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Editorial Office
Hospital of the University of Pennsylvania
3400 Spruce Street
Philadelphia, Pa. 19104

Telephone: 215-662-2017
215-688-7504

Manuscript No.: 90004.

Author: Maghbouleh, et al.

Title: A Critique of Studies on Mormon Cancer Mortality

From:

No. of pages 22 Illustrations

August 18, 1981

Dear Doctor Enstrom — We understand the authors sent you a copy as well and wonder if you would care to comment for us.

Would you please give me your opinion regarding the suitability of this manuscript for publication in CANCER? Write your comments in the space below and on the reverse side about its acceptability, and suggest any corrections, revisions or advice to the authors you think would be helpful. Please return this sheet with the manuscript to the Editorial Office.

Suggested Recommendations:

- ____ Accept as submitted
- ____ Accept with minor revisions
- ____ Accept with major revisions
- ____ Reject but recommend resubmission following revision or inclusion of more data
- x Reject
- ____ Recommend that the paper be published as a lead article
- ____ Recommend that the paper be published as soon as possible
- ____ Submit it to another (more appropriate) journal

Comments: Attached I have outlined why I feel this paper is not worthy of publication. However, if the other reviewers feel differently and you decide to publish it, I would appreciate being given the opportunity to make a full rebuttal.

(over)

COMMENTS TO THE AUTHORS:

This manuscript is totally without merit as a scientific research article or substantive critique. It contains no original research on Mormons and merely expresses the authors' opinions and speculations. The authors have made a very weak case in each of their three areas of concern: the overall impact of Mormon doctrine on health behavior, specific flaws in research on Mormon cancer mortality, and Mormons as a model for cancer prevention.

pp. 5-10: Are Mormons More Successful in Inducing Healthy Behavior?"

It is a highly implausible hypothesis that Mormonism induces alcoholism. Surveys in both Utah and California indicate overall rates of drinking among Mormons are about one half those of the general population (see Tables 1 and 7 of Ref. 1). Table 7 of Ref. 1 and Table 4 of Ref. 4 indicate that active Mormons abstain completely from alcohol and tobacco, whereas inactive Mormons use about as much alcohol and tobacco as the general population. These patterns have also been found in surveys by Lyon et al., CDC, and others. I am not aware of any published survey indicating excessive alcoholism among Mormons or former Mormons. If Maghbouleh and Greenland have such data then they should present it. Although some Mormons are alcoholics, the overall proportion appears to be small based on the available drinking surveys and the fact that the cirrhosis of the liver death rate, which is highly correlated with alcohol consumption, is relatively low in the 70% Mormon state of Utah (see, for instance, AJPH 52:1474, 1962).

My examination indicates that References 6-37 are compatible with Refs. 1-5 and do not contain any specific evidence of large scale alcoholism among persons exposed to Mormonism. For example, Skolnick (Ref. 15) states "The overriding conclusion is that religious affiliation seems to influence drinking behavior more than ideas arising out of any comparable variable...the abstinence recommendation is fulfilled mainly by committed churchgoers... those who are least involved in religious practice incline to ignore abstinence...." In other words, the overall rates of drinking are lowest among the abstinence oriented religions, and abstinence is practiced by the active members of these religions, consistent with what Enstrom has found. Maghbouleh and Greenland have misrepresented the complete findings of Skolnick and Bacon and Straus (Ref. 10) by discussing only drinkers who disregard church doctrine. I can find no "scandalous incidents of alcohol-related problems" among Mormon High Priests in Sorensen's book on alcoholic priests (Ref. 36). Furthermore, Enstrom has never addressed the issue of the relative success or failure of Mormon indoctrination in inducing healthy behavior. This part of the paper does not constitute a critique of his Mormon cancer mortality studies.

pp. 11-14: "Do the Published Mormon's Cancer Mortality Rates Reflect Accurately the Experience of All Those Exposed to Mormon Religious Education?"

The authors make several erroneous statements and implications. First, Enstrom has never examined cancer mortality among "all those exposed to Mormon religious education" and has never commented on what these cancer patterns might be. Second, previous studies on Mormons have not "implicitly analyzed their results as if the preventive exposure factor under consideration is 'Mormon Lifestyle' as a whole." References 1-4 contain descriptive (not analytic) epidemiology on cancer mortality rates and health practices for several well-defined Mormon populations. Analysis of specific preventive factors will presumably come from the prospective cohort study of Mormons described in Ref. 4. Third, the statement "individuals under study usually enter this exposure category at birth, but voluntarily leave the Mormon Church at different age" is false and shows ignorance of Mormonism. Up to one-half of all new Mormons become members through conversion, not birth, and relatively few ever officially leave the Church. Mormons who are excommunicated or who join another religious group are removed from the membership files, but inactive Mormons who do not adhere to Church doctrine still remain as members and are included in the overall membership files. About 10-15% of all members eventually go into the category of "lost and unknown" Mormons, but these people are not likely to affect the cancer mortality rates which Enstrom has calculated for California and Utah Mormons.

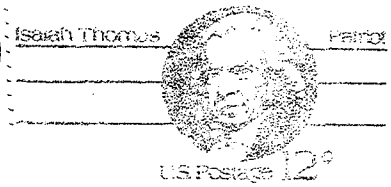
The authors' arguments about "selective loss" and "bias" in Enstrom's results are inaccurate and misleading. For instance, while claiming that the relatively high death rates among "Californians born in Utah" constitute evidence for bias, they have deliberately omitted mention of the numerous tests which Enstrom has done to confirm his mortality calculations. In 90% Mormon Utah County, the Mormon death rate is only 8% less than the county death rate as a whole, which does not depend on Church records (see Ref. 1, Table 3). Ref. 3, Table 1 shows that the Utah Mormon age-adjusted total death rate is only 10% less than the Utah state rate. For California Mormons, Ref. 3, Table 7 shows that direct follow-up of individual ward members yields an SMR consistent with the ward death records presented in Ref. 3, Table 1. For active Mormons, 30-year follow-up of the Muir cohort is confirmation of the Church mortality data (see Ref. 3, Tables 1 and 6). Estimates of the errors in the Mormon death rates are clearly stated. In addition, the studies by Lyon et al. on Utah Mormon cancer incidence and Utah cancer incidence and mortality agree with Enstrom's findings. If the authors' have any specific evidence of bias or errors they should present it.

The authors' notion of active Mormons being a "purified sample" is also misleading. In order to study Mormons you must by definition "purify" your sample to include only persons who identify with the Mormon Church. Similarly, active Mormons are a "purified" sample consisting of the subgroup active in the Church. However, this is similar to selecting a "purified sample" of persons who answered on a questionnaire that they are nonsmokers, as opposed to choosing the larger group who answered that they have been exposed to anti-smoking indoctrination. Obviously, the two groups are going to differ in several respects. As long as definitions are clear and precise there is nothing inappropriate with any specific cohort definition. The problem arises when one tries to overinterpret the reasons why a specific cohort differs from some other. Active Mormons are obviously a highly select group, but Enstrom has never said otherwise and has been very cautious about interpreting his findings.

pp. 14-15: "What Can be Established from Research on Active Mormons?"

Again the authors make spurious claims. It is not "epidemiologically improper to compare a highly select group such as 'active Mormons' to the general population." What is improper is to draw unwarrented conclusions from such a comparison. Self-selection applies to most epidemiologic studies, including those on other religious, occupational, or geographical populations. In observational studies, people self-select themselves in many ways. Enstrom has never stated or implied that the observed mortality differences could not be explained by all the ways in which active Mormons differ from the general population. However, in Ref. 2 he has clearly demonstrated that active Mormon males have the lower overall death rates than typical U.S. white males who never smoked cigarettes. In a reference not cited (Ca 29:352-362, 1979), Enstrom shows active Mormons have death rates similar to those of several other highly selected cohorts of nonsmokers. In any case, there are several sound reasons why Mormons are a good population for epidemiologic studies, regardless of how their cancer mortality rates compare with those of other groups.

The authors are entitled to their own opinion as to the value of another investigator's work. But they have tried to cast doubt on the validity and significance of Enstrom's research by citing data on other religious groups, by inappropriately citing from Enstrom's own papers, and by misrepresenting his findings. The authors have done no original research on Mormons and have found no specific errors in any of the publications on Mormon cancer mortality. This "critique," as currently written, has no place in a scientific journal.



JAMES E. ENSTROM, Ph.D.
SCHOOL OF PUBLIC HEALTH
UNIVERSITY OF CALIFORNIA
LOS ANGELES, CALIFORNIA 90024

Manuscript No. 90004, "A Critique of Studies on Mormon Cancer Mortality," which was reviewed by Dr. James E. Enstrom, is (please check):

✓ rejected and not under further consideration.

___ still under consideration.

___ accepted for publication.

Signed Mary Bissey

Mary M. Bissey

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World Views and Behavioral Patterns: A Study of Chinese and Other Cultures

by

Mitra D. Maghbouleh

Mitra Maghbouleh is a postdoctoral Trainee in the School of Public Health-Epidemiology, University of California, Los Angeles. She holds a Ph.D. from the University of California, Irvine, in Social Science, and has served as an instructor at the University in the School of Social Sciences.