Nieto and Coresh (1) are reviewing alternative methods for presenting adjusted survival curves. The proposed new method is a special case of the Ederer II method currently used at cancer registries worldwide. In addition, there are other frequently used methods for adjusting for deaths and censorings that are not mentioned.

It is important to state the context in which any adjusted survival curves are to be used. Age-adjusted survival curves are analogous to directly standardized incidence rates (2). These rates may be expressed as the weighted average of the age-specific survival curve for each study group, where the weights are chosen to be proportional to the age distribution of some external standard population. A term that helps to distinguish this type of adjustment from others and emphasizes that covariates other than age may be adjusted is externally covariate-adjusted survival curves.

Adjustment for deaths and censorings is frequently used in medical statistics as well as in cancer survival analysis (8, 9). To summarize, I think that adjusted conditional survival curves should not be presented without reference to Ederer et al. (5). Furthermore, it should always be stated whether externally, internally, or directly adjusted survival curves are used and how individual survival curves are evaluated.

**REFERENCES**


**THE AUTHORS REPLY**

We thank Dr. Zahl for his letter (1) regarding our paper on methods to obtain adjusted survival curves (2). We agree with Dr. Zahl that some of the procedures we discussed in our review as well as the new method we proposed are analogous to the methods for obtaining relative survival estimates proposed by Ederer et al. (3) and others (4–6). As Dr. Zahl points out, the latter methods have been widely used at cancer registries worldwide. In addition, there are other methods to obtain adjusted survival curves (2). We agree with Dr. Zahl that some of the procedures we discussed in our review as well as the new method we proposed are analogous to the methods for obtaining relative survival estimates proposed by Ederer et al. (3) and others (4–6). As Dr. Zahl points out, the latter methods have been widely used at cancer registries worldwide. In addition, there are other frequently used methods for adjusting for deaths and censorings that are not mentioned.
used during the last couple of decades, particularly by investigators interested in comparing survival of cancer patients with the expected survival based on general population rates, while taking into account competing causes of death. Our review, however, focused on methods that have been proposed and used for the internal comparison of the survival in different groups (e.g., exposed and unexposed) in a given cohort, while adjusting for covariates. To our knowledge, the methods of Ederer et al. and their variants have not been used in this context, although, as Dr. Zahl implies, they could be adopted for that purpose. A major motivation in writing our review (2) was to raise awareness of the existing methods and their limitations. Using a different terminology, Dr. Zahl reiterates some of the distinctions we made. Clearly, these distinctions are important, and a uniform terminology is desirable. We are not convinced, however, that the terminology proposed by Dr. Zahl is sufficiently clear to be widely adopted.

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F. Javier Nieto
Department of Epidemiology
School of Hygiene and Public Health
The Johns Hopkins University
Baltimore, MD 21205

Josef Coresh
Department of Medicine
School of Medicine
The Johns Hopkins University
Baltimore, MD 21205

RE: “EXPOSURE TO 50-HZ ELECTRIC FIELD AND INCIDENCE OF LEUKEMIA, BRAIN TUMORS, AND OTHER CANCERS AMONG FRENCH ELECTRIC UTILITY WORKERS”

I read with great interest the study by Guénel et al. (1) on the association between occupational exposure to 50-Hz electric fields and the risk of leukemia and brain tumors among French utility workers. The authors observed significantly elevated odds ratios associated with cumulative exposure to electric fields in three different latency period assumptions for cancer of the colon (cf. 1, table 6). In their discussion, they state that colon cancer had never been associated with extremely low frequency field exposure. However, two previous studies (2, 3) have reported a significant excess of colon cancer in those exposed to electromagnetic fields.

Tynes et al. (2) linked a cohort of male Norwegian electrical workers to the Cancer Registry and calculated the standardized incidence ratio (SIR) for total cancers and selected sites. The SIR for colon cancer (ICD 7 code 153) was elevated: SIR 1.13 (95 percent confidence interval (CI) 1.01–1.26). Verkasalo et al. (3) in a nationwide cohort study from Finland, investigated the risk of cancer in Finnish adults living close to high-voltage power lines. They calculated exposure using average annual magnetic fields taken from power company records. The risk of colon cancer showed a dose response in women as they observed an Odds Ratio of 2.77 for the highest exposure category (≥20 μT-years). At present, we are not certain whether the apparent increased risk of colon cancer is due to electric or magnetic field exposure.

There is also a possible biologic pathway through which the risk may operate. As to the possible carcinogenic mechanism in colon cancer, it has been suggested that the earlier observation of high stores of iron in the body increasing the risk might be explained by iron having a role in catalyzing oxygen radicals (4). However, 60-Hz magnetic fields have been reported to produce expression of transferrin receptors on human colon cancer cells in vitro (5), i.e., body iron could act as a cocarcinogen with extremely low frequency magnetic fields. Given the above, and with the similarity of results for occupational and nonoccupational exposures to electric and/or magnetic fields in epidemiologic studies that employed different designs, it may be worthwhile including colon cancer as an a priori outcome variable in ongoing as well as future epidemiologic studies on electromagnetic fields and adult cancer.

REFERENCES


P. Badrinath
Department of Community Medicine
Institute of Public Health
University Forvie Site
Cambridge CB2 2SR
United Kingdom