Determinants of Birth Outcomes: A Study Using Variation in Tornado Exposure

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Abstract

Previous literature finds that adverse events are correlated with poorer infant outcomes, but the timing and mechanisms by which this occurs are poorly understood. This paper uses tornadoes as a source of random variation in exposure to stressors. First, using detailed data on the date of conception, I find that among the 1999-2007 conception cohorts there is evidence of selection: exposure to tornadoes during the second trimester of gestation leads to fewer live births and to changed sex ratios among live births. Second, I argue that tornado destruction represents an exogenous decrease in wealth and investigate its impact on birth outcomes. Conditional on being exposed to tornadoes, tornado damages do not significantly affect live births outcomes. However, repeated exposure during the third trimester is associated with prematurity and lower weight at birth.

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Introduction

This paper studies the prenatal environment and its impact on infant health by exploiting tornadoes[1] as a natural experiment in maternal exposure to stressors. This line of research makes two distinct contributions. First, although some prior studies find that extreme weather affects the birth weight and gestation length of live births (Simeonova 2011), other papers find evidence only of labor complications, with no effect on birth weight or gestation (Currie and Rossin-Slater 2012). Using a more detailed dataset on tornadoes and careful matching of exposure by date of conception,[2] which reduces measurement error in the independent variable, this research documents that during the 1999-2007 period tornado exposure was associated with significant changes in the number of live births and in sex ratios among live births. Such selection effects explain how exposure to tornadoes can be associated with equal or better infant health when it leads to culling of the weakest. I also find evidence that repeated exposure affects birth outcomes over and above a single instance of exposure. Such non-linearities along with fetal attrition could explain inconsistencies in previous literature linking exposure to stressors in utero to birth outcomes.

Second, I argue that tornado destruction represents an exogenous decrease in wealth. Prior literature on the relationship between changes in wealth, as a stock variable, and infant outcomes is sparse; however, under consumption smoothing, a one-time wealth shock can be understood as a permanent decrease in income/consumption.[3] By comparing the birth outcomes

[1] Definition: A violently rotating column of air extending from a cumuliform cloud or underneath a cumuliform cloud, to the ground, and often (but not always) visible as a condensation funnel. In order for a vortex to be classified as a tornado, it must be in contact with the ground and extend to/from the cloud base. On a local scale, it is the most destructive of all atmospheric phenomena. Storm Data, National Oceanic & Atmospheric Administration.

[2] In this paper "date of conception" is used interchangeably with "date of last menstrual period" (LMP). Since the date of conception is only approximate (estimated to be around two weeks after the date of LMP), physicians use the date of LMP in calculating gestation.

[3] Insurance may ensure that tornado damages are only a temporary income shock to be resolved upon the resolution of claims.

of women exposed to tornadoes that were not accompanied by material damage with those of women exposed to tornadoes leading to damage, this analysis isolates the causal effect of income on health, an important topic that still remains poorly understood because of endogeneity concerns (Currie 2009; Almond et al. 2011).

This inquiry is motivated by a growing literature on the effect of stress on birth outcomes, a literature that does not yield consistent findings with respect to the timing and mechanisms of this effect. One possible reason is that some sources of stress affect women through their impact on nutrition, which, as shown below, is found to be most harmful during late pregnancy, while other sources of stress affect women by causing distress,[4] which is posited to have an impact in early pregnancy.

Another potential reason for the inconsistent findings is that omitted variables might confound and bias the association between maternal stress and nutrition on one hand, and birth outcomes on the other. For instance, when the source of stress is an economic shock (e.g., recession), the estimated effect will be biased if the mother's socioeconomic characteristics are correlated with the severity of the shock (Dehejia and Lleras-Muney 2004). For this reason, a compelling research design looks for exogenous shocks caused by conditions outside the mother's control. A new strand of literature has emerged that attempts to identify exogenous sources of stress, such as earthquakes (Glynn et al. 2001; Torche 2011), the September 11th attacks (Eskenazi et al. 2007), and the harassment of Arab and Arab-American women in California after the September 11th attacks (Lauderdale 2006). However, these results, which rely on variation from a one-time event, are not generalizable because of the potential socio-economic particularities of the demographic groups investigated. This paper uses variation in

[4] The term "stress" is used in this paper to denote the catchall effect of everything other than income/wealth changes. Thus, stress in this paper is a black box, because although we can speculate on what it may comprise, we do not in fact know.

exposure to tornadoes as a source of random variation in maternal exposure to stressors. Geographically, tornadoes can potentially affect most areas of the United States (Figure 1), and thus, their estimated impact can be interpreted as the impact on the average woman.

Using data on a one-time event has another important caveat. Differences in fertility by date of conception across racial demographic groups (Deschenes et al. 2009) and socioeconomic groups (Buckles and Hungerman 2010) could be correlated with mothers' health (see also Figures 2 and 3 for seasonal patterns in fertility across maternal age); thus exposure may have differential effects depending on the season when the stressor occurs. In addition, when the source of stress is a one-time event, its impact cannot be completely disentangled from the effects of other environmental changes taking place at the same time. These sources of confounding can be addressed only in a panel setting that enables the researcher to control for factors specific to the date of conception. I use time variation in exposure to tornadoes to compare outcomes across cohorts defined by the county, month, and year of conception.

Using tornadoes as a source of variation has other important advantages. Tornadoes are a frequent-enough phenomenon that at least some women are exposed several times during pregnancy, thus making it possible to test the hypothesis of adaptation. In addition, tornadoes affect all socio-demographic groups. Thus, not only are the results highly representative of the entire country, but they also allow for comparisons across groups and the subsequent identification of the most vulnerable.

By conditioning on county-by-month fixed effects and state-by-year fixed effects, I find evidence supporting the theory that exposure to tornadoes has significant selection effects. Exposure to tornadoes is associated with fewer births per woman and a lower sex ratio (proportion male), which are explained by fewer very premature live births. There are non-linearities in the effect of repeated exposure to tornadoes. In addition, conditional on being exposed to tornadoes, higher damages are not significant predictors of gestation and birth weight at conventional levels. All these effects vary by mothers' age, suggesting that estimates of the average effects in the population vary with the socio-demographic structure of population and the initial level of stress prior to the stressor investigated. Researchers, therefore, should be careful in extrapolating their results beyond the period and the sample investigated.

I. Background

1.1. Mechanism and timing of the effect of tornadoes on infant health. Pregnant women's exposure to tornadoes can have deleterious effects on the fetus for several reasons, which include but are not limited to: changing patterns of nutrition during and following the event, poorer access to health care because of travel restrictions, and psychological distress.

The biological transmission of maternal stress to the fetus is not yet fully understood. The main mechanism suggested in the literature is an increased placental Corticotrophin-Releasing Hormone (CRH) around weeks 30-33 of gestation, which has been shown to predict preterm delivery (Mulder et al. 2002; Sandman et al. 2006; Wadhwa et al. 2004). The link between maternal stress and heightened CRH levels in the early third trimester is established only for stress experienced *up to the early- to mid-second* trimester. Hobel et al. (1999) and Sandman et al. (2006) find maternal stress at 18-20 and 15 weeks of gestation, respectively, to be correlated with placental CRH levels in the early third trimester. Overall, these results suggest that distress is most likely to affect the probability of miscarriage, stillbirth, and prematurity when experienced up to mid-pregnancy. Studies of extreme events, such as earthquakes (Glynn et al. 2001; Torche 2011) or conflict (Mansour and Rees 2011; Valente 2011), are consistent with this literature. If distress is the main driving factor behind the impact of tornadoes, the timing of the effect might be relatively early in the pregnancy. Note, however, that extreme stress later in the pregnancy has also been associated with fetal loss. Wisborg et al. (2008) find that women who report experiencing a high level of psychological stress around 30 weeks of gestation are 80 percent more likely to suffer a stillbirth than those who do not.

In contrast, the evidence on the impact of maternal malnutrition on fetal and infant health is more suggestive of an impact in late pregnancy. Studies on the Dutch famine support this theory. Stein and Susser (1975), Roseboom et al. (2001), and Painter, Roseboom, and Bleker (2005) find that exposure to famine in late gestation is associated with decreased weight and length at birth, and shorter gestation, while the effects of exposure during early-to-mid pregnancy are smaller, potentially due to fetal loss (Roseboom et al. 2001). Interestingly, this explanation implies that nutrition may have significant selective effects earlier in pregnancy.

There is also some evidence of a higher incidence of low birth weight (LBW) among fasters during the second trimester (Cross et al. 1990). Siega-Riz et al. (2001) examine women's diets during the second trimester of pregnancy in North Carolina and find that women who do not follow the optimal guidelines of three meals and two snacks per day, are 30% more likely to deliver pre-term. In fact, Shahgheibi et al. (2005) find no significant effect of fasting during the third trimester on birth- weight. However, this particular study uses a small sample.

Only a minority of studies find an effect from changes in nutrition early in pregnancy. Among these, Gluckman and Hanson (2005) emphasize the importance of glucose supply during early embryonic development, and Almond and Mazumder (2011) find that the most deleterious effects of fasting occur during early-to-mid pregnancy.

Overall, with few exceptions, the previous literature is consistent with the idea that if tornadoes' effect on live births is mediated by changes in nutrition, the likely relevant timing is later in pregnancy. The study that most closely resembles this one, Simeonova (2011), finds that exposure to natural disasters, including tornadoes, is particularly deleterious in the second and early-third trimesters, which suggests a mostly nutrition-based effect. However, due to data limitations, that study uses only variation in storms that produced more than 50,000 USD in damages (in 1960 dollars) or at least one death.[5]

The caveat and possible explanation of some inconsistencies is that the timing of conception (or date of last menstrual period, LMP) is not always measured correctly, thus introducing significant measurement error into the exposure to stressors. A variable defined as exposure 7-9 months before birth would contain significant measurement error for premature births and introduce downward bias into the estimated effect of exposure during the first trimester.[6] Such measurement error leads to attenuation bias, which could at least partially account for inconsistencies in the estimated effect and the relevant time of exposure. I use detailed data on the date of LMP to pin down the timing of exposure.

Comparing cohorts defined by the date of conception also makes it possible to investigate the question of "missing" births, i.e. fetal attrition, among women exposed to tornadoes.

1.2. Do tornadoes provide valid exogenous variation in exposure to stress and income loss? Many sources of stress are unequally distributed along socioeconomic and racial groups lines. Therefore, results obtained from investigating such sources of stress may be limited to the

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[5] Because, in that study, the control group includes both women exposed to tornadoes without damages and women not exposed to tornadoes.

[6] For instance, the cohort of infants born in December in a county with tornadoes in the previous April would aggregate over full-term babies conceived in April and exposed during first trimester and premature babies conceived in May and not exposed and count them as having been exposed to stress. On the other hand, if in the following year, the same county experiences a tornado in July, the next December birth cohort would aggregate full-term babies not exposed during the first trimester and premature babies exposed, but count them as not exposed. An analysis comparing these cohorts may conclude that there is no effect from exposure to stress during the first trimester.

particular demographic group usually affected. Using tornadoes as a source of variation is particularly suitable to investigate the effect of stress because of the low potential for selectivity.

First, although certain areas are more likely to be affected by tornadoes than others, these areas are quite vast (see Figure 1), so avoidance is difficult, unless there is willingness to move very large distances. In addition, the probability of being directly affected by a tornado in any of these tornado-prone areas is too low to induce significant selection by risk aversion, which could be correlated with birth outcomes. The two regions with the most tornado days are northeastern Colorado and peninsular Florida, but even in these areas, the peak values of the total threat of tornado touchdown is 1.5 tornado days per year (Brooks et al. 2003).

It is possible, however, that destructive tornadoes produce ex-post displacement among people in their path. To control for this possibility, I retain the sample of births to women who reside in the same state where the birth took place. In addition, I report results using only variation in low-intensity tornadoes that were, thus, unlikely to have led to displacement.

Second, tornadoes are very hard to predict. Weather models provide indications that there is a higher risk on one day versus another, which contributes to the decision to issue a tornado watch (not predictions), but these models cannot predict the exact location where a tornado will form. Third, these events are highly stressful even in the absence of property damage or direct injury. If a tornado is sighted, a tornado warning is issued (and sometimes there is no warning), and the population is urged to take immediate measures of protection. Even when the area is not hit, because tornadoes are quite narrow and their paths unpredictable, the heightened risk of potentially significant loss can be stressful. The opposite might also be true. Because of the known low probability/expectation that a tornado would hit an area under a tornado watch, people might not react to a warning. Such behaviors explain some of the injuries and deaths that result from tornadoes. The surprise of an actual tornado following an ignored tornado watch would certainly be highly stressful.

Fourth, it is possible to identify in the data the tornadoes that were not associated with property damage, thus isolating the income effect from everything else, which for ease of exposition will be referred to as "stress" in this paper.[7] Fifth, the variability in exposure to tornadoes across time (tornadoes exhibit significant variability from year to year) and across geographical areas provides a plausible way to investigate the effect of repeated exposure. As shown in Figure 1, there is significant variation in the intensity of exposure in the same geographical area from one year to another, as well as significant variation across geographical areas. In the data section, I examine the tornado data in more detail.

1.3. Doing research with birth data. A complicating factor when studying the impact of shocks on birth outcomes is that such shocks not only may have an (adverse) scarring effect on those exposed, but also may lead to increased rates of mortality in-utero, and therefore to positive selection of survivors (see Bozzoli, Deaton, and Quintana-Domeque 2009 for a formal exposition of the offsetting effects of scarring and selection on childhood mortality). Selective effects of stress are expected to cull the weakest. Valente (2011), for example, finds that civil conflict increases the likelihood of miscarriage, and Wisborg et al. (2008) find that women who report experiencing a high level of psychological stress are more likely to suffer a stillbirth. Such selection effects could obscure the effects of shocks on measures of health obtained using surviving infants only. In particular, the adverse effect of exposure on the health of live births would be biased downward. I use data on live births per woman to conduct a comprehensive

[7] Clearly income loss due to tornadoes is also stressful, but so is income loss due to any other factor.

investigation into the effects of tornadoes on pregnancy outcomes by investigating the hypothesis of missing births.[8]

1.4. Stress and/or income. Throughout the paper I use the word "stress" as a catchall term for whatever processes affect birth outcomes when the mother is exposed to severe weather events (i.e., tornadoes). In addition to stress, some tornadoes cause property damage, while others do not, depending on their path and the quality of structures in their path. Thus, in addition to the stress of exposure, some women will suffer a sharp change in wealth, even if only a temporary one (if fully insured against such loss).

Existing evidence suggests that there is a significant correlation between parents' socioeconomic status and infant outcomes. However, the literature attempting to identify the causal effect of income on child health, especially in developed countries, is scant. Currie (2009) provides an excellent review of this literature and the problems of identification encountered in previous research, which I mention only briefly here. For instance, the literature using welfare-to-work programs cannot isolate the effect of changes in income from the effect of changes in employment (Smolensky and Appleton 2003). A similar problem is encountered by research relating recessions while in utero to infant health (Van den Berg, Lindeboom and Portrait 2006). Other studies, such as Duflo (2000), using pension reform in South Africa that brought black pensioners up to parity with white pensioners; Costello et al. (2003), using cash transfers that resulted from a casino opening at an Indian reservation; and Almond, Hoynes, and Schanzenbach (2011), using variation in income from food stamps availability, apply only to a restricted socioeconomic group. Other research using direct measures of income analyze only limited data on children's health (Berger, Paxon, and Waldfogel 2009) or make strong assumptions (Burgess

[8] Although data on fetal deaths is available, it is highly unreliable for fetal deaths of under 28 weeks of gestation, which precludes any analysis of early fetal attrition.

et al., 2004). Perhaps more closely related to this research, Conley and Bennet (2000, 2001) find that income has an effect on birth outcomes only among mothers of low-birth-weight infants, but they use a very small sample.

Overall, it appears that although the correlation between income and infant health outcomes is quite robust across various samples and estimation strategies, there is still more work to be done before a causal effect of income on infant outcomes can be established.

II. Empirical strategy

A substantial literature documents the existence of seasonality in birth weight. Specifically, the lowest mean birth weight appears in late spring and summer, and there is evidence to support that it is driven by both seasonal effects on intrauterine growth (Murray et al. 2000) and on gestational length (Matsuda et al. 1993). Because tornado season in much of Tornado Alley[9] occurs in April through June, disentangling the effect of exposure to low temperatures during the second trimester of pregnancy from the effect of tornadoes during the third trimester of pregnancy presents some challenges. It requires data across several years so that comparisons of health outcomes in the same season of birth can be made over time.

I utilize a quasi-experimental strategy that exploits variation in tornado density across counties and over time. The following equation is estimated:

$$h_{c,m,y} = \alpha + \sum_{j=1}^{3} \beta_j \, Tornado_{c,m,y,j} + \delta X_{c,m,y} + \mu Z_{c,y} + \lambda_{c,m} + \gamma_{s,y} + \varepsilon_{c,m,y}$$

where *h* is the prevalence of an outcome of interest among live births in each cohort defined by the county-year-month of conception. In this equation, *c* stands for county, *y* stands for year of conception, *m* for month of conception, and *j* for the pregnancy trimester of exposure to

[9] Tornado Alley signifies the area in the southern plains of the central U.S. that consistently experiences a high frequency of tornadoes. (National Climatic Data Center).

tornadoes. β is the coefficient of interest, measuring the impact of exposure to tornadoes. Tornado exposure is measured as the percent live births in a cohort that was exposed to tornadoes during each trimester of pregnancy. In specifications investigating the role of wealth shocks, tornado damages are measured in thousands of dollars (in 2007 dollars) losses per capita, where only damages associated with tornadoes affecting the births in our sample were retained. $X_{c,m,y}$ is a vector of mothers' demographic characteristics: percent under 25 (omitted), 25-34, or over 35 years old; percent married; percent White (omitted); percent Black; percent other race; percent Hispanic; percent with less than a high-school education (omitted); percent with a high-school education; percent with some college; percent with a college degree; and percent without prenatal care; and infants' characteristics: percent female, percent first child, percent second child, percent third child, percent fourth child, and percent fifth or subsequent child (omitted).

The $Z_{c,y}$ vector contains variables such as hospitals per 100,000 people, log per-capita personal income, and percent of children under 17 years of age living in poverty. The hospitals variable acts as a proxy for ease of access to medical care, which could mitigate the effect of tornadoes. The controls for income and poverty prevalence are included because one of the goals of this paper is to investigate the existence of an income effect due to damages. Because tornadoes are more likely to produce significant damage in areas with poor-quality structures, it is important to control for income as a source of confound. In addition, because practically all means-tested programs are administered at the state level, including the state-by-year fixed effects accounts for any effects of such programs that might be correlated with higher damages from tornadoes in poorer areas. $\gamma_{s,y}$, the vector of state-by-year fixed effects, captures time-varying differences in the dependent variable common to all counties in a state, such as changes in health-care policies.

All specifications include county fixed effects that are allowed to vary by month, $\lambda_{c,m}$. As a result, differences in permanent, season-specific determinants of pregnancy outcomes at the county level do not confound our estimates. $\varepsilon_{c,m,y}$ is the error term.

One of the outcomes of interest is prematurity. Being born prematurely reduces the probability of exposure in utero during the third trimester, because there are fewer days of potential exposure. This induces a negative correlation between exposure and prematurity, because exposure during the third trimester effect of tornadoes would reflect the ability to survive in-utero at least 27 weeks,[10] a measure of the health of the pregnancy. To account for this possibility, I follow Currie and Rossin-Slater (2012) and instrument exposure with potential exposure under the hypothetical scenario that all infants would reach 39 weeks of gestation.

The relationship between weather shocks and health could be highly nonlinear so some specifications test the impact of repeat exposure. In addition, this relationship between exposure and heath may differ across demographic groups. Previous literature suggests that mothers in poor condition are more likely to experience fetal loss, especially male fetal loss (Trivers and Willard 1973; Song 2012). This could have implications for the external validity of average estimates of the effect of stress when the effect is non-linear and the demographic structure of population changes. I investigate the heterogeneity of the tornado effect by maternal age, a factor known to correlate with a higher probability of male fetal loss (Almond and Edlund 2007).

Two additional issues about the estimation strategy should be mentioned. First, estimates obtained from counties with large populations are more precise than those from smaller counties. To control for this source of heteroskedasticity, this paper reports regressions weighted by the number of births. Second, the error terms may be correlated within counties over time.

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[10] The first trimester is the first 13 weeks after conception, the second trimester is weeks 14-26, while the third trimester starts with week 27 of gestation.

Misspecification of the autocorrelation process can lead to downward bias in the standard-error estimates (Bertrand, Duflo, and Mullainathan 2004). Consequently, robust standard errors clustered at the county level that allow for heteroskedasticity and autocorrelation of unspecified form are calculated and reported throughout the paper.

A similar specification is used to estimate the impact of tornado exposure on fertility (live births per 10,000 women of fertile age). In those specifications, $Z_{c,y}$ is a vector of demographic characteristics of women aged 18-44 in the county (percent Black, percent women 18-24 years old (omitted), percent 25-34, and percent 35-44 years old among women of fertile age, i.e. 18-44 years old), as well as a measure of access to medical care (hospitals per 100,000 people) and measures of income (log per-capita personal income and percent children under 17 years of age living in poverty).[11] These regressions are weighted by the number of women of fertile age (18-44 years old) in a county-year.

III. Data

This paper uses data from the National Center for Health Statistics (NCHS) Natality Files, which provide a census of all births. The data include information on newborns' health, parents' demographic characteristics, and mothers' pregnancy history and prenatal-care history. I retain data for singleton births, which insures better comparability across time. To reduce potential bias due to displacement, I retain only those women who gave birth in their state of residence.

These data are ideally suited for this research because, for most observations, Natality Files provide the exact date of the last menstrual period (LMP)[12], which permits the identification of exposure with a high degree of precision. Because the literature documents the

[11] In this specification, the vector of variables $X_{c,m,y}$ is empty.
[12] Starting 2009 Natality data not longer reports the day of LMP, so the last conception year used in his paper is 2007, leading to births in 2007 and 2008.

problem of selective reporting of LMP based on socioeconomic status (Hediger et al. 1999), for the remaining observations, the date of conception is calculated based on the date of birth and gestation (as estimated by the physician).[13] This procedure allows me to pin down the month, but not the day, of conception. For the day of conception, I impute the middle of the month (the 15th of the identified month of conception). All observations with missing data on gestation (less than 2% of observations) are dropped.

The weather data and Natality data are matched based on county of residence and date of conception. This ensures the precise identification of the degree of exposure during the first trimester. In addition, I use the information on gestation to pinpoint the intensity of exposure during the third trimester, which is especially important for premature babies.

The weather data come from the National Oceanic and Atmospheric Administration (NOAA) Storm Data. This dataset provides information regarding the latitude and longitude of the event, the intensity of the event, and other event details pertaining to injuries, fatalities, and property and crop damages. I follow Deschenes and Greenstone (2011) to measure exposure by retaining those tornadoes that landed close to the population centroid of the county of residence and, thus, are more likely to have affected a significant share of the population. In particular, I retain data for tornadoes that landed within 100 kilometers of each county's population centroid.[14] This reduces the measurement error associated with aggregating tornadoes that landed within the borders of a county (by reducing the likelihood that tornadoes landing far from the residence of most women are counted). Even this measure is not perfect, and thus the results reported in this paper may still suffer from measurement error, which if classical, lead to

[13] In this dataset, less than 5% of observations have a physician-calculated date of conception. The results are robust to dropping these observations.

[14] Using this definition of exposure approximately 2.5 % of births were exposed in each semester. Women in different counties could have been exposed to the same tornado if it landed close enough to two different county centroids.

attenuation bias. Consequently, these results likely still underestimate the true effect of tornadoes and are, thus, conservative.

This measure accounts not only for the stress associated with witnessing a tornado but also with the stress of responding to a tornado warning. A tornado warning is issued when a tornado has been sighted in the area or was indicated by radar. Warnings identify the location of the tornado at the time of detection, the area (usually the counties) that could be affected, and the time period covered by the warning. The length of this area is equal to the distance the tornado is expected to travel in one hour. A tornado moves along the ground at 50 to 80 km per hour (Hyndman and Hyndman 2010). Thus, the 100-kilometer radius around the population centroid accounts for exposure of women during their activities outside the home and the expected area of the tornado warnings associated with the tornadoes in our dataset. It does not capture the entire effect of tornado watches. A tornado watch bulletin states approximately where and for how long the tornado threat will exists and identifies an area about 225 to 390 kilometers long.[15]

A different source of measurement error comes from the fact that not all tornadoes are of equal intensity. This analysis does not rely on the dollar amount of damages to infer intensity, because higher-quality structures are likely to withstand more stress without damage. It could be the case that by retaining only tornadoes that produce damage, the researcher would in fact retain only tornadoes that hit areas with poor-quality structures. In that case, the treatment group, women exposed to tornadoes, would likely include mostly women of low socioeconomic status, a known determinant of poor infant outcomes. The Storm data used in this paper identifies six levels of intensity classified according to the Enhanced Fujita scale: an F0 level tornado is associated with 40-72 mph winds, F1 with 73-112 mph winds, F2 with 113-157 mph winds, F3 with 158-207 winds, F4 with 208-260 winds, and F5 with 261-318 winds. The Enhanced Fujita

[15] The results are robust to using a 200 km radius. Results not reported but available on request.

scale corrects the errors in the previous Fujita scale by taking into account the quality of structures when estimating the speed of the wind that produced that damage. This analysis retains all tornadoes classified as severe (F3, F4, and F5).[16]

After identifying the exact degree of exposure (measured at the county level) of each birth by merging the weather data at the individual level, I collapse the individual-level data into county-year-month of conception cells and perform the analysis at this level. Other control variables, such as per-capita personal income, percent children under 17 living in poverty, and hospitals per 100,000 people, vary only at the county–year level. Per-capita personal income data come from the Bureau of Economic Analysis. The U.S. Census Bureau's Small Area Income and Poverty Estimates provide county-level data on the percent of people of all ages in poverty and the percent of people ages 0-17 living in poverty. The number of hospitals comes from the U.S. Department of Health and Human Services - Area Resource Files.

Summary statistics for the main sample used for the analysis are reported in Table 1. Because early county level poverty data are sparse and consistently reported only after 1997, and because the 1997-1998 years were exceptional weather-wise,[17] only the 1999-2007 cohorts defined by year of conception are used in this study. The means and standard errors of variables used are shown for all births by exposure and timing of exposure to tornadoes. Cohorts exposed to tornadoes and those not exposed appear to be virtually identical with respect to demographic characteristics. The only difference is the prevalence of African-American population, and this is

[16] Tornadoes at levels F0 and F1 are considered weak. NOAA's National Weather Service provides the following description of the meaning of Enhanced Fujita Scale: F0 (Gale), F1 (Weak), F2 (Strong), F3 (Severe), F4 (Devastating), and F5 (Incredible). There are very few F4 and f5 tornadoes relative to the number of F3 tornadoes so the data used in this paper is quire homogenous in terms of intensity of exposure. The results are robust to excluding observations subject to exposure to F4 and F5 level tornadoes – results reported in Appendix not for publication.

[17] "Data tabulations, maps and time series of temperature and precipitation anomalies show the century-scale significance of the climate of 1997 and the December- January-February 1997-98 period for the U.S. and the globe. Throughout the winter a number of states and regions have experienced hundred year record rainfall and temperature." Special Climate and Weather Summary, US Department of Commerce, NOAA/National Weather Service, April 7, 1998.

driven by the fact that only the Eastern, Middle, and Southern parts but not the Western part of the United States are exposed to tornadoes (Figure 1). Similarly, there are no statistically significant differences in the demographic characteristics among the groups of mothers exposed during the first, second, or third trimester. The prevalence of poor infant outcomes appears to be slightly higher among cohorts exposed to tornadoes than among those not exposed (Table 1). Caution should be used, however, in drawing conclusions based on the relation between raw means.[18]

IV. The Impact of Tornado Exposure

4.1. Main Results

Using an OLS with fixed effects specification, I find evidence in support of the Trivers and Willard's (1973) hypothesis that mothers in poorer condition have more daughters.[19] As shown in Table 2, exposure to tornadoes in the second trimester leads to an increase in the share of female births. This appears to be achieved through the culling of the weakest, because I find that exposure to tornadoes is negatively correlated with the proportion of premature births. There is also evidence that exposure in the third trimester is associated with longer gestation. There is no evidence, however, of an increase in the share of female births from exposure in the third trimester. In addition, after instrumenting for exposure using potential exposure, under the hypothesis that all infants would have reached 39 weeks of gestation, all coefficients corresponding to the third trimester exposure become smaller and statistically insignificant. For the remainder of this paper, only the IV specifications are reported. The effects are, as expected, fairly small. Extrapolating from these results, cohorts in which all women were exposed to

[18] Controlling for county fixed effects is enough to remove the positive association between tornadoes and the prevalence of premature or very premature births (results not reported but available on request).

[19] In contrast, Helle et al. (2009) find that male births increased during World War II and also during warm years.

tornadoes during the second trimester would have ~0.15% more female births, ~0.11% fewer premature births, and an average gestation 1.8 weeks longer than cohorts I which no women were exposed to tornadoes.

It is possible that in the second trimester a fetus is viable outside the womb,[20] but the viability may differ by sex. Haig and others have suggested that the relation between mother and fetus can usefully be viewed as genetic conflict (Trivers 1974, Moore and Haig 1991). For instance, as in the case of humans, male elephant seal pups are heavier at birth than females, and the smallest elephant seal mothers only give birth to females, which suggests that they miscarry male pups. This may be an advantage if they are unable to raise a male pup to a viable size without jeopardizing their own survival and reproductive success (Fedak et al. 1996). Such a mechanism would make male mortality more responsive to the prenatal environment and increase the incidence of fetal death among males.

To test this hypothesis, I estimate the effect of exposure by gender (Table 3). A change in sex ratio would be consistent with steeper selection of the healthiest among male births. Results reported in Table 3 provide support for this hypothesis: One standard deviation around the estimated impact on premature male births excludes the estimated effect on female births.

4.1.1. Robustness checks

These results are robust to a wide series of specification tests, which are reported in the online Appendix Table A1. This section briefly summarizes these findings. Another potential interpretation is that the results are driven by displacement. By retaining only the births to mothers that report giving birth in their state of residence, I reduced the potential bias associated with displacement across state borders. However, displaced households may be moving across

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[20] At 24 weeks, the chance of survival is about 50% (Breborowicz 2001), and it increases afterwards.

counties within the same state. One way to test whether the results are driven by displacement is to investigate the effect of tornadoes that were not strong enough to trigger displacement. I remove all cohorts conceived in a state hit by tornadoes of F4 or F5 intensity in the year prior, of, or after the tornado landing. In this manner, I remove all cohorts potentially affected directly by high-intensity tornadoes and the cohorts that may suffer from selection due to prior displacement. I find qualitatively similar results, but perhaps surprisingly, the estimated effect on sex ratios is even larger. A possible explanation is that by removing the cohorts that experienced strong tornadoes, I retained only those cohorts born in areas less likely to be exposed to tornadoes. In these areas, the element of surprise is higher and, thus, perhaps the stress is more significant. This hypothesis finds some support in the findings following the investigation of the possibility that relative exposure matters.

If there is acclimatization/adaptation, exposure relative to the expected normal could be a better predictor of outcomes. I define "normal" as the average exposure in a county-month over the period investigated, and redefine exposure as deviations from this mean. The results are very similar, providing further reassurance that the results reported capture the effect of tornadoes.

I find that exposure in the months before conception is not a significant predictor of birth outcomes. At the same time, exposure in the months after birth should not matter. It should be noted here that I use weeks of gestation after conception to identify the date of birth, but gestation is measured in weeks, and thus, there is some measurement error in this variable.[21] There is no evidence of a significant effect from exposure after birth.

The results are robust to excluding western states that are not affected by tornadoes, and to excluding very small county-year-month cohorts.

[21] Natality data provide information only about the month and year of birth, and therefore, I could not solve that problem based on the reported date of birth in the data.

4.2. Effect on Fertility

All these results are consistent with the hypothesis that tornadoes have strong selection effects and lead to a smaller left tail of the birth weight distribution by reducing the left tail of the gestation distribution. The alternative interpretation is that tornadoes lead to an improvement in infant health. To further test the validity of the interpretation of the effect of exposure on live births outcomes, I estimate the effect of tornado exposure on the number of live births (Table 4). In these regressions, exposure is in fact potential exposure. If a tornado landed within 100 kilometers of a county population centroid, all women are assumed to have been exposed. For instance, the independent variable exposure during the first trimester is equal to one if a tornado landed within 100 kilometers from the county population centroid during the 13 weeks following the date of potential conception and zero otherwise. Similarly, exposure during the second trimester is equal to 1 in the case of tornado exposure during weeks 14-26, and exposure during the third trimester is equal to 1 in the case of tornado exposure during weeks 27-39 following each date of potential conception.

I find evidence that tornado exposure is associated with fewer births per 10,000 women but the effect is not significant at conventional significance levels. Exposure during the second trimester, however, is significant and negatively associated with male births but not female births (Table 4). This explains why the estimated effects of tornadoes on sex ratios among live births are statistically significant only for second trimester exposure.

V. Hit or Miss: Do Damages Matter?

In Table 5 I investigate whether wealth shocks affect birth weight and prematurity using exogenous variation in wealth driven by tornado property damage. The question of interest is whether the estimated effect of tornadoes is driven by the stress of exposure or by the income shock due to property damage.[22] Because there is a strong correlation between the intensity of exposure to tornadoes and the amount of damages, multicollinearity may make it difficult to disentangle the impact of damages from the effect of exposure in the framework used thus far. Consequently, I reformulate the question in a way that makes it possible to disentangle the effect of wealth shock. Specifically, I investigate whether, conditional on exposure to tornadoes, tornado damages affect pregnancy outcomes.

To estimate the effect of a wealth shock during the first trimester over and above just exposure to tornadoes, I retain the sub-sample of women who were exposed only during the first trimester but not in the second or third. These data are then collapsed into county-year-month of conception cohort cells. Within this sub-sample, I compare the outcomes of cohorts that also suffered property damage with the outcomes of cohorts that did not experience tornado-related damages. Because larger damages could be due to repeated exposure, I also control for the proportion of women in this sub-sample who were exposed to tornadoes more than once. This specification also allows us to investigate potential non-linearities in the effect of tornadoes, as previous literature suggests that repeated exposure to stressors may undermine the ability to cope and recover (Alderman, Hoddinott and Kinsey 2006; Alderman 1994). A similar exercise is done to estimate the effect of exposure in the second or third trimester.[23]

[22] Although weather data distinguish between "property damage" and "crop damage," by property damage I mean all damages to any type of property, i.e. the sum of reported "property damage" and "crop damage."
[23] Those that were exposed in two or more trimesters do not enter these regressions. It is more likely someone would have been exposed in the first and second trimester or in the second and third trimester but not in the first and third.

I find that repeated exposure during the third trimester is associated with shorter gestation and lower birth weight. It is possible that longer gestation improves the likelihood of survival in the event of a shock. There is thus significant variance in the effect of repeated exposure across trimesters of gestation.

It is important to note that such non-linearities imply that the results obtained using a linear specification on different samples are likely to be quite different function of the level of variation in tornado (or other stressors) exposure during the period investigated. In addition, such non-linearities could be driven by differences in expectations, stress-management strategies, etc., such that, again, the results could differ across samples and source of variation. To the extent to which the population investigated is exposed to stress repeatedly, researchers should always try to investigate the possibility of non-linear effects.[24]

I find that for the average woman tornado damages are not significant predictors of gestation and birth weight. The caveat is that this specification captures the effect of damages on births subjected to stress, which may not be the average birth. If the births in this sample are the surviving infants and, thus, are healthier, the results underestimate the effect.

Using a similar model specification, I find no evidence that the amount of damages has a significant effect on live births (Table 6).

VI. Discussion

Overall, the results reported in this paper suggest that both income and stress channels are significant predictors of pregnancy outcomes. However, it appears that the catchall "stress" has

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In addition, more babies are born in the summer and also, the tornado season is concentrated around the month of May; thus more births are exposed to tornadoes in the third trimester, than in the first trimester.

[24] These results could also explain the difference between my estimates and the estimated effect of tornadoes obtained by Simeonova (2011).

stronger selection effects than the wealth shocks. The income effect is small and not statistically significant at conventional levels.

It is important to note that these results cannot be extrapolated to conclude what is the effect of low socioeconomic status on health. Socio-economic status is associated with several differences in access to health inputs, some of which have and some of which do not have a counterpart in the effect of tornadoes. For instance, the income shock due to tornado destruction affects access to food and medical care, possible candidates for the mechanism of effect of socio-economic status on health. However, these estimates do not fully account for the higher opportunity cost of time of parents with higher wages. In addition, this paper identifies the effect on the average woman. The effect may differ across socioeconomic demographic groups because of the way each group chooses to use available inputs toward health production

Second, I identify the income effect from a temporary shock. Temporary shocks likely have smaller negative effects on health than persistent poverty (Currie 2009). Under consumption smoothing, a wealth shock can be understood as a permanent decrease in consumption. The availability of homeowners' insurance, however, could mean there is no long-term reduction in consumption (long-term here is to be interpreted as up to birth) but rather a larger short-term shock (if there are credit market failures) to be resolved upon the resolution of the insurance claim. Consequently, the effect of damages may vary with the degree of access to credit markets and the degree of coverage by homeowners/renters' insurance against tornado risk.

The results also suggest that repeated exposure matters. Repeated exposure during the third trimester leads to an increase in prematurity and low birth weight. This could explain differences in the estimated relevant timing of exposure and in the estimated impact of stress when the effect of a stressor depends on timing, and baseline level of stress.

In addition, the effect may vary with the demographical structure of women of fertile age. The question remains whether stress is associated with higher male mortality or with lower fetal attrition among demographic categories known to have a higher likelihood of a female birth for other reasons. To test this hypothesis, I investigate how the effect of stressors on the main population parameters differs among demographic groups defined by maternal age, which is known to be correlated with fetal loss (Almond and Edlund 2007).

I find evidence that the effect of tornadoes on live births is larger among younger women (Table 7). Similarly, the effect on prematurity is larger for young mothers i.e. under 25 years old. In fact, the percentage change in the prevalence of births under 37 weeks of gestation among women under 25 is 20 times greater than the change among births to women 25-34 years old. These results are consistent with theories regarding the genetic conflict between mother and fetus. If conditions are such that the survival of the current fetus will severely compromise the probability of survival of existing children or future offspring, then it is in the mother's interest not to invest in the fetus. However, if the situation is such that future reproductive success is low, a greater weight is given to the fetus instead of the mother. This would predict higher attrition among pregnancies to younger mothers with a longer reproductive period ahead than among older women who are exposed to the same degree of stress.

It is thus possible that statistically significant skewed gender ratios in response to stressors are identified only where there is higher proportion of births to younger women or of the type of women more likely to start fertility early.

VII. Conclusions and Future Research

This paper evaluates the health consequences of negative shocks during pregnancy. For this purpose, I exploit the natural experiment afforded by the incidence of one type of extreme weather event: tornadoes. I find that exposure to tornadoes is correlated with fewer live births among women of fertile age, and fewer live births in the lower tail of the gestation distribution. This study also finds that for the average birth most of the effect is due to exposure to tornadoes and not to the material damages associated with the exposure. It is possible that insurance against these events shields the average woman from the effect of such income shocks. Future research should investigate the role of insurance in mediating the impact of income/wealth shocks during pregnancy on infant outcomes.

Overall, the results of this research suggest that the shock of exposure to tornadoes, referred to in the paper as "stress," appears to influence mainly gestation and probability of survival, and thus most of the effect is concentrated at the left tail of the fetal-health distribution. Moreover, repeated exposure matters and thus a linear model may not be able to pick up the effect of exposure. This could explain the inconsistencies found in previous literature regarding the timing and mechanism of the effect of stressors on birth outcomes.

Our results are useful in identifying the causal effect of income changes and of one source of stress, tornadoes, on pregnancy outcomes. This study also points out an important fact that is perhaps sometimes ignored: The cost of negative shocks is not limited to poor infant outcomes, the medical cost and poorer educational and labor outcomes of surviving infants. The cost also includes the effect on mothers that experienced fetal loss, and this cost might be quite significant. Future research on the benefits/costs of policies targeting pregnancy outcomes should account for the loss in productivity experienced by these women.

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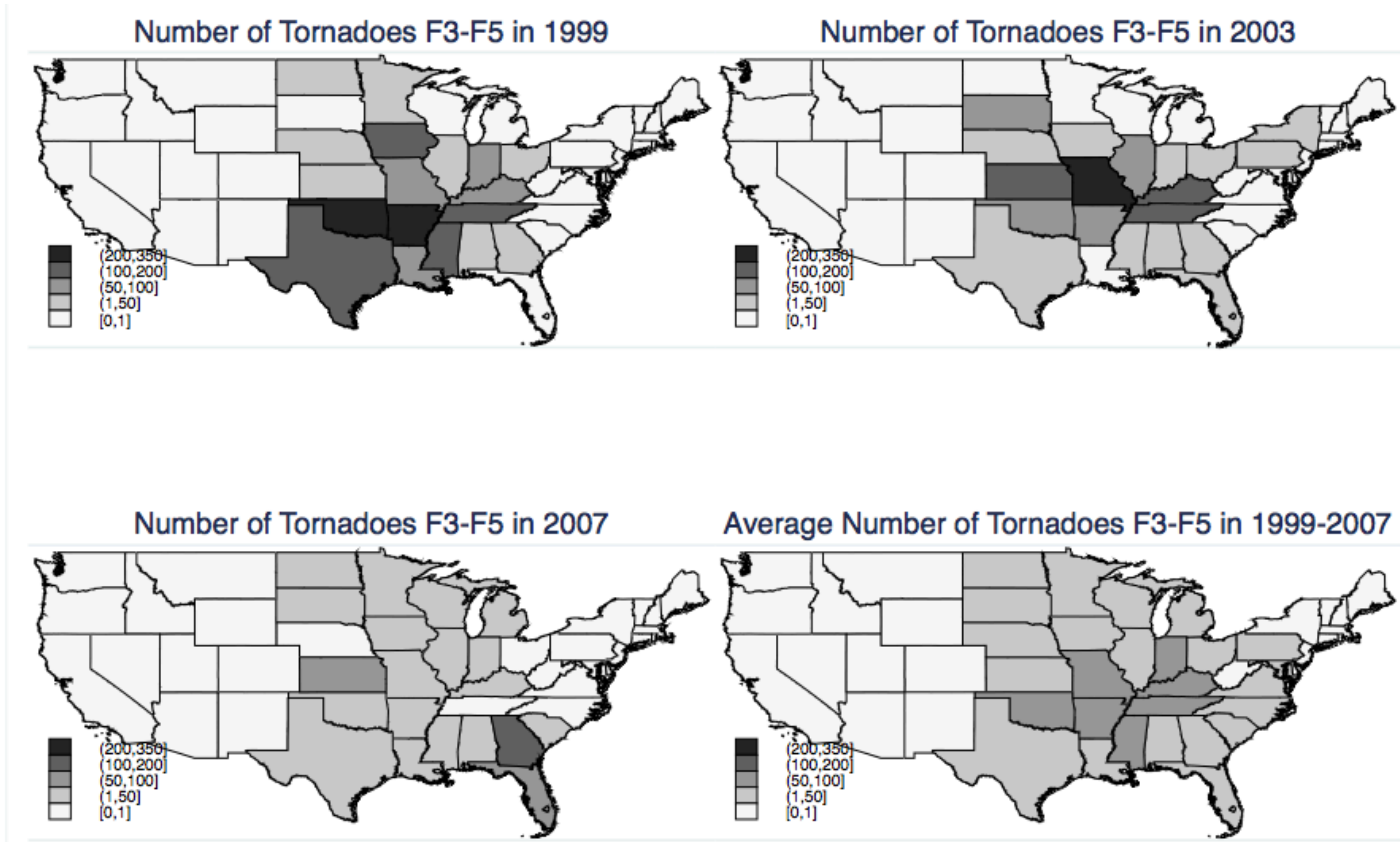

Figure 1. The Prevalence of Tornadoes Across States and Time
The data used in making these figures retains all tornadoes that landed within 100 km of each county population centroid (regardless of whether pregnant women were exposed to these tornadoes or not). The legend is the same for comparability, but not all ranges of tornadoes are represented in each graph.

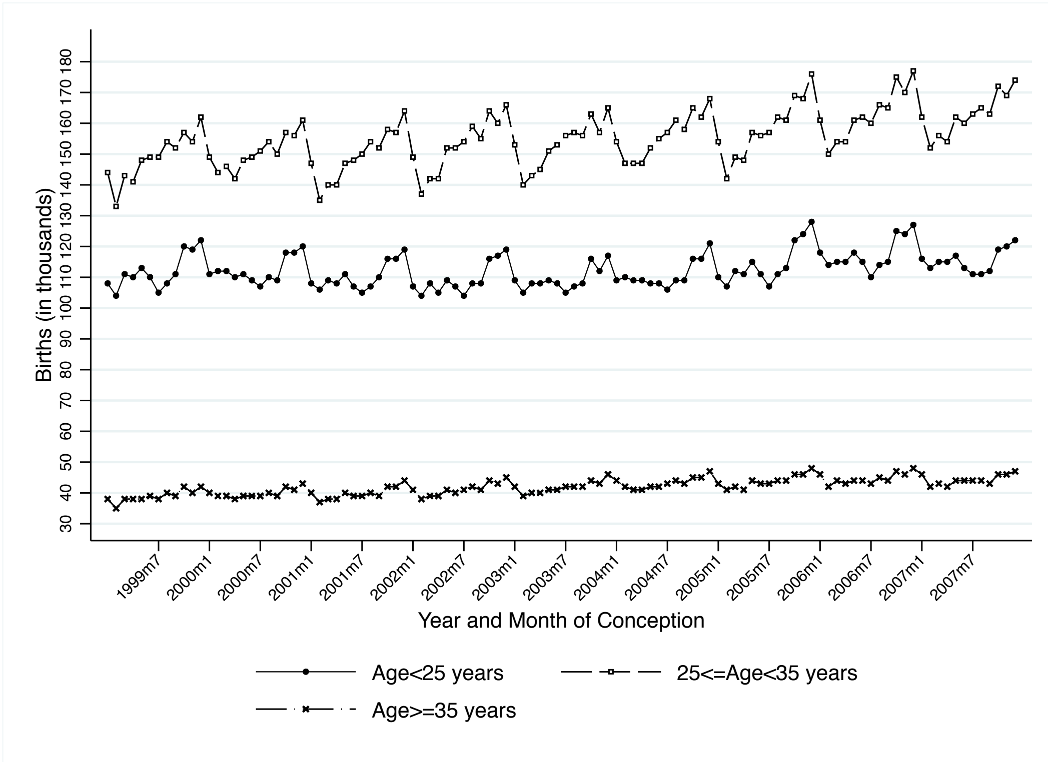

Figure 2. Number of Singleton Births by Mother's age, Year and Month of Conception

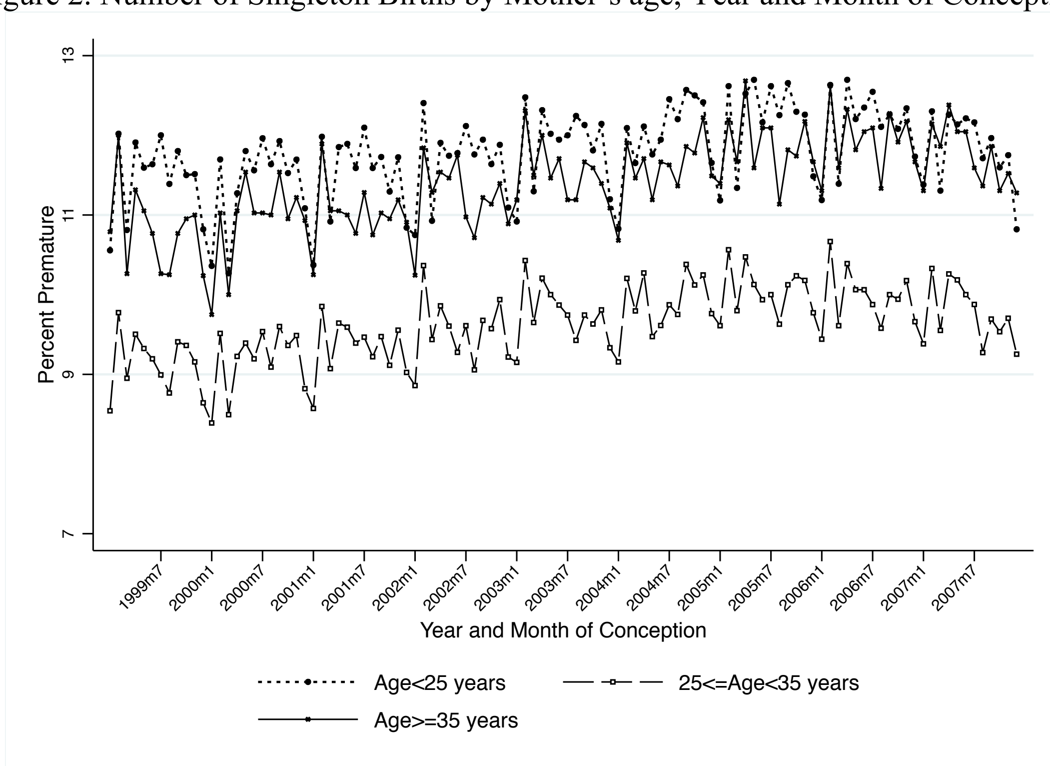

Figure 3. Percent Premature (<37 weeks) by Mothers' Age, Year and Month of Conception

Table 1. Weighted Summary Statistics

	Not exposed	Exposed 1st trim	Exposed 2nd trim	Exposed 3rd trim
%Gestation <37 weeks	10.562	11.476	11.429	11.505
	(3.799)	(4.686)	(4.622)	(4.550)
%Gestation <32 weeks	1.547	1.686	1.690	1.714
	(1.327)	(1.688)	(1.636)	(1.601)
%Birth Weight <2500 g	6.119	6.636	6.619	6.625
	(2.799)	(3.529)	(3.431)	(3.367)
%Birth Weight <1500 g	1.055	1.152	1.151	1.155
	(1.056)	(1.371)	(1.333)	(1.294)
% exposed 1st trimester	0.000	0.762	0.154	0.029
	(0.000)	(0.326)	(0.303)	(0.159)
% exposed 2nd trimester	0.000	0.161	0.758	0.132
	(0.000)	(0.308)	(0.327)	(0.285)
% exposed 3rd trimester	0.000	0.027	0.158	0.629
	(0.000)	(0.148)	(0.302)	(0.368)
% First child	33.671	34.211	34.115	34.045
	(5.676)	(6.406)	(6.287)	(6.186)
% Female	48.797	48.828	48.928	48.808
	(4.989)	(6.259)	(6.105)	(5.945)
% Age 25-34	50.136	48.330	48.736	49.091
	(7.538)	(8.969)	(8.824)	(8.822)
Age 35	13.879	11.173	11.361	11.390
	(6.002)	(5.429)	(5.376)	(5.298)
%High-school	29.699	31.285	30.799	30.378
	(8.416)	(9.375)	(9.289)	(9.150)
%Some College	22.590	22.566	22.648	22.817
	(6.632)	(7.444)	(7.461)	(7.395)
%College	25.618	24.569	25.171	25.599
	(12.162)	(12.322)	(12.330)	(12.252)
%Black	14.553	19.368	19.538	19.628
	(14.988)	(18.514)	(18.339)	(18.371)
%Married	63.741	62.543	62.412	62.343
	(11.559)	(12.765)	(12.606)	(12.725)
%No prenatal care	1.176	1.365	1.370	1.390
	(1.860)	(2.176)	(2.095)	(2.076)
Log Income	9.756	9.710	9.719	9.726
	(0.261)	(0.225)	(0.227)	(0.224)
Child poverty	17.745	17.709	17.668	17.506
	(7.593)	(7.251)	(7.273)	(7.239)
Hospitals	2.001	2.525	2.492	2.452
	(2.145)	(2.453)	(2.424)	(2.369)

Sample means for the 1999-2007 conceptions resulting in live births. All averages are obtained after weighting by the number of births. Exposure refers to tornadoes of intensity F3, F4, and F5 on the Enhanced Fujita scale. Standard errors are reported in parentheses. Observations included in column 2 and 3 and 4 overlap because some births were exposed to tornadoes in more than one trimester.

Table 2. The Impact of Exposure to Tornadoes of Intensity F3-F5 on Birth Outcomes

Dep. Var.	<37 weeks	<32 weeks	Gestation	<2500 g	<1500 g	Birth weight	% Female
				Panel A. OLS			
1st trim	-0.045	-0.038*	0.009*	-0.044	-0.028*	1.114	0.011
	(0.057)	(0.020)	(0.005)	(0.037)	(0.015)	(0.854)	(0.061)
2nd trim	-0.119*	-0.029	0.019***	-0.059*	-0.015	2.661***	0.159**
	(0.063)	(0.023)	(0.005)	(0.035)	(0.015)	(0.880)	(0.064)
3rd trim	-0.069	-0.049*	0.020***	-0.057	-0.018	1.386	-0.019
	(0.076)	(0.028)	(0.007)	(0.040)	(0.017)	(0.985)	(0.072)
Obs.	320,511	320,511	320,511	320,511	320,511	320,511	320,511
				Panel B. IV			
1st trim	-0.043	-0.037**	0.009*	-0.041	-0.028**	1.065	-0.052
	(0.054)	(0.019)	(0.005)	(0.035)	(0.014)	(0.814)	(0.070)
2nd trim	-0.108*	-0.025	0.018***	-0.050	-0.011	2.476***	0.148**
	(0.060)	(0.022)	(0.005)	(0.034)	(0.015)	(0.830)	(0.070)
3rd trim	0.028	-0.024	0.009	-0.018	-0.003	0.452	-0.049
	(0.072)	(0.027)	(0.006)	(0.038)	(0.016)	(0.931)	(0.082)
Obs.	320,451	320,451	320,451	320,451	320,451	320,451	320,451

The sample covers the 1999-2007 cohorts of conception. All singleton births regardless of gestation are retained. Tornado exposure is measured as the proportion of births exposed to tornadoes of level F3-F5 intensity during each trimester of gestation. Each column represents a different regression. As explained in the text each regression controls for: infant's gender (with the exception of the %female regression) and parity, mother's age, race, education, marital status and prenatal care, log per capita personal income, percent children under 17 living in poverty and hospitals per 100,000 people. In addition all regression include county-by-month fixed effects, and state-by-year fixed effects. Robust standard errors clustered at county level are reported in parentheses.

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

Table 3. The Impact of Tornadoes – An Investigation in the Mechanism of the Effect

Dep. Var.	<37 weeks	<32 weeks	Gestation	<2500 g	<1500 g	Birth weight
			Male			
1st trim	-0.058	-0.065***	0.008	-0.014	-0.046**	1.197
	(0.077)	(0.025)	(0.006)	(0.044)	(0.020)	(1.093)
2nd trim	-0.150**	-0.022	0.024***	-0.045	-0.019	2.593**
	(0.072)	(0.030)	(0.006)	(0.047)	(0.021)	(1.126)
3rd trim	0.063	-0.017	0.010	0.031	0.006	0.020
	(0.089)	(0.031)	(0.007)	(0.049)	(0.022)	(1.288)
Obs.	311,588	311,588	311,588	311,588	311,588	311,588
			Female			
1st trim	-0.042	-0.001	0.009	-0.059	-0.005	0.781
	(0.070)	(0.027)	(0.006)	(0.052)	(0.019)	(1.118)
2nd trim	-0.042	-0.026	0.012**	-0.053	-0.007	2.540**
	(0.079)	(0.028)	(0.006)	(0.051)	(0.021)	(1.131)
3rd trim	-0.011	-0.024	0.008	-0.057	-0.008	0.964
	(0.082)	(0.034)	(0.007)	(0.054)	(0.023)	(1.185)
Obs.	310,718	310,718	310,718	310,718	310,718	310,718

Each column in each panel represents a different regression. The results reported here use the same model specification as Table 2 Panel B.

Table 4. The Impact of Tornado on Fertility

Dep. Var.	Live births	Male live births	Female live births
1st trim	-0.027	0.006	-0.031
	(0.087)	(0.059)	(0.054)
2nd trim	-0.143	-0.164***	0.020
	(0.093)	(0.063)	(0.058)
3rd trim	-0.087	-0.022	-0.061
	(0.088)	(0.054)	(0.063)
Obs.	320,854	312,379	311,558

The sample covers the 1999-2007 cohorts. Each column represents a different regression. Data were collapsed into county/year/month of conception cells. "Live births" represent the number of live births per 10,000 women age 18-44 in each county/year/month of conception. Each regression controls for: percent Black among women 18-44 years old, age structure of women 18-44 years old: 18-24 (omitted), 25-34, over 35 years old, log per capita personal income, percent children under 17 living in poverty, and hospitals per 100,000 people. In addition all regression include county-by-month fixed effects, and state-by-year fixed effects. Robust standard errors clustered at county level are reported in parentheses.
* significant at 10% significance level; ** significant at 5% significance level; *** significant at 1% significance level

Table 5. Hit or Miss: Do Damages Matter for Infant Outcomes?

Dep. Var.	<37 weeks	<32 weeks	Gestation	<2500 g	<1500 g	Birth weight	% Female
			Panel A. Women exposed only during the 1st trimester				
% exposed more than once	-0.786	-0.523	0.084	-0.520	-0.640	16.483	-0.588
	(0.709)	(0.474)	(0.077)	(0.480)	(0.469)	(12.088)	(0.390)
Damages	0.457	0.196	-0.036	-0.300	0.053	7.133	-0.186
	(0.450)	(0.266)	(0.042)	(0.353)	(0.229)	(7.059)	(0.539)
Obs.	8,529	8,529	8,529	8,529	8,529	8,529	8,529
			Panel B. Women exposed only during the 2nd trimester				
% exposed more than once	-0.017	0.217	0.017	0.106	0.008	-3.892	-0.029
	(0.581)	(0.458)	(0.069)	(0.474)	(0.319)	(10.767)	(0.354)
Damages	-0.778*	-0.237	0.042	-0.579*	-0.373*	10.979	0.539
	(0.437)	(0.304)	(0.042)	(0.348)	(0.194)	(8.178)	(0.612)
Obs.	8,511	8,511	8,511	8,511	8,511	8,511	8,511
			Panel C. Women exposed only during the 3rd trimester				
% exposed more than once	1.368***	0.069	-0.228	0.587***	0.028	-27.830***	-0.649***
	(0.304)	(0.044)	(0.479)	(0.223)	(0.035)	(6.039)	(0.076)
Damages	-0.572	0.024	-0.077	0.057	-0.108	4.033	0.005
	(0.356)	(0.080)	(0.758)	(0.357)	(0.068)	(6.655)	(0.047)
Obs.	9,698	9,698	9,698	9,698	9,698	9,698	9,698

Each column represents a different regression. Each panel uses a different sample. With the exception of the additional controls of repeated exposure and tornado damages, the results reported here use the same model specification as Table 2 Panel B.

Table 6. Hit or Miss: Do Income Shocks Affect Fertility?

Dep. Var.	All Live Births	Male Births	Female Births
	Panel A. Women exposed only during the 1st trimester		
Avg. Potential	-0.115	-0.108	-0.000
Exposure	(0.372)	(0.223)	(0.194)
Damages	0.684	0.461	0.197
	(1.606)	(0.885)	(0.876)
Obs.	10,950	10,732	10,688
	Panel B. Women exposed only during the 2nd trimester		
Avg. Potential	-0.628	-0.206	-0.430
Exposure	(0.595)	(0.388)	(0.354)
Damages	0.244	0.077	0.382
	(1.740)	(1.347)	(1.069)
Obs.	7,235	7,098	7,088
	Panel C. Women exposed only during the 3rd trimester		
Avg. Potential	-0.113	-0.079	-0.061
Exposure	(0.257)	(0.188)	(0.170)
Damages	-0.647	-0.036	0.123
	(2.132)	(1.882)	(1.212)
Obs.	11,124	10,900	10,850

Each column represents a different regression. Each panel uses a different sample. The dependent variable is live births per 10,000 women. With the exception of the additional controls of repeated exposure and tornado damages the results reported here use the same model specification as Table 4.

Table 7. The Effect of Tornadoes by Mothers' Age

	Mother's Age < 25 years			25 ≤ Mother's Age < 35			Mother's Age ≥ 35		
Dep. Var.	*<37 weeks*	*<32 weeks*	*% Female*	*<37 weeks*	*<32 weeks*	*% Female*	*<37 weeks*	*<32 weeks*	*% Female*
1st trim	-0.090	-0.061*	-0.190*	0.038	0.015	-0.047	-0.179	-0.139**	0.239
	(0.071)	(0.032)	(0.114)	(0.074)	(0.023)	(0.100)	(0.152)	(0.058)	(0.231)
2nd trim	-0.245***	-0.068**	0.314***	-0.011	0.014	0.045	-0.156	0.017	-0.265
	(0.080)	(0.032)	(0.114)	(0.076)	(0.029)	(0.096)	(0.150)	(0.072)	(0.226)
3rd trim	0.050	-0.042	-0.185	0.075	-0.006	0.004	0.026	0.066	-0.110
	(0.091)	(0.038)	(0.132)	(0.084)	(0.032)	(0.112)	(0.190)	(0.071)	(0.234)
Obs.	306,846	306,846	306,846	309,841	309,841	309,841	240,198	240,198	240,198
Dep. Var.		*Live Births*			*Live Births*			*Live Births*	
	All	*Male*	*Female*	*All*	*Male*	*Female*	*All*	*Male*	*Female*
1st trim	-0.272	-0.004	-0.229*	0.383	0.213	0.163	0.071	0.017	0.064
	(0.205)	(0.135)	(0.133)	(0.360)	(0.241)	(0.217)	(0.065)	(0.050)	(0.046)
2nd trim	-0.254	-0.388**	0.117	-0.441	-0.430**	0.007	-0.024	-0.013	-0.034
	(0.239)	(0.160)	(0.143)	(0.333)	(0.209)	(0.226)	(0.063)	(0.048)	(0.048)
3rd trim	-0.394*	-0.121	-0.262	0.111	0.070	0.069	0.056	0.037	0.023
	(0.239)	(0.141)	(0.178)	(0.360)	(0.221)	(0.224)	(0.065)	(0.048)	(0.048)
Obs.	307,862	290,860	289,047	310,699	292,161	290,702	243,613	200,874	197,721

The results reported here use the same model specification as Table 2 Panel B.

APPENDIX NOT FOR PUBLICATION

Table A1. Robustness checks

	<37 weeks	<32 weeks	Gestation	<2500 g	<1500 g	Birth weight	% Female
1) Deviations from county mean							
1st trim	-0.043	-0.037**	0.009**	-0.041	-0.028*	1.065	-0.052
	(0.051)	(0.018)	(0.004)	(0.034)	(0.015)	(0.795)	(0.069)
2nd trim	-0.108**	-0.025	0.018***	-0.050	-0.011	2.476***	0.148**
	(0.052)	(0.021)	(0.004)	(0.034)	(0.015)	(0.797)	(0.067)
3rd trim	0.028	-0.024	0.009*	-0.018	-0.003	0.452	-0.049
	(0.059)	(0.022)	(0.005)	(0.037)	(0.016)	(0.865)	(0.074)
Obs.	320,451	320,451	320,451	320,451	320,451	320,451	320,451
2) 3 months	-0.027	0.010	-0.003	-0.053	0.000	-0.135	0.053
prior conception	(0.054)	(0.020)	(0.005)	(0.036)	(0.016)	(0.907)	(0.069)
3) 3 months	-0.025	0.008	0.011*	0.007	0.006	-0.140	-0.021
after births	(0.067)	(0.025)	(0.006)	(0.038)	(0.014)	(0.907)	(0.065)
4) Exclude state-years before/with/after F4 and F5 tornadoes							
1st trim	-0.067	-0.058	0.005	-0.132*	-0.063**	3.411**	-0.031
	(0.114)	(0.036)	(0.010)	(0.076)	(0.026)	(1.414)	(0.122)
2nd trim	-0.095	-0.022	0.009	-0.060	-0.009	2.292	0.251**
	(0.095)	(0.034)	(0.008)	(0.073)	(0.027)	(1.686)	(0.118)
3rd trim	-0.032	-0.054	0.011	-0.045	0.011	0.215	-0.112
	(0.123)	(0.037)	(0.009)	(0.078)	(0.031)	(1.799)	(0.142)
Obs.	170,711	170,711	170,711	170,711	170,711	170,711	170,711
5) Exclude western states							
1st trim	-0.042	-0.037**	0.009*	-0.041	-0.028**	1.066	-0.052
	(0.054)	(0.019)	(0.005)	(0.035)	(0.014)	(0.814)	(0.070)
2nd trim	-0.109*	-0.025	0.019***	-0.051	-0.011	2.482***	0.147**
	(0.060)	(0.022)	(0.005)	(0.033)	(0.015)	(0.828)	(0.070)
3rd trim	0.027	-0.024	0.010	-0.019	-0.003	0.464	-0.048
	(0.072)	(0.027)	(0.006)	(0.038)	(0.016)	(0.930)	(0.082)
Obs.	268,789	268,789	268,789	268,789	268,789	268,789	268,789
6) Drop very small cells							
1st trim	-0.048	-0.038**	0.009*	-0.044	-0.027**	1.085	-0.050
	(0.054)	(0.019)	(0.005)	(0.035)	(0.014)	(0.816)	(0.070)
2nd trim	-0.106*	-0.024	0.018***	-0.048	-0.010	2.456***	0.146**
	(0.060)	(0.022)	(0.005)	(0.034)	(0.015)	(0.831)	(0.071)
3rd trim	0.026	-0.024	0.010	-0.014	-0.004	0.489	-0.047
	(0.073)	(0.027)	(0.006)	(0.038)	(0.016)	(0.932)	(0.082)
Obs.	285,647	285,647	285,647	285,647	285,647	285,647	285,647

The results reported here use the same model specification as Table 2 Panel B.