Table 5 suggests that a husband's job loss has large effects on pre-birth incomes. That there seem to be relatively small impacts on infant health for women with lower levels of education who increase their work activity to mitigate the income shock is enlightening. It suggests that the lost income itself is an important mechanism and casts doubt on the hypothesis that adverse health effects are driven by work-induced stress.

5 Discussion

As a whole, the preceding analysis of husband's displacements reveals that this income shock negatively affects birth weights. The point estimates indicate that a husband's displacement reduces a child's birth weight by 4.7 percent (approximately five ounces) on average. To put this magnitude into context, Almond, Chay, and Lee (2004) find that smoking reduces a child's birth weight by 7.1 ounces on average. Given the wealth of studies identifying the causal link between birth weights and long-run outcomes, one would anticipate that the effect on children's health is likely to persist throughout their lives. For example, a back-of-

the-envelope calculation based on Royer's (2009) study of female twin pairs suggests that a five-ounce impact on birth weights will lead to a six-percent increase in the probability of hypertension.

If we assume that husbands' job losses only affect birth weights through their effects on family income in the year prior to birth, then my estimates imply an income elasticity of 0.32.[27] This would suggest that cross-sectional comparisons, such the estimates shown in Table 6 which regresses birth weights on family incomes, understate the importance of family income as they imply an elasticity of 0.04. At the same time, caution is necessary when interpreting the magnitude of my estimates given the small sample size offered by the PSID. Indeed, the 95 percent confidence intervals for my main estimates include the estimate one would expect based on the cross-sectional income elasticity.

Two other recent quasi-experimental studies offer a similar ability to obtain income elasticity estimates for birth weights. The only other study to consider the effect of a large negative income shock on birth weights finds similarly large estimates. In particular, the estimates reported in Burlando's (2011) examination of a prolonged blackout in Zanzabar, which reduced work by a twenty-five percent, imply an elasticity between 0.24 and 0.76. In a more similar context but focusing on a relatively small *increase* in income, Almond, Hoynes, and Schanzenbach (2011) estimate the effect of food-stamp benefits in the United States. Combined with Hoynes and Schanzenbach's (2009) estimates of the effect of food-stamp benefits on income, approximately ten percent, their estimates imply an income elasticity of 0.05.[28]

[27] This estimate is calculated as 0.047 (Column 5 of Table 2) divided by 0.145 (Column 2 of Table 5).

[28] In a related study, Currie and Cole (1993) consider the effects of AFDC receipt on birth weight, employing sibling fixed effects and instrumental variables models. The instrumental variables estimates, using variation generated by differences in generosity across states, imply that AFDC receipt increases birth weights by 4.5 ounces and 32 ounces for blacks and poor whites, respectively. The sibling fixed effects estimates suggest that AFDC increases birth weights by approximately one ounce. All of these estimates, however, are imprecise and none are statistically significant. In addition, these estimates might also reflect differences in Medicaid eligibility and/or food stamp receipt which were closely tied to AFDC.

Again noting that an income elasticity of 0.05 cannot be rejected based on my analysis, there are multiple reasons why my estimate may be larger than the estimate implied by the effect of food-stamp benefits. First, reductions in income may have greater effects than similar increases in income. Second, the change in income that I consider is relatively large in magnitude and the effects of income shocks may be non-linear. Third, my estimates focus on married women whereas seventy-five percent of food-stamp recipients are single. In addition, it is important to keep in mind that a job displacement leads to a negative income *shock* which might have more severe consequences than low income by itself. It is also important to note that, while the most salient feature of a husband's displacement is its large and permanent impact on family income, impacts on birth weights might be generated by aspects of the shock other than the loss of income. For example, a husband's job loss might reduce birth weights because of its impact on stress. At the same time, if the reason that stress increases is because of the lost income, then we would still be correct in interpreting the estimated effects as resulting from the income shock.

6 Conclusion

In this paper, I have examined the impacts of husbands' job displacements on children's birth weights. My findings represent a nice parallel with Sullivan and von Wachter (2009). Whereas there is evidence that mortality improves during recessions (Ruhm 2000), Sullivan and von Wachter (2009) show that individuals' job losses increase their mortality. Similarly, while Dehejia and Lleras-Muney (2004) present convincing evidence that birth weights improve during recessions, I find that husbands' job displacements have a negative effect on birth weights. Although these results chip away at the "why do birth weights improve during recessions?" question, much work remains to be done on this topic.[29] My results indicate

[29]Unfortunately, that there is not nearly enough power in the PSID to be able to meaningfully replicate Dehejia and Lleras-Muney's (2004) analysis of the effects of local unemployment, which uses vital statistics

that some aspects of adverse macroeconomic conditions besides husbands' job losses must play a major role. In fact, these other aspects must play a role so great that they more than offset the negative consequences of husbands' job losses.[30]

The results of this paper also shed light on the relationship between socioeconomic status and health. Like prior papers, one could think of the displacement as a plausibly exogenous shock to household income. In that sense, my results suggest that the positive cross-sectional relationship between income and infant health is indicative of the causal link. This in turn implies that policies that provide income support, in addition to increasing consumption, can be expected to have the additional benefit of improving health outcomes.

data to show that a one percentage point increase in the unemployment rate is associated with a 0.00018 reduction in the fraction of children with low birth weight, with a standard error estimate of 0.000063.

[30] As Sullivan and von Wachter (2009) point out, we should not necessarily be surprised that economic downturns and job displacements have different impacts on health. Displaced workers make up a small share of those affected by recessions and the health effects of recessions might result from reduced hours of work for employed workers or impacts on the employment rates of those who are weakly attached to the labor market.

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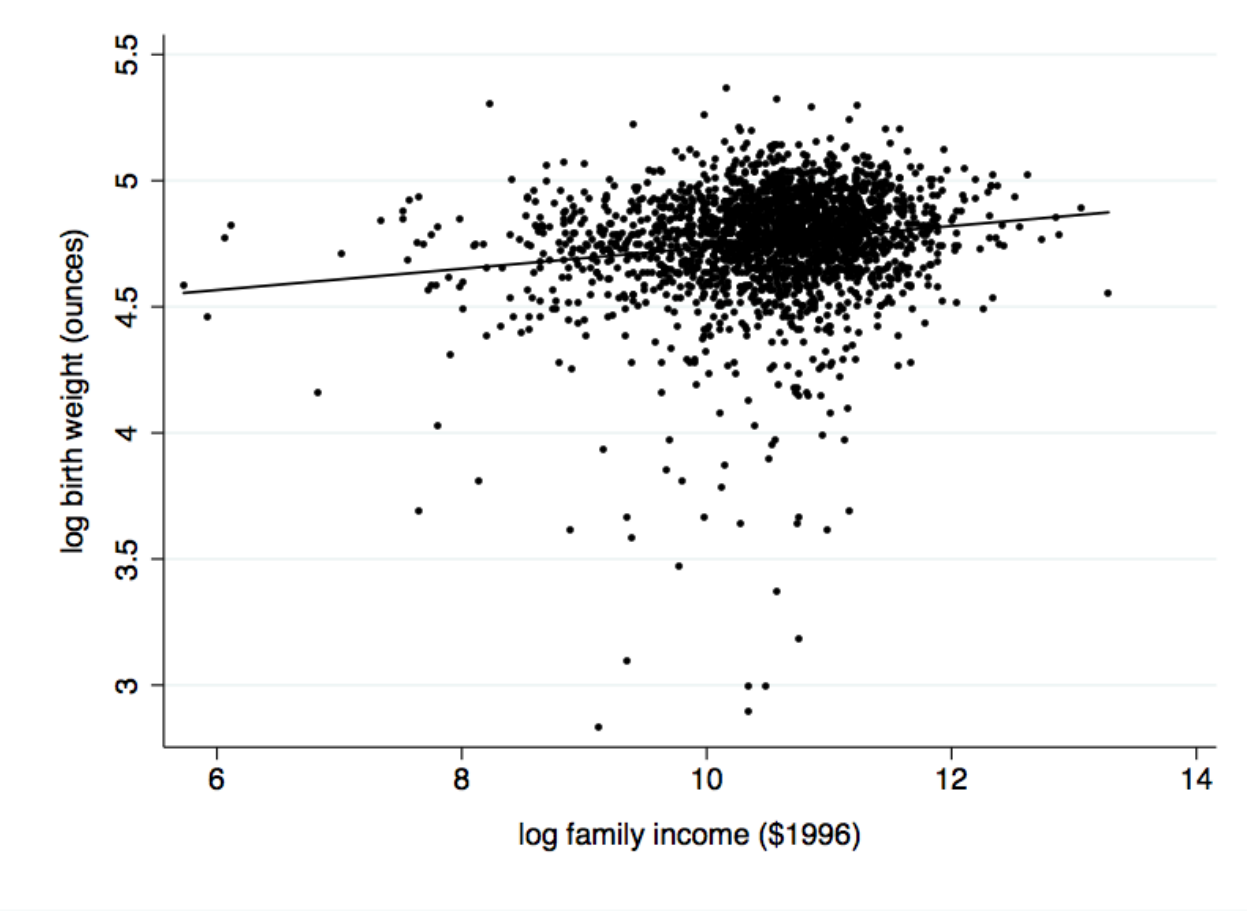
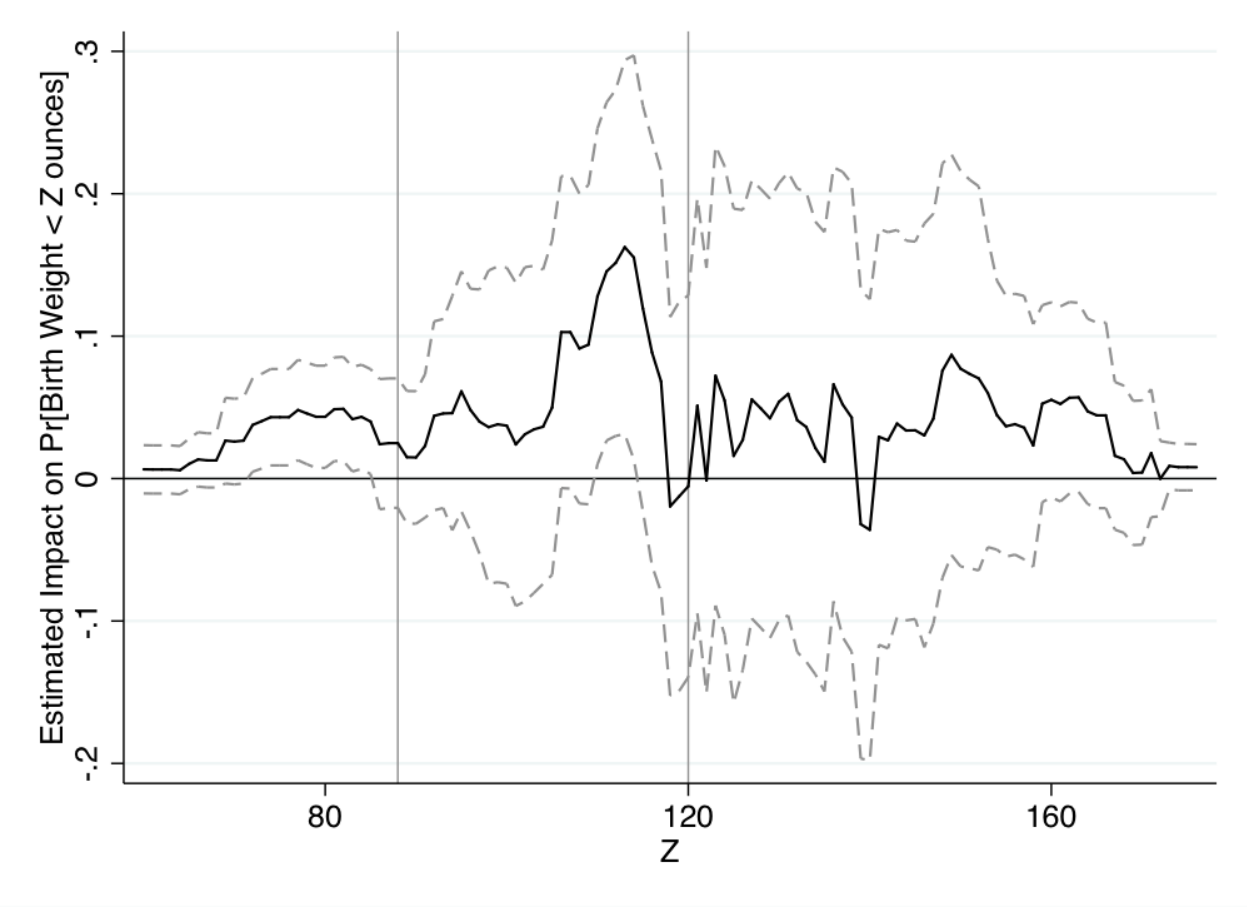
Figure 1
Income and Birth Weights



Notes: Data is from the PSID. The collection of dots represent the 2,263 births for which income is not missing in the year before the birth. The fitted line has a slope coefficient of 0.032 with a standard error estimate, clustered on the mother, of 0.010.

Figure 2
The Distributional Impact of a Husband's Displacement on Birth Weights



Notes: Data is from the PSID. This figure summarizes the results of over 100 regressions in which the dependent variable is an indicator variable taking one if a child's birth weight is less than Z ounces where Z is plotted on the horizontal axis. The regressor of interest is an indicator variable that takes a one if a child is born following a displacement. The estimated coefficients on this regressor are plotted in the figure along with the 95% confidence intervals. The regressions also include mother fixed effects in addition to controls for the mother's age, the year the child is born, sex, birth order, education group trends, and industry trends. Standard errors are clustered on the mother. Regressions are weighted by using children's sample weights. Vertical lines are drawn the conventional cutoff for low birth weight (88 ounces) and at the median birth weight (120 ounces).

Table 1
Summary Statistics By Husband's Displacement Status

	All mothers			Mothers with ≥ 2 children			Mothers with children before and after displacement	
	Mother never experiences displacement	Children born before displacement	Children born after displacement	Mother never experiences displacement	Children born before displacement	Children born after displacement	Children born before displacement	Children born after displacement
Mother characteristics:								
≤ High School Education	0.31	0.36	0.46	0.31	0.35	0.40	0.34	0.36
Some college	0.31	0.25	0.29	0.30	0.28	0.31	0.34	0.36
College degree	0.39	0.39	0.25	0.39	0.37	0.29	0.32	0.29
White	0.91	0.88	0.90	0.93	0.90	0.93	0.93	0.96
Black	0.09	0.12	0.09	0.07	0.10	0.07	0.07	0.05
Age when first married	23.2	22.9	21.8	23.2	22.9	22.1	22.4	22.9
Age at first birth	25.5	25.1	23.9	25.4	24.6	24.0	24.4	24.8
Child-specific characteristics at birth:								
Mother's Age	28.7	27.8	28.7	28.7	26.9	28.2	26.0	29.9
Year	1991	1989	1990	1991	1989	1990	1988	1992
Birth order	1.9	1.8	2.3	2.1	1.8	2.3	1.4	2.7
Husband's Earnings ($1996)	41,355	32,669	28,991	42,871	31,055	29,736	31,164	29,399
Parental Income ($1996)	60,938	50,379	45,354	62,232	49,084	47,429	50,418	44,945
male	0.51	0.54	0.52	0.50	0.53	0.52	0.52	0.46
Birth weight (ounces)	120.1	126.5	121.7	119.7	128.6	122.1	128.5	125.0
Low birth weight (<88 ounces)	0.05	0.02	0.03	0.06	0.02	0.03	0.01	0.02
Sample Size	1,812	237	797	1,073	142	451	75	83

Means are calculated using children's sample weights. The three columns are mutually exclusive from one another. In order, these columns demonstrate the means for children who are born to a mother who never experiences a husband's displacement, children born before their mother has a displaced husband, and children born following the displacement of a mother's husband.

Table 2
Estimated Impact of a Husband's Displacement on Log Birth Weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Born after a displacement	0.012	-0.039**	-0.041**	-0.059***	-0.047**	-0.045**	-0.048*
	(0.010)	(0.016)	(0.016)	(0.021)	(0.023)	(0.023)	(0.025)
Mother ever experiences displacement		0.058***	0.059***	0.085***			
		(0.017)	(0.017)	(0.022)			
Child observations	2,846	2,846	2,846	1,666	1,666	1,645	1,607
Mothers	1,929	1,929	1,929	749	749	745	729
Child-specific controls	no	no	yes	yes	yes	yes	yes
Siblings-only sample	no	no	no	yes	yes	yes	yes
Mother fixed effects	no	no	no	no	yes	yes	yes
Education group trends	no	no	no	no	no	yes	yes
Industry trends	no	no	no	no	no	no	yes

Notes: All regressions control for mother's age and the year of birth. Child-specific controls include sex and birth order fixed effects. Standard errors are clustered on the mother. Regressions are weighted using children's sample weights.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3
Estimated Impact of a Husband's Displacement on the Probability of Low Birth Weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Born after a displacement	-0.020**	0.008	0.011	0.017	0.018	0.018	0.025
	(0.009)	(0.012)	(0.013)	(0.015)	(0.023)	(0.024)	(0.023)
Mother ever experiences displacement		-0.032**	-0.033***	-0.042***			
		(0.013)	(0.013)	(0.016)			
Child observations	2,846	2,846	2,846	1,666	1,666	1,645	1,607
Mothers	1,929	1,929	1,929	749	749	745	729
Child-specific controls	no	no	yes	yes	yes	yes	yes
Siblings-only sample	no	no	no	yes	yes	yes	yes
Mother fixed effects	no	no	no	no	yes	yes	yes
Education-group trends	no	no	no	no	no	yes	yes
Industry trends	no	no	no	no	no	no	yes

Notes: Same as Table 2.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4
Estimated Impact of a Husband's Displacement on Log Birth Weight
By Timing of Birth, Child Gender, and Mother's Education

	(1)	(2)	(3)	(4)
Born 1-2 years prior to displacement	-0.014			
	(0.030)			
Born in year of displacement	-0.076*			
	(0.040)			
Born 1-4 years following displacement	-0.049			
	(0.041)			
Born 5+ years following displacement	-0.088*			
	(0.049)			
Born after layoff/fire		-0.043		
		(0.028)		
Born after plant/business closure		-0.064		
		(0.049)		
Born after displacement × Male			-0.032	
			(0.029)	
Born after displacement × Female			-0.061**	
			(0.027)	
Born after displacement × Mother's Education ≤ HS				-0.012
				(0.038)
Born after displacement × Mother's Education > HS				-0.068**
				(0.032)
Child Observations	1,607	1,607	1,607	1,607
Mothers	729	729	729	729
Mother fixed effects	yes	yes	yes	yes
Child-specific controls	yes	yes	yes	yes
Education-group trends	yes	yes	yes	yes
Industry trends	yes	yes	yes	yes

Notes: Same as Table 2.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5
Estimated Impact of a Husband's Displacement on
Other Circumstances at Birth

Dependent variable:	Log Husband's Earnings	Log Family Income	Husband Unemployed	Husband's Weeks Worked	Mother's Weeks Worked	Whether Mother Worked	Log Food Spending	Mother is Married	Mother's Self-reported Health	Short Interpregnancy Interval
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Overall effects										
Born after a displacement	-0.252**	-0.145**	0.010	-1.161	0.043	0.049	-0.218	-0.020	0.028	-0.007
	(0.114)	(0.069)	(0.010)	(1.676)	(3.202)	(0.063)	(0.175)	(0.034)	(0.142)	(0.037)
Child observations	1,213	1,332	1,309	1,396	1,394	1,394	958	1,586	1,448	1,607
Mothers	659	699	700	696	699	699	649	720	721	729
Mother fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Child-specific controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Education group trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Industry trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Panel B: Effects by mother's education										
Born after displacement × Education ≤ HS	-0.266	-0.035	0.025	-2.270	8.023*	0.173**	-0.361	-0.007	-0.018	-0.010
	(0.231)	(0.123)	(0.018)	(2.369)	(4.403)	(0.083)	(0.360)	(0.064)	(0.168)	(0.048)
Born after displacement × Education > HS	-0.245*	-0.201**	0.002	-0.564	-3.977	-0.014	-0.107	-0.026	0.051	-0.006
	(0.127)	(0.088)	(0.012)	(2.251)	(4.197)	(0.082)	(0.174)	(0.041)	(0.192)	(0.048)
Child observations	1,213	1,332	1,309	1,396	1,394	1,394	958	1,586	1,448	1,607
Mothers	659	699	700	696	699	699	649	720	721	729
Mother fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Child-specific controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Education group trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Industry trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: Same as Table 2.

Table 6
Family Income and Birth Weights

Dependent variable:	Ln birth weight		Weight < 88 ounces	
	(1)	(2)	(3)	(4)
Log Family Income	0.032***	0.040***	-0.030***	-0.036***
	(0.010)	(0.013)	(0.009)	(0.012)
Child observations	2,263	2,263	2,263	2,263
Mothers	1,614	1,614	1,614	1,614
Additional controls	no	yes	no	yes

Notes: Additional controls include year fixed effects and mother's age fixed effects. Standard errors are clustered on the mother. Regressions are weighted using children's sample weights.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table A1
Distribution of Birth Weights in Analysis Sample Versus 1990 Vital Statistics

	Fraction in Analysis Sample	Fraction in 1990 Vital Statistics
Birth Weight $<$ 18 ounces (500 grams)	0.00	0.00
Birth Weight $<$ 35 ounces (1000 grams)	0.00	0.01
Birth Weight $<$ 53 ounces (1500 grams)	0.01	0.01
Birth Weight $<$ 71 ounces (2000 grams)	0.03	0.03
Birth Weight $<$ 88 ounces (2500 grams)	0.06	0.07
Birth Weight $<$ 106 ounces (3000 grams)	0.22	0.23
Birth Weight $<$ 123 ounces (3500 grams)	0.55	0.60
Birth Weight $<$ 141 ounces (4000 grams)	0.85	0.89
Birth Weight $<$ 159 ounces (4500 grams)	0.97	0.98
Birth Weight $<$ 176 ounces (5000 grams)	0.99	1.00

Note: Author's calculations. 1990 is chosen as the comparison year because it is the median year of birth for the analysis sample.

Table A2
Estimated Impact of a Husband's Displacements Due to Plant/Business Closures on the Birth Weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable: Log Birth Weight							
Born after a displacement	0.020	-0.046	-0.045	-0.122***	-0.064	-0.062	-0.076
	(0.016)	(0.028)	(0.029)	(0.042)	(0.049)	(0.049)	(0.049)
Mother ever experiences displacement		0.068**	0.065**	0.134***			
		(0.026)	(0.027)	(0.038)			
Child observations	2,082	2,082	2,082	1,233	1,233	1,215	1,177
Mothers	1,403	1,403	1,403	554	554	550	534
Child-specific controls	no	no	yes	yes	yes	yes	yes
Siblings-only sample	no	no	no	yes	yes	yes	yes
Mother fixed effects	no	no	no	no	yes	yes	yes
Education group trends	no	no	no	no	no	yes	yes
Industry trends	no	no	no	no	no	no	yes
Dependent Variable: Probability of Low Birth Weight							
Born after a displacement	-0.019	0.028**	0.028*	0.051**	-0.005	-0.003	0.012
	(0.014)	(0.014)	(0.015)	(0.023)	(0.025)	(0.024)	(0.026)
Mother ever experiences displacement		-0.049***	-0.049***	-0.061***			
		(0.012)	(0.012)	(0.016)			
Child observations	2,082	2,082	2,082	1,233	1,233	1,215	1,177
Mothers	1,403	1,403	1,403	554	554	550	534
Child-specific controls	no	no	yes	yes	yes	yes	yes
Siblings-only sample	no	no	no	yes	yes	yes	yes
Mother fixed effects	no	no	no	no	yes	yes	yes
Education group trends	no	no	no	no	no	yes	yes
Industry trends	no	no	no	no	no	no	yes

Notes: Same as Table 2.

* significant at 10%; ** significant at 5%; *** significant at 1%