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PAPER FROM THE LANCET - UCHIDA ET AL. MATERNAL RADIATION AND CHROMOSOME ABERRATIONS

I've discussed the problems encountered with Drs. Bender and Kimball. They have successfully countered the few ideas I've expressed but perhaps they should be written for the benefit or lack thereof for future interested personnel. Most criticisms (but not all) could be in the area called "picky picky" or "stretching the point."

(1) Women with children in both the control and abdominally radiated (AR) groups are by definition older (perhaps much older) when they become members of the AR group (at least 1/3 of the sample). Moreover, it's very likely that mothers with multiple children would be in the AR group. Dr. Bender indicated that no one has shown an association (i.e. the third or more child of an older AR mother matched for age only to a control mother's earlier child) between child number and mongolisms. However, it is strange (Table 9) that all of the observed abnormal children are latter births (up to 13th or 14th children). The effect could be maternal age only as indicated in the same table.

(2) The additional 106 mothers (I'm not sure how many total control children this represents) obtained from the years 1960-1964 were necessary to find older control mothers to match with AR mothers of the 1956-1959 period.

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(a) A lot can happen in four years

(b) What about demographic, radiographic, and hospital-physician changes in Winnipeg during this time.

(c) It seems dangerous to select only control women who were "readily available" from the 1960-1964 hospital records. One could speculate this meant closer to the hospital, the city, and perhaps much better normal care. This cannot be passed off lightly because these 105 mothers are the older control sample where the apparent important differences are discussed.

(3) The 21 physicians who refused to participate in the study should not be passed off as "some nonresponses always happen." Rather ask why didn't they want to participate. If there are good uncorrelated reasons fine, but if ten were city obstetricians, internal medicine physicians, etc. it could be important. After all a doctor with three abnormal kids (genetic or not) in his practice (and for whatever his reasons) might not consent to any kind of survey.

(4) The statement "Whenever possible, peripheral blood samples were aspirated from abnormal children and their relatives" (page 1046, first paragraph) leaves much to the imagination.

(a) Who said or thought they were abnormal and at what point (i.e. birth, age 10, etc.)

(b) What was or is the definition of abnormal.

(c) Why were only 11 of 44 tested AR patients found abnormal based on a suspicion of abnormality and only 1 of 39 controls presumably based on the same kind of suspicions?

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(5) It seems to me that the appropriate percentage for (paragraph 1, results page 1046) classifying mothers should be based on the 1956-1959 sample of 755 mothers only (forget the 105 controls from 1960-1964). The adjusted percentage values 30% in both groups, 40% post (AR) only, and 30% control only indicates 10% more (75 mothers) in one group. The possible ramifications of this relative to the author's percentages could be meaningful.

(6) If the total abnormalities (calculable from Table III) vs time since AR are evaluated with an exact binomial test (not all independently) two or three are significantly different from one another. This (meaninglessly) makes questionable the author's statement of no effect. However, I'm sure they meant this in a prediction sense (i.e. an x,y plot). The point I'm trying to make is that differences comparable to their central idea of greater AR induced chromosomal damage are evident in Table III. They choose to ignore these differences (because they are not interesting?) but make a strong point of analogous differences in Table V.

(7) I can't really see much point in either their mention of:

(a) An implied sex difference in the two total groups (95% CI for $N = 1000$ $P = 1/2$ is 46.85 - 53.15).

(b) Their use of statistical tests (which one) to judge stillbirth difference. They could make a good case for AR because stillbirths are significantly higher in controls rather than the overall comparisons they chose to stress.

(8) The fact that control women had substantially less x-ray indicates that they may have substantially less quality of medical care. Even if this result is due solely to the definition of the control sample (no x-ray prior to delivery of the child) one could argue that these women were just from a healthier portion of the population (i.e. selection bias).

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