DDT and Breast Cancer Trends
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Cohn et al. (2008) suggested that birth cohort trends in breast cancer rates for women under 50 years of age are consistent with declining use of DDT (dichlorodiphenyltrichloroethane) after 1959. They cited Weiss (2007) in claiming that increased detection and treatment of in situ breast cancer must be considered when interpreting recent trends in breast cancer mortality rates in young women. The remarks of Weiss (2007) relate to women 40–49 years of age, and earlier detection and improved treatment of breast cancer has had a marked impact on breast cancer mortality rates in these women since 1990 (Berry et al. 2005; Chu et al. 1996). The birth cohort trends relevant to examining the possible impact of childhood DDT exposure on U.S. breast cancer rates, however, were firmly established well before 1990 in women <40 years of age (Tarone 2007).

Cohn et al. (2007) reported a large increase in breast cancer risk estimates for p,p′-DDT [1,1,1-trichloro-2,2-bis(p-chlorophenyl)ethane] exposure with successive birth cohorts after 1930. Their reported odds ratio estimates by period of birth for the highest tertile of p,p′-DDT exposure were 0.6 for women born in 1931 or earlier (i.e., >14 years of age in 1945), 3.9 for women born in 1932–1937 (i.e., 8–13 years of age in 1945), 9.6 for women born in 1938–1941 (i.e., 4–7 years of age in 1945), and 11.5 for women born in 1942 or later (i.e., <4 years of age in 1945) [Table 4, Cohn et al. (2007)]. In contrast, I have found no evidence of increasing breast cancer rates among young U.S. women born between 1930 and 1945 (Tarone 2007). I quantified trends in breast cancer mortality rates for U.S. white women 20–39 years of age (by 5-year age group) born during 1930–1945 using linear regression analyses with the logarithm of the age-specific rate as the dependent variable and year of birth as the independent variable (with two-sided p-values) [Surveillance, Epidemiology, and End Results (SEER) 2006; Tarone 2007]. The slope estimates did not differ significantly from zero for women in the three youngest age groups (p > 0.25), and there was a marginally significant decrease in rates for women 35–39 years of age (p = 0.04). Thus the trends in breast cancer mortality rates among women born in 1930–1945 are not consistent with the sharply increasing trend in odds ratios for childhood DDT exposure by birth period reported by Cohn et al. (2007). The most recent mortality rate contributing to the reported regression analyses (corresponding to women in the 35- to 39-year age group born in 1945) was for 1983, well before improvements in detection and treatment would have had any impact on breast cancer mortality rates.

Women born after 1945 were exposed to DDT for each of the first 13 years of life (and all years thereafter). In addition, DDT exposure increased from 1945 through 1959, when DDT use peaked (with dietary exposure peaking in 1965) (Wolff et al. 2005). If DDT exposure early in life markedly increases breast cancer risk, then some evidence of the increasing DDT use after 1945 might be expected in breast cancer mortality rate trends for young women born from 1946 through 1959 (Tarone 2007). Breast cancer mortality rates decreased significantly among women 20–24 years of age (p = 0.009) and 25–29 years of age (p = 0.0002) born between 1946 and 1959 (SEER 2006; Tarone 2007). The most recent rate contributing to these regression analyses was for 1987 (corresponding to women in the 25- to 29-year age group born in 1959). Breast cancer mortality rates decreased even more markedly (p < 0.0001) for women in the 30- to 34-year and 35- to 39-year age groups born from 1946 through 1959; some of the recent rates in these latter age groups were almost certainly affected by improved breast cancer detection and treatment, although decreasing trends were apparent in both age groups for rates well before 1990 (Tarone 2007). Thus, U.S. breast cancer mortality rates in women between the ages of 20 and 39 who were born between 1930 and 1959 show no evidence of an increase in breast cancer risk associated with their marked increase in DDT exposure during childhood.

The observed birth cohort trends in breast cancer rates do not refute a possible association between childhood DDT exposure and breast cancer risk, and contrary to the implication of Cohn et al. (2008), no such claim was made in my earlier letter (Tarone 2008). The regression analyses reported above suffer the weaknesses of all ecologic analyses, and in fact, the decreasing birth cohort risk of breast cancer in baby boomers has been observed in spite of trends in established risk factors (e.g., parity, age at first birth, and oral contraceptive use) that would predict increasing breast cancer rates among U.S. women born after 1945. If, as suggested by Cohn et al. (2007), the public health significance of DDT exposure early in life is large, then this would provide additional evidence that the factor or factors responsible for the paradoxical decrease in birth cohort risk of breast cancer observed among U.S. baby boomers must have a very powerful impact on breast cancer etiology, large enough to turn an expected increasing trend in breast cancer rates among baby boomers into a decreasing trend.

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Editor’s note: In accordance with journal policy, Cohn et al. were asked whether they wanted to respond to this letter, but they chose not to do so.

Beef Production and Greenhouse Gas Emissions
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In their article discussing the impacts of farm animal production on climate change, Koneswaran and Nierenberg (2008) called for “immediate and far-reaching changes in current animal agriculture practices” to mitigate greenhouse gas (GHG) emissions. One of their recommendations was to...
switch to organic livestock production, stating that
Raising cattle for beef organically on grass, in contrast to fattening confined cattle on concentrated feed, may emit 40% less GHGs and consume 85% less energy than conventionally produced beef.

These claims are terribly misleading. Koneswaran and Nierenberg (2008) compared organic beef produced in Sweden (22.3 kg of carbon dioxide-equivalent GHG emissions per kilogram of beef) with unusual and resource-intensive Kobe beef production in Japan (36.4 kg of CO2-equivalent GHG emissions per kilogram) (Cederberg and Stadig 2003; Ogino et al. 2007).

To achieve the ultra-high fat levels in meat preferred by Japanese consumers, Japan’s wagyu cattle are raised and fattened for more than twice as long as typical U.S. beef cattle (Cattle Marketing Information Service Inc. 2007; Ogino et al. 2007). Moreover, all of the feed and forage for the Japanese animals (from birth through slaughter) must be shipped especially long distances—> 18,000 miles in the example cited. Hence, this beef has ultra-high GHG emissions and energy requirements.

According to several analyses, typical nonorganic beef production in the United States results in only 22 kg of CO2-equivalent GHG emissions per kilogram of beef, which is 0.3 kg less than the Swedish organic beef system (Johnson et al. 2003; Subak 1999). These comprehensive life cycle analyses, which examined all aspects of beef production and all GHG emissions, seem to definitively rule out significant reductions in GHG emissions by switching to organic beef production.

In fact, if nitrous oxide and other emissions from land conversion are included in the analysis, a large-scale shift to organic, grass-based extensive livestock production methods would increase overall GHG emissions by nearly 60% per pound of beef produced.

According to Searchinger et al. (2008), each acre of cleared land results in 10,400 lb/ac/year of CO2-equivalent GHG (over a 30-year period, based on estimated emissions from a proportion of each land type converted to cultivation in the 1990s). Our own analysis (Avery and Avery 2007) using conservative beef production parameters from Iowa State University’s Leopold Center for Sustainable Agriculture shows that grain-finishing cattle is at least three times more land efficient per pound of finished beef compared to grass-finishing.

Cattle industry statistics [U.S. Department of Agriculture (USDA) 2008] show that, in 2007, the United States used 2 billion bushels of corn to produce 22.16 billion lb finished grain-fed beef (17.3 million head steers and 10.2 million head heifers at average dressed weights of 830.2 and 764.8 lb, respectively). At 150 bushels/acre corn, this means we used 13.3 million acres to produce the feed grains. Converting all beef production to grass-based finishing would require at least an additional 26.6 million acres of pasture/grass to produce 2007 U.S. beef output.

Using the 22 lb of CO2-equivalent GHG per pound of grain-fed beef from Johnson et al. (2003) and the 22.3 lb CO2-equivalent GHG per pound of beef for organic grass of Cederberg and Stadig (2003), each system producing 22.16 billion lb of beef would directly and indirectly result in 487.5 and 494.2 billion lb of CO2-equivalent GHG emissions, respectively.

Additionally, adding the “carbon debt” resulting from the additional cleared land required by the two-thirds less efficient grass finishing process (26.6 million acres × 10,400 lb/ac/year, or 276.6 billion lb/year) results in the organic system totaling 770 billion lb of CO2-equivalent GHG emissions; or 58% higher than the conventional system’s total of 487.5 billion lb.

In early 2007, the authors received funding from the GET IT (Growth Enhancement Technology Information Team) pharmaceutical companies that are members of the National Cattlemen’s Beef Association, to conduct an analysis of the environmental impacts and costs of various beef production systems.

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**Beef Production: Koneswaran and Nierenberg Respond**

Avery and Avery, who find comparing conventional Japanese and organic Swedish beef production misleading, propose relying on “comprehensive life cycle analyses” (LCAs) to quantify emissions from conventional U.S. beef production. However, neither study they cite (Johnson et al. 2003; Subak 1999) appears to be a comprehensive LCA, and it is unclear whether these studies considered emissions created by facets of beef production such as feed transport or pesticide manufacturing, as did Ogino et al. (2007). Additionally, contrary to Avery and Avery’s conclusion, Subak (1999) stated that

These results indicate that the intensification of beef production systems may be counterproductive because net emissions of carbon dioxide as well as nitrogen and other pollutants would increase.

For a more comprehensive analysis, additional production aspects must be considered. Ogino et al. (2007), for example, included the transportation of feed (> 18,000 km, not miles, as stated by Avery and Avery in their letter), which accounted for 8.3% of emissions.

A better comparison of conventional versus organic beef production may be an LCA of greenhouse gas (GHG) emissions from three Irish systems reported by Casey and Holden (2006). Conventional production generated the most GHGs, followed by agri-environmental, with the organic system producing the least GHGs.

In contrast to conventional production, organic farming can reduce nitrous oxide emissions by avoiding excessive amounts of manure, as stacking densities are limited to land available for manure application. Organic agriculture typically also uses less fossil-fuel energy, in part because thousands of feed transport miles may be reduced (Kotschi and Müller-Sämann 2004).

Pasture-based systems require less operational fuel and feed than do conventional systems, and they adeptly sequester GHGs in the soil, tying up 14–21 million metric tons of carbon dioxide and 5.2–7.8 million metric tons of N2O in

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pasture soil organic matter (Boody et al. 2006; Rayburn 1993).

Dourmad et al. (2008) concurred with our conclusion (Koneswaran and Nierenberg 2008) that more research is needed and noted that existing LCAs—other attempts to quantify GHGs from various systems (Avery and Avery 2007)—lack data on pesticide use and animal transport from farms or feedlots to slaughter. In our article (Koneswaran and Nierenberg 2008), we not only argued for refinement of agricultural practices but also for a concurrent reduction in animal product consumption in high-income nations, especially because the U.N. Food and Agriculture Organization has concluded that animal agriculture accounts for more GHGs than transport (Steinfeld et al. 2006). In addition to lowering GHG emissions, reducing animal product consumption could also decrease the incidence of cardiovascular disease, certain cancers, and obesity (McMichael et al. 2007). Given the developing global food crisis, it is important to note that did Baroni et al. (2007) in the European Journal of Clinical Nutrition, that plant-based diets “could play an important role in preserving environmental resources and in reducing hunger and malnutrition in poorer nations.”

Although Avery remains skeptical over the role of anthropogenic GHG emissions in global warming (2008), the Intergovernmental Panel on Climate Change (IPCC 2007) concluded that Most of the observed increase in global average temperature over the mid-20th century is very likely due to the observed increase in anthropogenic GHG concentrations.

The link between GHG mitigation and organic or extensive animal agriculture systems is well established, as are the other environmental and public health benefits of less-intensive production systems. Understanding the efficacy of less-technology-dependent mitigation strategies is critical as the effects of global warming become more evident.

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**Traffic-Related Air Pollution and Stress: Effects on Asthma**

Chen et al. (2008) examined the potential for social stressors to influence responsiveness to environmental pollution. Contrary to their initial hypothesis, and to results we reported previously (Clougherty et al. 2007), their findings indicated that chronic stress was associated with asthma symptoms and heightened inflammatory profiles only in low nitrogen dioxide areas. We would like to note several key issues in the emerging research on social susceptibility to environmental pollutants that should be considered as research on this work moves forward.

One key issue is that the relative timing of psychosocial stressors and physical exposures, which Chen et al. (2008) did not present, is critical for at least two reasons:  
- **Acute and chronic stress produce substantively different physiological sequelae.** Acute stress can induce bronchodilation with elevated cortisol (possibly masking short-term detrimental respiratory effects of pollution), whereas chronic stress can result in cumulative wear and tear (allostatic load) and suppressed immune function over time, increasing general susceptibility (McEwen and Seeman 1999).
- **Temporal relationships between stress and pollution exposures matter.** Depending on when measures are obtained, exposure misclassification is possible, which may influence the directionality of observed interactions. Chen et al. (2008) stated that the measured 6-month stress and NO2 periods do not overlap, but they did not specify whether the stress measure preceded the 1998–2003 NO2 exposure window or the amount of time that passed between exposures. If the stress interval occurred first, some increased susceptibility to subsequent pollution is plausible, provided that chronic stress effects predominate over acute effects. If, however, the stress interval occurred after NO2 exposures, the interaction is potentially problematic, because we must then assume that stress levels measured after the 6-year NO2 period (1998–2003) are relevant for the earlier time, which may not be the case. If, for example, respondents compared current stress to prior experience, an individual reporting high stress for one interval may have experienced lower stress previously, during those “reference” periods corresponding to the NO2 window—potentially producing a negative interaction, as Chen et al. (2008) observed. More broadly, careful attention to relative timing and durations of stress and pollution exposures is critical in maintaining directionality and interpretability as we progress with this research.

Second, Chen et al.’s finding of significant effects of stress only in low-NO2 areas (Chen et al. 2008) points to the possibility of nonlinear interactions and saturation effects at high exposures. Similarly, our group (Clougherty et al. 2006) reported that asthmatic children of families reporting higher fear of violence showed less symptom improvement in response to allergen-reducing indoor environmental interventions. Our results, counter to our initial hypotheses, suggested a saturation effect in our very high-exposure public housing cohort, where either high exposure alone may have been adequate to induce or maintain symptoms.

Third, Chen et al. (2008) did not address the spatial covariance among stress, socioeconomic status, and pollution, which can
confound geographic information system–
based air pollution epidemiology. In partic-
ular, communities near highways, with 
higher traffic-related pollution and lower 
property values, may be disproportionately
composed of families having lower socio-
economic status. Because of this potential for 
spatial autocorrelation and thus con-
fitting, accurate fine-scale exposure mea-
surement is critical. However, Chen et al. 
(2008) did not present pollution or stress 
maps, the NO2 model was not formally vali-
dated to this cohort’s specific spatial char-
teristics, and spatial patterns in stress were 
not explored; thus we are left wondering 
whether, and how, spatial misclassification 
and confounding may be at play. Relatedly, 
social–physical correlations may vary by geo-
graphic scale (e.g., across vs. within neigh-
borhoods); although a given neighborhood 
may have high mean pollution and stress, 
it is harder to argue that particular individuals 
(or residences) within these neighborhoods 
would be relatively more exposed to both 
(i.e., individuals living closer to highways 
are not necessarily more exposed to violence 
or family stress than are other community 
members).

Fourth, Chen et al. (2008) reported 
results for 73 asthmatic children. However, 
in the absence of information on disease 
chronicity, severity, or adequacy of medical 
treatment, it may be difficult to truly assess 
the influence of either stress or traffic-
related pollution. Relatedly, it is important 
to distinguish between processes related to 
illness onset from those related to progres-
sion or exacerbation, and whether the nega-
tive interaction observed in their study 
could be expected in healthy adolescents.

Finally, the cohort studied by Chen 
et al. (2008) varied considerably in age 
(9–18 years), but the authors did not con-
sider age-related asthma characteristics 
and responsiveness to family stressors and 
air pollution. Age stratification should have 
been used to compare the strength of indi-
vidual and combined effects at multiple 
ages. It would also be interesting to know 
whether non–family-related stressors would 
produce similar interactions at all ages.

The issues we have highlighted—tempo-
ral relationships between stressors and pollu-
tion, nonlinearity and saturation effects, 
spatial correlations, age-related susceptibility, 
and distinctions between illness etiology and 
exacerbation—will be critical in the further 
study of social–environmental interactions. 
These effects may distort observed associa-
tions (e.g., saturation effects may reverse 
interactions at high exposures), but with sus-
tained attention to these issues, we can better understand joint effects of social and physical 
environments on health.

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financial interests.

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Traffic-Related Air Pollution and Stress: Chen and Brauer 
Respond
doi:10.1289/ehp.11863R
We thank Clougherty and Kubzansky for 
their thoughtful review of our article (Chen 
et al. 2008). We view our article, as well as 
their article on exposure to violence, air pol-
lution, and asthma etiology (Clougherty 
et al. 2007), as suggestive regarding how the 
social and physical environments operate in 
asthma. Although the nature of the inter-
action effects were different in these two 
studies, the broader point—that there are 
interactive effects between the social and 
physical environments in asthma—is consis-
tent and is the key message that we wish to 
emphasize.

We would like to address their specific 
comments. First, regarding temporal issues, 
Clougherty and Kubzansky raise the possi-
bility that stress increases susceptibility to 
subsequent pollution. We agree that this is 
possible; we also recognize the possibility 
that chronic pollution exposure could 
heighten responses to subsequent stressors. 
As we stated in our “Discussion” (Chen 
et al. 2008), the time frame of assessments 
that were available to us for these analyses 
was not ideal, and future studies should 
more specifically coordinate the timing of 
exposures to both stress and air pollution.

Second, we agree it is possible that satu-
ration effects may occur at high levels of 
pollution exposure. However, because pollution 
levels in Vancouver (British Columbia, 
Canada) are not extreme (the range in our 
sample was 10–30 ppb nitrogen dioxide), 
we think this is an unlikely explanation.

Third, regarding spatial covariance, in 
our study (Chen et al. 2008), family stress 
was measured at the individual level; thus, 
we do not have neighborhood-level stress 
maps or information on spatial patterns in 
stress. Although spatial covariance between 
socioeconomic status and air pollution has 
the potential to lead to confounding, the 
availability of individual measures of stress 
and air pollution exposure estimates at the 
resolution of individual addresses allowed 
us to evaluate interactions. Our longitudi-
nal findings also diminish the likelihood of 
confounding. Further, previously published 
pollution maps (Henderson et al. 2007) 
have shown that, in our study area, air pol-
lution levels are not spatially correlated with 
neighborhood socioeconomic status [e.g., 
see UBC (University of British Columbia) 
Centre for Health and Environment 
Research 2008].

Fourth, we presented information about 
disease characteristics in Table 1 (Chen 
et al. 2008). We also controlled for asthma 
severity and medication use in all analyses, 
as described in our article under “Potential 
confounders.”

Finally, we agree that it would be inter-
esting to know whether stress by air pollu-
tion effects vary by age. However, given the 
limited sample size in our study, we were 
unable to test this possibility.

The authors declare they have no competing 
financial interests.

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