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HANFORD RADIATION STUDY III

A COHORT STUDY OF THE CANCER RISKS FROM RADIATION TO WORKERS AT HANFORD (1944 to 1977 deaths)

by the Method of Regression Models in Life-Tables

Word Count: Main Text 2400 words
Appendix 400 words

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PRECIS

This paper reports on results from the study initiated by Dr. T. F. Mancuso into the health risks from low level radiation in workers engaged in plutonium manufacture at Hanford Works, Washington State, U.S.A. Previous reports by the same authors on the same study, notably one in Health Physics, have aroused much controversy because the reported risk per unit radiation dose for cancers of radiosensitive tissues was much greater than the risk generally accepted on the basis of other studies and widely used in setting safety levels for exposure to low level radiation.

This report therefore attempts to answer criticisms of previous reports by an in-death study. This method, of regression models in life tables, isolates the effect of radiation after statistically controlling for a wide range of possible interfering factors. Like the lung cancer risk for uranium miners the dose-response relationship showed a significant downward curvature at about 10 rem. Therefore, there may be better agreement with other studies, conducted at higher doses, than is widely assumed. The findings with regard to cancer latency, of about 25 years, and to the effect of exposure age, increasing age increases the risk, are in general agreement with other studies. An unexplained finding is a significantly higher dose for all workers than for workers who developed cancers in tissues which are supposed to have low sensitivity to cancer induction by radiation.

INTRODUCTION

In 1977 a report was published[1] of a preliminary analysis of cancer risks from radiation to workers at the Hanford works, Richland, Washington. This report indicated a risk for bone marrow cancers among reticulo-endothelial system (RES) neoplasms, and for cancers of pancreas and, to a lesser extent, lung among solid tumours. These risks showed a definite relation to radiation doses of individual workers.

This report aroused controversy because the estimated increase in risk (per unit dose) at relatively low dose levels (less than 30 rads) was approximately 10 to 20 times greater than would have been expected by extrapolating downwards from somewhat higher doses analysed in previous studies, notably the Japanese atomic bomb survivors (ABCC data)[2]. Therefore, two independent analyses of essentially the same data by different scientists using different methods were made in order to see whether our findings could be confirmed[3,4]. Both studies essentially confirmed the findings in relation to bone marrow and pancreatic cancers but drew different conclusions.

Meanwhile we continued analysing the data[5] and showed that an increase in risk was still observable after simultaneous control for the following factors: sex, age at death, year of death, years worked and level of monitoring for internal exposure to radioactivity (see ~~below~~ footnote to Table 1). This paper introduced the important concept of concentrating on cancers in tissues which are known (by others) to be sensitive to cancer induction by radiation. In epidemiological studies it is often necessary to subdivide cancers because a particular agent may be inducing some cancers more than others. If this subdivision is done without previous knowledge of tissue sensitivity it will often be necessary to carry the subdivision so far that the subgroups are too small for an adequate statistical test. In the field of cancer induction by radiation this difficulty no longer exists because a wide body of previous experience has shown which tissues are most sensitive[6,7].

Previous reports by us[1,5] and the NCI report[3] used the methodology of proportionate mortality analysis to relate the proportion of cancers to the cumulative radiation doses. The Battelle[4] report used the Standardized Mortality Ratio method (SMR) and thus identified a substantial "Healthy Worker Effect" (or reduced risk of dying) which was presumably due to pre-employment health checks raising the standard of general fitness. According to this study the age and sex standardized death rates for Hanford workers were 75% of national rates for all causes and 89% for cancers. The question arises, how much of this difference is due to inefficient rejection of cancer-prone workers by the pre-employment health checks and how much to radiation? Clearly what is needed is a method of analysis in which nothing is assumed about cancer mortality of Hanford employees in the absence of radiation.

NATURE OF THE DATA

The variables recorded and the method of data collection have been described elsewhere[8] and only a few relevant facts are noted here. The present analysis includes employees up to 1975 who wore film badges (and deaths up to 1977) and the main epidemiological facts are summarised in Table 1.

The prime variable is the vector of annual dose of external (or penetrating) radiation as measured by the film badge. Formally these doses are measured in rems to the nearest centi-rem, not rads but this refinement is an illusion since prior to 1960 there was only one type of film in the badge and thus it is impossible to separate the effects of gamma rays, neutrons and x-rays, which have different quality factors. Only cohorts exposed prior to 1960 are yet old enough to have substantial numbers of deaths and this is a major limitation to possible conclusions from any analysis.

Files describing basic epidemiological facts about the population, death certificates and various kinds of radiation exposure are in a good state of quality control and very suitable for analysis. However, the file describing work histories is so poor that the Battelle scientists had to recode all the occupations before using them in their analysis[9]. We have adopted a different approach and decided to kill two birds with one stone by using the level of monitoring for internal exposure as an indication of the occupational risk. In any case this level is strongly correlated with the total external dose (as may be seen in Table 2) and therefore ought to be included in any analysis.

STATISTICAL METHODOLOGY

As already mentioned an ideal methodology ought to assume nothing about death rates in the absence of radiation. It ought also to be able to statistically control for any combination of relevant epidemiological variables, as a Mantel-Haenszel analysis can, and be able to include data on both live and dead workers. Ideally it ought also be able to estimate parameters of simple dose effect models (e.g. latent period, doubling dose, linearity of dose-response etc.) as well as testing the null hypothesis of no radiation effect.

A methodology satisfying these criteria was developed in the course of correspondence with interested scientists and an attempt was made to publish in Nature. However, a referee pointed out that the method of Cox[10] on the analysis of Regression Models in Life-Tables (originally supposed to be of use only in clinical trials) had simply been rediscovered. Therefore, the mathematical explanation, in the Appendix, is based on the paper by Cox.

The method divides into two parts: first, a relatively simple calculation to test the null hypothesis of no radiation effects, and second a more complex calculation, based on a transformation of the dose to estimate parameters of a specific dose-effect model. In both calculations the data are first divided into a large number of subgroups by levels of controlling variables. In each subgroup a life-table is constructed, giving for each year of follow-up, the total number at risk, the number of cancers dying in that year, and the mean doses (transformed doses in the second calculation) of these two

categories, cumulated to the year of follow-up or death. One then obtains summary variables for each subgroup by certain summations over years of follow-up and finally a grand summary by summation over all sub-groups. The result is, in the first case, a t-statistic with an approximately normal distribution if the null hypothesis is true and, in the second case, a log-likelihood which measures the goodness of fit of the specific dose-effect model according to which the dose transformation was calculated. By varying the parameters of the dose- effect model one can then calculate maximum likelihood estimates in the usual way.

RESULTS

Validation of the Controlling Factors

In table 1 are shown the levels of the controlling factors used, and in table 3, the definition of cancers of radiosensitive tissues. This definition is the same as the one in a previous paper[5] except that on the advice of experts we have included all RES neoplasms, all digestive cancers and breast cancer[7].

Before these definitions can be used in the analysis proper, it is necessary to show that the range of controlling factors is adequate. The reason for this necessity may be seen by considering the paper by Sanders[11]. He, in effect, used the same method, but without the mathematical basis and with fewer controlling factors.

He came to the conclusion that radiation exposure, if it did anything, increased longevity, because survivors had higher doses than non-survivors. In fact, using as controlling factors, only sex, year of hire and age at hire, our analysis finds a grand summary t-value for comparing all deaths with survivors of -4.6395 (table 4), which is highly significant and indicative of increasing longevity. But the methods employed in this paper can go further, and do what Sanders did not, namely estimate the magnitude of this effect by fitting a model. Practically any model will show that doses of less than 5 rads seem sufficient to reduce the death rate from all causes by more than half, or equivalently to extend longevity by 10 years! Inasmuch as a not insubstantial number of workers received over 30 rads they should have longevity extended by 60 years and live to be more than centenarians. This conclusion is contrary to the facts, but merely by adding exposure period and internal radiation monitoring to the controlling factors, the grand summary t-value for all deaths may be reduced to -0.4847 (table 4) which is non-significant.

Tests of the Null Hypothesis for Cancers

Having shown that the range of controlling factors is adequate, the appropriate tests now are those of the null hypothesis (of no radiation effect) using the definitions of Tables 1 and 3. Table 5 shows one of the life-tables that are intermediate in this calculation. The grand summary t-values obtained are as follows:- the one for cancers of radio-sensitive tissues is significantly positive

at 2.4701, while the one for other cancers is significantly negative at -2.1957. Why other cancers should show a negative effect is still a mystery: it may be that although the controlling factors are adequate for all causes of death, more control is needed for these cancers. Whatever the reason, the fact that the two different kinds of cancers give opposite results only strengthens the case that the result for cancers of radiosensitive tissues is genuine, since the distinction was made a priori and the test is of radiation effects per se.

Model Fitting: I

Having shown that cancers of radiosensitive tissues gave a significant positive result in the test of the null hypothesis, an attempt was made to fit a simple model (table 6). This first model only allows for variation of the assumed doubling dose and a parameter measuring non-linearity of dose-response. Equal weights were given to doses of radiation at whatever age they were received or at whatever interval before death, so no allowance was made for cancer latency. The log-likelihoods (relative to no radiation effect) for various combinations of the parameters are given in Table 6. These are plotted as log-likelihood curves in Figure 1, and sample dose-response curves equivalent to typical combinations of parameters are plotted in Figure 2. Both theoretical and practical considerations show that in plotting log-likelihood curves the parameter D (doubling dose) should be measured on an inverse scale for best possible interpolation on the curves.

Inspection of the curves in Figure 1 shows that for all values of D, the log-likelihood is higher for E=0.5 (corresponding to a half-power law for the dose-response) than for any other value of E, in particular E=1.0 (corresponding to a linear dose-response). This is interesting because a similar dose-response was found in a recent study of uranium miners.[12] Moreover, if the very differing estimates of doubling dose (assuming linearity) from ABCC and Hanford data were to be reconcilable, one would expect the true dose-response relationship to have such a downward curvature. By inspection of Figure 1 the maximum likelihood estimate of doubling dose is approximately 30 rads for the linear model and 15 rads for the half-power model.

Model Fitting: II

Following these encouraging results with a simple model a more complicated model (allowing for cancer latency and variation in sensitivity with age at exposure) was fitted (Table 7).

The results for this model are as follows:

(i) Non-linearity of dose-response (E):- a maximum likelihood estimate for E of 0.5 (half power dose-response law) with E = 1.0 rejected at the 1% level;

(ii) Doubling dose (D):- a maximum likelihood estimate for D of 15 rads with a 95% confidence interval of 2 to 150 rads;

(iii) Interval between cancer induction and death (L):- a maximum likelihood estimate of L = 25 years;

(iv) Effect of age on sensitivity to cancer-induction by radiation (S):- a maximum likelihood estimate of the amount by which age at exposure must increase in order to increase sensitivity by e (i.e. the base of natural logarithms) is given by S = 8 years (with equal sensitivity at all exposure ages, or $S = \infty$, rejected at the 1% level.)

CONCLUSIONS

Previous to the Hanford Study, the main body of data on the carcinogenic effects of penetrating radiation in man was the ABCC study[2] and the study of ankylosing spondylitics[13]. Both these studies broadly agree that the dose-response effect above 100 rem shows no evidence of curvilinearity within experimental error and that the doubling dose for radiosensitive cancers (see table 3) is in the region of 200 rem. The two sets of data on latent periods broadly agree with one and other (and with us), showing an effect continuing and in some cases still increasing after 20 years. Less has been written on the effects of age at exposure, but what has been published tends to show that the risk increases with age, though the measured effect is much less than our estimate.

It is also interesting to compare the estimates of this paper with those from the study of lung cancer in uranium miners[12] since in both studies an approximately square root relationship of effect to estimated exposure was found. However, it is extremely difficult to compare other parameters of the dose response because the uranium miners' exposure involved alpha particles from radon daughters which have very different LET and RBE from the penetrating gamma radiation measured in this study.

Thus the main area of disagreement between our second analysis of Hanford data[5] (which gave a doubling dose of about 15 rad assuming linearity of dose-response) and other human data on the effects of external or penetrating radiation lies in the dose-response effect and, specifically, the doubling dose which implies an effect about 15 to 20 times greater than earlier estimates.

If, however, the dose-response relationship estimated in this paper - which implies major downward curvature in the region of 10 rads - is extrapolated upwards to the dose levels covered in earlier studies (i.e. over 100 rems) then it predicts an effect two to three times lower than linear extrapolation. The effect of this is to halve the difference between the two estimates. For the reasons already given the Hanford Study cannot separate the greater radiobiological effect of neutrons from the lesser effects of gamma radiation. Therefore, although no precise figure can be given for the neutron effect, one should probably reduce the difference still further and thus be left with an unexplained component of the difference which is only two or three times higher than the earlier estimates.

This difference is ~~sufficiently~~ small enough to be accounted for by increased liability of pre-cancer in general and pre-leukaemia in particular to latent period deaths. Heightened sensitivity to infections during the terminal phase of cancer latency has recently been confirmed in children[14] and is probably a feature of adult cancers also. Therefore, changed reactions to other diseases during the pre-clinical phase of adult cancers could make all the difference since there is a strong "Healthy Worker Effect" at Hanford (see above) whereas the ABCC population were exposed the aftermath of a catastrophe and the patients with ankylosing spondylitis were all suffering from a disease which has strong associations with respiratory infections.

Thus putting all the data together can give a reasonably consistent explanation of observed differences and resemblances between several surveys. But one discrepancy remains to be

accounted for. That is, the prediction that background radiation, amounting to about one-tenth of a rem per year, would (by our estimates of risk) account for more cancers than actually exist. This apparent reductio ad absurdum can be taken account of by three factors. Firstly, progressive increase in sensitivity to cancer-induction by radiation with advancing age means that most of any one persons life-time exposure to background radiation is occurring at relatively insensitive ages. Secondly, long intervals between cancer-induction and death mean that any effects of background radiation will only find expression among individuals who live to an advanced age. Thirdly, the assumption that each cancer death has only one cause is certainly an over-simplification. The method of calculation employed in this paper is such that if, for example, radiation worked jointly with other chemicals to produce lung cancer, then radiation would have contributed to the risk even in the presence of a sufficient cause, namely, excessive smoking. In fact, smoking was not measured in Hanford data but for other industrial chemicals there are records which we hope will be incorporated in later analyses.

Since lung cancers account for a high proportion of radiosensitive cancers a further word should perhaps be said about the possibility of smoking being an interfering factor. As mentioned above, there is no record of the smoking histories of Hanford employees. It is hardly surprising that this item was not included in the workers' medical records when the plant was first set up in 1943, since on-site smoking was strictly prohibited. By 1964, when an epidemiologic study of this population was first promolgated, it was too late to obtain off-site smoking habits from workers who had left

the industry. But although we are not in a position to observe any joint effects of radiation and smoking it is still possible that off-site smoking was correlated with the radiation exposures. This remote possibility has been tested in a preliminary fashion by measuring the association between radiation exposures and deaths from chronic respiratory diseases other than cancer (which should include the majority of non-cancer deaths with smoking associations). This test showed no statistically significant evidence for the postulated association.

Finally, although the present paper has shown the importance of controlling for internal monitoring levels when testing for external radiation effects, it also shows that extensive monitoring of Hanford workers only identified 225 men with definite evidence of internal radiation (see footnote to table 1). This sample is clearly too small for measuring any health effects of internal radiation. However, an earlier analysis found that apparent effects from external contamination (revealed by monitoring for internal radiation) were much less after controlling for external radiation than in a crude analysis[5]. Therefore, we can safely assume that, compared with external radiation, any cancer effects of internal radiation were very small.

APPENDIX

Regression Models In Life-Tables

A life-table contains information on individuals exposed to various treatments and followed-up for several years. A characteristic feature is that the final fate of some individuals is not known; that is, their survival time is censored and all that is known is that they were alive at the end of follow-up. A crucial assumption is that this censoring time is statistically independent of the final fate whatever it may be. The question at issue is whether the survival curves differ between treatments. In the seminal paper by Cox[10] only one kind of ultimate fate was considered; in other words, if an individual was not alive at the end of follow-up any cause of death was considered of interest. The present problem differs in that only cancers are supposed *a priori* to be susceptible to radiation induction, so two kinds of ultimate fate, cancer and non-cancer, must be considered. The probability of non-cancer is assumed independent of any radiation, and if the plausible assumption is made that the probability of censoring is also independent of radiation (though it will obviously depend on other treatment factors such as work cohort), then the censored and non-cancers can be considered together, which greatly simplifies the statistical analysis.

Because the data give the radiation doses in yearly exposures and not more finely divided it is convenient to work in discrete time units of one year. The basic method is to divide the data into a large number of treatment sub-groups (480 in the present paper) by the cross-classification of non-radiation controlling factors. The survival curve of cancers in each sub-group in the absence of radiation is considered arbitrary and estimated by maximum likelihood. The survival curve in the presence of radiation is assumed related to that in its absence by a simple regression model whose parameters can then be estimated by maximum likelihood.

Derivation Of Likelihood Formula

Let the data be divided into G sub-groups indexed by g. Let the follow-up years be indexed by i and j. Let there be K individuals indexed by k. Let individual k be in sub-group G_k and be followed up to year I_k. Let a_k be one if individual k dies of cancer and zero otherwise. Let b_k be one if individual k dies of non-cancer or is censored and zero otherwise. Let $\lambda_A(i,g)$ be the probability of dying from cancer in sub-group g and follow-up year i. Let $\lambda_B(i,g)$ be the corresponding probability of dying from non-cancer or of being censored. Then $[1-\lambda_A(i,g)-\lambda_B(i,g)]$ is the probability of surviving year i in sub-group g. Let X_{ki} be the radiation dose of individual k in year i. Let $\underline{x}_k$ be a vector of length I_k containing these doses. Let the model of radiation effects be that the relative risk of cancer for individual k in year i is increased by the factor $(1+E(\underline{x}_k,i))$ where E is a simple function specifying the model. For example a very simple model has $E(\underline{x}_k,i) = (\sum_{j=1}^{i} X_{kj}/D)$ with equally weighted doses and a constant doubling dose D. Then the overall likelihood is given by

$$\prod_{k=1}^{K}\left\{\prod_{i=1}^{I_k}[1-\lambda_A(i,G_k)(1+E(\underline{x}_k,i))-\lambda_B(i,G_k)][\lambda_A(I_k,G_k)(1+E(\underline{x}_k,I_k))]^{a_k}[\lambda_B(I_k,G_k)]^{b_k}\right\}$$

Let R_{ig} be the survivors to the beginning of year i in sub-group g Let A_{ig} be the cancers dying in year i in sub-group g and B_{ig} be the corresponding number of non-cancers and censored. So the survivors to the next year are given equivalently by $R_{(i+1)g}$ or $(R_{ig}-A_{ig}-B_{ig})$ Then, using the notation $\sum_{k\in R_{ig}}$ to mean summation over the R_{ig} individuals surviving to year i in sub-group g and a similar notation for summation over the A_{ig} cancers dying in that year, the overall log-likelihood is given by

$$\sum_g\left\{\sum_{k\in R_{ig}}\ln[1-\lambda_A(i,g)(1+E(\underline{x}_k,i))-\lambda_B(i,g)]+A_{ig}\ln[\lambda_A(i,g)]+\sum_{k\in A_{ig}}\ln[1+E(\underline{x}_k,i)]\right.$$
$$\left.+B_{ig}\ln[\lambda_B(i,g)]\right\}$$

Optimum Test Of The Null Hypothesis

Since by year i the doses for years less than i and consequently $E(\underline{x}_k, i)$ and also R_{ig} are all fixed, the only term in the log-likelihood that actually depends on any connection between the doses and the number of cancers is $\sum_{ig}\{\sum_{k\in A_{ig}} \ln[1+E(\underline{x}_k, i)]\}$ and consequently by sufficiency arguments the difference between two such terms is the optimum statistic for testing which of two fully specified models, corresponding to two forms for E, is the better fit. If the null hypothesis of no radiation effect is true the function E and the term it specifies are both identically zero, and so the term corresponding to the model of some effect is the optimum test of that model compared to the null hypothesis. For the very simple model with equal weights and a constant doubling dose the test statistic becomes $\sum_{ig}\{\sum_{k\in A_{ig}} \ln[1+(\sum_{j=1}^{i} x_{kj}/\ldots)]\}$ If the doubling dose under test is large and fixed, then by expanding the logarithm and neglecting a constant of proportionality, the effective statistic becomes $\sum_{ig}\{\sum_{k\in A_{ig}} (\sum_{j=1}^{i} X_{kj})\}$ or the total dose of the cancers. Its distribution under the null hypothesis of no radiation effect may be found from the following considerations. If the null hypothesis is true the A_{ig} cancers dying in year i in sub-group g will be a random sample of the R_{ig} survivors who started the year. Therfore the mean under the null hypothesis of the test statistic will be $\sum_{ig}\{(A_{ig}/R_{ig})\sum_{k\in R_{ig}} (\sum_{j=1}^{i} X_{kj})\}$ and its variance can be found by finite population sampling formulae. Hence a t-statistic can be constructed from the observed value and its mean and variance under the null hypothesis. If the number of cancers is reasonably large this t-statistic will be approximately normally distributed under the null hypothesis.

Fitting A General Model Of The Radiation Effect

If one is attempting to fit a general model with adjustable parameters in the function E, because the null hypothesis has been rejected by the previously derived test, one cannot use sufficiency arguments that work for fully specified models since the function E appears in more than one place in the expression for the log-likelihood. So an approach via general maximum likelihood theory appears suitable. Because of the number of parameters involved it would be better to estimate the parameters in λ_A and λ_B by maximum likelihood for a fixed function E, substitute these estimates in the likelihood and then estimate the parameters in E. This approach is made more simple if the likelihood function is first suitably approximated

Let $E_{ig} = \sum_{k \in R_{ig}} E(\underline{x}_k, i)/R_{ig}$ be the estimated mean excess relative risk in year i in sub-group g. Then if $\lambda_A(i,g)E_{ig}$, the estimated proportion of radiogenic cancers in the R_{ig} individuals who started year i in sub-group g, is small compared with one, the term in the expression for the log-likelihood involving summation over R_{ig} can be approximated by $\sum_{ig}\{R_{ig}\ln[1-\lambda_A(i,g)(1+E_{ig})-\lambda_B(i,g)]\}$ With this approximation the maximum likelihood estimate for $\lambda_A(i,g)$ is $A_{ig}/[(R_{ig}+A_{ig}+B_{ig})(1+E_{ig})]$ and the corresponding value for $\lambda_B(i,g)$ is $B_{ig}/(R_{ig}+A_{ig}+B_{ig})$. The justification for using maximum likelihood estimates at all if R_{ig} is small, when the estimates will be very erratic, is given in terms of the power it gives against the most general forms for λ_A and λ_B in the paper by Cox[10]. Substituting these estimates into the expression for the log-likelihood, simplifying and neglecting constant terms, the log-likelihood becomes $L = \sum_{ig}\{\sum_{k \in A_{ig}} \ln[1+E(\underline{x}_k, i)] - A_{ig}\ln[1+E_{ig}]\}$ or, in other words, the sum over the cancers of the difference between the logarithms of the actual estimate of the relative risk and the mean estmate of matching individuals.

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ACKNOWLEDGEMENTS

This analysis of Hanford data was financed by the Environmental Policy Institute, 317 Pennsylvania Avenue, S.E. Washington DC 20003, U.S.A. Earlier analyses and the cost of data collection were supported by the Division of Biology and Medicine, and Division of Occupational Safety of the former Atomic Energy Commission, A.E.C. Contract No. AT(30-1)-3394 and No. CH-AT(11-1)-3428, and ERDA Contract No. E(11-1)-3428.