LETTERS TO THE EDITOR

RE: "LEUKEMIA FOLLOWING OCCUPATIONAL EXPOSURE TO 60-HZ ELECTRIC AND MAGNETIC FIELDS AMONG ONTARIO ELECTRIC UTILITY WORKERS" AND "CANCER RISKS ASSOCIATED WITH OCCUPATIONAL EXPOSURE TO MAGNETIC FIELDS AMONG ELECTRIC UTILITY WORKERS IN ONTARIO AND QUEBEC, CANADA, AND FRANCE: 1970–1989"

I read with great interest the recent paper by Miller et al. (1) on the risk of leukemia among Ontario electric utility workers. Their paper appears to be an addendum to the paper published previously by Thériault et al. (2) on cancer risks among electric utility workers in Ontario, Québec, and France. Thériault et al. presented data on the risk of occupational exposure to extremely low frequency magnetic fields and found a higher risk for leukemia mainly in Ontario Hydro workers. Miller et al. present data from the same study but only for Ontario, and they add 5 cases of leukemia to the 45 cases studied by Thériault et al. and consider possible confounders not taken into account in the Thériault et al. paper (2,4-dichlorophenoxyacetic acid and 2,4,5-trichlorophenoxyacetic acid). Results from electric field measurements taken by means of the model 378108 Positron personal exposure meter (Positron Industries, Montréal, Québec, Canada) were also considered by Miller et al. to supplement the data on magnetic fields presented by Thériault et al.

The results of Miller et al. differ from those of the previous paper, and thus it might be important for the authors to reconsider both data sets and discuss how and why the new data contradict or support the previous findings. Because the two papers present the data quite differently, this exercise is difficult for outsiders, and it should therefore be done by the authors of the two papers themselves. For example, Thériault et al. found an odds ratio of 3.14 for total leukemia for Ontario workers exposed to cumulative magnetic fields intensity ≥3.1 μT-years. At this exposure level, Miller et al. found odds ratios of the same order of magnitude (2.04 for cumulative exposure between 3.2 and 7 μT-years and 2.84 for exposure ≥7.1 μT-years), but these odds ratios were substantially reduced after control for confounders (1.67 and 1.56, respectively). Because Thériault et al. found “very little” change in their data after adjustment for nearly the same variables, it is necessary for the authors of this new paper to specify which variables were confounders in their data. Does consideration of pesticide exposure make the difference? This information might be very important for future studies on this subject.

Miller et al. also found that electric fields carry a greater risk than magnetic fields. This is a very important finding, and it corroborates recent data from a study of childhood leukemia in the United Kingdom (3). Because electric field measurements, as recognized by the authors themselves, are less precise than magnetic field measurements, such results must be carefully evaluated, and results on the effect of electric fields exposure for the complete data set seem paramount. The possible interaction between electric and magnetic fields also makes it important to gain further insight into the data published by Thériault et al. Would consideration of electric fields and other confounders have changed the results previously published? It seems logical now to review the entire data set of the Ontario/Québec/France study to clarify these aspects.

REFERENCES

Patrick Levallois
Département de médecine sociale et préventive
Faculté de médecine, Université Laval et Centre de Santé Publique de Québec 2400, D'Estimauville
Beaupre, Québec
Canada G1E 7G9

MILLER ET AL. REPLY

We appreciate the comments by Dr. Levallois (1) in regard to our paper (2). However, we would caution against giving too much attention to the apparent differences between the findings on magnetic fields from the analysis of the tri-utility data set from Ontario and Quebec, Canada, and France of Thériault et al. (3), and the corresponding findings from our more detailed consideration of the expanded Ontario Hydro data set (2).

It is our interpretation that the findings for magnetic fields and leukemia in the two reports are consistent. The differences noted by Dr. Levallois are largely due to different approaches to analysis and exposure assessment of electric and magnetic fields, which we reported in our recent paper (2). In particular, there was little evidence of confounding from any of the potential occupational confounders we considered, including the herbicides 2,4-dichlorophenoxyacetic acid and 2,4,5-trichlorophenoxyacetic acid. Individually, they contributed nonsignificant leukemia risks. Of all confounders, ionizing radiation was the only one that showed a borderline statistically significant association with leukemia.

The tri-utility data set constructed for the Thériault et al. analysis (3) has now been dissolved, in accordance with the original agreement between the sponsors. Thus, it is not possible to make the comparison suggested by Dr. Levallois. However, as we pointed out in our recent paper