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Evaluation of Canadian Forces Survey on Homosexual Issues

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## EVALUATION OF CANADIAN FORCES SURVEY ON HOMOSEXUAL ISSUES

## The Mandate

In the cover letter accompanying the report, I am asked to provide a written evaluation of

- (a) the theoretical rationale of the study
- (b) the adequacy of the measures
- (c) the tenability of the conclusions
- and (d) other comments (e.g. sampling, psychometric issues).

I see (a) as encompassing the background and justification set out in the introduction, (b) and (d) as covering methodology and results, and (c) as most relevant to the discussion. Consequently, this review deals with each major section of the report in the order in which it appears, with an overall evaluation at the end.

## Review and Evaluation of the Report

Introduction

The introduction begins by setting out the background to the survey, which was designed to obtain opinions of CF members concerning the potential impact of a change in policy on the enrolment of homosexuals in the CF. It is stated that the military position is

"that a number of factors directly related to the operational effectiveness of the CF such as cohesion, morale, discipline, confidence in leadership,

recruiting and personnel attrition could be affected by a change in CF policy to allow homosexuals to enrol and serve in the CF." (p. 1)

On page 2, five specific goals of the survey are set out.

My first comment is that the factors in the above quotation are not connected to the five goals and to subsequent measured variables via operational definitions, as is standard scientific practice (Christensen, 1988). For example, what exactly is meant by cohesion and morale? Which specific opinions are supposed to reflect these abstract terms? The final discussion implies some of these links, but the theoretical rationale would have been considerably enhanced by an initial statement operationalizing the terms. I spent a considerable amount of time trying to understand the author's interpretation of his results because I had not been given a clear set of operational definitions.

Second, if potential attrition is a concern, this question should have been asked directly. As far as I can see, it was not; the only evidence that did appear occurred spontaneously in free comments (p. 28).

Third, if recruitment is an issue, data might have been obtained from the general public in order to tap the opinions of potential recruits. Only recent candidates were queried here, a point which I recognize is later defended in the discussion (p.43).

Related research is then described. Care is taken to point out the possible errors in previous estimates of the proportion of homosexuals in the population. Notably, it is observed that "most Canadians view homosexuality as a sickness rather than a lifestyle", although the the next sentence (p.4 "More ... ..authority") is not clear because of poor expression. Given this information, I was surprised that the survey did not permit respondents this choice. Only the attitude towards the mental illness model was measured.

It is also stated that Canadians seem to have more difficulty with the employment of homosexuals when children are involved. The author might have used this to draw a contrast between schools and the military, where children are not directly involved, or to make the theoretical point that, in principle, Canadians may wish to limit equal opportunity in some situations.

Research on the determinants of attitudes towards homosexuals is then considered. Although the direction of most previous findings was stated, the relationship between age and attitude (p. 5) was not. It is interesting that, on p. 6, the sex difference is linked to a general female trait (presumably to be less prejudiced), but that males seem relatively more negative towards male homosexuals. I am not sure why religious variables (p. 6) were included under experience and not under demographics, but the stated finding that contact usually leads

to more positive attitudes is important. Unfortunately, although it is noted that this relationship does not always hold (p.6), the circumstances were not described. The author correctly points to the necessity of having a measure of quality of the contact, and is to be applauded for introducing it in this survey.

I also believe that the previous work on authoritarianism is relevant, and was pleased that the balanced F-scale was used. However, one potential difficulty, which was not raised, is that correlations between F-scores and other variables may be affected by a restriction in range; it is plausible that members of CF are not representative of the general population on this variable.

The next section of the introduction deals with attitudes and their measurement. It is observed (p. 7) that attitudes are usually measured in applied research in order to predict behaviour or intentions to do so. I think that this section ought to have been expanded. First, if only intention can be predicted, the applied implications of this study are seriously weakened. Indeed, in the discussion (p. 40) it is recognized that "the relationship between verbal and actual behaviour is not exact". Thus, I think that the circumstances under which attitudes do predict behaviour ought to have been spelled out, so that we can evaluate whether or not the factors are present in this case. Research indicates that attitudes are more likely

to relate to actual behaviour if the situational context is common to attitude and behaviour, if the measurement of the attitude contains a specific behavioural intention, and if the attitude is based on personal experience (Atkinson, Atkinson, Smith, & Bem, 1990, p. 707; Zimbardo, 1988, p. 618). In my opinion, the first requirement is partly met, the second is met in some places in the survey, and the last one is possible to evaluate since personal experience data were obtained.

Second, although the attitude-behaviour link is clearly relevant to the present study, the author fails to mention the behavior-attitude link, which demonstrates the attitudes can change as a result of engaging in action (Zimbardo, 1988, pp. 618-619). I think that this omission has important implications for interpretation of the results, and I shall return to it later.

The author then sets out four criteria for selecting his measures. I think I accept these, although I found the first ("multidimensionally"; should it be "multidimensionality"?) unclear, and the second ("continuous") to be badly described (the scales contained 5 categories were not dichotomous, but they were also not continuous). These are picky points pertaining to expression rather than substance, however, and I think that the Halpern scale and the balanced Ray F-scale were reasonable choices.

Finally, the author reviews the purposes of the study,

making clear that behavioural prediction was the main goal. This implies that one of the other goals (e, p. 2, establishing determinants) was secondary. I accept this, but note that introducing the causal issue renders relevant the behavior-attitude link mentioned above. At the end of the introduction, the author correctly observes that results with the general population cannot be generalized automatically to the military situation; however, I do not understand what he means (p. 9) by "consequence of error, and unlimited liability".

Evaluation of Theoretical Rationale (Introduction)

I have noted one or two points that were not clear to me, but I do not think that they seriously undermine my understanding of the author's introduction. Also, the material included is relevant to the stated purposes of the survey. Given the issue of concern (possible policy change), I also think that it is reasonable to obtain an assessment of opinion in the CF. My only criticisms here are that, for recruitment, further data from the general public would be needed and, for the attrition estimate, a specific question ought to have been included.

I have two additional criticisms. First, I found that the military concepts (cohesion, morale, discipline, confidence in leadership) relating to operational effectiveness were not clearly operationally defined in terms of the variables measured here. Some of these links are implied later in the discussion, but since they were introduced on p. 1 as the major starting

point of the study, they should have been clearly connected to the six goals. Second, given that one of the goals, albeit secondary, was to study the determinants of both reactions and attitudes, the theoretical link between behaviour and attitudes ought to have been stated. The attitude-behaviour link, which is mentioned (although in insufficient detail) is relevant to the prediction of behaviour, the major goal of the study, but the behaviour-attitude relationship is crucial to the issue of causation.

#### Method

Given the normal practices in conducting surveys and opinion polls, the present total sample size and all of the subtotals in Table 1 are adequate. In addition, the sampling procedures outlined on pp. 10-11 are acceptable. I understand why some oversampling occurred (p. 10). The proportions in the sample set out in Table 1 do not correspond precisely to the CF proportions in the same table (indeed  $\chi^2$  statistical tests on the first two were significant), but I do not judge the discrepancies to contribute seriously to sampling error. The one thing I cannot judge is the representativeness of samples in combined categories: for example, English males who were officers in land operations. Thus, any conclusions drawn about such subgroups must be made with caution.

The administration procedures and instructions to



respondents are outlined on pp. 12-13. I was impressed by the care taken to assure them on the matter of confidentiality and anonymity, particularly the revision (p. 13) to deal with certain individual concerns. One question here: were these initial data included in the results? This is not stated.

However, I have two reservations about administration and instructions. The surveys were given by local officers, who were themselves instructed in written form (although some units were visited as a check). It is difficult to evaluate the extent to which these officers followed procedures to the letter (a basic requirement of standardized testing). It might have been useful if they had had some other test administration experience (e.g. giving group intelligence tests), which I assume that they did not. Another concern, however, is the possibility that the presence of officers as administrators might itself have biased responses in unknown ways. As is clearly recognized by the author of the report, the issue being addressed is a sensitive one, and one wonders to what extent "experimenter effects" might have influenced the answers given. I recognize that the use of known officers might have elicited good cooperation, but would the results have been the same if the survey had been administered by a non-military researcher? It is difficult to tell. Nevertheless, honesty of response was emphasized in the test instructions, perhaps mitigating the criticism somewhat.

It is the combined effect of an officer administrator and

instructions that gives rise to most concern here. LGen. Manson's introductory letter to respondents clearly states why the Charter Task Force was formed:

"to make recommendations to the CDS concerning the change of CF policies so as to enhance individual rights and freedoms without adversely affecting the operational effectiveness of the CF".

He goes on:

"The information you give through this survey will provide the Charter Task Force with the essential scientific data they require to carry out their mission on this critical issue."

The instruction on p. 56 also state:

"The Charter Task Force has identified several areas in which survey research is immediately required to serve as a basis for policy recommendations..."

From p. 57 of the report, it is also clear that respondents knew that the survey as directed at CF personnel to find out what they think about employing homosexuals in the CF.

The problem here is this: To what extent were responses to the survey influenced by the knowledge that CF members were being questioned in order to obtain data about a potential (possibly likely?) change in policy towards homosexuals? It is impossible to tell, but the author himself notes on p. 32 that some people felt that the survey was a way of preparing them for

a change. He also observes that many felt that the results would not be used, perhaps implying that the change had already been decided upon. The overall results of the survey might have been different if it had been presented by a non-military person as part of a general population survey of opinion for research purposes. One possible way of checking on the accuracy of responses might be to compare the data on questions that have been used by other researchers. I shall return to this point. Of course, I recognize that disclosure of the purpose of the survey satisfies the requirement that subjects in research be fully informed; but this information could have been provided afterwards.

The 10 parts of the survey are then described. Part 1 gathers relevant facts, but I would have thought that it would be important to establish whether or not the respondent was married. This might be a factor in responding, particularly on questions about privacy, which may be less relevant to the married member if he or she lives with the family. Of course, if the vast majority of the CF members were unmarried, this omission may not alter the overall results. In Part 2, respondents were given past data on beliefs and actual estimates of the proportion of homosexuals in society. I do not understand why. It means that the present estimates do not simply reflect the opinions of the CF members. In addition, it might have been useful to employ data from this question to

provide an estimate of the validity of present responses. If the CF members gave a similar estimate of homosexuals in the general population as found in past research, it would increase our confidence in their other responses. However, the obtained estimate of the number of homosexuals in the CF itself is probably less contaminated. In Part 3, I think (as already mentioned) that there should have been a question on attrition, and it should have been directed at all respondents, particularly full-fledged CF members. Also, it would have been useful to list various reasons for joining, and to ask respondents to rank them in importance; the relative position of the homosexual issue could then be clearly seen. The more information here the better, because the author subsequently devotes half of his final discussion to the issue of recruiting. Parts 4 and 5 are the Halpern scales. It is not clear why the two changes (dropping of some questions, reducing response categories from 7 to 5) were made, but they probably do not pose serious validity threats to the scales. Parts 6 and 7 were constructed for this survey. My only comment here is that, in Part 6, questions 63 to 72, were worded either negatively ("would no more") or positively ("would"). This means that the derived harassment scales were all worded negatively, and are therefore unbalanced. On the other hand, the questions on security and on interference with work effectiveness were worded only positively. This may be a picky point, but usually items

are constructed to balance out question format (re: Ray's balanced F-scale). Also, I liked the comparative perspective introduced in Part 7, where a heterosexual question was included. I think that this technique could have been used more often in the survey, perhaps to obtain data on other minority groups. It is difficult to know if the present opinions are specific to homosexuals; they might represent attitudes towards all minorities. This point is implicitly recognized by including the F-scale (Part 8), but greater use of the technique might have provided another way in which to assess the specific validity of the homosexual opinions. Part 9 seems fine; again, I liked the inclusion of items dealing with quality of contact.

#### Evaluation of the Measures and Procedures (Method)

I think that the items could have been improved in a number of ways (married, estimates, attrition, changes, imbalance, comparative), but overall I judge the measures used to be adequate for the main purposes of the study. Also, the sampling was acceptable, particularly for inferences concerning the CF population as a whole. However, I have some concern about the personnel administering the survey and when this factor is combined with the specific instructions containing the purpose of the survey, I worry about the validity of the responses. I have no evidence that answers were contaminated, but the general effects of demand characteristics and the importance of expectations introduced by instructions are factors that are

considered important in research (Anastasi, 1988; Christensen, 1988).

### Results

Here, I divide my comments into two sections, corresponding to technical considerations and the results themselves.

Technical considerations. On pp. 17-18 and 112-115, a number of technical points are presented. On p. 17, it is mentioned that there were differences in opinions expressed by purely heterosexual and other respondents, but they were not described. I think that it would have been interesting to see these but, since the vast majority of the sample was purely heterosexual, this omission is not serious. Also, the description of the psychometric properties of the scales is satisfactory (p. 18, Annex E), as was the reason for dropping data from the F-scale - a personal disappointment to me, given my earlier remarks re: prejudice. The treatment of missing data and demographic data (p. 112) are acceptable, and I liked the two criteria for statistical significance (p. 113).

Although I accept most of the procedures used in the multiple regression analyses, I also have some concerns. First, I understand that four different such analyses were conducted, for each of the following predicted variables: outcomes (D1, D2), enrolment (D3), attitudes (D4, D5). In addition, there seems to have been another analysis for effectiveness (described on p. 27), but no table is provided as was done with the other

three. Second, it is not always clear to me which variables were entered as potential predictors. In fact, the only analysis in which the initial variables are specified clearly is that for attitudes (p.30). I cannot tell what was entered for the others. I was provided with one set of zero-order coefficients that pertained to the outcomes analysis, but even this did not inform me what was initially entered. Moreover, it is not clear whether the coefficients given are for male or female respondents. Third, although the attitudes analysis involved Parts 9 and 1 as predictors, the other three all involved attitudes as a predictor (along with other Parts). I am not an expert on multiple regression, but I suspect that it is problematic to arbitrarily designate a scale as both a predictor and a predicted variable. At the very least, the four analyses are not independent of each other. If something is to be predicted, a clear case should be made, and justification provided for the variables entered as predictors. Perhaps it is permissible to "data snoop", but it should be made clear that the overall analysis is an exploratory one.

Again with a rider for my inexperience in this area, I would suggest that a path analysis ought to have been conducted. With this technique, various initial variables such as quality of contact or sex could be specified, then linked to an intermediate variable such as attitudes, which in turn could be used to predict the other three (outcomes, effectiveness,



enrolment).

Obtained data. The first descriptive statistics refer to sexual orientation or, rather, to reported sexual orientation. I emphasize the latter because, as the author recognizes, some respondents may have been reluctant to be honest. In fact, I would like to have seen more frequent reminders that the data came from self-reports. As has already been noted, measured opinions may not correspond to behaviour, but measured opinion may not even correspond to true opinion. This is where honesty is crucial and where we require confidence that responses have not been contaminated in any systematic fashion.

Although the 11.8% (p. 18) estimate of homosexuals in the general population may have been contaminated, the estimate of 5.4% for the CF is interesting since it is close to the actual self-reports (3.5%). This lends some support to the validity of responses to the survey, as does the negative view of males towards male homosexuals (p. 20), since it also reflected a trend in the general population.

I noted a small slip on p. 20, where a mean score of 3.2 was reported (last line). It should be 3.0. Also, on p. 25, it is claimed that mental disorder is a factor predicting harassment of male and female homosexuals. However, this also seems to be an error, since Tables D1 And D2 do not list this as a predictor. Again, on p. 26, at the end of para. 51, it is stated that "...male heterosexual respondents disagreed...that



sexual desires would cause the same interference...." (emphasis mine). This statement should read "....would cause no more interference". See wording of question 68 on p. 75.

Of more importance is the reporting of the statistic that "80% of heterosexual male respondents .....felt the employment of homosexuals would decrease the effectiveness of the CF." (p. 26, para. 52). I presume that this number combines the "somewhat" and "greatly" response options, which I would have preferred to have seen separately. Indeed, I think that it would have been useful to have most of the data presented for inspection in this form, although I mention it here because this statistic is discussed later in the report (p. 36). Also, are we to assume that respondents spontaneously used the terms "morale" and "cohesion" (p. 26, para. 52) in the free comments?

With regard to the data on attitudes, I note that males were "moderately negative" (p. 28 and 3.7 in Table 5) on homosexuality as a mental disorder. This piece of information is difficult to interpret, particularly in view of the fact (pp. 31-32, free comments) that more respondents thought that homosexuality was a mental disorder rather than a lifestyle.

Also on p. 28 (para. 58), it is stated that

"The strongest negative attitudes were related to Personal Anxiety and Moral Reprobation indicating a a strong revulsion of direct or indirect contact with homosexual males and a strong belief that homosexuality is morally

wrong."

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I agree that the scores for Personal Anxiety (4.1) and for Moral Reprobation (3.9) were the highest for males (see Table 5). However, they correspond to the designation "moderate" (4), not "strong" (5). Moreover, emphasizing "highest" is misleading, since the scores are only 0.4 and 0.2 higher than 3.7, which the author already designated as "moderate" (see immediately above); also, these differences do not meet the statistically significant criterion of 0.5 of a scale unit (p. 113). Note also that the reference to Table 6 at the bottom of p. 28 should be to Table 5. Finally, I am not sure why the author suddenly deems it important to note a lack of Anglophone-Francophone differences at the bottom of p. 29; this contrast had not previously been given any weight, and it does not receive further emphasis in the report.

#### Evaluation of Results

I have noted a number of small slips in wording and one instance of a misleading description (strength of attitudes). I have also observed that it would have been useful to have more information on the number of cases in each response category. Overall, however the data presentation is adequate, and even provides two pieces of support for the validity of the responses.

Although the main purpose of the survey was to obtain descriptive data and predictions about behaviour, the

specification of determinants of both reactions and attitudes, was a secondary goal. In addition, for me, the latter has potential importance for the discussion, where interpretations were made and conclusions drawn. Unfortunately, I think that the presentation of the multiple regression analyses is lacking in detail and that the conduct of four separate analyses may be questionable. My suggestion is a path analysis, which might reveal more insight into the pattern of determinants.

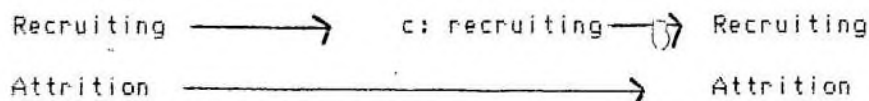
### Discussion

The discussion is divided into two sections: cohesion and morale, then recruiting and retention. I will comment briefly on this framework, then discuss each section.

I have already registered my disappointment that the original concepts set out in the introduction (cohesion, morale, discipline, confidence in leadership) were not operationally defined.

By dividing the discussion in this fashion, the author seems to imply the following links between original concepts, six goals and variables measured:

<u>Concept</u>	<u>Goal</u>	<u>Variable</u>
Cohesion	a: reactions	Privacy, will, to work, blood, harassment
Morale		
Discipline		
Confidence in leadership	b: effectiveness	effectiveness
	d: attitudes	attitudes



That is, the first four concepts are related to goals a, b and d and to the first six variables, and the last two concepts are linked to goal c and to the last two variables. I present this to demonstrate my understanding of the operationalization of the original concepts; I still believe the author should have been clearer in this regard. Given this ambiguity, the author might have kept simply to a description of the data, without referring to these higher-order constructs. I think that the implications of the study for operational effectiveness could have been discussed just as easily.

Cohesion and morale. On pp. 35-36, it is claimed that homosexuals are likely to hamper "team spirit". This may be so, but that variable was also not operationalized here. However, the author goes on (p. 36) to correctly point out the potential for disruption from the data on privacy. I would prefer that his language had been less categorical ("would certainly be a disruptive force" on p. 36 might have been "could be"), but the broad thrust of his argument seems reasonable.

It is then pointed out that "Fully 80% of the male.... heterosexual respondents ...felt the presence of homosexuals would decrease the effectiveness of the CF". I do not deny this result, but repeat my interest in knowing the response breakdown here, particularly since it is categorically claimed (bottom of

p. 36) that "both cohesion and morale would (my emphasis) be affected adversely by the members' loss of confidence in the effectiveness of the organization".

On p. 37, it is stated that "Known homosexuals would be verbally and physically harassed by heterosexual members". In view of the some of the free comments, I do not doubt this likelihood, but I again urge some caution in the use of such categorical terminology (see also "will lead" and "will not have" on the same page). On the 5-point scale, there was "moderate" not "strong" agreement here. I do agree that these facts imply an extra burden on leaders, but it is not clear how much extra. Also, it could be argued that it is part of the leader's role to mediate potential conflict. However, I also agree that the data suggesting that members would not be willing to take orders from known homosexuals could provide trouble for operational effectiveness.

I found the argument in para. 75 (p. 38) difficult to follow. If the first sentence is recognizing that the instructions given to respondents re: policy change may have had an impact on the validity of the data obtained in the survey, I cannot but agree. Secondly, at the end of the page, he states "without an exclusion policy these personnel problems would not easily be rectified". I have trouble with this point. It implies that, with an exclusion policy, certain problems can be rectified, although it is not stated how. However, if this is

() so, and if many homosexuals would remain discreet (para. 75), why might the same procedures not work? By the way, I assume that the "not" related to this point on p. 39 (para. 76) is a mistake and should be omitted.

Finally, in this section of the discussion dealing with cohesion and morale, I accept the point (p. 39, para. 75) that small reductions in operational effectiveness may be important. In fact, such a possibility is one of the reasons that I would have preferred the author to stick to such variables in his discussion, because they were measured directly in the survey.

Recruiting and retention. It is stated (p. 39, para. 77) that "results of this study predict that such a policy (i.e. enrolment of homosexuals) would negatively affect recruiting directly and indirectly".

"Negatively" is a loaded term, which the author may or may not have used intentionally. The essential point is that the results suggest a lower number of recruits. Whether this is bad or good is a separate issue, which can be judged on such considerations as whether the drop is significant in size and whether the quality of the lost members is strong or weak. We can comment on the first of these points; the second is largely a matter of conjecture.

The author argues that opening the door to homosexuals would increase the available pool of recruits, but probably not by much, since many homosexuals may not be attracted to the

military. However, he does not emphasize one implication of this: if very few homosexuals would actually join, their impact might be very small, especially if they continued to be discreet. It should be remembered that the survey suggests that there are already a small number in the CF. Unfortunately, members were not quizzed about any personal CF experience in this regard.

On p. 40, para. 79, it is stated that "27% of ..candidates would not (my emphasis) have joined the CF" with a policy to enrol homosexuals. This is not strictly accurate. The results showed that 11% reported that they would have been very unlikely and 17% that they would have been somewhat unlikely to have joined. Nevertheless, I agree that that a certain number probably would not join; I cannot tell if this number is "significant" (p. 40, para. 79). Given the characteristics (pp. 40-41) of those unlikely to join, however, it should be considered how serious their loss would be. A judgement about the impact of loss of recruits should not be based solely on their number. In addition, the author's arguments concerning the characteristics of those lost contains an error. He mentions "loss of privacy" as a concern (p. 40, para. 79), but this is only true for female respondents; it did not appear as a predictor of enrolment for males (see Table D3, p. 119).

On p. 41, the author writes: "..heterosexual CF servicemenbers felt that employing homosexuals in the CF would



3 dramatically (my emphasis) reduce the effectiveness of the CF..." Again, I question the characterization of the data here. The results were that 80% reported that effectiveness would be reduced, but no breakdown of this figure into the response categories of "somewhat" and "greatly" decrease was given. I do agree, however, that the author's overall conclusion that removal of the exclusionary policy would be likely to reduce perceived effectiveness in the CF in the eyes of many CF members and of some of the public.

On pp.42, we find that figures concerning recommendations taken from a national poll (34% males, 22% females) are said to be similar to results obtained here. However, the present overall result is lacking; it was only stated (p. 28) that between 18% and 56% would be less likely to recommend joining the CF. Nevertheless, I agree that the number more likely to recommend joining was almost identical and very small (1-2%), so that the present results are similar to those from the national poll. This fact adds additional support (see my p. 16) to the validity of the respondents' replies. At the same time, it may be premature to claim that a new homosexual policy "will have a strong negative impact on recruiting" (pp.42-43). Apart from my reservation about the loaded term, I find it difficult to translate the above data into a quantified statement about the size of the impact.

On p. 43, the author recognizes the possibility, already



noted in my introduction, that potential recruits (presumably from the general population) might have been polled. I think that he gives a reasonable defence of the choice of respondents here, although a further study of the opinions of the general population would still be useful. I found some difficulty understanding the last sentence in para. 83, and wonder if the word "than" in the last line should have been "then".

Finally, it is observed at the end of p. 43 that "there is also some evidence that allowing homosexuals in the CF will cause some current members to leave the CF". Perhaps so. However, this conclusion is based on free comments; no specific question to this effect was asked.

#### Evaluation of Discussion

As a result of my difficulties with the operational definitions, I am hesitant to agree that cohesion and morale will decline if homosexuals are enrolled. In addition, I take issue with a number of categorical statements, which I would have expressed more tentatively, and with such judgements as "significant", "negative" and "dramatic".

However, I agree that the survey shows that CF members express concern about privacy, harassment, taking orders, and that they report that perceived effectiveness would be likely to fall. Also, I agree that small declines in operational effectiveness might have serious consequences. In addition, I accept the conclusion that there is likely to be a decline in

the absolute number of recruits.

At the same time, I also present some alternative interpretations of the data pertaining to the role of leaders, size of the impact from enrolling homosexuals, and implications for effectiveness from the loss of certain potential recruits.

Finally, the goal to estimate attrition has not been met, since the key question was not posed to enrolled CF members.

On pp. 44-45, the final summary and conclusions are presented. On the basis of my analysis, I would have expressed them as follows:

- 86a. There is evidence of concern for privacy from homosexuals.
- b. Many servicemembers report being unwilling to work with known homosexuals, particularly when supervised.
- c. CF members report that harassment is likely.
- d. There is some concern about disease and taking blood.
- e. Members think that there is likely to be a decline in perceived effectiveness.
- 87a. The recruit pool would be potentially expanded by 10%.
- b. There is likely to be a reduction in the number of heterosexual recruits.
- c. Recommendations to join would probably decline.

Note that I have avoided the following terms and phrases:  
cohesion, morale, CF image, bonding, team-spirit, confidence in leadership eroded, would cause personnel problems, serious

threat to military effectiveness. I find them too categorical or inadequately linked to the measured variables.

#### Overall Evaluation and Commentary

##### Theoretical Rationale

The review of background information is adequate, and provides a summary of previous evidence and of determinants of attitudes towards homosexuals. Also, the attitude-behavior link was mentioned. However, initial military concepts were not clearly defined operationally, and the behaviour-attitude link was not considered.

##### Adequacy of Measures, Methods and Results

I found that there was room for some improvement in the measures used, but that they were satisfactory overall. Also, sampling was acceptable, particularly for data based on each sex considered separately. However, I pointed out that there were potential difficulties with officer administrators and the instructions given. It is possible that the reports may have overestimated perceived problems with homosexual enrolees, since respondents were told that there may be a policy change, and that their data would contribute to such recommendations. This fact contributes to my concern about the categorical fashion in which some conclusions were expressed. At the same time, I note three pieces of data in the survey which correspond to findings from other sources, thereby offering some support for the validity of the present responses.

I was also critical of the multiple regression methods, and suggested that a path analysis might have been more revealing. However, having noted some slips in the presentation of results, some lack of detail on certain facts, and with further caution regarding the characterization of some findings, I concluded that the results section was acceptable overall.

#### The Tenability of the Conclusions

Given my previous concerns, I summarized the main facts in language that was more tentative than the author's, and avoided reference to the higher-order concepts such as cohesion. The degree to which the findings can be used to predict actual behaviour in the CF if homosexuals were enrolled hinges on two factors: the extent to which they may have overestimated actual problems, and the extent to the present opinions are likely to correspond to real life. In the latter case, I suggest that there are three grounds for optimism: the context of the survey and of actual behaviour are similar; some of the opinions related to specific behaviour; and 60% of the sample claimed to have had some contact with homosexuals. On the other hand, I also think that the initial instructions may have permitted an exaggerated estimate of problems.

Second, despite my criticisms of the multiple regression analyses, I was sorry that the final summary and conclusions made no attempt to include them. In fact, they provide some grounds for the conclusion that members' views on outcomes can

be related to their attitudes, particularly personal anxiety, which appeared as a predictor for privacy, willingness to work, harassment and perceived effectiveness. In addition, there was considerable evidence that relations (quality of personal contact) predicted attitudes. This suggests an interesting causal path among the three variables.

Final Comment

It seems to me that there is an underlying assumption in the report that attitudes and, consequently future behaviour, would be unlikely to change. This could be true or false. For example, is it the case that there have been hostile attitudes towards women in the military? Could many of the present concerns also have been applied to women? Has the introduction of women changed such attitudes? The point is that there is evidence that changes in behaviour can alter attitudes. In a widely-cited study, Sherif (1956) showed that hostility between groups could be broken down when they were forced to cooperate to complete a task. He writes:

"What our limited experiments have shown is that the possibilities for achieving harmony are greatly enhanced when groups are brought together to work towards common ends. Then favourable information about a disliked group is seen in a new light, and leaders are in a position to take bolder steps toward cooperation. In short, hostility gives way when groups pull together to achieve

overriding goals which are real and compelling to all concerned."

On p. 35 of his report, the author emphasizes the importance of superordinate goals in the military. Moreover, he also notes a belief (p. 5, 32, 40) that homosexuals do not lack ability. One could argue that, if steps were taken to ensure that heterosexual and homosexual members worked jointly towards the solution of tasks, then many of the concerns expressed by the CF respondents might be mitigated.

I cannot conclude that this would happen. However, the analyses are consistent with the possibility that good contact could lead to lower personal anxiety, and more favourable views on outcomes.

As the author himself observes at the end of his report: "It remains for the Charter Task Force to evaluate these findings along with evidence from other sources before a recommendation can be made trying to balance the enhancement of individual rights and the operational effectiveness of the CF."

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