Economic contraction and birth outcomes: an integrative review

Running title: Economic contraction and birth outcomes

CE Margerison Zilko
Division of Epidemiology
School of Public Health
University of California, Berkeley
Berkeley, CA, USA 94720

Corresponding author:
CE Margerison Zilko
cmargerison@berkeley.edu

Table of contents

Abstract

Background: Previous research has demonstrated an association between economic contraction, at both the individual and aggregate level, and adverse health outcomes. Proposed mechanisms include increased psychosocial stress and loss of resources. The aim of this review is to assess the quantity, validity, and consistency of empirical evidence examining economic contraction and birth outcomes. *Methods:* Empirical, English-language articles examining the effects of economic change at either the aggregate or individual level on birth weight, length of gestation, neonatal mortality, and the secondary sex ratio were identified using PubMed and ISI Web of Knowledge. Studies were organized by level of analysis and birth outcome and evaluated for internal and external validity. *Results:* One individual-level study reported a strong association between individual shift to inadequate employment and decreased birth weight. Of seven aggregate-level studies on birth weight, five exhibited moderate to strong validity but reported inconsistent findings. Similarly, findings from five studies (four with moderate to strong validity) examining rates of neonatal mortality reported inconsistent findings. Three of four moderate to strong studies reported a reduced secondary sex ratio following economic contraction. *Conclusion:* Associations between economic contraction and birth weight, neonatal mortality, and the secondary sex ratio remain speculative. Consensus on methodology is needed to compare findings across studies. Further research on economic contraction and the secondary sex ratio as well as individual-level birth weight and length of gestation is warranted.

Key words: birth weight, neonatal morality, secondary sex ratio, economic contraction, job loss

Introduction

Much research reports that pregnant women who experience stressful life events have worse birth outcomes than similar women without such stressors. Maternal stress has been implicated in fetal loss as well as in premature delivery (i.e., < 37 completed weeks of gestation) and attendant low birth weight (i.e., < 2,500 grams) (Paarlberg *et al.*, 1995; Hobel *et al.*, 2008). Empirical work also finds that economic contraction (i.e. a reduction in the ability of the economy to provide secure employment for those who desire it) precedes increased incidence of such stressful events (Catalano and Dooley, 1977). Economic change at the national, regional, and individual level has also been examined in relation to several aspects of mental and somatic health (Catalano, 1991; Dooley *et al.*, 1996). A subset of this literature focuses on the association between stress due to adverse economic change and the outcomes of gestation. The current recession, which has proved unusually deep and widespread, has heightened both scholarly and clinical interest in this relationship (Catalano, 2009). The objectives of this integrative review include 1) to assess the quantity, validity, and consistency of empirical evidence supporting the hypothesized connection between economic contraction and any of the following birth outcomes: birth weight, length of gestation, neonatal mortality, or alterations to the secondary sex ratio; and 2) to identify gaps and inconsistencies in the existing literature in order to provide direction for future studies. To accomplish these objectives, I will organize the literature by birth outcome and level of analysis (i.e. individual- or aggregate-level) and evaluate the internal and external validity of each study as well as the extent to which findings from studies assessing the same outcome at the same level of analysis report consistent findings. Where lack of consistency exists, possible explanations will be discussed.

Plausible mechanisms connecting economic contraction to gestational outcomes

Figure I illustrates the multiple pathways through which contracting economies may affect health in general, and birth outcomes in particular. Economic contractions may lead directly to individual job loss (a), followed by a subsequent decrease in income or resources (e), self-esteem or social networks (f) (Pearlin *et al.*, 1981; Brenner, 1983; Kong *et al.*, 1993). Contracting economies can also impact individuals who do not lose their jobs (Catalano, 2007). Those who remain employed (b) may face pay cuts (e) or experience psychosocial distress (f) due to fear of job loss (Kasl, 1975; Cobb and Kasl, 1977) or increased hours and responsibility at work (Brenner, 1983). Other members of families or households in which someone loses a job (c) may also suffer a loss of resources (e) or increased stress (f) (Rook *et al.*, 1991). Rising unemployment can negatively impact communities (d) by reducing funding for public or social services, decreasing philanthropy (e) (Brenner, 1983), or changing community networks and social support (f) (Catalano, 2007). Finally, economic contraction may induce changes in environmental hazards (g) such as air pollution or traffic congestion (Whitten, 2009; Ruhm, 2007).

Loss of resources and increased psychosocial stress may, in individuals, lead to worsened nutrition (Rogers, 1998) or decreased attention to personal health (i) or to negative coping behaviors, such as smoking (j). Evidence links these intermediary factors to physiological changes in the immune, endocrine, and cardiovascular systems of pregnant women that may affect gestation and subsequent birth outcomes. For example, maternal nutrition and weight gain during pregnancy affect birth weight and length of gestation (Institute of Medicine, 1990, 2007), and corticotrophin releasing hormone (CRH), thought to be an indicator of psychosocial stress, has been implicated as a factor in preterm delivery (Hobel 1999, 2008; Wadhwa, 2005). Maternal stress may also increase hypertension, which can affect fetal growth (Hobel, 2008), or

increase susceptibility to infections implicated in at least 20-30% of preterm deliveries (Wadhwa, 2001). Air pollution appears to be a risk factor for low birth weight and preterm birth (Sram, 2005; Stillerman, 2008) as well as for infant mortality (Chay and Greenstone, 1999).

Some researchers also hypothesize that adverse maternal conditions during pregnancy may induce changes in the secondary sex ratio (ratio of males to females at birth) (Trivers and Willard, 1973). Less-hardy males are thought to require more maternal investment, yet yield fewer grandchildren, compared to less-hardy females. Natural selection, therefore, may have conserved a mechanism that enables mothers to manipulate offspring sex in response to environmental stress. Hypothesized mechanisms for this manipulation include increased male fetal death (Catalano and Bruckner, 2006); alterations in the secondary sex ratio may also result from changes in the sex ratio at conception (primary sex ratio) due to sperm characteristics (Fukuda *et al.*, 1996; Zorn *et al.,* 2002) or coital day or frequency (James, 2008; Martin, 1995, 1997; Renkonen, 1970; Seagraves, 1998).

It is important to note that some researchers have proposed that economic contraction could have beneficial effects on health due, for example, to increased leisure time or decreased income available to pursue such risky behavior as smoking or substance abuse (Ettner, 1997; Ruhm, 2003). Those who remain employed during economic contractions may also curtail behaviors, such as drinking or drug use, that make them less desirable employees (Catalano, 1993). Furthermore, women, especially those with limited resources, may chose not to reproduce during difficult economic times, thereby changing the demographic distribution of pregnant women (Dehejia and Lleras-Muney, 2003).

Methods

Articles were identified for this integrative review using a PUBMED search of keywords related to the economy including "economy", "unemployment", "employment", "job loss", "lay off", "recession", "financial problems", "financial difficulties", and "financial stress"; in all possible combinations with outcome-related keywords "birth weight", "preterm", "gestational age", "neonatal mortality", "neonatal death", "fetal mortality", "fetal death", and "sex ratio". Studies published prior to August, 2009 were included in the search. The reference lists of identified articles were then searched for other relevant articles, manually and using ISI Web of Knowledge (www.isiknowledge.com). English language studies that were empirical in nature and not obviously irrelevant based on the title were then further reviewed to identify those meeting the following inclusion criteria.

Qualifying birth outcomes included those gauging length of gestation, birth weight, neonatal mortality (death before 28 days), and the secondary sex ratio. Literature concerning infant mortality was not included because post-gestational factors reportedly play an important role in the incidence of death after 28 days of life (D'Angleo and Colley Gilbert, 2002).

Only those studies measuring some facet or indicator of economic contraction at either the population (i.e., national, state, regional, etc.) or individual level were included. Studies using cross-sectional exposure variables such as occupation or per-capita or household income were excluded because of the difficulty in determining whether such variables arise from economic dynamics or reflect reverse causation, in which individuals' poor health leads to unemployment or lower socioeconomic standing (Smith, 2004).

I first separated the literature into two categories: (1) those that used individuals as the unit of analysis and (2) those that used population aggregates as the unit of analysis. Individual-level studies attempt to determine whether an exposure, (e.g. maternal job loss) increases the risk

of an outcome (e.g. low birth weight) among mothers who have the exposure compared to those who do not, whereas aggregate-level (i.e. ecologic) research estimates the association over time between characteristics of an economy (e.g. unemployment rate) and the incidence of an outcome in the population it supports.

Individual-level studies can explore multiple mechanisms and pathways through which an exposure affects an outcome but must address potential confounding by individual-level characteristics and cannot account for reverse causation unless data on both economic change and birth outcomes are collected longitudinally. Individual-level findings can be useful for clinicians and public health practitioners in identifying at-risk individuals. These studies may not, however, prove as useful for formulating or evaluating public policy because they are unable to estimate the "net effect" of economic contraction on population health. As noted above, contracting economies may cause some persons to reduce their risk of illness while others experience an increase in morbidity. The "net effect" of economic contraction on the incidence of illness in the population, therefore, equals the illness induced (e.g., by loss of resources or increased stress) less that averted (e.g., through reduced unhealthy behaviors).

Although aggregate-level studies are more useful for developing and evaluating public policy because of their ability to estimate these net effects, these studies can lead to the mistaken inference (i.e., the "ecological fallacy") that the association estimated between two characteristics of a population (e.g., rate of involuntary job loss and incidence of low birth weight) describes an association at the individual level (e.g., the risk of low weight birth attributable to involuntary job loss among pregnant women). Both aggregate- and individual-level studies are included in this review, but the reader is reminded that they have different strengths and weaknesses and ultimately answer different questions (Catalano, 1991).

Each study was evaluated on how well it defended its inferences against threats to internal and external validity (Campbell and Stanley, 1963). Internal validity, or the ability of a study's findings to provide inference about the target population, was assessed as a function of how well studies addressed bias due to confounding, measurement error, and selection bias. External validity was assessed primarily on the degree to which the study population, if a sample, represented the population to which the author intended to generalize results. For many aggregate-level studies, the study population is not a sample but is presumed to be the entire population in a given labor market or economy; in this instance, the extent to which the authors acknowledged or discussed the generalizability of their findings to other similarly defined populations was assessed. A second, but less well-understood, threat to external validity that was not assessed in this review arises from the issue of whether the confounders controlled at a particular time in either individual or aggregate studies similarly confound associations at times, typically in the future, to which the author intends to generalize. I refrain from assessing "temporal generalizability" because the literature offers no agreed response to this threat to external validity.

Threats to the internal validity of aggregate time-series studies have received less attention in the literature than threats to individual level studies and, therefore, deserve brief discussion here. Aggregate-level variables often come from archival sources (e.g., the US Bureau of Labor Statistics), rather than original data collection. Because these secondary data were not gathered to test theory, they rarely include measurement of confounding variables such as maternal smoking, pre-pregnancy body mass index, or socioeconomic status. In addition to evaluating the authors' use of adjustment to address measured confounders, I, therefore, assessed how well each aggregate study implemented strategies for defending against confounders

omitted from a test either because the author did not suspect or could not measure them (Catalano *et al.,* 2007).

The first strategy, comparison, adjusts for omitted confounders that would presumably affect other populations in addition to the test population. These generally-occurring confounding variables can be addressed in studies with a test and comparison population(s) by including, as a covariate in the model for the test population, a measure of the outcome in the comparison population. A comparison population is defined as one exposed to the suspected confounder but not to the same economic contraction as the test population. This method is similar to matching methods employed in individual-level studies in that it attempts to arrive at a counterfactual without randomization. Another method for addressing generally-occurring confounders in studies with many observed populations (e.g., states) exposed to economic contraction at different times, is to include an indicator variable for each time period (e.g., months, years), thereby controlling for generally-occurring changes in the outcome over time.

Although comparison can correct for generally-occurring confounders, it cannot reduce the threat of confounders peculiar to the test population or when the dependent variable has not been measured in a comparison population. A second strategy, decomposition, can be used in such circumstances, but applies only to omitted variables that exhibit patterns, referred to collectively as "autocorrelation," over time. Autocorrelation includes secular trends, cycles, and the tendency for a time series to remain elevated or depressed, or to oscillate, after high or low values. One type of autocorrelation, linear trends, may be controlled by adjusting for a population-specific slope over time. This technique may not adequately address other types of autocorrelation such as cycles or oscillations. The most rigorous decomposition methods are those originally developed by Box and Jenkins and known as ARIMA models (Box and Jenkins,

1994), which involve estimating the value of any datum from the best-fitting model of autcorrelation in the data as a whole. This process "decomposes" the original data into expected (from autocorrelation) and, by subtracting the expected from the observed, residual values. The residual values presumably express the variable adjusted for omitted confounders that exhibit autocorrelation, or the "unexpected" value of the variable.

Decomposition and comparison cannot defend against omitted third variables that exhibit no autocorrelation and affect only the test population. Only replication of findings in multiple populations can reduce the threat of this class of omitted variable. If an association survives decomposition and comparison as well as control for suspected and measured covariates, finding it again in other populations implies that the association did not arise from an omitted confounder unique to the original test population. Although one published article can include a finding and its replication (e.g., Catalano and Serxner, 1987), research more typically progresses through the publication of a finding and subsequent attempts to replicate it in other populations.

In order to summarize the quantity, validity, and consistency of empirical evidence for each outcome, I characterized each hypothesized association as supported, speculative, or unsupported. I use "supported" to describe associations reported by more than one study at the same level of analysis with high internal and external validity. Hypothesized associations for which findings are inconsistent or that are supported only by studies with low internal and external validity are described as "speculative". Hypothesized associations rejected by more than one null finding and without any supporting findings are considered "unsupported".

Results

The search yielded 594 results, of which 506 (85%) were either non-empirical, not written in English, or were obviously not relevant. Of the 88 remaining studies, 73 (83%) were

236 excluded because they used a static or cross-sectional exposure variable. Such studies, while of
237 potential interest in identifying women at risk for adverse birth outcomes, were not included in
238 this review because they are unable to refute the rival explanation of reverse causation. Also
239 excluded were studies that explored associations between adverse birth outcomes and counts of
240 stressful life events (SLEs) that often included undesireable job or financial experiences. I
241 excluded these studies because the associations of job or financial stressors with the outcome
242 were not separately tested or reported (for a review, see Paarlberg *et al.*, 1995 or Beydoun and
243 Saftlas, 2008). An additional study published after the initial search was added upon a
244 reviewers' suggestion (Helle *et al.*, 2009).

245 Of the 16 studies meeting the inclusion criteria, only one study (Dooley and Prause,
246 2005), which examined birth weight in US women, was based on individual-level analyses. Of
247 the 15 aggregate-level studies, six examined birth weight (Fisher *et al.*, 1985; Joyce, 1990; Joyce
248 and Mocan, 1993; Catalano and Serxner, 1992a; Catalano *et al.*, 1999; Bremberg, 2003), four
249 examined neonatal mortality (Brenner, 1973; Catalano and Serxner, 1992b; Neumayer, 2004;
250 Lin, 2006), four examined the secondary sex ratio (Catalano, 2003; Catalano and Bruckner,
251 2005; Catalano *et al.*, 2005, Helle *et al.*, 2009), and one examined both birth weight and neonatal
252 mortality (Dehejia and Lleras-Muney, 2003). Several of these studies also examined other
253 outcomes, such as infant mortality, that will not be discussed in this review. Eight of the studies
254 used US data, three used data from Sweden and/or Norway, two used data from Germany, and
255 one used data from Taiwan. Table I summarizes the time period, population, study design,
256 measurement of economic contraction and birth outcome, and quantitative findings of the 15
257 studies. Table II characterizes the studies' efforts to address internal and external validity for
258 individual- and aggregate-level studies, respectively.

Table I here

Birth weight

Individual level

As described above, only one individual level study concerned with any gestational outcome was identified. Using data from the National Longitudinal Survey of Youth (NLSY79), Dooley and Prause (2005) investigated the associations between individual-level birth weight and both the local unemployment rate and mother's shifting from adequate employment (defined as employment that is not poverty wage or involuntary part-time) to either unemployment, involuntary part-time employment, or poverty wage employment during pregnancy (Table I). The NLSY79 is a nationally representative sample of individuals who were 14 to 22 years old at enrollment in 1979. The authors report that women who moved from adequate employment to unemployment or involuntary part-time employment during pregnancy delivered significantly lower weight (β (SE) = -185.43 (77.2) and -418.05 (165.2) grams, respectively) infants compared to women who remained adequately employed. The odds of having a low weight (< 2,500 g) infant were also significantly higher (odds ratio [OR] = 7.38, 95% confidence interval [95% CI] = 1.82, 29.89) for women moving to involuntary part-time employment. The unemployment rate of the mother's standard metropolitan statistical area (SMSA) was significantly associated with mother's shifting from adequate to inadequate employment but not significantly associated with birth weight after controlling for such shifts.

The quality of the NLSY79 data confers high internal validity on the Dooley and Prause analyses (Table II). These data allow specification of many potential confounders including maternal pre-pregnancy weight, alcohol use and smoking during pregnancy, trimester of prenatal care initiation, weight gain during pregnancy, and length of gestation.

The NLSY79 did not, however, allow for control of all potentially important confounders. The authors acknowledge that other traits of women may have caused both adverse unemployment change and low birth weight, citing life-management skills and resources as examples. Other unmeasured confounders suspected to predict both the weight of offspring and mother's ability or choice to remain adequately employed include mother's birthweight (Conley and Bennett, 2000; Currie, 2007) and complications of pregnancy.

Measurement error may also threaten the internal validity of Dooley and Prause (2005) study. Employment shift and birth weight variables come from self report, and socioeconomic factors such as education may affect the respondent's interpretation of both the employment question and infant's birth weight. Whether respondents would agree with all of the authors' judgments concerning the desirability of employment shifts also remains unclear. Some women may have chosen low wage jobs during pregnancy to avoid the greater physical and psychological demands of higher-paying jobs. Depending on a woman's financial and family circumstances, these changes may or may not have resulted in significant loss of resources or greater stress.

Although the NLSY79 had very high retention rates (Center for Human Resource Research, 2004), selection bias could have affected the results of the analyses. Inclusion in the study was dependent on follow-up after pregnancy, which may be related to both adverse employment changes during pregnancy and infant birth weight.

The NLSY79 data have high external validity in that, weighted correctly, they describe the experiences of women born in the U.S between 1957 and 1965 (Table II). Inclusion in the Dooley and Prause (2005) study, however, depended on adequate employment prior to

pregnancy, and their findings, therefore, may not describe the implications of shifting from "inadequate" employment prior to pregnancy to unemployment during pregnancy.

The Dooley and Prause (2005) study exhibited high levels of both internal and, assuming generalization only to women adequately employed prior to pregnancy, external validity. Replication with other data validated beyond self-report and measurements of maternal birth weight and pregnancy complications would make these findings a compelling estimation of the risk of low birth weight attributable to adverse employment changes during pregnancy, as defined by Dooley and Prause.

Aggregate level

In an early aggregate-level study, Fisher, *et al.* (1985) found that the proportion of low birth weight increased during the recession of 1982 (compared to 1980) in Washington State within low-income (crude relative risk [RR] = 1.18, 95% CI = 1.00, 1.25) but not high-income census tracts (crude RR = 0.98, 95% CI = 0.77, 1.25), suggesting that the association with economic contraction may depend on factors such as personal or community resources (Table I). The authors did not find evidence of confounding using analyses stratified by maternal age, race, marital status, parity, time since last birth, and previous fetal or infant loss. The possibility of multivariate confounding was not, however, addressed (Table III), and the authors did not address potential confounding by omitted third variables using decomposition or comparison methods. The fact that this study sample included only the highest and lowest income census tracts in three metropolitan counties in Washington State raises questions of external validity.

Two additional early aggregate-level studies by Joyce (1990) and Joyce and Mocan (1993) investigated the association between unemployment rate and race-specific rates of low

birth weight in New York City (Joyce, 1990) and in Tennessee (Joyce and Mocan, 1993). Both studies report null associations (Table I).

The Joyce (1990) and Joyce and Mocan (1993) studies addressed autocorrelation using methods to adjust for secular trends and, in the Tennessee analysis, cycles in unemployment. The New York study also adjusted for several measured third variables (Table III). The authors did not address the possibility of confounding by generally-occurring omitted third variables that exhibited autocorrelation other than secular trend and cyclicity. The two studies used similar years of data and can possibly be viewed as replications of each other. This early work is notable in that the authors identified several key analytical issues such as trends in the data and the possibility for mediating factors to affect the relationship between the economy and health. A replication of these analyses using more recent data and comparison methods would improve the validity of these findings.

Catalano and Serxner (1992a) investigated the effects of "ambient threats to employment" on rates of low birth weight in two California counties (Table I). The authors first examined a "natural experiment," in which state workers in Sacramento County, California were unexpectedly told to prepare for pay cuts and lay-offs in June, 1978. The decision to reduce state workers was, however, reversed, and none actually lost their jobs. In a second test, the authors examined the effect of unexpectedly low total monthly employment (derived using ARIMA methods) in the Los Angeles (LA)-Long Beach SMSA, California on odds of low birth weight.

Both tests demonstrated a significant increase in rates of low birth weight among male infants in gestation during "threats to employment" (Table I). The threatened lay-offs in Sacramento were associated with increased low birth weight among white and Hispanic infants exposed in the fourth and third month of gestation, respectively. The authors estimated the

number (% of total births exposed in the appropriate month) of low weight births attributable to the threatened lay-offs as 9 of 342 (2.6%) white male and 4 of 54 (7.4%) Hispanic male infants. The number of low birth weight infants attributable to economic contraction in the month with the lowest level of employment in LA was eight white and six Hispanic male infants (no denominator reported). The authors acknowledge the small magnitude of these associations.

The authors used ARIMA modeling to identify and remove seasonality and other forms of autocorrelation and included the total number of live births as a control variable to account for potential changes in fertility rates (Table III) but did not employ comparison methods. The authors addressed external validity by acknowledging that, although the event in Sacramento County was extreme in nature, other communities that rely heavily on one or a few employers may have similar experiences when these employers threaten to reduce jobs. The LA County analysis may be more generalizable to large, diverse metropolitan areas. The Sacramento test is of note because it contributes evidence that the stress response may be triggered by the threat of unemployment, even if actual jobs are not lost.

Catalano *et al.* (1999) also report a significant positive association between increases in quarterly numbers of unemployed males and rate of very low birth weight (<1500 g), in separate analyses of data from Norway and Sweden between 1973 and 1995 (Table I). The authors estimated that approximately 2.1% of all very low weight births (188 of 8,924 and 329 of 15,272 low weight infants in Norway and Sweden, respectively) were attributable to increased male unemployment in this period.

This study controlled for potential confounding by both measured and unmeasured variables (Table III). First, the authors controlled for the total number of live births (in the country of interest, either Norway or Sweden) to account for potential changes in rates of

conception. Second, the authors used ARIMA decomposition methods to control for autocorrelation, and third, they used comparison methods to control for confounding by generally-occurring phenomena. Finally, the replication of these results in both countries diminishes the potential for confounding by a locally-occurring variable. This combination of methods to reduce potential confounding by third variables confers high internal validity on the findings of this study. This study is notable in that it may provide evidence that economic contraction can indirectly affect pregnant women when unemployment increases among males.

Dehejia and Lleras-Muney (2003) report that the annual unemployment rate in 50 US states was significantly associated with decreased rates of low birth weight and very low birth weight (data not shown). When stratified by race, these decreases were significant among black mothers only (β [standard error, SE] = -0.00078 [0.00016] and -0.00020 [0.00006] for low and very low birth weight, respectively) (Table I). Dehejia and Lleras-Muney (2003) controlled for measured confounders, used year indicator variables to control for national time trends, and addressed secular trends (one type of autocorrelation) by adjusting for state-specific slopes on time (Table III). These methods, however, may not have completely accounted for such types of autocorrelation as cycles, seasonality, or non-linear time trends.

The use of data from 50 U.S. states across a 15-year time period potentially increases the generalizability of this analysis, although the authors did not address the question of whether state unemployment rates reflects the experience of any labor market in a state. Furthermore, the use of annual data may have induced measurement error when unemployment change occurred within the same year as births but before conception or after delivery.

Bremberg (2003) examined rates of low birth weight in the Stockholm, Sweden area in periods before, after, and during a recession (Table I). There was no significant difference

(p=0.85) in the rate of low birth weight in the recession period (44.0 per 1,000) compared to the mean rate in the previous and following periods combined (44.4 per 1,000). Bremberg did not adjust for any other potentially available covariates, nor did he address potential confounding by omitted third variables. Although the findings regarding low birth weight are null, this inadequate control for confounding leaves open the possibility that an effect is being masked by an omitted third variable.

Neonatal mortality

Five aggregate-level studies were identified that examined the effects of adverse economic change during gestation on neonatal mortality. In one of the earliest studies examining the economy and mortality, Brenner (1973) compared national-level unemployment to perinatal mortality rates from 1915 to 1967 (Table I). He reports that increased unemployment in the US was associated with increased fetal mortality in the same year and that neonatal mortality at less than one day increased one year after unemployment increased. No findings regarding neonatal mortality within the first 28 days were reported. Brenner also found that these associations had increased in strength since World War II and that nonwhites had a higher risk of fetal mortality than whites during periods of high unemployment.

Brenner did not adjust for any measured confounders in these analyses. He did employ several analytic methods to attempt to examine and control for trends and multiple year lag periods, but he did not use extensive decomposition methods or any form of comparison (Table III). Brenner's methods have subsequently been criticized for the lack of control for measured variables, misspecifications of models, and the lack of interpretability of the time lags between rises in unemployment and mortality (Gravelle *et al.*, 1981). Although Brenner does not attempt to make generalizations about his findings beyond the study population (the U.S.), he does risk

the ecological fallacy when arguing that mortality prevention might be focused on "…individuals or families who have recently sustained major economic loss." Despite these issues, this study is of note because it was the first to attempt to examine the relationship between unemployment and perinatal mortality.

Catalano and Serxner (1992b) examined the effects of periods of unexpectedly high or low employment on rates of neonatal mortality (stratified by race and gender) using monthly data from LA and Orange counties in California (Table I). The only significant finding in both counties was that neonatal mortality rates were significantly higher than expected among black males when employment was unexpectedly low in the second trimester of gestation. The authors estimated that, among black males, 10 neonatal deaths out of 2,643 live births could be attributed to this economic contraction, representing 21% of all black male neonatal deaths in the cohorts exposed to low employment in the second trimester.

Catalano and Serxner adjusted for potential confounding by weather conditions, implicated in neonatal health (Lawlor, 2005), using measured average daily noon temperature (Table III). They also utilized ARIMA decomposition methods and implemented comparison by adjusting for neonatal death rates in Orange County in the model for LA County. Additionally, they repeated the analyses with Orange County as the test county to address confounding by a locally-occurring phenomenon. This study adequately controlled for potential confounding by third variables, but leaves open the question of why only one of the subgroups examined demonstrated a significant effect. The authors acknowledge that their findings may be limited to Southern California and/or the time period examined, although the diversity of the population and labor markets in LA and Orange counties increases the external validity of these findings.

Three studies used similar methods to examine the association between rates of unemployment and neonatal mortality (Table I). Two of these studies found no evidence of an association between annual, state unemployment rates and neonatal mortality in the US (Dehejia and Lleras-Muney, 2003) and in Germany (Neumayer, 2004) (β [SE] = -1.815 [2.038] and -0.0193 [0.53], respectively). In contrast, Lin (2006) reports a significant increase in the neonatal mortality rate associated with city (β [SE] = 0.057 [0.027]) and national (β [SE] = 0.07 [0.019]) unemployment rate in Taiwan.

All three studies adjusted for several measured variables such as age structure and socioeconomic status of the population and used year indicator variables as a method of comparison (Table III). As discussed above, Dehejia and Lleras-Muney addressed state-specific linear time trends (one form of autocorrelation) and attempted to replicate their findings in other populations. Lin does not discuss the external validity of findings in Taiwan to other countries, while Neumayer acknowledges that socioeconomic or other factors particular to Germany may explain differences between his findings and those from other places. All three studies also relied on yearly data, which may have reduced precision and possibly induced measurement error.

Secondary sex ratio

Four studies describe the relationship between economic change and changes in the secondary sex ratio (i.e. the ratio of male to female live births). The first study (Catalano, 2003) examined the secondary sex ratio in East Germany during the economic collapse of 1991 (Table I) and found that it was 1.5% lower than the expected value of 1.059, based on history and the West German sex ratio in the same year.

Catalano utilized both decomposition and comparison (by controlling for the sex ratio in West Germany) methods to address potentially omitted third variables (Table III). He also discusses the potential for confounding due to migration of women who gave birth to males from East to West Germany during 1991. Catalano points out that for migration to explain these findings, the sex ratio in West Germany would have also increased in 1991, and the East German sex ratio would have remained low after 1991, neither of which is demonstrated in the data. Although this study exhibits high internal validity, the nature of this economic change is so extreme that these results may not be generalizable to more typical economic contractions. The author acknowledges this fact and suggests replication in other populations to address external validity.

A subsequent study by Catalano and Bruckner (2005) does replicate the East German finding that economic contraction reduces the secondary sex ratio. This study examined the association between the annual percentage change in household consumption of goods and services in Sweden between 1862 and 1991 and the secondary sex ratio (Table I). Results showed that each 1% decrease below the expected value of annual consumption was associated with 25 fewer male births in that year, translating to 2217 male births attributable to increases in consumption over the time of the study.

Catalano and Bruckner controlled for total number of live births to address the potential for confounding due to changes in fertility (Table III). Additionally, these authors used ARIMA decomposition methods to address potential confounding due to autocorrelation. The authors did not utilize either comparison or replication, however, and they acknowledge that replication would increase the external validity of these findings.

Because these first two studies used annual data, the authors could not discriminate between selection against males *in utero* and alterations in the sex ratio at conception. Therefore, Catalano *et al.* (2005) directly tested the ratio of male to female fetal deaths (≥ 20 weeks) in response to unexpectedly high rates of monthly unemployment in California. Male fetal deaths were found to significantly increase when unemployment increased (Table I). The authors estimate that each 1% increase in the de-trended and de-seasonalized unemployment rate was associated with 33 male fetal losses, translating to 370 of 10,710 (3.4%) of male fetal deaths during the study period.

The model used in this study controlled for the number of female fetal deaths to address potential third variables that would affect conception or gestation in both sexes (Table III). The authors also used ARIMA decomposition methods to address autocorrelation, although they did not include comparison states or regions as control variables. Catalano and colleagues acknowledge that replication in other populations is needed to generalize their findings beyond the time and place described in this data. The use of a recent and relatively long time period and the large, diverse nature of the California economy increase the external validity of this study.

Helle *et al.* (2009) examined the effect of multiple population-level stressors, including economic contraction, on the secondary sex ratio in Finland between 1865 and 2003 (Table I). The authors measured economic contraction as the annual percentage change in gross domestic product (GDP) in the same and previous year as births. Helle and colleagues addressed potential linear, secular trends in the data by subtracting the previous value of both the secondary sex ratio and the percentage change in GDP from the current value ("first differencing") and by adjusting for a year-specific slope of GDP. The authors included multiple covariates in their model and then removed autocorrelation from the multivariate adjusted model using ARIMA methods

(Table III). These methods may not adequately address potential confounding by generally- or unpatterned locally-occurring third variables. The authors did not find any significant effect of change in GDP on the secondary sex ratio; however, the effect estimates are difficult to interpret due to the first differencing of a percent change variable and the inclusion of a year-specific slope.

Summary of results by birth outcome

Birth weight

These eight studies used a range of methods, measurements, and study populations to investigate the effects of economic contraction on birth weight. The individual-level study (Dooley and Prause, 2005) provided evidence of a strong association between maternal adverse employment change during pregnancy and decreased birth weight. Without replication, however, it is difficult to draw conclusions about the individual association between economic contraction and birth weight. Findings from the aggregate-level studies, even those with high or moderate internal and external validity, were inconsistent and appeared to differ by author and methodology. The evidence for an association between economic contraction and birth weight therefore remains "speculative".

Neonatal mortality

The five studies examining neonatal mortality assessed the economy at different levels (city, state, and nation) and in different geographic locations (the U.S., Taiwan, and Germany), making it difficult to draw conclusions regarding the effects of adverse economic change on neonatal mortality. The four studies with high or moderate validity also demonstrated inconsistent findings, leaving the association between economic contraction and neonatal mortality classified as "speculative".

Secondary sex ratio

The sum of the three studies by Catalano and colleagues supports the hypothesis that economic stress, as measure by several indicators, is associated with a lower secondary sex ratio, although Helle *et al.* (2009) present conflicting evidence. This inconsistency may exist because Catalano and colleagues estimate the total effect of measures of economic contraction on the sex ratio, while Helle *et al.* estimate the direct effect, controlling for several factors that may be on the causal pathway, such as famine or mortality. Findings from all four studies exhibited moderate to high internal and external validity; however, the inconsistent findings also lead to a "speculative" classification.

Discussion

This review of the literature examining the effects of adverse economic change on birth outcomes found that all but one of the studies meeting inclusion criteria tested associations at the aggregate level. These aggregate-level studies, moreover, varied widely in methodology, study population, and measurement of key variables. Hypothesized associations between economic contraction and birth weight, neonatal mortality, and the secondary sex ratio all remain "speculative" due to inconsistent findings, even among studies with moderate to high internal and external validity.

Several possible explanations for the inconsistency of findings in the aggregate-level studies deserve note. One, there is no consensus on a convention for aggregate time-series analysis, and findings appear to vary by methods used to address potential confounding by omitted third variables. For example, with regard to the effect of economic contraction on low birth weight, Joyce and colleagues reports null findings in two studies using similar methods (1990; 1993); Catalano and colleagues, using primarily ARIMA decomposition methods, report

553 positive findings (Catalano and Serxner, 1992a; Catalano *et al.*, 1999); and Dehejia and Lleras-
554 Muney (2003), using indicator variables to address potential confounding, find negative
555 associations. Clearly, more agreement is needed on the most appropriate analytic methods to
556 allow more direct comparison across studies.

557 Second, the studies reviewed here examined the effect of economic contraction in various
558 geographic locations and at various times. The effects of economic change on human biology
559 may differ greatly from place to place and time to time, depending on the populations'
560 understanding of, or participation in, the economy. Studies that include multiple states (e.g.,
561 Dehejia and Lleras-Muney, 2003; Brenner, 1973; Neumayer, 2004) face the problem that the
562 unemployment rate (or other economic indicator) in a small state with a dominant labor market
563 (e.g., Rhode Island, Delaware, New Mexico) may be a more meaningful indicator of all
564 individuals' experience than the rate in large states with multiple labor markets (e.g., California,
565 New York, Florida).

566 Other characteristics of the study population and time period, such as access to health
567 care, governmental income transfers, or behaviors such as maternal diet or smoking may also
568 mitigate or increase individuals' response to economic change. In fact, Lin (2006) reports that
569 the association between the unemployment rate and low birth weight was stronger prior to the
570 provision of National Health Insurance (NHI) in Taiwan and within areas with fewer health care
571 resources. The possibility that net effects differ by time and place is further impetus for
572 individual-level research that might elucidate more of the mechanisms underlying these
573 differences.

574 Although several studies stratified their analyses by race or ethnicity (Brenner, 1973;
575 Catalano and Serxner, 1992a, 1992b; Joyce, 1990; Joyce and Mocan 1993; and Dehejia and

Lleras-Muney, 2003), there is very little research examining differences in the relationship between economic change and birth outcomes by subgroups defined by income or education, area of residence, access to medical care, etc. Substantial response heterogeneity due to differences in personal or community resources, as suggested by Fisher *et al.* (1985) and Dehejia and Lleras-Muney (2003), may be likely. More disaggregation of subgroups may be a useful line of inquiry for future researchers interested in identifying vulnerable populations.

The unit of time by which data are aggregated also plays an important role in this type of research. Studies using annual data may be threatened by measurement error in that the measured economic contraction may not actually occur during gestation. Studies that use monthly data (e.g. Joyce (1990); Joyce and Mocan (1993); Catalano and Serxner (1992a, 1992b); Catalano *et al.*, 1999; and Catalano *et al.*, 2005) are able to specify that the economic change occurred after conception and lessen the potential for this error. Changes in rates of conception during economic contractions could also influence observed birth outcomes (Dehejia and Lleras-Muney, 2003) and cannot be distinguished from effects during gestation without monthly data.

Indicators of economic contraction differed widely across the reviewed studies and included the unemployment rate, number of employed workers, threats to employment, consumption of goods and services, GDP, and "recession." The unemployment rate equals the number of people seeking work divided by the sum of employed persons and those seeking work. Despite this variable's widespread reporting and intuitive appeal as an economic indicator, changes in the unemployment rate do not necessarily measure changes in the size of the employed population or of the capacity of the economy to provide employment. The unemployment rate often increases in the same month that the number of employed persons increases because the number of persons looking for work tends to increase when the economy

599 expands. Contracting economies, moreover, can discourage persons from seeking work, causing
600 the unemployment rate to drop even when the total number of employed persons decreases.
601 Researchers, therefore, have often used variables such as total employment (Catalano and
602 Serxner, 1992a, 199b) or the value of goods and services consumed (Catalano and Bruckner,
603 2005) to avoid the ambiguity of the unemployment rate. Others have examined multiple
604 economic indicators (Joyce, 1990; Dehejia and Lleras-Muney, 2003; and Neumayer, 2004) to
605 determine which, if any, predict health outcomes. The meaning of the independent variable is an
606 important consideration for any study, and findings related to the unemployment rate may not be
607 comparable to those related to consumption or total employment. Authors should identify why
608 and how they manipulated the economic variable, if relevant. Catalano and Serxner (1992a), for
609 example, provide an *a priori* argument for using "unexpectedly low" levels of employment as an
610 indicator of adverse economic change.

611 The appropriate choice of birth outcome to gauge the effect of economic contraction on
612 the process of gestation also deserves careful consideration. While empirical evidence links
613 economic events to psychosocial stress (Catalano and Dooley, 1977; Rook *et al.*, 1991; Pearlin *et*
614 *al.*, 1981) and psychosocial stress during pregnancy to length of gestation (Hobel, 2008), studies
615 examining the effects of economic contraction on length of gestation or preterm birth were
616 notably absent from the literature. Future research should assess these outcomes to determine
617 whether findings are consistent with literature linking maternal psychosocial stress to length of
618 gestation (Hobel, 2008). Although researchers still debate the utility of low birth weight as an
619 independent outcome (Wilcox, 2001), the reported association between apparent adverse labor
620 market experiences and reduced birth weight (Dooley and Prause, 2005) combined with evidence
621 linking stress-related behaviors (e.g. tobacco smoking) during pregnancy to reduced birth weight

(Hobel, 2008) also supports a need for further research at the individual level on the relationship between economic contraction and birth weight.

The reported association between economic contraction and lower secondary sex ratios, attributable to declines in the primary sex ratio and/or to the spontaneous abortion of male fetuses, is consistent with the larger literature concerned with population stressors and the sex ratio. With the exception of war, for which the evidence remains mixed (Graffelman and Hoekstra, 2000; Polasek *et al.*, 2005; Zorn *et al.,* 2002) much research reports an association between acute, exogenous stressors and declines in the secondary sex ratio. For example, sex ratios decline after natural disasters (Fukuda *et al.,* 1996; Lyster, 1974; Sadat, 2008), and terrorist attacks (Catalano *et al.,* 2005, 2006).

The outcomes presented in this review might be considered steps in the gestational process rather than endpoints in themselves. For example, shorter gestations are associated with increased risk of low birth weight and neonatal death (Callaghan, 2006), and the proportion of fetal deaths in a birth cohort will directly affect the number of live births available for analyses of birth weight or neonatal mortality. Therefore, findings related to one outcome may not provide a complete picture of the impact of economic change on gestation; future studies should attempt to more comprehensively examine the process of gestation.

Agreement on and improved attention to these methodological issues may lead to more consistent findings at the ecological level. If so, these findings could be useful for estimating the "net" association, if any, between a contracting economy and the incidence of adverse birth outcomes and may contribute to estimates of net costs and benefits of economic policies. The possibility remains, however, that no universal net effect of economic contraction on birth outcomes exists, and that aggregate-level studies will continue to find associations that vary by

time and place. These aggregate-level analyses also do not provide information about individual women's risk of experiencing adverse birth outcomes when exposed to economic contraction at either the group or individual level. Indeed, to interpret findings from aggregate studies as individual risk invokes the "ecologic fallacy." More individual-level studies are needed that examine the effects of individual, family, community, and macro-level economic changes on the process of gestation. Findings from such individual-level analyses could help identify women at risk of adverse birth outcomes in contracting economies and could thereby improve the planning and delivery of preventive and treatment services.

Given the ongoing debate over whether and how much to regulate economies and the emerging research on the developmental origins of disease (Almond, 2006; Hanson and Gluckman, 2008), it would seem important that we establish a more complete understanding of how, if at all, the macro economy and its repercussions for individuals affect maternal, and subsequently, fetal health, during gestation. A better understanding of the pathways through which economic contraction, for example, affects outcomes of gestation may allow clinicians and public health practitioners to identify at-risk women and plan preventative services. Finally, an understanding of how fluctuations in the economic environment affect reproduction could further our understanding of the basic biology of conception, selection, and gestation.

Acknowledgement

The author thanks Ralph A. Catalano, PhD for his assistance and insight in the development and preparation of this manuscript.

665 **References**

666 Almond D. Is the 1918 influenza pandemic over? Long-term effects of in utero influenza
667 exposure in the post-1940 US population. *J Polit Econ* 2006:**114**;672-712.

668 Beydoun H and Saftlas AF. Physical and mental health outcomes of prenatal maternal stress
669 in human and animal studies: a review of recent evidence. *Paediatr Perinat Epidemiol*
670 2008:**2;**438-466.

671 Box G, Jenkins G, Reinsel G. Time Series Analysis: Forecasting and Control. 3rd edn, 1994.
672 Prentice Hall, London, UK.

673 Bremberg S. Does an increase of low income families affect child health inequalities? A
674 Swedish case study. *J Epidemiol Community Health* 2003:**57;**584-588.

675 Brenner MH. Fetal, infant, and maternal mortality during periods of economic instability. *Int*
676 *J Health Serv* 1973:**3;** 145-159.

677 Brenner MH and Mooney A. Unemployment and health in the context of economic change.
678 *Soc Sci Med* 1983:**17;** 1125-1138.

679 Callaghan WM, MacDorman, M. F., Rasmussen, S. A., Qin, C., Lackritz, E. M. The
680 contribution of preterm birth to infant mortality rates in the United States. *Pediatric*s
681 2006:**118;**1566-1573.

682 Campbell DT and Stanley JC. Experimental and quasi-experimental designs for research on
683 teaching. In Gage NL (ed) Handbook of Research on Teaching. 1963. Rand McNally,
684 Chicago, IL, pp.171 246.

685 Catalano R. The health effects of economic insecurity. *Am J Public Health* 1991:**81;**1148-
686 1152.

687 Catalano RA. Sex ratios in the two Germanies: a test of the economic stress hypothesis. *Hum*
688 *Reprod* 2003:**18;**1972-1975.
689 Catalano R. Economic factors and stress. In Fink G (ed) Encyclopedia of stress. 2007.
690 Academic Press, New York, NY, pp.9-15.
691 Catalano R. Health, Medical Care, and Economic Crisis. *N Engl J Med* 2009:**360;**749-751.
692 Catalano R, Ahern J, Bruckner T. Estimating the health effects of macrosocial shocks: a
693 collaborative approach. In Galea S (ed) Macrosocial Determinants of Population Health.
694 2007. Springer, New York, NY, pp.375-397.
695 Catalano RA and Bruckner T. Economic antecedents of the Swedish sex ratio. *Soc Sci Med*
696 2005:**60;**537-543.
697 Catalano R and Bruckner T. Secondary Sex Ratios and Male Lifespan: Damaged or Culled
698 Cohorts. *Proc Natl Acad Sci U S A* 2006:**103;**1639–1643.
699 Catalano R, Bruckner T, Anderson E, Gould JB. Fetal death sex ratios: a test of the economic
700 stress hypothesis. *Int J Epidemiol* 2005:**34;**944-948.
701 Catalano R, Bruckner, T, Gould J, Eskenazi B, Anderson E. Sex ratios in California
702 following the terrorist attacks of September 11, 2001. *Hum Reprod* 2005:**20;**1221-1227.
703 Catalano R, Bruckner T, Marks AR, Eskenazi B. Exogenous shocks to the human sex ratio:
704 the case of September 11, 2001 in New York City. *Hum Reprod* 2006:**21;**3127-3131.
705 Catalano R and Dooley CD. Economic predictors of depressed mood and stressful life events
706 in a metropolitan community. *J Health Soc Behav* 1977:**18;**292-307.
707 Catalano R, Dooley D, Wilson G, Hough R. Job loss and alcohol abuse: A test using data
708 from the Epidemiologic Catchment Area Project. *J Health Soc Behav* 1993:**34;**215-226.

709 Catalano R, Hansen HT, Hartig T. The ecological effect of unemployment on the incidence
710 of very low birthweight in Norway and Sweden. *J Health Soc Behav* 1999:**40**;422-428.
711 Catalano R and Serxner S. Time-series designs of potential interest to epidemiologists. *Am J*
712 *Epidemiol* 1987:**126**;724-731.
713 Catalano R and Serxner S. The effect of ambient threats to employment on low birthweight. *J*
714 *Health Soc Behav* 1992a:**33**:363-377.
715 Catalano R and Serxner S. Neonatal mortality and the economy revisited. *Int J Health Serv*
716 1992b:**22;**275-286.
717 Center for Human Resource Research. NLSY79 Child & Young Adult Data Users Guide.
718 2004. Center for Human Resource Research, Ohio State University, Columbus, OH.
719 Conley D and Bennett NG. Is biology destiny? Birth weight and life chances. *Am Sociol Rev*
720 2000:**65;**458-467.
721 Currie J and Moretti E. Biology as destiny? Short- and long-run determinants of
722 intergenerational transmission of birth weight. *J Labor Econ* 2007:**25;**231-263.
723 Chay KY, and Greenstone M. The Impact Of Air Pollution On Infant Mortality: Evidence
724 From Geographic Variation In Pollution Shocks Induced By A Recession. Working Paper
725 7442. 1999. National Bureau of Economic Research, Cambridge, MA.
726 Cobb S and Kasl S. Termination: The consequences of job loss. 1977. NIOSH, Cincinnati,
727 OH.
728 D'Angelo D and Colley GB. From Data to Action: Using Surveillance To Promote Public
729 Health, Examples from the Pregnancy Risk Assessment Monitoring System (PRAMS),
730 Executive Summary. 2002. Division of Reproductive Health, National Centers for Chronic

731 Disease Prevention and Health Promotion, Centers for Disease Control and Prevention,
732 Atlanta, GA.
733 Dehejia R and Lleras-Muney A. The timing of births: is the health of infants counter-
734 cyclical? Working Paper No. 10122. 2003. National Bureau of Economic Research,
735 Cambridge, MA.
736 Dooley D, Fielding J, Levi L. Health and unemployment. *Annu Rev Public Health*
737 1996:**17;**449-465.
738 Dooley D and Prause J. Birth weight and mothers' adverse employment change. *J Health Soc*
739 *Behav* 2005:**46;**141-155.
740 Ettner SL. Measuring the Human Cost of a Weak Economy: Does Unemployment Cause
741 Alcohol Abuse? *Soc Sci Med* 1997:**44(2);**251-260.
742 Fisher ES, Logerfo JP, Daling JR. Prenatal care and pregnancy outcomes during the
743 recession: the Washington State experience. *Am J Public Health* 1985:**75;**866-869.
744 Fukuda M, Fukuda K, Shimizu T, Yomura W, Shimizu S. Kobe earthquake and reduced
745 sperm motility. *Hum Reprod* 1996:**11;**1244-1246.
746 Graffelman J and Hoekstra RF. A statistical analysis of the effect of warfare on the human
747 secondary sex ratio. *Hum Biol* 2000:**72;**433-445.
748 Gravelle HSE, Hutchinson G, Stern J. Mortality and unemployment: A critique of Brenner's
749 time series analysis. *Lancet* 1981:**ii;**675-679.
750 Hanson MA and Gluckman PD. Developmental origins of health and disease: New insights.
751 *Basic Clin Pharmacol Toxicol* 2008:**102;**90-93.

752 Helle S, Helama S, Lertola K. Evolutionary ecology of human birth sex ratio under the
753 compound influence of climate change, famine, economic crises and wars. *J Anim Ecol*
754 2009:**78;**1226-1233.
755 Hobel CJ, Dunkel-Schetter C, Roesch SC, Castro LC, Arora CP. Maternal plasma
756 corticotropin-releasing hormone associated with stress at 20 weeks' gestation in pregnancies
757 ending in preterm delivery. *Am J Obstet Gynecol* 1999:**180;**S257-S263.
758 Hobel CJ, Goldstein A, Barrett ES. Psychosocial stress and pregnancy outcome. *Clin Obstet*
759 *Gynecol* 2008:**51;**333-348.
760 Institute of Medicine. Nutrition during pregnancy. 1990. National Academies Press,
761 Washington, DC.
762 Institute of Medicine. Influence of Pregnancy Weight on Maternal and Child Health
763 Workshop Report. 2007. National Academies Press ,Washington, DC.
764 James, W. H. The variations of human sex ratio at birth with time of conception within the
765 cycle, coital rate around the time of conception, duration of time taken to achieve conception,
766 and duration of gestation: A synthesis. *J Theor Biol* 2008:**255;**199-204.
767 Joyce T. A time-series analysis of unemployment and health. The case of birth outcomes in
768 New York City. *J Health Econ* 1990:**8;**419-436.
769 Joyce T and Mocan N. Unemployment and infant health: time series evidence from the state
770 of Tennessee. *J Hum Resour* 1993:**28;**185-203.
771 Kasl SV, Gore S, Cobb S. The experience of losing a job: reported changes in health,
772 symptoms and illness behavior. *Psychosom Med* 1975:**37;**106-122.
773 Kong FY, Perrucci CC, Perrucci R. The impact of unemployment and economic stress on
774 social support. *Community Ment Health J* 1993:**29;**205-221.

775 Lawlor DA, Leon DA, Smith GD. The association of ambient outdoor temperature
776 throughout pregnancy and offspring birthweight: findings from the Aberdeen Children of the
777 1950s cohort. *BJOG* 2005:**112;**647-657.
778 Lin SJ. The effects of economic instability on infant, neonatal, and postneonatal mortality
779 rates: evidence from Taiwan. *Soc Sci Med* 2006:**62;**2137-50.
780 Lyster WR. Altered sex ratio after London smog of 1952 and Brisbane flood of 1965. *J*
781 *Obstet Gynaecol Br Commonw* 1974:**81;**626-631.
782 Martin JF. Hormonal and behavioral determinants of the secondary sex ratio. *Soc Biol*
783 1995:**42;**226-238.
784 Martin JF. Length of the follicular phase, time of insemination, coital rate and the sex of
785 offspring. *Hum Reprod* 1997:**12;**611-616.
786 Neumayer E. Recessions lower (some) mortality rates: evidence from Germany. *Soc Sci Med*
787 2004:**58;**1037-47.
788 Paarlberg KM, Vingerhoets A, Passchier J, Dekker GA, Vangeijn HP. Psychosocial factors
789 and pregnancy outcome: a review with emphasis on methodological issues. *J Psychosom Res*
790 1995:**39**;563-595.
791 Pearlin LI, Menaghan EG, Lieberman MA, Mullan JT. The stress process. *J Health Soc*
792 *Behav* 1981:**22;**337-356.
793 Polasek O, Kolcic I, Kolaric B, Rudan I. Short Communication: Sex ratio at birth and war in
794 Croatia (1991-1995). *Hum Reprod* 2005:**20;**2489-2491.
795 Renkonen KO. Heterogeneity among first post-nuptial deliveries. *Ann Hum Gen*
796 1970:**33;**319-321.

797 Rogers I, Emmett P, Baker D, Golding J, Team AS. Financial difficulties, smoking habits,
798 composition of the diet and birthweight in a population of pregnant women in the South West
799 of England. *Eur J Clin Nutr* 1998:**52;**251-260.
800 Rook K, Dooley D, Catalano R. Stress transmission: the effects of husbands' job stressors on
801 the emotional health of their wives. *J Marriage Fam* 1991:**53;**165-177.
802 Ruhm CJ. A healthy economy can break your heart. *Demography* 2007:**44;**829-848.
803 Ruhm CJ. Good times make you sick. *J Health Econ* 2003:**22;**637-658.
804 Saadat M. Decline in sex ratio at birth after Bam (Kerman Province, Southern Iran)
805 earthquake. *J Biosoc Sci* 2008:**40;**935-937.
806 Segraves RT. Psychiatric illness and sexual function. *Int J Impot Res* 1998:**10;**S131-S133.
807 Smith JP. Unraveling the SES-health connection. *Popul Dev Rev* 2004:**30**;108-132.
808 Sram RJ, Binkova BB, Dejmek J, Bobak M. Ambient air pollution and pregnancy outcomes:
809 A review of the literature. *Environ Health Perspect* 2005:**113;**375-382.
810 Stillerman KP, Mattison DR, Giudice LC, Woodruff TJ. Environmental exposures and
811 adverse pregnancy outcomes: A review of the science. *Reprod Sci* 2008:**15;**631-650.
812 Trivers RL and Willard DE. Natural selection of parental ability to vary sex ratio of
813 offspring. *Science* 1973:**179**;90-92.
814 Wadhwa PD, Culhane JF, Rauh V, Barve SS, Hogan V, Sandman CA, Hobel CJ, Chicz-
815 DeMet A, Dunkel-Schetter C, Garite TJ *et al*. Stress, infection and preterm birth: a
816 biobehavioural perspective. *Paediatr Perinat Epidemiol* 2001:**15;**17-29.
817 Wadhwa PD. Psychoneuroendocrine processes in human pregnancy influence fetal
818 development and health. *Psychoneuroendocrinology* 2005:**30;**724-743.

819 Whitten D. EPA Pollution Cases to Increase Amid Recession, Nakayama Says
820 http://www.bloomberg.com/apps/news?pid=email_en&sid=a0XJ.Epk.xkI 2009. Accessed
821 August 22, 2009.
822 Wilcox AJ. On the importance - and the unimportance - of birthweight. *Int J Epidemiol*
823 2001:**30;**1233-1241.
824 Zorn B, Sucur V, Stare J, Meden-Vrtovec H. Decline in sex ratio at birth after 10-day war in
825 Slovenia. *Human Reprod* 2002:**17;**3173-3177.

826 **Table I.** Description of included studies: study design features, measure of key variables, and findings

827

	Time period	Population	Study design	Measure of economic contraction	Birth outcome	Quantitative finding
Bremberg (2003)	1987-1998	Stockholm county, Sweden	Comparison of recession period to comparison periods	Recession period defined as 1991-1996, comparison periods as 1987-1990 and 1997-1998	Rate of LBW[1]	Recession mean= 44.0, non-recession mean = 44.4 (p=0.85)
Brenner (1973)	1915-1967	United States	Annual time series	National, annual unemployment rate	Fetal, neonatal (<1 day) and neonatal (≤28 days) mortality rates[1]	Unemployment positively associated with: - Fetal mortality in same year - Neonatal mortality (<1 day) in next year No association with neonatal mortality (≤28 days) *Actual effect and error estimates not reported.*
Catalano and Serxner (1992a)	a) June – November, 1978 b) March, 1972 – December, 1984	a) Sacramento County, CA b) Los Angeles (LA) – Long Beach SMSA	Monthly time series	a) threat of lay-offs to state workers due to Proposition 13 b) total monthly employment[2]	Rate of LBW[1,2]	a) White male infants: β=0.0254 (p<0.025) Hispanic male infants: β=0.077 (p<0.025) b) OR for LBW for month with lowest employment: 1.071 for White male

						1.067 for Hispanic male (p<0.025)
Catalano and Serxner (1992b)	January 1972 – December 1984	Los Angeles and Orange Counties	Monthly time series	Total monthly employment, averaged over three month periods[2]	Neonatal mortality rate[1] stratified by male/female and black/white	β=3.7976 (p<0.5) for second trimester, black neonatal mortality
Catalano *et al.* (1999)	1973-1995	Norway and Sweden	Quarterly time series	Monthly change in number unemployed men summed over three month periods[2]	Rate of VLBW[1,2]	β (SE): Norway, male infants: 0.4669 (0.1078) Norway, female infants: 0.4794 (0.1446) Sweden, male infants: 0.5116 (0.1190) Sweden, female infants: 0.3664 (0.0970)
Catalano (2003)	1991	East and West German	Interrupted times series	Economic collapse in East Germany	Secondary sex ratio in East Germany compared to West German[2]	β (SE): -0.2716 (0.1340)
Catalano and Bruckner (2005)	1862–1991	Sweden	Annual time series	Annual percentage change in value of goods and service consumed by private households[2]	Secondary sex ratio[2]	β (SE): Same year: 0.0002 (0.0001) Previous year: 0.0003 (0.0001)
Catalano *et al.* (2005)	January 1989 – December 2001	California	Monthly time series	State unemployment rate[2]	Monthly count of fetal deaths[2]	β=33 for 1% increase in unemployment rate (no variability estimate reported)
Dehejia and Lleras-Muney	1975-1999	50 U.S. states & District of Columbia	Aggregate-level fixed	Yearly unemployment rate by state	Rates of: LBW, VLBW,	Black infants: β (SE)

(2003)			effects analysis		and neonatal mortality[1] - total (data not shown) and stratified by black and white	(change in SD): % LBW: -0.00078 (0.00016) (-3.58%) % VLBW: -0.00020 (0.00006) (-2.06%) Neonatal mortality rate: -1.815 (2.038) (-1.86%)
Dooley and Prause (2005)	1981–1994	Singleton first births to women in NLSY79 adequately employed at pre-pregnancy interview	Panel study	Individual: shift from adequate to inadequate (involuntary part-time, poverty wage, or unemployment) employment in year prior to birth Aggregate: unemployment rate in SMSA	Birth weight in grams (g) LBW	<u>Individual</u> Shift to unemployment: β (SE^3) = -185.43 (77.2) OR (95% CI) = 1.76 (0.71, 4.39) Shift to involuntary part-time employment: β (SE^3) = -418.05 (165.2) OR (95% CI) = 7.38 (1.82, 29.89) <u>Aggregate</u> No association with birth weight ($F(df) = .871(2,1162)$, $p < .5$)
Fisher *et al.* (1985)	1980-1983	Births to women in poorest (based on proportion of tract under 200% federal poverty) census tracts in King, Pierce, and Spokane counties in WA state compared to births to women in least poor census	Aggregate comparison of two time periods	Recession defined as 1982, comparison group defined as 1980	Rates[1] of: LBW VLBW LBW at term (≥37 weeks)	Crude relative risk (95% CI) comparing 1982 to 1980: Low income tracts: LBW: 1.18(1.00,1.25) VLBW: 1.06(0.68, 1.38) LBW at term: 1.18(0.93,1.50) High income tracts: LBW: 0.98(0.77,1.25)

		tracts.				VLBW: 1.29(0.68,2.47) LBW at term: 0.96(0.67,1.37)
Helle *et al.* (2009)	1865-2003	Finland	Annual time series	Annual percentage change in real GDP (minus percentage change in previous year)	Secondary sex ratio	β (95% CI): GDP in same year: 0.00004 (-0.00002, 0.00010) GDP in previous year: -0.00004 (-0.00011, 0.00002)
Joyce (1990)	1970-1986	Births in New York City (NYC)	Monthly time series	NYC monthly unemployment rate (reported as sum of 12 months)	Ln(%LBW) - stratified by black/white	β (95% CI): Whites: -0.053(-0.19) Blacks: -0.315(-1.22)
Joyce and Mocan (1993)	1971-1988	Tennessee (TN)	Monthly time series	Structural unemployment rate Cyclical unemployment rate (both adjusted for previous values of LBW)	Rate of LBW[1] - total and stratified by black and white (data not shown)	Total LBW β (SE[3]): Structural unemployment: -0.180 (0.17) Cyclical unemployment: -0.087 (0.09)
Lin (2006)	1979–2002	Taiwan cities	Aggregate-level fixed effects analysis	Annual city unemployment rate	Neonatal mortality rate[1]	β (SE): City unemployment: 0.057 (0.027) National unemployment: 0.07 (0.019)
Neumayer (2004)	1980–2000	German states	Aggregate-level fixed effects analysis	Annual state unemployment rate	Neonatal mortality rate[1]	β (SE): -0.0193 (0.53)

828

829 Abbreviations: Low birth weight: (LBW); very low birth weight: (VLBW); regression coefficient (standard error): β(SE); standard

830 deviation (SD); National Longitudinal Survey of Youth 1979 (NLSY79); Standard Metropolitan Statistical Area (SMSA); odds ratio

831 (95% confidence interval): OR(95% CI); F-test (degrees of freedom): F(df)

832

833 [1] rate = number with outcome/1000 live births

834 [2] "unexpected" portion of variable remaining after autocorrelation removed using ARIMA methods

835 [3] SE calculated from reported t-statistic

836
837 **Table II:** Internal and external validity characteristics of aggregate-level studies

	INTERNAL VALIDITY				EXTERNAL VALIDITY	STRENGTH OF STUDY
		Treatment of omitted third variables				
	Adjustment for measured confounders	**Variables that would affect multiple populations (generally-occurring)**	**Variables in test population that exhibit autocorrelation**	**Variables in test population that do not exhibit autocorrelation**	**External validity of sample or authors' discussion of generalizability to other populations**	
Bremberg (2003)	None	None	None	None	No discussion	Low internal and external validity
Brenner (1973)	None	None	Adjusted for non-linear trends, specific lag times, and cycles	None	No discussion	Low internal and external validity
Catalano and Serxner (1992a)	Adjusted for total number of births	None	Used decomposition methods	Two tests may be viewed as replications	Authors acknowledge limitations	High internal validity and adequate discussion of external validity
Catalano and Serxner (1992b)	Adjusted for noon temperature	Comparison: used each county's rate of neonatal mortality as control in model for other county	Used decomposition methods	Replicated results in two counties	Authors discuss generalizability	High internal validity and adequate discussion of external validity

Catalano, Hansen, and Hartig (1999)	Adjusted for total live births (potential changes in fertility)	Comparison: used each country's rate of LBW as control in model for other country	Used decomposition methods	Replicated results in two countries	Authors acknowledge limitations	High internal validity and adequate discussion of external validity
Catalano (2003)	None	Adjusted for sex ratio in West Germany	Used decomposition methods	None, but addressed concerns about confounding by migration	*Weakness:* extreme event, not typical	High internal validity and low external validity
Catalano and Bruckner (2005)	Adjusted for number of female live births	None	Used decomposition methods	None	Author addresses limitations	Moderate internal validity and adequate discussion of external validity
Catalano, et al. (2005)	Adjusted for number of female fetal deaths	None	Used decomposition methods	None	Author addresses limitations	Moderate internal validity and adequate discussion of external validity
Dehejia and Lleras-Muney (2003)	Adjusted for: - Age distribution - Educational attainment - Prenatal care - Government transfers	Year fixed effects	State-by-year interaction term	None	*Weakness:* economy may not affect individuals in the same way in all states	High internal validity and adequate discussion of external validity
Fisher *et al.* (1985)	Examined bivariate confounding by maternal: - age - race	None	None	None	*Weakness:* Only includes lowest and highest census tracts	Low internal and external validity

	- marital status - parity - time since last birth - previous infant or fetal loss Did not address multivariate confounding					
Helle (2009)	Adjusted for: - Average family size - Temperature anomaly - Total mortality rate - Famine - War	Included year-specific slope of GDP and covariates	First-differenced explanatory variable and sex ratio Used ARIMA methods to remove autocorrelation from adjusted model	None	No discussion	Moderate internal validity and low external validity
Joyce (1990)	Adjusted for: - proportion of prenatal care - unmarried mothers (proxy for social support)	None	Examined all key variables for stationarity (trend)	None	No discussion	Moderate internal and low external validity

Joyce and Mocan (1993)	Adjusted unemployment rate for previous values of LBW	None	Stated that data did not exhibit trend Smoothing to remove cyclical variation	Can be viewed as replication of 1990 study in NYC	No discussion	Moderate internal and low external validity
Lin (2006)	Adjusted for - education and income of cities - percent of population <5 yrs - number of hospitals	Year fixed effects	None	None	No discussion	Moderate internal and low external validity
Neumayer (2004)	Adjusted for: - % of population <5 and >65 yrs - % foreign born - Gini coefficient[1] - per capita income	Year fixed effects	None	None	Acknowledges findings may be specific to Germany	Moderate internal and adequate discussion of external validity

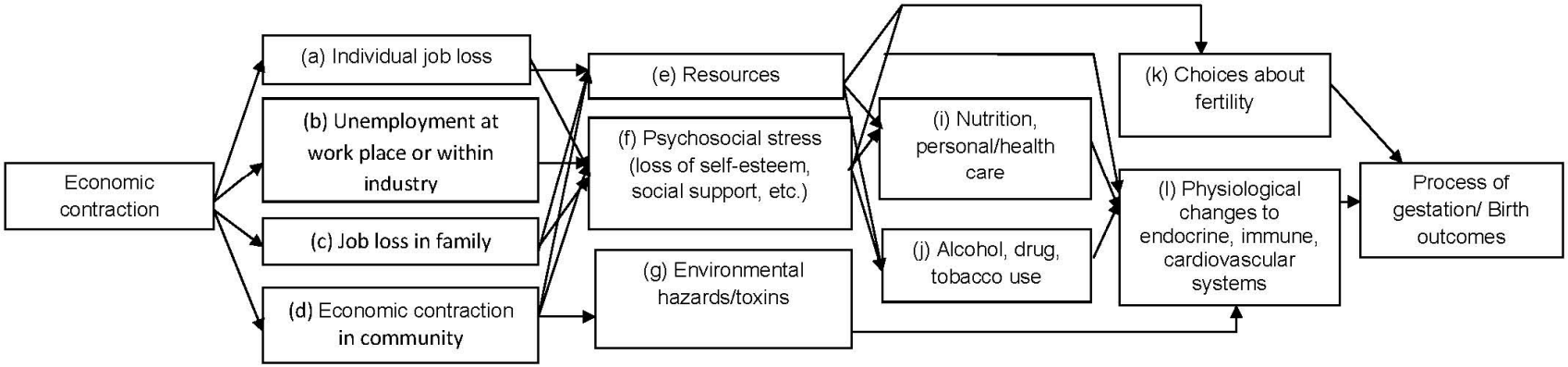
838

839 Abbreviations: Low birth weight: (LBW); Gross domestic product (GDP)

840 [1]Measure of income inequality ranging from 0 to 1 where 0 represents perfect equality and 1 represents perfect inequality

841 **Figure I.** Plausible mechanisms connecting economic contractions to gestational outcomes.

842



843