The Centers for Disease Control (CDC) has estimated that the risk of HIV transmission from an infected health care worker to a patient is 2.4 to 24 per million procedures.[1] In fact, except for one dentist whom the CDC has documented to have used substandard infection-control technique,[2] such transmission remains undocumented. Yet a consistent application of Angell's "right to know" would require the disclosure of all such risks in health care delivery — remote, undocumented, or both. Thus, health care workers could be required to inform patients of many individual disabilities or conditions affecting themselves, such as side effects of continuing medications, chronic conditions, sleeplessness, psychiatric conditions, even surgeon-specific postoperative and perioperative mortality rates and surgeon-specific rates of wound infection. All are arguably much more relevant to patients' safety than a health worker's HIV infection.

The law of informed consent has long held that only "material" or "significant" risks need to be disclosed to patients before treatment.[3,4] The materiality or significance of the risk is determined in turn by the standard of a "reasonable patient" or, in some states, by the customary practice of the medical community. Although many and perhaps most patients would probably like to know such information, the medical, legal, and public health communities must bring their professional judgment to bear on the reasonableness of that desire. These communities should be mindful of their obligation to distinguish between how much of that desire can be attributed to overreaction and how much to a rational apprehension of harm. I also question whether, if disclosure is mandated, the information disclosed should concern the individual practitioner's record and practice of infection control, rather than his or her known serologic status with regard to blood-borne pathogens.

If improving the safety and choice afforded to patients is our goal, we should not begin by singling out HIV infection of health workers, from which the aggregate harm to patients is truly remote.

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2. Update: transmission of HIV infection during an invasive dental procedure — Florida. MMWR 1991; 40:21, 25-7.
3. Information that need not be disclosed. In: Rozovsky FA. Consent to treatment: a practical guide. 2nd ed. Boston: Little, Brown, 1990:59-64.
4. The legal requirements for disclosure and consent: history and current status. In: Appelbaum PS, Lidz CW, Meisel A. Informed consent: legal theory and clinical practice. New York: Oxford University Press, 1987:50-4.

Editor's reply: In my editorial I proposed a dual approach to the AIDS epidemic: on the one hand, I advocated stronger measures to protect those with HIV infection from social and economic deprivation; on the other, I recommended the routine screening of certain population groups that are accessible to the health care system. Rubenstein et al. imply that I believe the two elements of this dual approach can be separated; I do not.

Elsewhere in this issue of the *Journal*, Rogers and Osborn put forth a different view.* They are, I believe, too optimistic about voluntary testing and universal precautions (which, of course, contrary to their implications, do not exclude routine screening for certain groups). In addition, I find it puzzling that they and some of the correspondents whose letters appear here seem to object to my proposals because they do not represent perfect solutions to problems. Thus, they object to funding the treatment of HIV infection because we do not fund the care of everyone. They see the fact that screening would yield a small number of false negative results as a reason to remain ignorant about the status of the great majority in whom the test would be accurate. (Somehow, the issue of false negative results does not stop them from calling for more widespread voluntary screening.) As is too often the case in this epidemic, the rights of sexual partners are all but overlooked; Houston suggests that because contact tracing is imperfect, it should not be undertaken, although he does acknowledge that there may be easily identified contacts, such as spouses.

It is particularly disturbing that some doctors seem to focus disproportionately on resisting proposals to test health care providers. To be sure, the risk of HIV transmission from doctors to patients is exceedingly small — much smaller than the risk of transmission from patients to doctors. But there are two reasons for testing doctors that have nothing to do with the magnitude of the risk. First, agreeing to routine screening would give doctors the moral leadership necessary to ask for reciprocity from their patients; they would in effect not be asking anything of them that they were not willing to do themselves. Second, the public overwhelmingly believes that certain groups of health care providers should be screened for HIV infection, and there is no overriding reason not to do so. Rogers and Osborn believe it "draconian" to divert HIV-positive doctors to noninvasive professional pursuits, but the CDC and the American Medical Association think otherwise, and I agree. The further admonition that a screening program would make doctors reluctant to care for patients with AIDS is odd; it suggests that doctors would be more worried about having to retrain than about contracting a lethal disease.

The best approach to containing the spread of a transmissible disease is to find out who is infected and who is at risk and to treat the former and try to protect the latter. The system I proposed is not perfect, but it is better than our current nonsystem. We should not make the perfect the enemy of the good. To borrow the metaphor of Rogers and Osborn, the fact that we can search for a lost wallet only under a street lamp is no reason not to search at all — particularly if there are many wallets and many street lamps.

Marcia Angell, M.D.

*Rogers DE, Osborn JE. Another approach to the AIDS epidemic. N Engl J Med 1991; 325:806-8.

VIDEO DISPLAY TERMINALS AND SPONTANEOUS ABORTIONS

To the Editor: We were surprised by the conclusion of Schnorr and her coworkers (March 14 issue)[1] that exposure to electromagnetic fields emitted by video display terminals (VDTs) was not associated with an increased risk of spontaneous abortion.

We agree that their data show no link between the use of VDTs and miscarriages, but as the study abstract itself clearly states, all operators had "similar" levels of abdominal exposure to extremely-low-frequency fields. If both the subjects who used VDTs and the controls who used light-emitting diodes and neon glow tubes for display were equally exposed to extremely-low-frequency electromagnetic fields, nothing can be deduced from this study about the impact of electromagnetic fields on pregnancy.

This methodologic flaw stems from the fact that the study was not designed to assess the risk of miscarriage due to electromagnetic fields emitted by VDTs. Schnorr and her coworkers did not originally plan to measure such fields, as indicated by their draft protocol,[2] nor were sources of possible confounding exposure investigated in the design or feasibility stages of the study. Electromagnetic fields were measured only in the spring of 1990, more than 1½ years after the last subject was interviewed and more than 3 years after the last pregnancy included in the study.

Measurements of electromagnetic fields included in the published paper and presented in greater detail in a report by Tell,[3] the consultant who took the readings, provide only a rough and unreliable indication of the operators' actual levels of exposure. As Schnorr and her coworkers point out, there was considerable variation in the fields emitted between the two different models of VDTs used by the operators and sold by International Business Machines (IBM) and Computer Controls, Inc. (CCI). What they do not point out, however, is that even among the seemingly identical IBM terminals, Tell found a difference of more than 15-fold in very-low-frequency emissions and a difference of more than 3-fold in electromagnetic field emissions. Tell attributed the variation to

the way two different manufacturers, Motorola and Zenith, assembled the IBM VDTs.[3] It should also be noted that the operators who used IBM VDTs (as well as those who used the CCI VDTs) used different terminals from one shift to the next (Schnorr T: personal communication).

The measurements show that the extremely-low-frequency fields to which the operators who used non-VDT units were exposed did not diminish with distance from the displays. This would suggest, as Tell has noted, that ambient fields in the offices were an important source of exposure to electromagnetic fields. Little more can be deduced from the available data, because measurements of electromagnetic fields were made in only 1 office where light-emitting diodes were used and in only 1 office where near-glow-tube units were used[3] — out of a total of 38 offices using these two types of display units. In addition, the workers' employer could not say how many of the controls used each type of display (Schnorr T: personal communication). The control group may have been exposed to electromagnetic fields from building wiring or other types of electrical office equipment. These sources of exposure for the control subjects may have offset the exposure to electromagnetic fields emitted by the VDTs for the case group.

Another argument for caution in interpreting the role of electromagnetic fields is that, as Schnorr and her coworkers concede, the study was not able to assess the incidence of early, subclinical pregnancy loss. This may be an important deficiency, since such early losses make up a large proportion of miscarriages and since laboratory experiments with pregnant mice and chick eggs indicate that the effects of electromagnetic fields are most pronounced during the early stages of development.[4,5]

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Matthew Connelly
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2. Schnorr T, Thun M, Halperin W. Draft protocol for a reproductive study of female video display terminal operators, Cincinnati: National Institute for Occupational Safety and Health, 1986.
3. Tell R. An investigation of electric and magnetic fields and operator exposure produced by VDTs: NIOSH VDT epidemiology study, final report. Cincinnati: National Institute for Occupational Safety and Health, 1990. (NTIS publication no. PB91-130-500.)
4. Martin AH. Magnetic fields and time dependent effects on development. Bioelectromagnetics 1988; 9:393-6.
5. Frölen H, Svendenstål BM, Paulsson LE. Effects of pulsed magnetic fields on the developing mouse embryo. Bioelectromagnetics (in press).

To the Editor: In their interesting study of the possible reproductive hazards of VDTs, Schnorr et al. accrued two groups of women, exposed and unexposed, who were commendably well matched in terms of most variables, though apparently not with regard to the propensity to become pregnant during the study period. There was a considerable difference in the mean number of pregnancies per woman (0.135 in the exposed group and 0.182 in the unexposed group), amounting to an apparent deficit of 126 pregnancies in the exposed group relative to the pregnancy rate in the unexposed group. This discrepancy arose partly because fewer women in the exposed group ever became pregnant during the study period (11.9 percent vs. 14.3 percent) and partly because the mean numbers of pregnancies for the women who did become pregnant were different between the two groups (1.13 vs. 1.27), although the mean lifetime number of pregnancies was the same (2.6 in each group).

Can the authors explain this discrepancy? It may be partly attributable to the overrepresentation of Hispanics in the unexposed group, although, conversely, racial and ethnic groups other than whites and Hispanics were underrepresented. The results presented could conceal a potentially serious hazard associated with the use of VDTs, if the difference in the rates of pregnancy resulted from excess early, undetected fetal loss in the exposed group.

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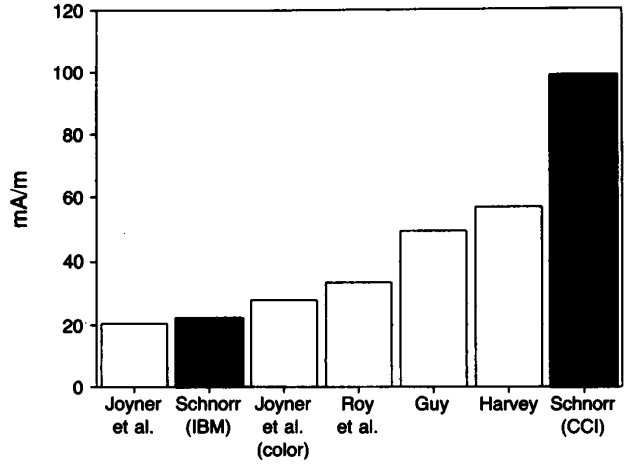
The above letters were referred to the authors of the article in question, who offer the following reply:

To the Editor: The fertility rates cited by Newcombe and Coles assume the same exposure status for all the pregnancies of each woman in the study. However, 397 women classified as VDT users in Table 1 in our study worked in both operator jobs during the study period, contributing 77 VDT-exposed pregnancies and 104 unexposed pregnancies. To obtain compatible numerators and denominators, one should exclude these women and their pregnancies from the calculations. When we did this, there were no significant differences between the exposed and unexposed groups. For example, the mean number of pregnancies was 0.338 (289 among 855 women) in the exposed group and 0.350 (412 among 1178 women) in the unexposed group. These numbers provide only a rough estimate of fertility. The optimal analysis would consider both the length of time at risk of becoming pregnant and changes in job status. We lacked data on covariates, such as contraceptive use, to



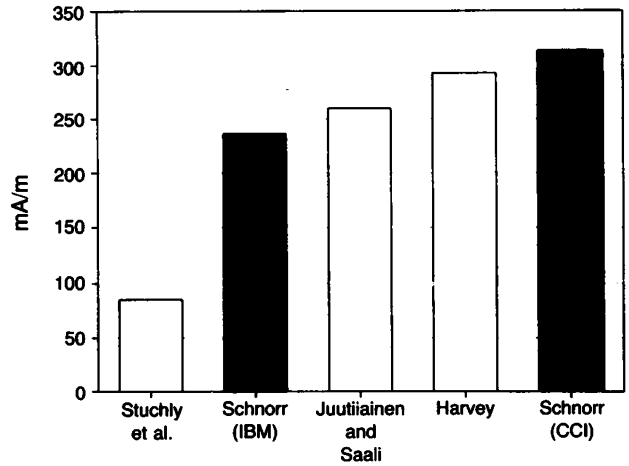


Figure 1. Mean Strengths of Magnetic Fields Emitted by VDT Models.

The mean field strengths in all studies were measured 30 cm from the VDT screen. The measurements made in our study are indicated by solid bars. Those made by other investigators are indicated by open bars; the sources of these measurements are as follows: Joyner et al.,[8] Roy et al.,[7] Guy,[6] Harvey,[5] Stuchly et al.,[3] and Juutilainen and Saali.[4] VLF denotes very low frequency, and ELF extremely low frequency.

perform this survival analysis. Other investigators are conducting studies to address this issue.

Slesin and Connelly address only one type of electromagnetic field produced by VDTs, extremely-low-frequency fields. VDTs also produce very-low-frequency fields, which are characteristic of cathode-ray-tube VDT units and are not generally emitted by other office equipment.[1] Extremely-low-frequency fields are produced by many electrical devices, including VDTs and electrical wiring. In our study, the women in both the VDT-user group and the comparison group were exposed to extremely-low-frequency fields, since both groups worked in buildings with electrical wiring. However, only the VDT users were exposed to very-low-frequency fields.[2] By selecting a comparison group that was very similar to the VDT-user group except for the presence of the VDT at the workstations, our study was able to focus on the risk of spontaneous abortion due to the VDTs (which are typified by the emission of both very-low-frequency and extremely-low-frequency fields). It would not have been useful to select a comparison group without electricity in the workplace (i.e., without exposure to extremely-low-frequency fields), since this group would not have been comparable in many other important factors, such as job duties and socioeconomic status.

As compared with the mean magnetic-field strengths found in other VDT surveys,[3-8] the CCI model in our study had the highest mean very-low-frequency and extremely-low-frequency values measured 30 cm from the screen. The IBM model in our study had mean values at the low end of the ranges (Fig. 1). As expected,[1] field strengths varied both between VDT models and among units of the same model in our study. This variation was considered in our sampling scheme and was described by means of geometric standard deviations. Because field strengths decrease with distance from the source, we also quantified the exposure levels at the operator's normal position (about 50 cm from the screen). Other studies have not documented exposure at the operator's normal position. On the basis of these data, the VDT users in our study had, as a group, typical exposures to electromagnetic fields emitted by VDTs.

This study was not designed to assess the risk of subclinical fetal loss. It should be noted, however, that it was clusters of recognized (not subclinical) miscarriages that initially raised concern about the reproductive effects of VDTs. Our conclusion remains that the use of VDTs was not associated with an increased risk of spontaneous abortion in our study.

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1. Kavet R, Tell RA. VDTs: field levels, epidemiology, and laboratory studies. Health Phys 1991; 61:47-57.
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5. Harvey SM. Characterization of low frequency magnetic fields produced by video display units. Ontario Hydro Research Division report no. 83-504-K. Toronto: Ontario Hydro Research Division, 1983.
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SOLID TUMORS IN CHILDREN

To the Editor: Drs. Crist and Kun gave an excellent overview of the status of pediatric oncology in their review article (Feb. 14 issue).[1] Their statement concerning the role of radiation therapy in patients with neuroblastoma should, however, be modified. The authors stated that children with lymph-node involvement benefit from regional irradiation. They referred to a paper in press, which has since appeared,[2] that deals with children over one year of age in Pediatric Oncology Group Stage C. Hence, the statement does not apply to patients under one year of age. Moreover, Stage C in the Pediatric Oncology Group system includes Stages II and III in the International Neuroblastoma Staging System (INSS).[3] Patients in INSS Stage IIB may have positive lymph nodes, but such nodes are restricted to the ipsilateral side. Since the Pediatric Oncology Group study included only 7 patients with Stage II disease out of a total of 62 patients, it cannot be concluded that radiation therapy is useful for patients with node-positive Stage II disease.

The need for caution is emphasized when one recalls the Children's Cancer Study Group data reported by Matthay et al.[4] That report dealt with children in Children's Cancer Study Group Stage II. The criteria for Stage II in this system and the INSS are similar, except that biopsy proof of lymph-nodes status is not required in the Children's Cancer Study Group system. Matthay et al. found that no form of therapy — surgery, radiation therapy, or chemotherapy — improved survival for patients with Stage II disease, including those over one year of age. Such children had an excellent prognosis (progression-free survival rates in the 90 percent range) regardless of treatment (including radiation therapy). It would be incorrect for readers of the *Journal* to infer that radiation therapy should be used for node-positive children of any age, especially those with Stage II disease. On the contrary, the evidence indicates that such children would undergo irradiation needlessly and without benefit.

It is to be hoped that future clinical reports on neuroblastoma by the Pediatric Oncology Group, the Children's Cancer Study Group, and others will use the international staging criteria so that this kind of confusion can be avoided.

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1. Crist WM, Kun LE. Common solid tumors of childhood. N Engl J Med 1991; 324:461-71.
2. Castleberry RP, Kun LE, Shuster JJ, et al. Radiotherapy improves the outlook for patients older than 1 year with Pediatric Oncology Group stage C neuroblastoma. J Clin Oncol 1991; 9:789-95.
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The above letter was referred to the authors of the article in question, who offer the following reply:

To the Editor: Drs. D'Angio and Matthay correctly describe the uncertainties regarding the indications for radiation therapy in children with neuroblastoma. The Pediatric Oncology Group study cited to support irradiation for children with regional lymph-node involvement was limited to those more than one year old[1]; there are no data supporting radiation therapy for infants less than one year of age. The issue of radiation treatment in Stage II disease, defined initially by Evans et al. and subsequently adopted as part of the INSS, is less clear.[2,3] The Pediatric Oncology Group staging system used to define eligibility for the study we cited relied primarily on the presence of documented regional nodal disease.[1] The effect of nodal involvement has been controversial. Its potential importance is indicated in the INSS, in which Stages II and III are divided into