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Report/Article Title Minutes of the Meeting on September 24, 1986,
Science Panel of the Agent Orange Working Group
(AOWG) with attachments

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Description Notes Attachments include: Letter to Ronald W. Hart, Director, National Center for Toxicological Research, from Neil A. Holtzman, Office of Technology Assessment, dated October 3, 1986; Letter to Ted Stevens, Chairman, Office of Technology Assessment, from Senator Frank H. Murkowski, dated March 11, 1986; Report of OTA Health Program, "Critical Analysis of: "Delayed Effects of the Military Draft on Mortality" N. Hearst, T.B. Newman, and S.B. Hulley"; and a copy of the article from the New England Journal of Medicine, 314 (10), March 6, 1986, 620-624.

MINUTES OF THE MEETING ON SEPTEMBER 24, 1986
SCIENCE PANEL OF THE AGENT ORANGE WORKING GROUP

The Science Panel met from 2:00 pm until 3:00 pm in room 729G of the Humphrey Building in Washington, D.C. Dr. Ronald W. Hart, Director of the National Center for Toxicological Research and Acting Chairman of the AOWG Science Panel, presided. Members present at the meeting are listed on the attached sign-in sheet.

The purpose of the meeting was to review the CDC protocol entitled "Validation Study Comparing Military Records-based Estimates of Likelihood of Exposure to Agent Orange with Plasma Levels of 2,3,7,8-TCDD".

Dr. Vernon Houk presented a brief history leading up to the preparation of this protocol. He emphasized that blood levels alone could not be used to pick the exposed and unexposed groups since that might involve analysis of plasma from all of the men who were in Vietnam. This is just not practical or possible. Therefore, some sort of exposure assessment must be available to pick the cohorts; blood levels can then be used, if necessary, to validate the choices. Dr. Houk also emphasized their concern for the treatment of the Vietnam veteran study subjects in that they feel that someone was interested in their well being; therefore, he felt it was important that the initial RTI interview and the subsequent full physical exam at Lovelace be conducted with the utmost care. He then introduced his staff that prepared the protocol and Dr. Bob Worth continued the briefing. Dr. Worth stated that this protocol should provide the necessary information so that a firm decision can be made as to the feasibility of conducting the full Agent Orange study.

Dr. Marilyn Fingerhut, NIOSH, distributed a written response to the protocol and the remainder of the meeting centered around the discussion of her concerns (copy attached). Her first point was to consider including samples from the non-Vietnam veterans as part of the main study rather than as a possible add on at the end. Dr. Jeffrey Lybarger, CDC, mentioned that it was very possible that non-Vietnam veterans may have a higher TCDD background than the normal USA population. Therefore, this type of data would be very pertinent to the interpretation of the results from the main body of the protocol. Dr. Dana Flanders, CDC, explained the assumptions and considerations that had gone into the pooled non-Vietnam veteran samples that were to be used to address this point. Further discussion on the various pros and cons ended with Dr. Worth and his staff volunteering to assess the various means of getting non-Vietnam veteran samples (cost considerations also being a factor) and then using their judgement as to the most feasible approach.

Dr. Fingerhut's second concern was with the meaning of a 71% difference in geometric means (protocol p. 34). Again Dr. Flanders gave an example as to the meaning and assumptions that were made. He gave as an example a low mean of 6 ppt; then a 71% difference could be detected with a 95% confidence if the high mean were above 10.2 ppt. However, further unresolved discussion centered around the possible range of background and what were the implications if the background spanned the range of both the high and low geometric means from the two exposure groups. This point can not be

resolved at this time but must be consider when the data is interpreted at the end of this study.

Another unanswerable concern was voiced by Ms. Hellen Gelband. She stated that the proposed study includes only veterans with high and low exposure scores. Ms. Gelband suggested that CDC consider including some men with moderate scores, anticipating that ultimately, it might not be possible to find enough men with very high scores to do the AO study. If a positive correlation is found between the TCDD levels and one of the exposure indices in this study but then an insufficient number of Vietnam veterans can be found to make up a cohort for the next phase, what happens then? The CDC personnel indicated that there was no real answer to this concern at this time but could possibly be a problem in the future and would have to be addressed in the proper context with the added information from this study.

Dr. Houk reiterated that the main cohort study is intended to examine the relationship between potential Agent Orange exposure in the field troops with long term health effects. Dr. Carl Keller, NIEHS, pointed out that this protocol was not designed to relate TCDD levels to health effects, but was designed to see if TCDD could be used as a biomarker to verify one of the exposure indices.

Dr. Peter Greenwald, NCI, made the motion that the protocol be accepted as written with consideration being given to Dr. Fingerhut's suggestion regarding the non-Vietnam veteran plasma controls. The motion was seconded by Dr. Renate Kimbrough, CDC. After some further discussion, the motion passed unanimously with the understanding that CDC would act appropriately in making needed adjustments to the study design and making further decisions as they become necessary.

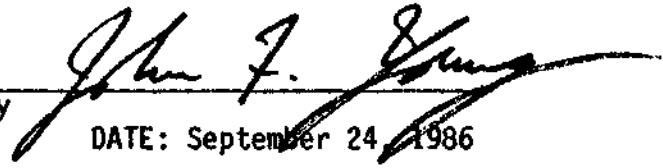
Dr. Houk informed the Science Panel of a meeting scheduled for September 30th between his staff at CDC and Dr. Al Young, OSTP, concerning obtaining plasma samples from some of the Ranch Hand personnel. If portions of stored plasma could be made available, as well as current samples from the same individual, much needed and very pertinent information could be garnered concerning the population variance in halflife values for TCDD. Dr. Hart emphatically agreed that this was a very critical issue and this effort should be supported fully. Dr. George Stebbing, DoD, felt that this was not an unreasonable request and would work to obtain the necessary information. However, there were certain considerations that would have to be dealt with, not least of all was a monetary consideration. The process of obtaining the needed samples could be greatly aided with a timely task order from AOWG Chairman Newman. No vote was called for by Dr. Hart, but no dissenting opinions were forth-coming when requested.

Dr. Houk volunteered the information and requested that it be made part of the record of this meeting that no Agent Orange funds had gone into the development of the GC/MS plasma assays. That work was initiated and completed by his staff at the CDC.

The protocol, appendices, and correction pages prepared by CDC and the requested written responses from Drs. Marilyn Fingerhut, Al Young, Renate Kimbrough and Ron Hart are included as part of these minutes. Most of the concerns addressed in these reviews were discussed during the meeting. Other issues cited relative to points of clarification, e.g., sensitivity levels, phraseology, etc., were to be incorporated into the final protocol.

Dr. Hart requested and received a motion and second to adjourn at 3:00 pm. The vote again was unanimous.

Prepared by John F. Young, Ph.D.
Acting Executive Secretary
AOWG Science Panel



A handwritten signature in cursive script, reading "John F. Young", written over a horizontal line.

DATE: September 24, 1986

Approved by Ronald W. Hart, Ph.D.
Acting Chairman
AOWG Science Panel



A handwritten signature in cursive script, reading "Ronald W. Hart", written over a horizontal line.

DATE: September 24, 1986

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Congress of the United States
OFFICE OF TECHNOLOGY ASSESSMENT
WASHINGTON, DC 20510-8025

*Read
10/9/86
JH*

October 3, 1986

cc: Gilberg

Dr. Ronald W. Hart
Director
National Center for Toxicological Research
Jefferson, AR 72079

Dear Ron:

Here is a copy of the OTA staff paper reviewing the article by Hearst et al, "Delayed Effects of the Military Draft on Mortality." We are sending this to you in your capacity as Acting Chair of the Science Panel of the Agent Orange Working Group. The letter from Senator Murkowski requesting this evaluation is also enclosed.

When you complete the paper you told me about on risk assessment, please send a copy.

Sincerely,

Neil

Neil A. Holtzman, M.D., M.P.H.
Senior Analyst

enclosures

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COMMITTEE ON VETERANS' AFFAIRS

WASHINGTON, DC 20510

March 11, 1986

Hearst
3/17/86
J.H.K.
A/O'S
CPA
McGurn
Swain
Stevens
3/19/86

Letter only

Honorable Ted Stevens
Chairman
Office of Technology Assessment
600 Pennsylvania Avenue, S.E.
Washington, D.C. 20510

Dear Ted:

I am writing to request that the Office of Technology Assessment (OTA) evaluate the scientific validity of the assumptions made, the methods used, the data evaluated and the conclusions reached in the study of the effect of military service during the Vietnam era reported by Drs. Norman Hearst, Thomas Newman and Stephen Hulley in the March 6, 1986 New England Journal of Medicine. I have attached the study abstract for your convenience.

Does the use of men "involved" in the draft lottery for the study population avoid selection bias? Does this method obviate the need for a control population?

Is the assumption valid that the excess mortality found in the "draft eligible" population is due to military service? Can this assumption be tested? Does this assumption have any effect on the earlier assumption that selection bias has been avoided?

Is the assumption valid that military service is the only difference between those who served and those who were eligible for the draft? Are there socioeconomic, psychological or other differences between those in the draft eligible population who served and those who did not (by either fortune or design) which may confound the assumptions and conclusions of the study? For example, student deferments were available until September 1971. Existing student deferments could be continued after that date. Deferments were continuously available for Reserve service. Are there differences between those who were able to obtain and hold a deferment and those who did not which would affect the results?

Could the results of the study be affected by the fact that the authors excluded from their study population foreign born men on the incorrect assumption that the foreign born were exempt from the draft? The authors incorrectly excluded 11.8% of total deaths on this basis.

Is the method used to determine combined relative risk valid and effective?

Is the method used to project relative risk due to military service valid and effective?

Were available data appropriately applied to the study methodology?

Are there alternative assumptions equally reasonable which would lead to different results?

Do the published conclusions follow logically from the data available? Are there alternative conclusions consistent with the data? Are there other relevant data available which were not considered?

Is the method used for the case control study valid? Does it lead to the stated conclusions? Are there alternative conclusions?

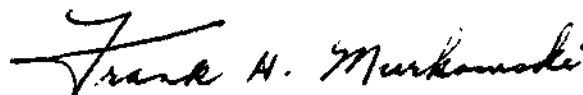
Have the New York and Massachusetts studies cited in the article been subject to critical review or replication? What were the results?

Are you aware of other studies addressing this subject? If so, what conclusions were reached?

Would it be feasible to determine which of the members of the "draft eligible" population studied actually served in the military? in Vietnam? in combat? If this data were available could it be used to confirm or deny the conclusion that the observed mortality rate is due to military service?

Answers to the questions posed in this letter and any other OTA views relative to published conclusions of the New England Journal of Medicine article will assist the committee in evaluation of the study results and any impact they may have on current or future VA programs or benefits. Thank you for your cooperation in this important matter. If you have questions please contact Chris Yoder at 224-9126.

Sincerely,



Frank H. Murkowski
Chairman

Enclosure

CRITICAL ANALYSIS OF:

"DELAYED EFFECTS OF THE MILITARY DRAFT ON MORTALITY"

N. Hearst, T.B. Newman, and S.B. Hulley

**Staff Paper
prepared by
Special Projects Office
OTA Health Program**

September 1986

**The views expressed in this Staff Paper
do not necessarily represent the views of
the Technology Assessment Board, the
Technology Assessment Advisory Council,
or individual members thereof.**

(9/30/86)

CRITICAL ANALYSIS:
"DELAYED EFFECTS OF THE MILITARY DRAFT ON MORTALITY"¹

Executive Summary

The purpose of this study was to determine the effect of the military draft during the Vietnam era on subsequent mortality. In an effort to avoid "selection bias," the authors limited their analysis to men born in 1950-52; beginning in 1970, men born in these years were subject to the draft lottery. Selection bias was not completely eliminated by this approach, but the bias that might be present cannot account for the significant excess of post-discharge deaths from suicides and motor-vehicle accidents among men born on dates that made them eligible to be drafted under the lottery. The data presented by the authors do not permit concluding that this excess is accounted for by men who actually served in the military. Even if this were proven to be the case, the only inferences that could be drawn relate to the effect of military service under the lottery system and not to military service per se.

How military service as a result of the lottery could exert an effect on subsequent mortality, if any, remains unknown. Men who enlisted to avoid being drafted, who accounted for more than half of all draft-eligible men who joined the Armed Forces after the lotteries were published, could have had a different chance of post-discharge death than draftees. Although combat experience in Vietnam might be a factor in post-war risk of dying, only 14.5 percent of draft-eligible and 9.7 percent of draft-exempt men in the cohort studied by the authors, and who were accepted for military service after July 1, 1970, served in Southeast Asia. Using the authors' estimate, about one-fifth of those men would have seen combat.

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The authors' findings are based on men dying in two states, California and Pennsylvania. The cohorts studied by the authors include men who entered the Armed Forces before the lottery went into effect, as well as men who had been rejected for military service, or who would have been had they been called. A study designed, as the authors' was, to determine the effect of being drafted by the lottery system ideally should exclude such men since they could not have been drafted and might have a different chance of post-discharge death than men who were drafted. Unfortunately, neither the authors nor we know who these men are. They almost certainly comprise at least 50 percent of the men included in the study. Including these men does not change the nature of the finding, that of an increased risk, but it does suggest that the magnitude of the risk may not be as great as is reported.

In a separate analysis, the authors compare the risks of dying from motor vehicle accidents and suicide among Pennsylvania Vietnam-era veterans (without reference to whether they served in Southeast Asia or elsewhere) with the risks of non-veterans dying from those causes. Inherent in this analysis is the same bias that the authors sought to avoid with their first analysis: the bias introduced because men who served may have had different characteristics from men who did not serve, and that those characteristics, apart from any influence of military service, may affect their risk of dying from specific causes. The quantitative risk estimates from this analysis are probably inflated because of this and other methodological factors.

The analysis in this paper supports the authors' finding that draft eligibility confers a small increase in the risk of post-discharge death from suicide or motor vehicle accidents among veterans dying in California and Pennsylvania. This study cannot shed light on whether the increase is

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actually associated with military service, however, and cannot necessarily be generalized to the population of U.S. Vietnam-era veterans. By itself, this study does not contain enough information to support conclusions about the risk of death among Vietnam veterans, but the findings reported are generally consistent with the few completed studies of mortality among that group of veterans. The Veterans Administration Mortality Study, which will be published soon, will add greatly to this information base.

Introduction

The paper by Hearst et al, "Delayed Effects of the Military Draft on Mortality"² has three components. The first, based on data from California and Pennsylvania, shows that the risk of dying from suicide or motor-vehicle accidents after the Vietnam war was greater for men born in 1950-52 on days that made them eligible for military service under the draft lottery in 1970-72 than for those born on days that exempted them from being drafted. The second component projects risk of death from the same causes assuming "that the excess risk among men who were eligible for the draft was entirely due to the excess risk among those who actually served." The third, based on data from Pennsylvania, finds that the odds of dying from suicide or motor-vehicle accidents are significantly greater among veterans than among non-veterans. We will consider each component separately.

I. Risk of death associated with draft eligibility

Reportedly high rates of morbidity and mortality in Vietnam veterans, cited by Hearst et al, may not be due to military service but to pre-existing socioeconomic, psychological, and other characteristics associated with

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entrance into the Armed Forces. For instance, death rates from many causes are higher in men of low socioeconomic status than in others regardless of military service. They may also have a relatively greater chance of serving in the military. It does not follow that military service is a cause of their higher mortality. To avoid such "selection bias" the study by Hearst et al is limited to men who were subject to the draft lottery. It is based on the assumption that the principal difference between those who served after the lottery went into effect and those who did not was the date on which they were born. Consequently, finding that the chance of post-war death from certain causes was higher in men whose dates of birth made them draft-eligible than in men whose birthdates exempted them would be consistent with the hypothesis that military service as a result of the lottery system increased the chance of death from these causes.

The draft lottery of 1970-72

Draft eligibility was determined by separate lotteries for men born respectively in 1950, 1951 and 1952 who would be subject to the draft in the year of their twentieth birthday: 1970, 1971, and 1972 respectively. Prior to each of these years, each date in the year was assigned a number (1-366) by a drawing that was intended to be random.³ The drawings were held, and the results made public, on December 1, 1969, July 1, 1970, and August 5, 1971. Shortly before the beginning of each month, beginning in January, 1970, the Selective Service System was told by the Department of Defense how many men were needed for the ensuing month. It would then select a sufficient number of birthdates, beginning with the one assigned the number "1" to meet that quota, based on an estimate of the number of births per day (about 5400) and

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the number of men who would be rejected or have bona fide deferrals. In 1970, men with the lowest 195 numbers were eligible; in 1971, men with the lowest 125 numbers; in 1972, men with the lowest 95 numbers. All males residing in the U.S. (except legally admitted non-immigrants) for at least 12 months were eligible. Men who were called had to report within one month. All draftees were recruited for a two year tour of duty in the Army.

Study Method

Hearst et al obtained the birthdates of draft eligibility from the Selective Service Commission. They obtained causes of death from computerized death-certificate registries of men dying in California and Pennsylvania. As the death records contain date of birth, the authors could assign draft-eligible or draft-exempt status to each man who died. The analysis was restricted to men born in 1950-52 who died at least three years after the beginning of the year of their draft eligibility. Men born outside the United States were excluded "because most would not have been eligible for the draft." In the analysis, deaths were stratified by year of birth, but a combined relative risk conferred by draft eligibility was computed for the three years, using standard statistical methods. The calculations did not depend on knowing death rates, although these were estimated.⁴ Veteran status, which was recorded on Pennsylvania death certificates from 1979 onward, was not used in this component of the study.

Findings

The relative risk of death conferred by draft eligibility, although greater than 1.0 for suicide (1.16) and motor-vehicle accidents (1.05), was

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not significantly increased in Pennsylvania. The relative risk was significantly elevated in California (with a larger number of deaths) for suicide (1.12), motor-vehicle accidents (1.10) and all causes (1.05). The latter finding is due to the high proportion of all deaths in this age group from suicide and motor-vehicle accidents, 16.7 % and 24.0 % respectively. Together, the Pennsylvania and California data represent a statistically significant increase. The relative risk of suicide was significantly elevated for draft-eligible whites (1.13), who constituted 81 percent of decedents, but the relative risk of motor-vehicle accidents (1.07) was not. For nonwhites, only the relative risk of motor-vehicle accidents (1.33) was significantly elevated. The relative risk of dying from cirrhosis of the liver was significantly less than 1 in draft-eligible compared to draft-exempt men.

Comments by Hearst et al

The authors attribute the significant increase in relative risk of post-war suicides and deaths from motor-vehicle accidents to the greater likelihood of military service in the draft-eligible than draft-exempt men. They concede, however, that the greater chance of death from violent causes after Vietnam may not have been in draft-eligible men who served in the military, but in those who did not, "due, for example, to guilt or psychological stress resulting from the lottery." (Guilt could result from being draft-eligible and evading the draft or not being drafted.) They maintain that the third component of their study makes this unlikely (see below). Although the increased relative risks conferred by draft eligibility observed in men dying in California and Pennsylvania may have been offset by decreases elsewhere in the country, the authors comment that their "results

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were similar in California, which has in-migration, and Pennsylvania, which has net out-migration."

In view of the association of motor-vehicle accidents with alcohol intake, and the finding in studies cited by Hearst et al that alcohol abuse is more common among veterans than nonveterans, the authors have difficulty explaining the significantly lower relative risk of cirrhosis conferred by draft-eligibility. Pointing out that significantly lower rates of death from cirrhosis have not been found in other studies of veterans, the authors think this significant finding is most likely due to chance.

OTA Comment

A substantial proportion of the cohort studied by the authors consists of men who would not have been subject to randomization under the lottery system, and others who, although subject to randomization, would not have been accepted for military service. The authors ignore these groups. Although this does not have an appreciable effect on this component of the study, it does affect the second component as well as the authors' conclusions, as will be considered later.

Men became eligible for the lottery in the year of their twentieth birthday, but they were eligible to volunteer for military service at seventeen-and-a-half (with parental consent). From data supplied to us by the Veterans Administration, we calculated that 56 percent of 7419 deceased veterans who were born in 1950-52 entered the military prior to the date on which their eligibility under the lottery was published.⁵ In 1970, 45 percent of draftees were rejected for medical or mental reasons, or both, as were 6.8 percent of enlistees.⁶ In comparison to men who were, or would have been,

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accepted for military service under the lottery system, post-discharge death rates may well have been different in men who volunteered prior to the lottery, and in men who were medically unacceptable for military service. There is no reason to believe, however, that post-discharge death rates for men in these two groups who had birthdates that made them draft-eligible were any different than the rates for men whose birthdates made them draft-exempt. Removing these men from the analysis--if they could be identified--would reduce the size of the cohort, but should not reduce the relative risk conferred by draft eligibility.

As shown in Table 1, reducing the size of the cohort raises rather than lowers the relative risk of suicide conferred by draft eligibility. The confidence intervals broaden, but the lower limit moves further above a relative risk of 1.0. The probability that the findings are due to chance becomes less rather than greater despite the smaller sample sizes. The calculation is based on the assumption that the suicide rate in men not subject to military service under the lottery was the same as the observed, combined rate for draft-eligible and draft-exempt men.⁷ A higher rate in the men excluded from the analysis would result in lower relative risks than those shown in Table 1. It is probable that the death rates from certain causes, particularly chronic diseases, would be higher in men who were rejected for military service. The authors had no way of identifying decedents who served in the military prior to institution of the lottery system or those who were medically unacceptable for military service.

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Although the epidemiological method used by the authors, which we used in preparing Table 1, is appropriate for analysing the data, an alternative method entails determining whether a disproportionate number of deaths occurred among draft-eligible men. Since there is no reason to believe that birthdate per se influences cause of death, the expected proportion of all deaths that occurred in draft-eligible men equals the proportion of draft-eligible days in the three years of the lottery, assuming that the average number of births per day on draft-eligible and draft-exempt dates were equal. This method also gives statistically significant positive ratios of observed to expected deaths from suicides and motor-vehicle accidents, as well as all causes, although they are somewhat lower than the ratios calculated by the authors' method.

Following institution of the lottery system, a greater proportion of draft-eligible than draft-exempt men may have obtained classifications other than 1A, for example, by going to, or remaining in, college. Their birthdates would not change, so that the finding in this component of the study is not affected, although it is possible that post-discharge death rates in draft-eligible men who avoided military service differed from those who served. It is also possible that a greater proportion of draft-eligible than draft-exempt men left the country in order to avoid military service. In this case, those men who returned and died in California or Pennsylvania would be counted. The contribution that such men would make to the excess of deaths in draft-eligibles cannot be determined by the data available to the authors.

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The data analysed by Hearst et al are limited to two states. The excess of post-discharge deaths from suicide and motor-vehicle accidents is not significant in Pennsylvania, but it is in California, as well as for the two states combined. Despite the absence of statistical significance, the authors argue that the increased relative risks in Pennsylvania, a state which has net in-migration, together with the significant increase in California, which has net out-migration, strengthens the generalizability of the study to other states. They maintained that if both states had in-migration it is possible that "draft-eligible men who were at high risk of suicide and motor-vehicle accidents were more likely than comparable draft-exempt men to move to California or Pennsylvania."⁸ This would leave fewer such men in states with net out-migration so that for the nation as a whole the relative risk of post-discharge death from these causes conferred by draft eligibility would not be increased. From migration data analysed by the Veterans Administration, it seems likely that there was a net in-migration of veterans to Pennsylvania and a net out-migration of them from California between 1975 and 1980.⁹ Although opposite from the civilian migration, this still supports the authors' argument for generalizability. Replication of the study in other states, which Hearst et al are currently attempting, will help to determine the extent of generalizability.

The authors do not comment about the racial differences. A larger number of men of all races should be studied before concluding that these differences are real.

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The authors are probably incorrect in stating that most decedents born outside the United States were not eligible for the draft. The only foreign born who would have been excluded were those residing in the country less than one year and non-immigrant aliens.¹⁰ The exclusion does not affect their analysis, which does, however, apply only to men born in the United States. Death records in Pennsylvania from 1974-78 did not include place of birth. The inclusion of deaths in foreign born men for this period (estimated to be 12.5 percent of all deaths) would probably not change the findings.

Although misclassification of cause of death on death certificates probably occurred, there is little reason to expect that it would be different in men with draft-eligible than with draft-exempt birthdates. In response to this study, Webb¹¹ suggested that men of higher socioeconomic status who committed suicide might not have their deaths coded as suicides. Although this would result in an underestimate of the suicide rate in both draft-eligible and draft-exempt men, it should not influence the relative risk of suicide conferred by draft eligibility; unless the suicides in which miscoding took place were related to military service (and Webb argues that this is not the case) or avoiding it, there is no reason to expect that they would have been made more frequently in decedents with birthdates that made them draft-eligible than in those with birthdates that exempted them. If such deaths were related to military service under the lottery, or to avoiding it, they would have been more likely to occur in draft-eligible than draft-exempt men. Their removal from the suicide category would result in an underestimate rather than an overestimate of the relative risk of suicide conferred by draft eligibility. This would reduce the projected effect of military service in the second component of the study.

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The significantly lower relative risk of cirrhosis conferred by draft eligibility is unexpected, and points out the role that chance can play in obtaining significant findings. Men in the cohort studied may still be too young to have died from cirrhosis. However, that does not explain why they should be protected from cirrhosis.

In summary, draft eligibility conferred a small but significant increased chance of dying from suicide or motor-vehicle accidents in California and Pennsylvania combined, but not in Pennsylvania alone. To extend the result to the nation as a whole, or to attribute the finding to a specific factor, such as military service, is speculative. The data do not indicate that the excess of deaths occurred in men who actually entered the Armed Forces as a result of the draft lottery.

II. Estimated effect of military service on subsequent mortality

The authors derive an equation to project the relative risk conferred by military service of death following discharge from the Armed Forces. It is based on the assumption that the increased relative risk of post-discharge death conferred by draft eligibility is due entirely to a higher death rate in men who served in the military. Solution of the equation depends on knowing the proportions respectively of draft-eligible and draft-exempt men who actually entered the Armed Forces after publication of the lotteries. As discussed below, the authors' failure to exclude men who were not subject to the lottery in estimating these proportions inflates the projected relative risk of post-discharge death associated with military service.

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Study method

Using Bureau of Census estimates of the total number of males who were 20 years old in 1970, 1971, and 1972, and data provided by the Defense Manpower Data Center (DMDC), the authors estimated that 25.6 percent of draft-eligible men entered the military by being drafted or volunteering compared to 9.3 percent of draft-exempt men.¹² These percentages, together with the relative risks determined in the first component of the study from data from California and Pennsylvania, are used to project the relative risk of post-discharge death conferred by military service (authors' equation (3)).

Authors' projection

If all of the excess deaths observed for draft-eligible men were in men who served, then the relative risk of military service for suicide, motor-vehicle accidents and all causes would be 1.86, 1.53, and 1.25 respectively.

OTA comment

The authors' estimates of the percentages of draft-eligible and draft-exempt men who joined the military do not take into consideration that men born in 1950-52 could have volunteered for the Armed Forces before they knew their draft eligibility. As already pointed out, such men may comprise over one-half of the entire cohort. Men born on January 1, 1950 could have entered the military as early as July 1, 1967 and men born on December 31, 1952 could have entered as early as June 30, 1970. None of these "accessions" appear in the computerized data base of the Defense Manpower Data Center from which the authors made their estimates of the proportion of the cohorts who actually served; these records begin with accessions on July 1, 1970. Nor are the

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authors correct in assuming that the men born in 1950-52 who, according to the DMDC records, entered before January 1 of the year of their twentieth birthday all entered after the results of the drawing were published for the year in which they would be subject to the draft lottery.

Using complete data for accessions from July 1, 1970 to December 31, 1973 for men born in 1950-52, which were made available to us by DMDC, we calculated that 18.7 percent of draft eligible men born in 1950-52 joined after the lottery for their respective year was announced, compared to 5.7 percent of draft exempt men.¹³ As shown in the first row of Table 2, these percentages give higher projections of the relative risk of post-discharge suicides than those calculated by the authors. (The corresponding relative risks for deaths from motor-vehicle accidents and all causes are 1.64 and 1.31 respectively.) However, the calculation is based on the assumption that all men born in 1950-52 would be subject to the lottery and would be accepted for military service if their number was called. As already discussed, this is not the case. In addition to men excluded from the lottery because they already were in the Armed Forces, others would be rejected for physical or mental reasons. They might have a different risk of death in the years following the Vietnam War than those who were in the military. Neither of these men should be included in the pool of men subject to randomization by the lottery.

Instead of using the entire cohort of men to calculate the proportion of draft-eligible and draft-exempt men who actually served after the lottery for their respective year was published, we reduced the size of the cohort subject to the lottery by 10, 40 and 70 percent. As shown in the middle columns of the last three rows of Table 2, this results in a progressive

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increase in the proportion of both draft-eligible and draft-exempt men who served after the lotteries were published. The effect, as shown in the last column, is to reduce the projected relative risk conferred by military service.

The equation derived by the authors to project the risk of post-discharge death conferred by military service does not take into consideration that draft-exempt men who volunteered and were accepted for military service after the lottery might have characteristics that made their chance of post-discharge death different than draft-eligible men who were drafted under the lottery system or who enlisted after learning that they had a high probability of being drafted.

Regardless of what the risk of post-discharge death conferred by military service actually is, the only valid inference from the calculation used by the authors relates to the effect of military service under the lottery system and not to military service per se. The authors do not always make this distinction. In the last sentence of the paper they seem to acknowledge it,¹⁴ but in the Abstract they conclude, "the most likely explanation for these findings is that military service during the Vietnam War caused an increase in subsequent deaths from suicide and motor-vehicle accidents."

If military service as a result of the draft lottery did increase the risk of post-discharge death, the study does not indicate what aspects of military service were responsible. From the DMDC data, we estimate that about 44 percent of the draft-eligible men born in 1950-52 who entered the military between the date the lottery pertinent to them was published and December 31 of the year in which they were subject to the draft did so by enlisting. The

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rate of enlistment was twice as high in men with draft-eligible birthdays during their year of vulnerability as in men with draft-exempt birthdays. It is possible that men who enlisted to avoid being drafted had characteristics that were different than in men who waited to be drafted and that might differentially affect their chance of post-discharge deaths. For instance, draft-eligible men who enlisted in the Navy or Air Force may have done so perceiving correctly that they had less of a chance of dying than if they were drafted into the Army. This "risk-averse" decision might also influence their behavior in subsequent civilian life. On the other hand, men who joined the marines may have been risk-seeking.¹⁵

According to DMDC data, only 14.5 percent of draft-eligible men (born in 1950-52) who were accepted into the military from July 1, 1970 to December 31, 1973, served in Southeast Asia. (The comparable percentage for draft-exempt men was 9.7 percent.) In their reply to comments, the authors suggest that a "graded risk seems most likely, with combat veterans at greater risk than noncombat veterans, who in turn are at greater risk than nonveterans." According to the authors, only one-fifth of the Americans who served in Indochina saw combat duty. If the entire effect observed in the first component of the study is due to an excess of post-war deaths in men who fought in Vietnam they might be observed in the Vietnam Experience Study currently being conducted by the Centers for Disease Control, and in the Vietnam Veterans' Mortality Study nearing completion by the Veterans Administration. These studies are not limited to the cohorts studied by the authors, which would, in fact, comprise a small proportion of their total study population.

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We conclude that the projections by Hearst and his colleagues of the risk of post-discharge deaths conferred by military service are too high because they are not limited to men who could have been randomly assigned to military service under the lottery system. Moreover, the projections depend on the applicability of data from two states to the country as a whole. Because data are not available to determine whether decedents actually served, or how they joined (draft versus enlistment), or where they served (Southeast Asia or elsewhere), there is no way of saying whether, in fact, military service plays a role, or how.

III. Odds of death from specific causes in veterans compared to non-veterans

In this component of the study, the authors used death records on which veteran status was recorded in order to determine whether the cause of death differed in veterans compared to non-veterans.

Study method

The Pennsylvania death certificate registry, used by the authors to ascertain cause of death, contained information on veteran status for men dying from 1979 onward. The authors used these data for 1979-83, restricted to deaths in men born in 1950-52, to determine whether the odds of dying from suicide, motor-vehicle accidents or cirrhosis compared to dying from other causes was higher in veterans than in non-veterans. Men dying of causes that might be associated with military service (presumably based on the findings in the first component of the study) were excluded in calculating the odds ratio of other causes. For instance, men dying of motor-vehicle accidents and cirrhosis were excluded in calculating the odds ratio for suicide. The data

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were not examined separately by race.

Findings

The odds for veterans of dying from suicide (1.65) and motor-vehicle accidents (1.49) compared to other causes were significantly increased. The odds of dying from cirrhosis for veterans, 1.05, was not significantly increased.

Authors' comments

The authors use these results to suggest that their findings in the first component of the study--a significant increased risk of deaths from suicides and motor-vehicle accidents in draft-eligible compared to draft-exempt men--is more likely attributable to men who served than to draft-eligible men who didn't serve.

OTA comments

The authors have not shown that veterans have an increased chance of dying from suicide or motor-vehicle accidents than non-veterans. To do that they would need the number and ages of veterans and non-veterans living in Pennsylvania from 1979-1983. Their study shows that the causes of death were differently distributed in veterans compared to non-veterans. Because characteristics associated with acceptability for military service, and hence veteran status, can also be associated with deaths from specific causes, this is not surprising. In their introduction, the authors point out that "Comparisons between veterans and non-veterans may be biased because men who enter the military differ in important ways from men who do not." Since the

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authors do not know which veterans included in their study entered the Armed Forces as a result of the lottery this phase of their study suffers from this shortcoming as much as other studies.

Non-veterans probably had a significantly greater chance of dying from heart disease and other causes than did veterans at the young ages included in this study; men with evidence of such diseases would have been rejected for military service. Including men who died of such causes as "controls" in the analysis, as the authors did, yields higher odds ratios of dying from suicide or motor-vehicle accidents in veterans than if these deaths were excluded. In the relatively young age group studied by the authors, the excess of deaths from violent causes in men healthy enough to have served in the military a decade or so earlier is not surprising. The authors of the New York study cited by the authors, which also compared deaths from specific causes in veterans to non-veterans, recognized that such "comparison(s) should be evaluated cautiously since the screening procedure associated with induction into the military services produces inherent incomparability."¹⁶

The exclusion of men dying of causes that are associated with military service, based on increased odds ratios in veterans compared to non-veterans, will elevate the odds ratios for other causes. Without such an adjustment, factors for which the association is real might not give statistically significant increased odds ratios. If, however, men dying of causes that are not truly associated are excluded, the odds ratio for other causes could attain statistical significance when the association is not real.

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Both the Massachusetts study¹⁷ cited by the authors, and the New York study previously mentioned, suffer from the same problems of including deaths from causes that would be associated with rejection for military service. Both of these studies also exclude deaths from causes alleged to be associated with military service in calculating the odds ratio for other causes.

In their analyses, the authors fail to adjust for different ages of death. This is not a serious a problem since the age at death of men included in the "case-control" study could only be between 27 and 33 years.

Differential migration of veterans compared to non-veterans could also influence the odds ratios. For instance, if veterans born in 1950-52 moved into Pennsylvania while non-veterans born in those years moved out, fewer deaths from chronic disease in non-veterans might result and the odds ratio for deaths from acute causes among veterans might be inflated.

We do not think that this component of the study, or similar studies cited by the author, provides confirmation of the hypothesis that military service contributes to subsequent deaths from certain causes.

Summary and Conclusions

The finding by Hearst et al, that men with birthdates that made them eligible for the draft under the lottery system have a small but significant increased chance of dying from suicide or motor-vehicle accidents after the Vietnam war appears sound. Whether it holds for men dying in states other than California and Pennsylvania remains to be demonstrated. The data provide no information on whether the excess of deaths occurred in men who actually entered the Armed Forces as a result of the draft lottery.

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Even assuming that the entire excess of post-war deaths in draft-eligible men was due to military service, the relative risks projected by the authors for military service in the second component of the study are too high because they are not limited to men who could have been randomly assigned to military service under the lottery system. The projections apply only to men who served as a result of the lottery. Data are not available to indicate whether military service itself plays a role, or how.

Comparisons of cause-specific deaths in veterans compared to non-veterans are subject to selection bias and other problems. Consequently, neither the authors' finding of an increased odds ratio for suicides and motor-vehicle accident deaths in veterans compared to non-veterans, nor similar findings by others, offers conclusive support for the hypothesis that military service contributes to subsequent deaths from these or other causes.

The effect of military service under the lottery system might be determined by studying only those men born in 1950-52 who could have been randomly assigned to military service under the lottery system and who died following discharge from the Armed Forces. The first step, as in the authors' study, would be the collection of death records of men born in 1950-52 who died following the Vietnam war (preferably from more than two states). Men who were deferred for physical or mental reasons (but not for reasons that might have been used to avoid the draft), or who had previously served in the military, would be excluded from further study. Such men could be identified from their selective service system classification status at the time they would have been subject to the lottery. This information is available at Federal Record Centers. For men remaining in the study, those who served in the military--and, for those who did, whether they enlisted or were drafted,

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and whether they served in Southeast Asia--could be identified from information available at the National Personnel Records Center in St. Louis. The method used by the authors in the first component of the study could be used to determine whether there was an excess of deaths in men with draft-eligible birthdates. If so, the data could be further analysed to determine the contribution of military service, means of accession (by enlistment or the draft), and service in Southeast Asia. The data could also be used, as in the third component of the authors' study, to determine whether the odds of dying from certain causes of death were higher in men who served in the Armed Forces than in men who did not, but who probably would have been acceptable for military service.

Such a study would require an extensive examination of records. A finding that military service increased the risk of post-war death could not be generalized to other wars, or even to men who joined earlier during the Vietnam conflict. The unpopularity of the war at the time of the lottery could well be a factor in explaining why men who joined the military suffered more on their return to civilian life, if that finding is, in fact, obtained.

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FOOTNOTES

¹ Prepared by Neil A. Holtzman of the OTA Health Program Special Projects Office

² Hearst N, Newman TB, Hulley SB. New Engl J Med 1986; 314:620-24

³ A greater-than-expected proportion of men with birthdays in December and November drew low numbers, probably because the capsules containing the dates were entered into the vessel by month and were not thoroughly mixed. The authors were asked whether the nonrandomness of the lottery could explain their findings; men with birthdays late in the year may have been more likely to die from suicide or motor-vehicle accidents than others. (Hooper RR. (Letter) New Engl J Med 1986; 315:453-54.) There is no evidence for this hypothesis. The authors replied that even a 10 percent higher suicide rate among men born in November and December would have lowered the findings by less than 5 percent. Hearst N, Newman TB, Hulley SB. (Letter) New Engl J Med 1986; 315:454.)

⁴ Relative risk = deaths per eligibles/deaths per exempts. If N = total population of 20 year old men and p = the proportion of days of the year that are draft eligible and (1 - p) the proportion that are draft exempt, then eligibles = pN and exempts = (1-p)N. Therefore:
Relative risk = deaths per p/deaths per (1 - p).
N cancels out. N is needed in the calculation of confidence intervals. In their calculations, the authors used man-years rather than men. This would not change the calculation appreciably.

⁵ Forty-two percent of decedents who had birthdates that made them draft-eligible, and 71 percent of men with birthdates that exempted them, entered prior to the date on which they could first have known their likelihood of being drafted under the lottery system. Kang HK, Breslin PP. (Letter) New Engl J Med 1986; 315:454.)

⁶ U.S. Army Patient Administration Systems and Biostatistics Activity, Ft. Sam Houston, TX. Letter from Col. T.E. Bowen, July 18, 1986. An additional group of men would have been deferred because of hardship, or their student or occupational status.

⁷ The suicide rate per 100,000 for draft-eligible and draft-exempt men born in 1950 was 30.94; for men born in 1951, 32.95; and for men born in 1952, 35.37.

⁸ One explanation for such a movement would be that draft-eligible men who served in the military, and who had health problems that predisposed them to suicide or motor-vehicle accidents, migrated to these two states at higher rates than non-veterans with these problems because of better veterans benefits.

⁹ Heltman LR, Veterans Administration. A comparison of civilian and veteran interstate migration; improving veteran population estimates. Paper presented at American Sociological Association, August, 1984, Philadelphia. Tabulations

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prepared by Research Division, Veterans Administration. While the net migration of civilians in the cohort studied by Hearst et al was into California between 1975 and 1980, that of veterans was outward. In Pennsylvania during the same period, the net movement of veterans who were born in 1950 was probably outward, together with the civilian population of the same age, but veterans born in 1951 and 1952 may have been moving in with sufficient frequency to offset the out-migration of those born in 1950. Unfortunately, the data are presented by five-year age groups.

10 Personal communication, Col. Richard Hein, Selective Service System.

11 Webb J. Viet vets didn't kill babies and they aren't suicidal. Washington Post April 6, 1986, Page C1.

12 The authors kindly provided a detailed description of their method. The census estimates they used apparently were restricted to native-born men. In calculating the number of men who actually served they did not have sufficient information to adjust for foreign born men. The estimates we used are the census estimates for all 20 year old males in 1970, 1971, and 1972 respectively. They are about 6 percent higher than those of Hearst et al.

They calculated the number of males who were draft-eligible by multiplying each estimate by the fraction of draft-eligible days for that year, for example, $195/365$ for 1970, and the number of males who were draft-exempt by multiplying by one minus the fraction, for example $(1 - 195/365)$.

The data from the Defense Manpower Data Center (DMDC) consisted of a random sample of 3000 men who entered military service in 1971 to 1973. By adding together the percentages of men born in 1950, 1951 and 1952 who were accepted in to the armed forces in any of the three years 1971, 1972 and 1973 they obtained their estimates of the percentage of eligible and exempt men who served.

In additional material kindly supplied to us, the authors estimated that 7.7 percent of draft-eligible men (included in the 25.6 percent) entered prior to January 1 of their twentieth year, compared to 4.7 percent of draft-exempt men (included in the 9.3 percent). They assume, however, that all of these men entered after the results of the drawing were published for the year in which the men would be subject to the lottery. In the description they sent to us they wrote, "...the lottery was held in the previous year. It was not unusual, therefore, for men who drew low lottery numbers to enlist in the year prior to their year of draft eligibility." In fact, some of them could have entered the military without knowing what their lottery status would be; the results of the lottery were not made known until August 1971. But men born in 1952 who are included in the DMDC accessions printout could have joined between July 1, 1970 and August, 1971. From data supplied to us by DMDC, we calculated that 5.7 percent of draft-eligible men born in 1952 joined the military between July 1, 1970 and the month the lottery results were announced one year later. As expected, an almost identical percentage, 5.8, of draft-exempt men entered during that period.

13 The percentages of men who joined from the date the lottery pertinent to them was published to December 31 of the year they were subject to the lottery (excluding men who joined afterwards) are 15.6 percent of draft eligible and

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4.4 percent of draft exempt men. As the difference between these two percentages is smaller than those of Hearst et al, they give still higher relative risks projected for military service. The percentage of draft-eligible and draft-exempt men who joined after their year of draft eligibility expired was 3.1 percent and 1.4 percent respectively. The difference is probably due to delayed entrance into the military of men drafted in their year of eligibility.

14 They write, "The casualties of forced military service may not be limited to those that are counted on the battlefield (emphasis added)." "Forced" service includes not only the draft, but enlistment as an effort to avoid the draft.

15 From 1961 to 1971, total deaths (and death rates, in percent) in the Army, Navy, Air Force, and Marines were respectively: 38,175 (2.05), 2543 (1.57), 2581 (0.77), and 14,826 (4.04). (Source: Combat area casualties file. Archives of the United States, machine readable data file.)

16 Lawrence CE, Reilly AA, Quickenton P, Greenwald P, Page WF, Kuntz AJ. Mortality patterns of New York State Vietnam veterans. Am J Public Health 1985; 75:277-79.

17 Kogan MD, Clapp RW. "Mortality among Vietnam veterans in Massachusetts, 1972-1983." Boston: Massachusetts Department of Public Health, 1985.

TABLE 1
EFFECT OF REDUCING SIZE OF COHORTS ON THE RELATIVE RISK OF POST
DISCHARGE SUICIDES ASSOCIATED WITH DRAFT-ELIGIBILITY*

Percent of men <u>excluded</u>	Relative risk of suicide associated with draft-eligibility	
	<u>R.R.</u>	<u>C.I.**</u>
0	1.13	1.04, 1.23
10	1.14	1.04, 1.25
40	1.22	1.10, 1.36
70	1.48	1.27, 1.73

* We are indebted to the authors for providing us with the cohort-specific deaths, which did not appear in their article, so that we could calculate the combined relative risks using the Mantel-Haenszel method that they employed.

** R.R. - relative risk; C.I. - lower and upper 95 % confidence intervals

TABLE 2
EFFECT OF REDUCING SIZE OF COHORTS ON THE PROJECTION OF RELATIVE RISK OF
SUICIDE ASSOCIATED WITH MILITARY SERVICE

<u>Percent of men excluded</u>	Percent of remaining men who served after the lotteries		<u>Projected relative risk of suicide associated with military service*</u>
	<u>Eligible men</u>	<u>Exempt men</u>	
0	18.7	5.8	2.07
10	20.8	6.5	1.97
40	31.2	9.8	1.65
70	62.4	19.5	1.32

* Calculated with equation (3) of Hearst et al, using the observed relative risk associated with draft eligibility (1.13) and the percentage of remaining draft-eligible and draft-exempt men shown on each row. The baseline percentages are lower than the authors,' giving a higher projected relative risk than that calculated by the authors. See supporting data for explanation.