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The New Danger of Thirdhand Smoke: Why Passive Smoking Does Not Stop at Secondhand Smoke

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Passive smoking exposure is a topic of great concern for public health because of its well-known adverse effects on human health (International Agency for Research on Cancer 2004). Two news articles on this topic were published in the February 2011 issue of *Environmental Health Perspectives* (Burton 2011; Lubick 2011). Lubick (2011) discussed the global health burden of secondhand smoke, and Burton (2011) emphasized a new and alarming consequence of smoking in indoor environments—"thirdhand smoke"—a term first coined in 2006 (Szabo 2006).

Secondhand smoke is defined as "the combination of smoke emitted from the burning end of a cigarette or other tobacco products and smoke exhaled by the smoker" (World Health Organization 2007). Thus, secondhand smoke exposure consists of an unintentional inhalation of smoke that occurs close to people smoking and/or in indoor environments where tobacco was recently used.

Thirdhand smoke is a complex phenomenon resulting from residual tobacco smoke pollutants that adhere to the clothing and hair of smokers and to surfaces, furnishings, and dust in indoor environments. These pollutants persist long after the clearing of secondhand smoke. They are reemitted into the gas phase or react with oxidants or other compounds present in the environment to form secondary contaminants, some of which are carcinogenic or otherwise toxic for human health (Matt et al. 2011). Thus, thirdhand smoke exposure consists of unintentional intake (mainly through inhalation but also via ingestion and dermal routes) of tobacco smoke and other related chemicals that occurs in the absence of concurrent smoking. Exposure can even take place long after smoking has ceased, through close contact with smokers and in indoor environments in which tobacco is regularly smoked.

Lubick (2011) considers secondhand smoke synonymous with passive smoking, as do the majority of the authors publishing on this topic. However, in light of new evidence about thirdhand smoke (Matt et al. 2011), it is no longer appropriate to use the term "secondhand smoke" as a synonym for passive smoking or environmental tobacco smoke, because it represents a *pars pro toto*. In other words, using the term "secondhand smoke" mistakes one part of the problem for the whole. Instead, we propose that "passive smoking" or "environmental tobacco smoke" be used as a more inclusive term to describe any tobacco smoke exposure outside of active smoking.

This question of terminology is of particular concern for researchers evaluating passive smoking exposure in indoor settings, especially in domestic environments. Since numerous countries have introduced smoking bans in enclosed public places, domestic environments have become the main sources of passive smoking exposure (World Health Organization 2007). We believe researchers should determine the independent contributions of secondhand and thirdhand smoke when they assess the magnitude of pollutant intake due to passive smoking exposure.

The authors declare they have no actual or potential competing financial interests.

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Time-Dependent Exposures and the Fixed-Cohort Bias

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Hwang et al. (2011) showed an interesting association between air pollution and stillbirth. The authors examined births between 1 January 2001 and 31 December 2007 in Taiwan using a case–control design, with each of 9,325 stillbirths matched to 10 controls. They examined exposures from all three trimesters. For cases and controls born before September 2001, some exposures could have occurred in March–December 2000. Other pregnancies with exposures during this period could not be included in the study because the births occurred before January 2001, thus having the potential to bias the estimates of time-dependent exposures such as air pollution. We previously labeled this bias the "fixed-cohort bias," but it applies equally to case–control designs using fixed dates of birth to recruit subjects (Strand et al. 2011).

For example, for pregnancies in their first month during June 2000, we can assume that some will result in stillbirth. Stillbirths often have relatively short gestations; therefore, some of these stillbirths would not be included in the cohort because they would have occurred before January 2001. In contrast, live births from this time could have made it into the cohort. In Table 1 of Hwang et al. (2011), mean gestation time was 26.9 weeks for the stillbirth subjects and 38.5 weeks for the control subjects. So for pregnancies in their first month in June 2000, the mean date of birth for stillbirths would be in December 2000 (outside the cohort), whereas the mean date of birth for live births would be in February 2001 (inside the cohort). This means that first trimester exposures during June 2000 may look remarkably protective, as the number of stillbirths would be very small. The bias for a study of air pollution would then depend on what exposure occurred in June 2000 and what the true association is. If it was a month with a particularly high level of air pollution, this would bias any true association between pollution and stillbirth towards the null. If there was no association between pollution and stillbirth, the bias would be toward a false finding of a protective effect.

The bias can also occur at the end of the cohort, with the longer pregnancies missed and the shorter pregnancies captured.

There is a simple way to avoid the bias: by excluding case and control subjects with estimated conception dates 20 weeks (shortest gestation) before the data collection started or 43 weeks before it ended (assuming a longest gestation time of 43 weeks). This ensures that the exposures examined during any gestation period could equally apply to cases and controls. The cost is a loss of sample size, which may widen any confidence intervals. I estimate that around 7% of pregnancies would need to be excluded by Hwang et al. (2011), but the benefit would be the removal of a potentially serious bias.

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Time-Dependent Exposures and the Fixed-Cohort Bias: Hwang et al. Respond

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Barnett expresses concerns about a potential bias in our article (Hwang et al. 2011) related to use of a fixed study period based on the date of delivery: on average a shorter duration of gestation among stillbirths compared to live births in combination with seasonal variation of exposure. We acknowledge the complexity of assessing effects of exposure with seasonal variation on the risk of stillbirth and thank Barnett for his suggestion to avoid a possible bias, which he with his colleagues illustrated through simulations of a retrospective cohort study (Strand et al. 2011). We reanalyzed the data, excluding case and control subjects following Barnett's suggestion to quantify the "fixed cohort bias." This led to loss of approximately 4.7% (4,480/102,575) of the subjects. The point estimates were similar with those from the original analyses, but some confidence intervals became wider (Table 1). This shows that the role of the fixed cohort bias was minimal in our study.

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Table 1. Adjusted ORs (95% CIs) for stillbirth by average pollutant concentrations, by trimester and for the whole pregnancy (single pollutant models), following Barnett's suggestion to address the "fixed cohort bias."

Air pollutant	All births (gestational age > 20 weeks) Model 1[a]	Preterm births (gestational age < 37 weeks) Model 2[b]	Term births (gestational age ≥ 37 weeks) Model 3[b]
PM_{10} (10 μg/m^3)			
1st trimester	1.02 (0.99–1.05)*	1.03 (1.00–1.07)	1.00 (0.96–1.04)
2nd trimester	0.97 (0.94–0.99)*	0.99 (0.95–1.03)*	0.95 (0.92–0.99)*
3rd trimester	0.97 (0.95–1.00)*	0.97 (0.92–1.02)	0.97 (0.92–1.02)*
Whole pregnancy	0.97 (0.95–1.02)*	1.01 (0.96–1.06)*	0.96 (0.91–1.01)*
SO_2 (1 ppb)			
1st trimester	1.02 (1.00–1.04)	1.04 (1.01–1.06)*	1.00 (0.97–1.03)*
2nd trimester	1.00 (0.98–1.02)	1.02 (0.99–1.04)*	0.99 (0.96–1.02)
3rd trimester	1.00 (0.98–1.02)	1.01 (0.97–1.04)	1.01 (0.97–1.04)*
Whole pregnancy	1.01 (0.99–1.03)	1.03 (1.00–1.06)	0.99 (0.97–1.02)*
NO_2 (10 ppb)			
1st trimester	1.01 (0.96–1.07)	1.05 (0.97–1.13)*	0.98 (0.90–1.06)
2nd trimester	0.97 (0.92–1.02)*	1.00 (0.93–1.08)*	0.95 (0.88–1.02)
3rd trimester	0.98 (0.92–1.04)	0.98 (0.89–1.08)	0.98 (0.89–1.08)*
Whole pregnancy	0.98 (0.93–1.05)*	1.02 (0.94–1.11)*	0.96 (0.88–1.05)
CO (100 ppb)			
1st trimester	1.00 (0.98–1.02)	1.00 (0.97–1.02)*	1.01 (0.98–1.04)
2nd trimester	1.00 (0.98–1.02)*	0.99 (0.96–1.01)*	1.01 (0.98–1.03)
3rd trimester	1.01 (0.99–1.03)*	0.98 (0.95–1.02)	0.98 (0.95–1.02)
Whole pregnancy	1.00 (0.98–1.02)	0.99 (0.96–1.02)*	1.01 (0.98–1.04)
O_3 (10 ppb)			
1st trimester	1.01 (0.96–1.06)	1.01 (0.94–1.09)*	0.99 (0.92–1.06)
2nd trimester	0.96 (0.91–1.01)	1.01 (0.94–1.08)*	0.92 (0.85–0.98)*
3rd trimester	0.99 (0.93–1.04)*	0.98 (0.90–1.08)*	0.98 (0.90–1.08)*
Whole pregnancy	0.97 (0.91–1.04)	1.01 (0.92–1.11)*	0.94 (0.85–1.03)*

Abbreviations: CO, carbon monoxide; NO_2, nitrogen dioxide; O_3, ozone; PM_{10}, particulate mattter ≤ 10 μm in aerodynamic diameter; SO_2, sulfur dioxide.
[a]Logistic regression analysis adjusting for sex, maternal age, gestational age, municipal-level socioeconomic status (SES), season of conception, and year of birth. [b]Logistic regression analysis adjusting for sex, maternal age, municipal-level SES, season of conception, and year of birth. *Point estimates were similar with those from the original analyses, but some confidence intervals were wider.

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DDT Paradox

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Bouwman et al. (2011) characterized anti-DDT, centrist-DDT and pro-DDT positions, and stated that they "could find no current outright anti-DDT activities." This conclusion is false and misleading.

Several activist groups currently promote an anti-DDT agenda, routinely hyping supposed human health and environmental harm from DDT and ignoring studies that find no association between DDT and such harm. For instance, the description of Biovision's "Stop DDT" project states that "Biovision is engaged to achieve a worldwide ban on DDT" (Biovision 2011). Such a statement could be ignored if it were not for the fact that Hans Herren, president of Biovision, was a member of the Stockholm Convention's DDT Expert Group, as were two of the authors of Bouwman et al. (2011)—Bouwman and van den Berg. Furthermore, Bouwman et al. ignored the Secretariat of the Stockholm Convention's promotion of an arbitrary deadline for cessation of DDT production by 2020 (United Nations Environment Programme 2007). The Secretariat's promotion of this deadline undermines use and production of DDT and is *ultra vires,* because the convention excludes any deadline.

In identifying the "pro-DDT" faction, Bouwman et al. (2011) attempted to characterize it as a minority view while ignoring national malaria control programs and ministers of health who repeatedly proclaim the importance of DDT for disease control programs in countries with high incidence of malaria. Indeed, the Southern African Development Community (SADC) Ministers of Health agreed at their November 2010 meeting that DDT was still required (SADC 2011). In addition, at the recent fifth meeting of the Conference of Parties to the Stockholm Convention, Namibia and the