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Comment on "Background Ionizing Radiation and the Risk of Childhood Cancer: A Census-Based Nationwide Cohort Study"

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We read with interest the article by Spycher et al. The authors claim their results suggest an increased risk of cancer among children exposed to external dose rates of background ionizing radiation of ≥ 200 nSv/h, compared with those exposed to < 100 nSv/h. However, all that the data show is a positive correlation rather than a causal result, which the word "risk" implies. Besides, these dose rates correspond to annual exposure levels of approximately 1.8 and 0.9 mSv, respectively. Considering that the average natural background exposure rate in the world is on the order of 2 mSv annually, with regions that range up to as much as 260 mSv (Ghiassi-Nejad et al. 2002), these are very low doses.

Importantly, the background exposure rates were based not on actual measurements at children's homes but on a geographic model. The authors noted they could not "exclude biases due to inaccurate exposure measurement." It comes as no surprise, therefore, that the various hazard ratios are for the most part extremely low, and most of the 95% confidence intervals include the value of unity. Essentially, for children putatively exposed to a background dose rate exceeding 200 nSv/h, only the confidence intervals for all cancers, leukemias, and acute lymphoblastic leukemias exclude unity.

This, taken seriously, would suggest a markedly increased cancer risk for these children, based on those exposure rates, but only if one begins by assuming that these levels of radiation contribute to producing cancers. There are numerous studies that show that such levels, in fact, elicit protective biological responses that lower the risk of cancer (Doss and Little 2014; Luckey 2008). Furthermore, given the very low attributed exposure rates and the imprecision in the actual exposure estimates, it is more likely than not that this increased childhood cancer occurrence is due to causes other than the background radiation exposure.

For example, it is of interest that those children experiencing the highest estimated background dose rates are those who live in rural areas and in neighborhoods of lowest socioeconomic status. The authors state that adjustments were made for these two confounding factors, but since not much detail was provided regarding the adjustments made, the adequacy of the removal of these factors as causative contributions cannot be independently verified. Nevertheless, it is far more likely that these two factors are more important causes of childhood disease than the extremely low background exposures involved.

Moreover, if it were true that exposure rates above 200 nSv/h, low though they be, were to somehow result in such a markedly increased cancer risk for children, the only reasonable governmental policy action would be to evacuate those children living in rural areas and poor neighborhoods, and relocate them to areas with lower radiation exposure in order to save lives. Failure to act in this manner would leave the government liable for allowing its younger citizens to die at an alarming rate. Studies like this cannot be taken seriously without such public health policy implications being likewise taken seriously.

All authors are members or associate members of SARI (Scientists for Accurate Radiation Information, http://radiationeffects.org). SARI, as an organization, has no expenses and thus no funders. SARI's aim is to provide empirical evidence and scientific reasoning to counter the linear no-threshold paradigm followed by radiation-related regulatory agencies around the world.

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Siegel and colleagues object to our use of the word "risk" on the basis that it implies a causal relationship. This is not so. In epidemiology, risk is simply the probability of developing the disease. Comparing risks across exposure strata is a natural way of assessing associations in a cohort study and does not imply causality. Our conclusions regarding causality are, in fact, very cautious.

The authors correctly point out that we are investigating low doses. The comparison they make with worldwide averages is, however, misleading. The worldwide annual dose of 2 mSv represents total background radiation and includes inhaled radon gas and ingested radionuclides. The appropriate comparison is with cosmic and terrestrial gamma radiation, which together contribute an annual average of 0.9 mSv worldwide (UNSCEAR 2000). This figure is on par with our lowest exposure category. Their comments on the use of a geographic model instead of measurements to estimate exposure reiterate limitations that we discuss in the paper.

Siegel and colleagues argue that the point estimates for the highest exposure category are unreasonably high and contradict literature showing protective effects of radiation on cancer. However, they base their argument mainly on ecological studies (Doss and Little 2014; Luckey 2008), which are prone to bias. Our study results are in line with a recent case–control study of 27,447 childhood cancer cases from the United Kingdom, which also observed a risk increase for gamma radiation (Kendall et al. 2013).

The authors suggest that other factors such as socioeconomic status and degree of urbanization are likely to explain our results. However, when we adjust for these factors, our results remain virtually unchanged. Consider the estimated response to cumulative dose, adjusted for sex and birth year (Table 4): For all childhood cancers we estimated an increase in risk per mSv cumulative dose of 2.8% (95% confidence interval [CI]: 0.8%, 4.8%) for the entire cohort and 4.0% (95% CI: 1.7%, 6.4%) for children with stable residence. After adjusting for socioeconomic status—using the Swiss neighbourhood index of socioeconomic position (Panczak et al. 2012), which is based on the education and occupation of household heads, rent, and crowding—and for degree of urbanization (urban, peri-urban, rural), the corresponding estimates were 2.9% (95% CI: 0.9%, 5.0%) and 4.0% (95% CI: 1.7%, 6.3%), respectively. The authors confuse the effects of socioeconomic status on mortality with those on cancer incidence in children. Only the latter could confound our results, but the evidence for their existence is far from conclusive (Adam et al. 2008).

The public health action proposed, i.e., the relocation of children to areas with lower radiation, is nonsensical. Childhood cancer is rare, and we are not dealing with deaths at "alarming rates." In the whole of Switzerland, there are about 200 new cases per year, of whom more than 80% survive (SCCR 2015). Only a small proportion of the population is living in highly exposed areas. The attributable fraction, assuming a causal relationship, is therefore small. Public health action is better targeted toward modifiable environmental factors leading to larger numbers of deaths from several causes, such as exposure to radon, air pollution, and secondhand tobacco smoke.

It seems to us that the "Scientists for Accurate Radiation Information" *a priori* exclude the possibility that low-dose radiation could increase the risk of cancer. They will therefore not accept studies that challenge their foregone conclusion.

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

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