all right," Rahwan says. "But if there is an increased risk for 10,000 people a year, then it would be unacceptable." Rahwan says a decision to relax the standards will be taken as a signal that sloppiness in handling toxic chemicals will be tolerated. "If anything, we should be tightening the standards," he concludes.

Although not many people are worried about radioactive pollution, quite a few are concerned about the toxic chemicals involved. The most commonly used scintillation liquid, toluene, is flammable, immiscible in water, and suspected of being a carcinogen. Some fear that if the NRC ceases to demand that this chemical be collected and carefully handled, laboratory workers may be inclined to pour it down the drain, even though this is forbidden by other hazardous waste regulations.

Joseph Rosenberg, the president of a waste-handling firm in Massachusetts called Interex, worries about the side effects of the NRC's proposed rule. The new proposal, he thinks, is "cavalier." It is like "clearing your driveway by shoveling the snow onto the next guy's driveway." About 20 to 30 local water systems in Massachusetts have been contaminated by chemicals, he says. Relaxing the NRC's standards will send the wrong signal and make it more difficult to control the kind of chemical pollution found in his state. If the NRC fails to grapple with the mess, some other agency will have to do the job.

Philip Lorio, Columbia's radiation safety officer, welcomes the NRC's proposed change as a "very intelligent move." According to Lorio and several others, the safest and most sensible solution would be to burn low-level radioactive wastes in specially designated incinerators. This approach would disperse the radioactivity and destroy the chemicals at the same time. But Lorio thinks the public is just too "paranoid" about radiation to permit it.

—ELIOT MARSHALL

The Case of the Unmentioned Malignancy

Radiobiologists are under fire from cell researchers for not reporting suspected cell contamination

That even a minuscule amount of radiation can cause cancer, genetic damage, and cell death has long been suspected by one school of biologists. In 1979, Paul Todd and Paul S. Furcinitti of Pennsylvania State University published a report in *Science* (26 October, page 475) that added new evidence to back up this belief. They took human kidney cells, exposed them to gamma radiation, and calculated that cell death occurred even at minute doses. The results were widely reported, as they were a pioneering effort in the low-dose area and they clearly contradicted the competing theory that holds no damage occurs below a certain radiation threshold. "Low radiation doses do cause cancer," read the headline in a *New Scientist* report on the research.

There was just one problem. It turned out that the cells were not normal but malignant—a fact that has subsequently cast a cloud over the whole experiment and raised a chorus of accusations and retorts concerning scientific credibility and candor.

The experimental ambiguity is significant in itself, for the results originally had application to both radiation therapy and the setting of radiation standards. In addition, the incident appears to be just one of a rash around the world in which the same "normal" cells used by the Penn State researchers were inadvertently used to explore the biological effects of radiation. One upshot of this will undoubtedly be a concerted attempt at stricter verification of cell line authenticity and possibly more explicit references in routine scientific reports about these problems.

Also significant is the fact that the Penn State researchers for more than 3 years after being alerted to the possibility that theirs was a malignant cell line took no action to pin this possibility down. Defending themselves today, these researchers say that the change in cells makes no difference in the outcome of the experiment. Even so, they tried in the past to alert the scientific community to the problem, but some journal editors, apparently feeling that such research already had enough interpretational latitude, had the Penn State researchers clean up their manuscripts, removing speculations about the malignant origins of the cells.

The troublesome cells suspected of infiltrating normal cell lines are those of Henrietta Lack, a 31-year-old black woman whose cells were isolated in 1951 when researchers from Johns Hopkins University succeeded in making cells from her uterine cervix grow in laboratory dishes. Though Lack later died of cervical cancer, her cancerous cells achieved immortality and were dubbed HeLa, after the first two letters of her first and last name.

First public disclosure of the probable HeLa origin of the Penn State researchers' cell line came from Walter Nelson-Rees, a cytogeneticist at the University of California Naval Biosciences Laboratory in Oakland who, after reading the Penn State report in *Science*, suspected that the cells were not normal. Publishing his findings in *Science* (8 August 1980, page 719), he reported not only that the Penn State cells, supposedly normal kidney cells known as T-1, were most likely descendants of HeLa cells, but that four other samples of T-1 cells from around the world, including those from the laboratory of J. van der Veen, who originally isolated the cell line, showed the same indications. Nelson-Rees noted that HeLa cells were present when T-1 cells were originally isolated from an 8-year-old Dutch boy in 1957. Apparently the T-1 cells were contaminated by the HeLa strain, and ever since, all T-1 cells have in reality been HeLa descendants.

In fact, Todd and the researchers at Penn State first got wind of this probable state of affairs back in March 1977 when the American Type Culture Collection (ATCC) outside of Washington, D.C., notified them that the T-1 cells they tried to deposit were probably HeLa descendants. This fact was not mentioned in their subsequent published research, nor did they try further to track down the cell lineage—a job left to Nelson-Rees more than 3 years later. "We felt that the ATCC evidence was skimpy," says Todd. "Besides, we were in the business of radiobiology, not the cytogenetics

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business." Todd says the question of descent was moot and that the radiobiological literature is full of examples where possible HeLa descent in relation to other cell lines is mentioned. In their *Science* report there is no such mention, and the cells are called "cultured human kidney (T-1)." There is nothing in the text to indicate to the reader that the cells are malignant; on the other hand, the report nowhere states that the cells are normal.

The nub of the scientific issue is whether use of cancerous cells in place of what were thought to be normal cells makes any difference. Todd maintains that it does not. Others say it does. "I found it devastating that Todd knew they were tumor cells and did not say that in the [*Science*] article," says Elizabeth L. Lloyd, a biophysicist at Argonne National Laboratory. "That fact is so vital, I think, to any understanding of what's going on."

Lloyd says work in her laboratory shows that, at least in some cases, cancerous human cells in culture are less sensitive to radiation than normal cells. For Todd's experiment, she says, this might change the relationship between radiation dose and cell death—a key parameter that Todd was investigating.

Todd, on the other hand, holds that changes in cell type do not alter the outcome. "When you get right down to it," he says, "the radiobiological conclusions are independent of cell type. The linear relationship [between radiation dose and cell death] applies to every cell type that has been investigated. The use of HeLa as T-1 might have affected the slope of the line slightly, but not the general conclusions."

Such speculations are precisely why the results of Todd's experiments are up in the air, according to Robert E. Stevenson, director of the ATCC. The most important part of Todd's report concerned not the relationship between radiation dose and cell death (the so-called linear hypothesis) but whether or not there was a threshhold at which no observable cell death occurred—an independent parameter that may have been changed by the slope of the line. In any event, says Stevenson, the burden of proof is on Todd. It is not enough to assume that there would be no differences or that observed differences would not affect the conclusions. These assertions need to be proved. "You've got to study the results, not speculate," he says. "I just don't buy [Todd's] assertions that irradiating transformed cells has the same result as regular materials."

Though Todd now makes little of the malignant nature of the T-1 cells, 3 years ago, when he first learned of the possible inadvertent substitution of HeLa descendants for T-1, he was apparently worried. He tried to warn editors and readers in his research reports.

Not wanting to muddy the waters of radiobiology, journal editors frowned on full disclosure. Todd in at least two manuscripts tried to point out the suspected problem, but reviewers and editors recommended that the references be cut, one anonymous reviewer saying that "details of cell culture folk lore are out of place in this journal." The first incident occurred in 1977, soon after Todd had received evidence from the ATCC of the probable HeLa origin of the T-1 cell line. An anonymous reviewer for the *International Journal of Radiation: Oncology-Biology-Physics* wrote that "if you [Todd] really want the punchline to reach the therapists, the manuscript needs to be simplified and detail omitted." The reviewer suggested that the paragraph concerning possible HeLa descent be cut, and Todd subsequently struck it.

The second incident occurred in 1979, when *Photochemistry and Photobiology* published a report by Todd from which a full page of speculations about HeLa descent (that Todd in manuscript called "potentially troublesome") had been deleted. In August 1980, responding to accusations by ATCC director Stevenson of "shoddy" scientific practice, Todd in reply referred to these attempts at disclosure, saying that the incidents indicated that "not all attempts by scientists to be honest and thorough are accommodated by journal editorial policies." To Nelson-Rees, who originally made the problem public, these attempts at full disclosure are only half-heartening. "I don't think that they [Todd *et al.*] swallowed the whistle," he says, "But they certainly didn't blow it."

To Stevenson, such incidents could be avoided in the future if journals demanded proof of cell line authenticity. *In Vitro*, he notes, the Journal of the Tissue Culture Association, demands that specific tests used by authors for verification of purported cell line origins be explicitly stated. If the tests have not been done, *In Vitro* demands that this be stated in the materials-and-methods section. Another possibility that Stevenson foresees is that the ATCC will set up an exhibit at the next national meeting of radiobiologists. "I think we'll have some cell cultures on display and hand out some catalogs," he says. "I think we need to do a little missionary work."

—WILLIAM J. BROAD

For the Weapons Labs, a Countdown of Regents?

Like the Roman general Quintus Fabius Maximus, who won few victories against the Carthaginians but finally wore them down, critics of the University of California's management of the Livermore and Los Alamos nuclear weapons laboratories seem to be counting on outlasting the opposition.

The critics lost again in September when the UC board of regents voted by a two to one margin to open talks with the Department of Energy on renewal of the UC contract to manage the labs. And on 21 November the regents voted for creation of new oversight arrangements for the labs on lines favored by UC President David Saxon. The critics of UC management prefer an alternative proposed by Governor Jerry Brown.

The critics, nevertheless, have taken heart from Brown's opposition last year to UC involvement with weapons work and from his power to appoint regents. Some see a successful strategy of keeping the pressure on and waiting for the balance of opinion and power to tip as Brown appoints new regents who are likely to be less sympathetic to the UC link with the laboratories.

Meanwhile, the critics are also taking direct action. They filed a suit charging conflict of interest against seven members of the board of regents. The suit contends that the regents named stand to benefit financially from their various associations with companies doing business with the labs. The suit asks that the seven be enjoined from voting on matters involving the lab and that the UC management contract, which expires in September 1982, be nullified. Several months of legal argumentation are expected before the court decides whether the suit should go to trial.

Saxon's proposal on oversight has antecedants in a debate over management of the labs that dates back at least to the late 1960's. This September, Brown put forward his own proposals for oversight. An effort was made to reconcile the Brown and UC administration approaches, but Brown declined to endorse the compromise version favored by Saxon.

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