Exposure to a Fungal Volatile Compound: Significance of Effects

In contrast to what Wålinder et al. (2005) concluded in their article “Acute Effects of a Fungal Volatile Compound,” I interpret the article to report essentially no effects beyond chance. In all, the authors carried out some 76 comparisons (each one representing a time point and an exposure vs. control measurement) if you take blink frequency as a single comparison. The authors reported finding 5 “significant differences” out of 76 comparisons. Of the reported significant differences, one (blink frequency) is misleading, as discussed below. Of the remaining 75 comparisons, 4 differences at a \( p \)-value of \(< 0.05 \) might be expected by random chance. This is without applying Bonferroni’s adjustment for multiple comparisons; using this adjustment, a \( p \)-value of approximately \(< 0.0007 \) would be required for a single comparison to be statistically significant. None of the differences reported reached this level.

Wålinder et al. (2005) reported that the subjects showed increased “blink frequency” during 3-methylfuran (3-MF) exposure (Table 1), but the frequency was higher in the exposure phase at time 0, about 9 for 3-MF exposure versus 6.5 for the control air phase. From the data in Table 1, it appears that both groups had fewer overall blinks per minute compared with baseline during the trial (Figure 2). Reporting that blinking was higher during exposure and not noting that it was higher at baseline is disingenuous.

Wålinder et al. (2005) may have mislabeled tear break-up, but as it reads in the legend for Table 1, a negative value indicates a decrease; therefore, the 6 sec given for measured break-up time after 3-MF exposure (Table 1) indicates that it was increased (i.e., longer to tear break-up), which is better. Is this correct? Also, was the observer who measured the tear break-up blinded to the exposure?

Finally, of the four lung measurements taken, the only comparison with a \( p \)-value of \(< 0.05 \) was the small 100-mL change for forced expiratory volume in 1 sec (FEV1)?

The authors have provided expert testimony in mold litigation.

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Exposure to a Fungal Volatile Compound: Wålinder et al. Respond

There are different opinions on the use of Bonferroni’s corrections. Everitt (1995) stated that it gives too conservative estimates if there are more than five tests performed. In environmental medicine, one exposure can have different health effects, so it is reasonable to test for different types of effects on different organs. We prefer to perform conventional statistical tests, without Bonferroni correction, and look at the pattern of significant effects and their biological plausibility.

We did not perform 76 comparisons (Wålinder et al. 2005); we actually performed 25 tests on 13 physiologic variables based on differences before and after exposure. A fourteenth variable (vital staining) was tested only after exposure. Repeated measurement analysis was performed on blink frequency (60 consecutive measurements of 2 min each) and one questionnaire with 10 questions was administered at six different times. This is a total of 37 tests performed on 25 variables, having five significant values, of which one was highly significant (\( p < 0.001 \)).

Moreover, all tests point in the same direction—mucosal effects of the exposure. We did not find the same effects over time for control exposure. There is, of course, the possibility that some of the significant effects were due to chance, and we were quite modest in our conclusions, using the words “may” or “might be.” Because our study is the first exposure-chamber study on 3-methylfuran (3-MF), more studies are needed to determine final conclusions.

Blink frequency was measured only during the first 2 min of exposure to 3-MF inside the chamber, possibly followed by later adaptation (8.8 blinks/min during the first 2 min, compared with a mean of 7.6 blinks/min during the whole period of exposure).

It is true that there was a numerical increase in break-up time at exposure, which could be in agreement with increased blink frequency. The fatty layer on the tear film is produced by the glands of the eyelids. Therefore, an increased blinking frequency could produce more secretion from the meibomian glands and therefore a longer break-up time.

Regarding lung function, transient effects of environmental exposure (as well as diurnal variation, which we controlled for by performing the experiment at the same time) may affect either forced vital capacity (FVC) or forced expiratory volume in 1 sec (FEV1), or both. Physiologically and numerically, the decrease was of the same order, but statistically the outcome was different. The increases were 0.1 L for FVC and 0.08 L for FEV1 after exposure to 3-MF. The magnitude of the effect was clinically small, but it was significant at group level for FVC. Small pulmonary effects may have large health effects in a population (Künzli et al. 2000).

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Organophosphates and Outdoor Air Concentrations

Harnly et al. (2005) suggested that measured air concentrations of organophosphate insecticides may pose a particular concern for

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Organophosphates and Outdoor Air: Harnly et al. Respond

Peterson is incorrect in stating that we did not conduct a risk assessment. We summarized in our article (Harnly et al. 2005) and detailed in a previous article (Lee et al. 2002) a human health risk assessment demonstrating elevated acute and subchronic risks for children’s exposures to ambient chlorpyrifos air concentrations in agricultural communities. Compared with the assessment by Peterson, we used a more refined probability distribution analysis, included air levels of the degradation product (chlorpyrifos oxon), and presented exposures relative to reference values. The chlorpyrifos reference values, however, were based on the same no-observed-adverse-effect levels (NOAELs) and the same 10-fold intraspecies and interspecies, and child uncertainty factors used in Peterson’s calculations.

In our article (Harnly et al. 2005), we suggested that our risk assessment may underestimate risks for several reasons, such as a) risk assessments that use NOAELs, and not the entire dose–response curve, tend to underestimate risks (Castorina and Woodruff, 2003); and b) the true ranges of intraspecies and interspecies variability are unknown and may be larger than the factors used (Eskenazi et al. 1999; Faustmann et al. 2000). In a very recent study, some

children’s health. However, when considering the current scientific weight of evidence, their conclusions cannot be supported for two reasons: first, they did not demonstrate a particular concern for children based on their results, and second, they cited incomplete and inappropriate literature to support the notion that recent toxicologic and epidemiologic studies indicate a health concern.

Harnly et al. (2005) did not conduct a risk assessment to demonstrate a concern for children. Rather, they detected pesticides in air concentrations in agricultural areas and then suggested there may be a concern for children because of recent toxicologic and epidemiologic studies. Their observed median exposures of all three active ingredients (chlorpyrifos, diazinon, and malathion) were all low and well within established regulatory limits. A risk assessment approach would have been quite useful. For chlorpyrifos, Harnly et al. (2005) detected a 20-day median concentration in air of 0.000033 mg/m³. A tier-1 risk assessment assuming an air concentration of chlorpyrifos at 0.000033 mg/m³, the mean body weight of a 1- to 2-year-old child of 12.3 kg, a child inhalation rate of 6.8 m³/day, and 24-hr outdoor respiration results in a chlorpyrifos inhalation exposure of 0.0000182 mg/kg/day. Margins of exposure (MOE) would be 5,495 [acute inhalation no observed adverse effect level (NOAEL) = 0.1 mg/kg/day], 27,473 (acute NOAEL = 0.5 mg/kg/day), and 1,648 (chronic NOAEL = 0.03 mg/kg/day). All MOEs are greater than the U.S. Environmental Protection Agency (EPA) target MOE of 1,000 for infants, children, and females 13–50 years of age (U.S. EPA 2002).

More problematic is that Harnly et al. (2005) stated that recent cellular, animal, and human evidence of toxicity, particularly in newborns, supports the public health concern indicated by initial risk estimates.

The authors did not provide a sufficiently thorough review of the literature relevant to risk assessment to support or refute their statement. In the case of chlorpyrifos, this statement cannot be supported by the available evidence. The principal problem with Harnly et al.’s approach is not unique to their article. Appropriate risk assessment requires appropriate data, and, as simple as this relationship sounds, it is often ignored. Harnly et al. (2005) cited toxicity and epidemiologic studies, but these particular studies are not appropriate for use in risk assessment. This problem has become so pervasive that Conolly et al. (1999) clarified the basic features of toxicology studies that are and are not appropriate for use in risk assessment.

Harnly et al. (2005) should not have included the findings of Qiao et al. (2002) because the high doses, subcutaneous route of administration, and carrier were inappropriate for toxicologic risk assessment (Conolly et al. 1999; Zhao et al. 2005). Indeed, Slotkin (2004), a coauthor of Qiao, has written that there is little academic interest in relevant routes of exposure or pharmacokinetics. He stated that practical issues that are critical to standardized testing are de-emphasized, such as pharmacokinetics/toxicokinetics, the matching of routes of exposure to those of humans in industrial, agricultural or domestic settings, or the development of biologically-based dose response models of established hazards. In that sense, the academic approach is entirely deficient in those attributes that are necessary components of the application of research findings to regulatory science.

Harnly et al. (2005) cited Eskenazi et al. (1999) as a source of concern for adverse consequences of organophosphate exposure. To be complete, Eskenazi et al. (2004) provide more information. They stated, “We failed to demonstrate an adverse relationship between fetal growth and any measure of in utero organophosphate pesticide exposure.” An association was found for a couple of variables and decreased gestational duration, but the conclusion was that these potential pesticide effects appear to have little clinical impact at the population level.

Finally, air concentrations have been shown to translate poorly into systemic exposure. Hore et al. (2005) showed that children in houses treated with chlorpyrifos had no detectable increase in urinary 3,5,6-trichloropyridinol (TCP), whereas median peak ambient air chlorpyrifos increased > 10-fold (median of 14 ng/m³ pretreatment, 196 ng/m³ on day of treatment). If a 10-fold increase in air chlorpyrifos does not cause a detectable increase in urinary TCP, then the 1-fold background air cannot be contributing measurably to the children’s background levels of urinary TCP.

The author was employed by Dow AgroSciences, a registrant of chlorpyrifos, from 1995 through 2001. Since he has been at Montana State University, he has received one unrestricted grant from Dow AgroSciences and one from The Dow Chemical Company to support his environmental risk assessment research. Additionally, he has had one research contract with Dow AgroSciences. None of the funding has involved research or other activities on chlorpyrifos.

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newborns were 65–130 times less able to metabolize diazoxon and chlorpyrifos oxon than their mothers (Furlong et al. 2006). To further support the concern for children indicated by our quantitative risk assessment, we cited toxicologic studies establishing that in addition to cholinesterase inhibition, on which the NOAEL for chlorpyrifos is established, chlorpyrifos and chlorpyrifos oxon have other neurodevelopmental toxicity mechanisms (Huff et al. 1994; Qiao et al. 2002). We also noted that cell death has been induced at the reference dose for drinking water (Greenlee et al. 2005).

Peterson argues that the toxicologic studies we cited (Castorina and Woodruff, 2003; Eskenazi et al. 1999; Faustmann et al. 2000; Greenlee et al. 2005; Huff et al. 1994; Qiao et al. 2002) are an insufficient review of the “literature relevant to risk assessment” and that these studies are not appropriate for use in risk assessment. However, in missing the fact that we conducted a quantitative risk assessment, Peterson is misinterpreting our citations as the only basis for our public health concern. We consider it our public health responsibility to at least qualitatively consider recent toxicologic data in addition to a quantitative risk assessment based on established reference values. Others have argued for a complete restructuring of risk assessment for children, including toxicokinetic modeling and assessment of cellular and molecular outcomes over the entire life span of experimental subjects (Landrigan et al. 2004).

For many reasons we disagree with the suggestion that the epidemiologic fetal growth and gestational duration findings of Eskenazi et al. (2004) may be used to disregard concern for in utero and child organophosphate exposure highlighted by Eskenazi et al. (1999). The associations of reduced gestational duration with dimethyl organophosphate urinary metabolites and cholinesterase inhibition were not clinically significant in the California population studied (recent Mexican immigrants who tend to have very healthy birth outcomes). However, a shortened gestational age of a half-week would represent, for some women, a risk of preterm delivery (Eskenazi et al. 2004). Clearly, this finding and the absence of any adverse association between fetal growth and measures of in utero pesticide exposure need to be confirmed or refuted. To be complete, however, we also cited the association found in a New York City population between low birth weight and length and cord plasma levels of chlorpyrifos and diazinon (n = 314) (Whyatt et al. 2004). Further, effects of organophosphate pesticide exposure on early child neurodevelopment have been found (Young et al. 2005) and are continuing to be evaluated in the California and the New York City cohorts. Finally, public health policy is typically developed to protect against a 1 in 1,000, or lower, risk, and the epidemiologic studies cited here are below the sample size necessary to detect such risks.

Peterson notes that a study of children in 10 homes did not demonstrate an association with child urine metabolite levels of chlorpyrifos and ambient air levels following crack and crevice treatment (Hore et al. 2005). Yet, the authors of that study were careful to note a number of study limitations, including the variability and accuracy of the child urinary metabolite readings. We also note that chlorpyrifos oxon, which also breaks down into the measured urinary metabolite, was not measured in air; air concentrations in four of the study homes were not elevated compared to pretreatment levels; and personal air samples were not collected (Hore et al. 2005). Among mothers in New York City (n = 314) in another study, 48-hr personal air samples collected during pregnancy were associated with cord and maternal blood levels of chlorpyrifos (Whyatt et al. 2004). This is the same study population within which an association with adverse birth outcomes and pesticide cord blood levels has been demonstrated, and the chlorpyrifos air levels are in the same (average, 15 ng/m^3) range, if not lower, as those evaluated in our health risk assessment (Whyatt et al. 2004).

The authors declare they have no competing financial interests
Effects of BPA in Snails: Oehlmann et al. Respond

We welcome critical appraisals that help to provide balance; however, Dietrich et al. gave an unjustified reproach. We feel that Dietrich’s position is severely compromised because he serves as an expert for the bisphenol A (BPA) Industry Group (Brussels, Belgium). We would like to respond to the issues raised by Dietrich et al., as well as to their oversights and inappropriate interpretations of our findings.

The source of test animals was clearly provided in our “Materials and Methods” (Oehlmann et al. 2006). All animals were dissected and sexed at the time of sacrifice. Sex distribution was known for each time-point of the experiment. We supposed a 1:1 sex ratio for dead snails, although historical data (n > 14,000) indicate a slight prevalence of females (1.3:1); therefore, our assumption was conservative. Egg production was corrected for the number of females in the tanks, and snail densities were equal for all groups at each time-point.

Semistatic designs are widely applied in scientific and regulatory ecotoxicology [Organization for Economic Development and Co-operation (OECD) 1998]. The actual exposure concentrations of BPA were measured and clearly communicated in our Tables 1 and 2 (Oehlmann et al. 2006). Because 17α-ethinylestradiol (EE2) is more stable than BPA ( Larsson et al. 1999), exposure to the positive control is also guaranteed in our 24-hr renewal test. Interestingly, Dietrich himself coauthored a semistatic study on snails (Czech et al. 2001) with several shortcomings: they used no analytical verification of exposure concentrations, no replicates, and inconsistent group size.

Analysis of covariance (ANCOVA) analyses of fecundity, development, and other cumulative data are widely used (Boehdiansky and Bollens 2004; Dziminski and Alford 2004; Schärer and Wedekind 1999). In our experiment 2 with replicates (Oehlmann et al. 2006), ANOVA confirmed the
Correspondence

The responsiveness of organisms. Non-Schulte-Oehlmann et al. (2001) without EE2 initial evidence for specific estrogen binding the binding study was performed appropriate-lying responses to BPA in four prosobranch species, including A 342 species, including because the focus of that work was compar-
table with findings of Chou and Dietrich (1999), who also performed their experiments in duplicate. This percentage might be due to homogenization of large amounts of tissue with high protein content but a limited degree of specific cytosolic binding sites. In our study (Oehlmann et al. 2006), we did not intend to deliver a complete binding study in which saturation experiments with Scatchard analysis are needed, but to provide indications for the presence of estrogen receptors by a specific binding of ligands to cytosolic extracts (a widely used practice). Tamoxifen was not disqualified as an antiestrogen because it elicited a binding higher than that of BPA. Furthermore, in vitro ligand affinities have a limited predictive value for biologic potencies in vivo (Kloas et al. 1999). In summary, the binding study was performed appropriately for the desired purpose and provides initial evidence for specific estrogen binding sites with high affinity for BPA.

Data presented in our Figure 1B (Oehlmann et al. 2006) were published in Schulte-Oehlmann et al. (2001) without EE2 because the focus of that work was comparing responses to BPA in four prosobranch species, including Marisa. Because the article was published in German, the distribution was not large enough to bring the issue to a wider audience. In the current article (Oehlmann et al. 2006), EE3 data were included to demonstrate the masking of BPA effects during the spawning season. Because future BPA industry-sponsored studies intend to investigate BPA effects under conditions maximizing reproduction, the problem of masked effects and an associated loss of sensitivity is of vital importance.

Responses in Marisa (ruptured oviducts, increased spawning) are estrogen specific and opposite of androgenic effects (imposex, reduced spawning). This and evidence communicated in our article (Oehlmann et al. 2006) justify the use of EE3 to demonstrate the responsiveness of organisms. Non-monotonic concentration responses have also been reported for estrogen-regulated end points in EE3-exposed fish (Pawlowski et al. 2004), supporting our view that estrogen-specific binding sites in Marisa may represent functional receptors.

Dietrich et al.‘s charges that our “Introduction” and “Discussion” were “imbalanced and indeed alarmist” and that we selectively used literature are unjustified.

We hope that the evidence presented here serves to refute the unjustified claims made by Dietrich et al. We leave it to the readers to make final judgment, but we feel that with the ever-increasing body of evidence showing effects of BPA on reproduction in various animal species, common sense will eventually prevail on this issue.

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ERRATUM

In the article by Colbert et al. [Environ Health Perspect 113:700–707 (2005)], the authors discovered an error in the units of exposure estimates in the tenth paragraph of the “Discussion.” These estimates are expressed in milligrams but should be expressed in micrograms. The corrected sentences are as follows:

It is estimated that children 1–6 years of age are exposed to 0.167 µg Vz/kg body weight/day … Given this chronic exposure estimate, a 2-year-old boy who weighs 13 kg … would consume an average of 2.17 µg Vz/day, whereas a 6-year-old with a body weight of 21 kg would consume an average of 3.51 µg Vz/day.

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