
SEXUAL ASSAULT AND SEXUAL HARASSMENT IN THE U.S. MILITARY

Volume 4. Investigations of Potential Bias
in Estimates from the 2014 RAND
Military Workplace Study

Andrew R. Morral, Kristie L. Gore, Terry L. Schell, editors

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Preface

The Sexual Assault Prevention and Response Office within the Office of the Secretary of Defense selected the RAND Corporation to provide a new and independent evaluation of sexual assault, sexual harassment, and gender discrimination across the U.S. military. As such, the Department of Defense (DoD) asked the RAND research team to redesign the approach used in previous DoD surveys, if changes would improve the accuracy and validity of the survey results for estimating the prevalence of sexual crimes and violations. In the summer of 2014, RAND fielded a new survey as part of the RAND Military Workplace Study.

This report, Volume 4 in our series, contains discussion of methodological studies of nonresponse bias, total survey error, the effects of our sample weighting methods, analyses of survey breakoff, and other studies of the precision and validity of the survey and its results. The complete series that collectively describes the study methodology and its findings includes the following reports:

- *Sexual Assault and Sexual Harassment in the U.S. Military: Top-Line Estimates for Active-Duty Service Members from the 2014 RAND Military Workplace Study*
- *Sexual Assault and Sexual Harassment in the U.S. Military: Top-Line Estimates for Active-Duty Coast Guard Members from the 2014 RAND Military Workplace Study*
- *Sexual Assault and Sexual Harassment in the U.S. Military: Volume 1. Design of the 2014 RAND Military Workplace Study*
- *Sexual Assault and Sexual Harassment in the U.S. Military: Volume 2. Estimates for Department of Defense Service Members from the 2014 RAND Military Workplace Study*
- *Sexual Assault and Sexual Harassment in the U.S. Military: Annex to Volume 2. Tabular Results from the 2014 RAND Military Workplace Study for Department of Defense Service Members*
- *Sexual Assault and Sexual Harassment in the U.S. Military: Volume 3. Estimates for Coast Guard Service Members from the 2014 RAND Military Workplace Study*
- *Sexual Assault and Sexual Harassment in the U.S. Military: Annex to Volume 3. Tabular Results from the 2014 RAND Military Workplace Study for Coast Guard Service Members*

- *Sexual Assault and Sexual Harassment in the U.S. Military: Volume 4. Investigations of Potential Bias in Estimates from the 2014 RAND Military Workplace Study.*

These reports are available online at: www.rand.org/surveys/rmws.

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Summary

In the summer of 2014, the RAND Military Workplace Study (RMWS) survey was fielded to more than half a million members of the U.S. military, including active and reserve component members from each Department of Defense (DoD) service and the U.S. Coast Guard. The primary objective of the survey was to establish valid estimates for the prevalence of sexual assault and sexual harassment in the U.S. military, and to characterize the nature and circumstances of these crimes and violations of military equal opportunity regulations. The survey questionnaire and many of the methods used in the RMWS were substantially revised from those used in the Workplace and Gender Relations Survey of Active Duty Members (WGRA) and the Workplace and Gender Relations Survey of Reserve Component Members (WGRR) that were previously used by DoD to assess sexual assault and harassment.

The 2014 RMWS was the largest such study ever conducted. With more than 145,300 survey responses from active-component members, the prevalence estimates generated from the study frequently had 95-percent confidence intervals that spanned less than one-half a percentage point, suggesting extraordinary precision. However, these confidence intervals, which only assess the uncertainty due to random sampling variability, could be misleading. Sampling variability is unlikely to be the primary source of error for our estimates. Instead, larger errors could result from several factors, including specification errors, if, for instance, our sexual assault screening module misclassifies individuals; coverage errors, due to the inclusion criteria used in the sample frame; and survey nonresponse, if our sample weights fail to fully adjust for important differences between those who chose to participate in the study and those who did not.

The goal of this volume, the fourth volume of the *Sexual Assault and Sexual Harassment in the U.S. Military* series, is to examine the influence and magnitude of these less-easily quantified sources of error that may affect the study's results (see Volumes 1, 2, and 3 for details of the study design and results).

The Sexual Assault and Sexual Harassment Experiences of Survey Nonrespondents

If survey nonrespondents have different sexual assault or sexual harassment experiences than our weighted sample of respondents, it would result in biased survey estimates. This possibility is a particular concern for surveys like the WGRA and most other surveys of military populations which have, for many years, had response rates below 50 percent. To investigate how nonrespondents differed from respondents, we conducted follow-up studies of the sexual harassment and sexual assault experiences of three samples of service members who were nonrespondents in the main study. All nonrespondents were randomly assigned to one of three follow-up methods: (1) those who were recruited by phone for a phone interview; (2) those who were recruited by mail for a self-administered paper survey; and (3) those who were given additional time to complete the survey on the web, but were not subject to additional recruitment efforts beyond those of the main study. Across these three follow-up studies of RMWS nonrespondents, more than 6,500 members were assessed for sexual assault, sexual harassment, and gender discrimination experiences. With each follow-up study, we examined differences in the sexual assault and harassment experiences of RMWS nonrespondents compared with matched samples of respondents from the main RMWS survey, and evaluated whether our nonresponse weighting methods properly accounted for these differences.

The results of these studies identified no consistent pattern of nonresponse bias. We did find that nonrespondents who were asked about their sexual assault and harassment histories over the phone acknowledged sexual assault and harassment experiences less often than RMWS survey respondents, even after weighting adjustments. However, this apparent difference may result from well-documented survey mode effects, in which respondents speaking with live interviewers tend to underreport sensitive or stigmatized experiences.

Because of this response bias, results from the sample of nonrespondents randomly assigned to the mailed survey condition likely offer a more valid estimate of the experiences of nonrespondents. This study found no significant evidence of nonresponse bias on sexual assault and harassment outcomes after sample weighting. For gender discrimination, a small possible bias was detected after weighting, suggesting that the RMWS prevalence estimates may be too low. The third study, of late web respondents, suggested that if there is a bias in the RMWS estimates, it is a downward bias. That is, the RMWS estimates of all three primary outcomes may be lower than the true population rates.

Efficacy of RMWS Nonresponse Weights

The nonresponse weighting methods used for the RMWS are novel and allowed us to account for differences in a wider range of known characteristics of respondents and nonrespondents than has previously been possible without unacceptably elevating the variance of survey estimates. In addition to accounting for key service and demographic characteristics that have previously been included in past WGRA sample weights, we included many more demographic, military service, environment, and fieldwork metadata characteristics.

In analyses comparing sample weights generated using our new method with the earlier WGRA weighting approach, we found that the new weights reduced differences between the analytic sample and the population on a wide range of factors associated with both nonresponse and key outcomes that were not satisfactorily addressed using the earlier methods. Moreover, nearly all of these factors drove bias in the same direction—specifically, service members with characteristics associated with a higher risk of sexual assault also had the lowest likelihood of responding to the survey. Therefore, we can be confident that the differences in survey estimates resulting from the RMWS weights, in contrast to those generated using the traditional WGRA weighting method, reflect a reduction in nonresponse bias as they adjust the prevalence estimates upward, thus providing more-accurate estimates of actual prevalence.

Finally, these reductions in bias were achieved with only modest inflation of variance in the survey estimates. Whereas the overall design effect associated with the traditional WGRA weights was 2.62, the RMWS weights produced a design effect about 40 percent larger (3.69). An assessment of the trade-off between bias and variance suggested that any increase in variance was offset by reduction in bias. Furthermore, we would argue that for a survey with such a large sample size and extraordinary precision, a small loss of precision associated with the increased variance due to weighting is justified by the bias reduction that we achieved.

Assessment of Total Survey Error Using an Administrative Records Benchmark

If our sampling, measurement, weighting, and analysis methods performed correctly, the true rates of sexual assault and sexual harassment in the military should lie within the confidence interval of our estimated rates. In reality, we cannot observe the true values for these rates. However, we included one question on the survey that does have an objective, observable benchmark value maintained in the administrative records of the Sexual Assault Prevention and Response Office (SAPRO). Specifically, we asked those who qualified as having experienced a sexual assault in the past year whether they completed a DD2910 Victim Reporting Preference form. All victims of sexual assault

who come to the attention of a mandated reporter at DoD are asked to complete this form to indicate how they want their case handled. Thus, the comparison of our survey's estimate of the number of DD2910 forms completed in the past year with the actual number found in SAPRO's records offers a test of total survey error, not only nonresponse bias, but also error due to sampling variability, sample coverage, specification errors associated with misclassifying a respondent's past-year sexual assault experiences, computational, and other types of error.

Our weighted survey estimate of 2,435 forms filed during the year underestimates the 2,997 forms actually recorded by SAPRO. Thus, the estimated value is 81 percent of the true value, which we view as a relatively small error in light of the wide range of factors that could contribute to this discrepancy. As with the results from our studies of nonrespondents, the conclusion about total survey error from this analysis suggests that bias in our estimates is relatively small and it is in a direction that implies the RMWS estimates of prevalence are lower than the true rates in the population.

The precise causes of the discrepancy cannot be fully identified. However, one source that is not actually survey error may contribute. The SAPRO count of DD2910 forms assesses the number of incidents that were reported, while the survey estimate counts the prevalence, i.e., the number of individuals reporting one or more incidents. A more accurate assessment of survey error would exclude this double counting from the official records. In addition, one known source of survey error is the sample frame exclusion criteria, which are discussed below. Specifically, the survey excludes service members who left the military prior to sampling, as well as those who joined in the prior six months. Any Victim Reporting Preference statements filed by such individuals will not be counted in the survey estimate, which partially explains the observed survey error.

Undercounting and Overcounting of Sexual Assault in the RMWS

Our sampling plan, survey questionnaire, and analysis plan all required judgments that affected how we counted incidents and the prevalence of sexual assault. We examined several such key decisions to evaluate how much effect they may have had on our survey estimates. For instance, because we asked about past-year sexual assault experiences and included service members with just six months of service, it is possible that some of their past-year sexual assaults occurred before they joined the military. We found, however, that 98 percent of the assaults against service members with fewer than 12 months of active-duty service were committed in a military setting, during training, or by another member of the service. If we assume that the remaining 2 percent of the assaults occurred before the service member joined the military, the effect of excluding these cases from the overall estimates is small, suggesting our population estimate of 20,300 members experiencing sexual assaults in the past year could be an overestimate by approximately ten members.

On the other hand, the fact that we excluded service members with fewer than six months of service means we failed to count any assaults against them. Using the experiences of those members with six to 12 months of service to set bounds on the likely magnitude of underreporting attributable to the exclusion of those with fewer than six months of service, we estimated that it could cause our estimate of 20,300 active-duty members exposed to sexual assaults in the past year to be underestimated by 25 to 190 people.

A more significant underestimate may result from the exclusion of members who experienced sexual assaults in past year that separated from the service before our sample frame was drawn. Using reasonable assumptions about rates of sexual assault among those who separate from the military, we found that correcting for the omission of these cases would cause our estimate of 20,300 active-duty members experiencing sexual assaults in the past year to increase by 900 to 2,800 members or more.

We counted as sexual assaults some events about which the expert legal opinions we received were strongly divided. For instance, if a person said they had an unwanted sexual experience, but could not recall the details of the event because of the effects of too much alcohol, we counted such events as having occurred without consent. Similarly, if none of the offender coercion behaviors occurred, but the respondent indicated they could not consent because they were frozen in fear, we counted this as meeting the criteria for sexual assault as well. The net effect of these decisions was very small. Had we excluded all such cases from our estimates of the prevalence of past-year sexual assaults, our estimate that 20,300 service members were sexually assaulted in the past year would have been reduced by about 50 members.

As specified in Article 120 of the Uniform Code of Military Justice (UCMJ), we included the mouth as one of the orifices that, when violated, would be counted as a penetrative assault. Some reviewers suggested that such offenses may involve unwanted kissing with the tongue. While this is clearly the intent of the law, we assessed how our estimates of sexual assault would change if all penetrations of the mouth that involved something other than a penis were excluded, and our population estimate of 20,300 service members experiencing sexual assaults in the past year would decline by about 250 members.

Differences in the Events Counted Using RAND and WGRA Questions

Whereas most survey respondents received a version of the new RAND survey form (the RAND form), 29,541 respondents were randomly assigned to a questionnaire that included the sexual harassment and unwanted sexual contact questions used in earlier administrations of the WGRA survey (the prior form). This survey design allowed us to compare estimates derived from the RAND form with those from the prior form, providing a direct comparison of the types of events counted and not counted by each measurement approach.

Sexual Assault

The RAND form and the prior form used different approaches to establishing that counted sexual assaults occurred in the past year. Both forms, however, included an item at the end of the sexual assault (or unwanted sexual contact) module asking how confident respondents were that the event occurred in the past 12 months. Of those who received the RAND form, 6.8 percent responded by saying they were sure the event actually occurred more than a year ago (i.e., should not be counted as a past-year event), compared with 23.0 percent of those receiving the prior form. All who indicated they were sure that no sexual assault occurred in the past year were excluded from the prevalence estimates generated from the RAND form, but—consistent with prior methods—these cases were included in the prior form prevalence estimates. We present analyses demonstrating that the past-year prevalence estimate generated by the prior form is likely overestimated by 20 percent due to the inclusion of service members whose most recent unwanted sexual contact occurred more than a year earlier.

Similarly, we demonstrate that the prior form identifies only about one-half as many service members who experienced penetrative assaults as the RAND form (4,200 versus 7,800), an effect particularly acute for male service members among whom the RAND form identifies three times as many experiencing penetrative assaults (1,200 versus 3,700). We interpret these differences as likely attributable to the RAND form identifying more sexual assaults that occur in the context of hazing or that are not perceived as sexual by the service member.

On the other hand, the prevalence of unwanted sexual contacts that are not penetrative (assessed on the prior form) is substantially higher than the prevalence of nonpenetrative sexual assault (assessed on the RAND form). Indeed, the prior form counts 5,600 more individuals in this category than the RAND form. If the experiences of these individuals does not, in fact, meet the criteria for a UCMJ sexual assault, as suggested by the fact that the RAND form did not identify a similar proportion on nonpenetrative assault victims, this would suggest that 25 percent of all unwanted sexual contacts counted using the prior form were not, in fact, crimes. Many may not have met even the criteria for unwanted sexual contact, as we find that 18 percent of all those with “one event” on the prior form that is not penetrative positively affirm that their unwanted sexual contact experience met none of the behavioral descriptions defining unwanted sexual contact.

Sexual Harassment

The RAND sexual harassment module, unlike the WGRA sexual harassment module, does not require respondents to know the definition of sexual harassment and correctly apply it to their experiences in order to have those experiences counted as sexual harassment. This WGRA “labeling” requirement proves to significantly reduce estimates of sexual harassment prevalence. Indeed, if the RAND form required correct labeling to count instances of sexual harassment, our overall prevalence rate for past-year sexual

harassment would have fallen by 30 percent and rates for men would have fallen by 50 percent.

Another difference between the forms is that the RAND form, unlike the prior form, includes unwanted sexual touching by a coworker as one type of event that could be classified as sexual harassment (even if it also qualifies as a sexual assault). In practice, however, we find that this difference does not lead to meaningful differences in prevalence estimates.

When we adjusted the RAND form past-year sexual harassment prevalence rates to match the criteria used in the prior form (implementing the “labeling” requirement, excluding sexual touching and other adjustments), we found that the RAND form identified fewer cases of sexual harassment against women and comparable numbers for men compared to the prior form.

The fact that the over- and undercounts described here for the prior form approximately cancel one another out should not be taken as evidence that the prior form and RAND form provide equivalent results or are equally satisfactory measures of sexual offenses. For purposes of tracking the effectiveness of DoD policies or for estimating the total number of offenses occurring against service men and women, measures must accurately and precisely count people or events that are the target of training, prevention, or other policies or programs.

Sample Attrition and Breakoff Across the Survey Instrument

When respondents stop answering questions, or “break off,” before reaching the last question in the survey, it reduces the precision with which questions later in the survey are measured. It also can introduce nonresponse bias on later survey questions if respondents who break off differ systematically from those who do not in ways that are not addressed by sample weighting. We examined patterns of survey breakoff on the RAND form and provide an innovative analysis of the sample characteristics of those who make it through each step in the survey—from accessing the web form, to completing the informed consent, to answering each question presented to them.

Results of this analysis show that survey breakoff before the sexual assault module (which was required for the survey to be counted as completed) was less than 4 percent of those who began the RAND forms. This compares favorably to the 2012 WGRA instrument, in which 13.9 percent broke off prior to the mandatory item assessing unwanted sexual contact, and the prior form used in the current study, which had 6.5 percent breakoff before the unwanted sexual contact item.

We found little evidence that overall nonresponse was a reaction to the survey content. Only a small proportion of study nonrespondents dropped out after being directly informed about the survey content or seeing the survey questions; those who dropped out after that point did not, on average, have characteristics that put them at

higher or lower predicted risk for sexual assault than those who completed the survey. The primary difference in predicted risk between respondents and the intended sample was due to those who never answered a single survey question, rather than those who broke off after beginning the survey.

Survey Complaints and the Costs and Benefits of Using Survey Language Some Respondents Find Offensive or Distressing

Our use of behaviorally and anatomically specific language in the sexual assault module offended some service members, was distressing for some victims of sexual assault, and raised questions among DoD leadership about whether the problems the language created (including stories in the press questioning DoD's approval of the survey) were justified by the benefits of using precise language.

We examine data we collected on complaints about survey language as a proxy to understand who was offended and the likely harms associated with participating in the survey. We found that the RAND form, with 122 complaints per 100,000 completed surveys, was far more likely to trigger survey language complaints than was the prior form. The offense experienced was not sufficiently severe or widespread, however, to cause a surge of breakoffs during the sexual assault screening module, where the language drawing complaints occurred. Indeed, more respondents broke off in the uncontroversial sexual harassment screening module than in the sexual assault screening module, and breakoffs overall were similar for the combined RAND forms and prior form (which drew few language complaints).

Participants with characteristics putting them at the lowest risk for sexual assault tended to object to the survey language at the highest rates. These include men, officers, and those with more-senior pay grades. Possibly, the risk of assault feels so remote to these respondents that the inconvenience of being asked questions about whether they themselves have experienced such violations outweighs any benefits they can imagine the survey producing. Alternatively, perhaps these groups are more likely to express their complaints about any topic, possibly because they are representing the views of, or complaints from, their subordinates. In that case, their higher complaint rates would have nothing to do with their lower risk of sexual assault.

Obtaining accurate data on the proportion of service members who are sexually assaulted each year is critical for sound policy on sexual assault prevention and response. Sexual assault is a technical legal construct that is defined in the UCMJ using anatomically and behaviorally specific language. To accurately identify events that meet the UCMJ definitions, similarly specific language must be used in surveys. This approach is widely used in sexual assault research in civilian populations, and it is the approach recommended by the National Research Council for surveying sexual assault experiences (National Research Council, 2014).

Conclusions

Our investigations of a range of possible sources of error found no conclusive evidence of substantial bias or error in the previously reported RMWS estimates. However, there was a general pattern across these investigations, suggesting that our primary RMWS study estimates of sexual assault, sexual harassment, and gender discrimination are more likely to be underestimates than overestimates of true population values. In particular, three types of evidence suggest that the survey estimates could underestimate the true values: (1) the nonresponse follow-up studies, (2) the analysis of individuals excluded from the sample frame, and (3) the comparison between survey estimates of officially reported sexual assaults to the number of actual reports.

In contrast, we found little evidence that the study was overcounting these outcomes. For example, although we concluded that a small number of pre-service sexual assaults may be captured in our estimates, this number is almost certainly lower than the larger number of assaults that go uncounted because we excluded members with fewer than six months of service and those who left the military shortly before the survey fielded. Similarly, our analyses of the performance of the sexual assault and sexual harassment modules provides no indication that more incidents were counted as crimes or violations than should have been.

Our conclusion that the study is more likely to underestimate than overestimate the true values is stronger for the estimated counts of individuals who experienced these violations (e.g., 20,300 service members experienced a sexual assault in the past year) than for the estimated prevalence of these crimes (e.g., 1.5 percent of service members experienced a sexual assault in the past year). This is because the strongest evidence for bias comes from the fact that the survey sample frame clearly excluded some individuals who served in the military in the past year and who may have experienced these outcomes (e.g., members who separated before the sample was drawn).

Therefore, even if our rate estimates are unbiased, when we multiply them by the total number of members represented by the sample frame, we know the product will underestimate the total number of service members sexually assaulted in the past year because the sample frame excludes some members who were exposed to this risk. This source of bias may explain a substantial proportion of the total survey error identified by our comparison of survey-estimated counts of reported sexual assault to official reports of sexual assault (see Chapter Four). In contrast, evidence of bias in the estimated prevalence of sexual assault, sexual harassment, and gender discrimination is weaker; the incomplete coverage of the sample frame necessarily has smaller effects on prevalence rates than on population counts. The three nonresponse follow-up studies (Chapter Two) provide some limited evidence that the reported prevalence underestimates the true value. However, those effects were descriptively small, and were not consistent across follow-up methods.

In addition to the previously reported RMWS estimates, the RAND study also replicated WGRA methods to produce time-trend data using the same measurement and weighting methods as WGRA surveys conducted in past years. The current investigations of bias provide stronger evidence that the WGRA methods underestimated the true rate of sexual assault. In particular, the analysis of nonresponses weights found that the WGRA system of weights resulted in the underrepresentation of a number of groups of service members who have a high risk for sexual assault and harassment. In addition, the prior form identified substantially fewer penetrative sexual assaults than the RMWS, particularly among men. However, this classification error was partially offset by telescoping errors, or the tendency of respondents to report events as having occurred more recently than they actually did. These errors result in a substantial proportion (23 percent) of respondents being counted as experiencing unwanted sexual contact in the past year, when they later admitted that their last such experience occurred more than 12 months before the survey.

Recommendations

The findings described in this report and our experiences conducting the 2014 RMWS support several recommendations for future WGRA surveys and the analysis of the data they collect.

- *Measurement approach.* Future WGRA surveys should use the RMWS measurement approach, or comparable survey questions that use behaviorally and anatomically specific language to clearly define criminal sexual assault and violations of equal opportunity law and policy. In future WGRA surveys, the Defense Manpower Data Center (DMDC) should consider supplementing the RMWS measure of gender discrimination with additional questions to establish (a) whether the discrimination was legally mandated by the service (e.g., the exclusion of women from combat occupations), (b) the specific nature of the career harm suffered, and (c) the evidence that a coworker's gender biases harmed a service member's career.
- *Sample frame.* Omitting service members who recently separated from the military could lead to significant bias in estimates of past-year sexual assaults, sexual harassment, and gender discrimination. As a result, we recommend including past-year separations in the sample frame of future WGRA surveys, or developing analytic approaches for estimating the number of crimes and violations those who separated experienced in the past year. Minimally, separations that occur after the WGRA sample frame is drawn should not be counted as ineligible, as has been the practice in earlier versions of the WGRA. In addition, because recent separations appear to have elevated risk of past-year sexual assaults, sexual harassment,

and gender discrimination, the Office of the Secretary of Defense (OSD) should evaluate (1) what effect such violations have on military careers and retention and (2) whether making an official report or receipt of available services reduces the separation rates of service members who have been sexually assaulted, harassed, or discriminated against.

- *Sampling plan.* DMDC should design future surveys to include sufficient numbers of men in the sample to ensure ongoing assessment of the nature of sexual assaults against them. In practice, this means large sample surveys that may not oversample women at rates as great as in the RMWS or previous WGRA studies. This can be done without reducing the precision of women's estimates below those of men.
- *Sample weighting.* DMDC should build on approaches developed for the RMWS to include a wider set of factors in future nonresponse weighting models than has previously been possible for military surveys like the WGRA.
- *Improving response rates.* OSD, the Defense Information Systems Agency, and the services should collaborate to improve the coverage and reliability of email contact information in the personnel systems used for survey recruitment. Also, DMDC should investigate additional modes of recruitment (phone or text message) that improve outreach to members who do not routinely use email as part of their military duties.
- *Further study of nonresponse bias in future surveys.* In future administrations of the WGRA, DMDC should continue to compare survey estimates with actual numbers of filed Victim Reporting Preference forms as a measure of nonresponse bias and total survey error more generally. The procedure we used could be further refined to better match the survey's sample frame with the victim preference statements counted in the SAPRO database.
- *Survey frequency.* OSD should conduct the survey no more frequently than once every two to four years.

Acknowledgments

For this methodological volume of the *Sexual Assault and Sexual Harassment in the U.S. Military* series, we wish to highlight the invaluable methodological guidance and support we received from the members of our scientific advisory board. We are also grateful for the expert advice provided to us by the Defense Manpower Data Center (DMDC), especially Elizabeth Van Winkle, who shared DMDC experience from prior administrations of the Workplace and Gender Relations surveys and who served as a liaison between RAND and other parts of DMDC.

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Finally, we again want to acknowledge the service men and women who took the time to complete the RAND Military Workplace Study survey and share their experiences, even when those experiences were painful to recount.

Introduction

Andrew R. Morral, Kristie L. Gore, and Terry L. Schell

In the spring of 2014, RAND was asked by the Sexual Assault Prevention and Response Office (SAPRO) in the Office of the Secretary of Defense (OSD) to conduct the 2014 Workplace and Gender Relations Survey of Active Duty Members (WGRA) and the Workplace and Gender Relations Survey of Reserve Component Members (WGRR), biennial surveys of the state of gender relations in the military required by Congress. The terms of the project required that RAND make any necessary changes to the measurement approach, sampling plan, and analytic plan to ensure that the survey results would represent the best available information on the prevalence of criminal sexual assault and military equal opportunity (MEO) sexual harassment and gender discrimination violations in the U.S. military.

In consultation with experts at RAND and other institutions, a scientific advisory board, the Defense Manpower Data Center (DMDC), and Sexual Assault Prevention and Response (SAPR) program officials from each service, RAND completely redesigned the survey questions used to assess each of the principal outcomes, developed a new approach to sample weighting designed to reduce nonresponse bias, and designed a follow-up study of survey nonrespondents to examine whether their exposure to sexual assault and sexual harassment differs systematically from the weighted sample of respondents.

Because results from the 2014 WGRA were required for a report to the President on Department of Defense (DoD) progress addressing sexual assault—to be delivered no later than December 1, 2014—RAND had to make these changes and field, analyze, and report results on the survey in a span of eight months. This left little time for pretesting many of the changes we introduced to the survey design beyond some basic assessments of whether the target population correctly understood and was able to tolerate the new survey questions.

Instead of pretesting, in several cases we were able to design the study to include experiments and substudies that could be examined after survey fielding to evaluate the performance of the new survey instrument and other aspects of the study design. This fourth volume of the *Sexual Assault and Sexual Harassment in the U.S. Military* series presents the results of these experiments and additional analyses we conducted to evaluate the quality and credibility of the findings that the new survey design pro-

duced (see Volumes 1, 2, and 3, as well as their annexes, for details of the study design and results).

About the 2014 Survey

DoD, in consultation with the White House National Security Staff, stipulated that the sample for the new study—which became known as the RAND Military Workplace Study (RMWS)—was to include a census of all women and 25 percent of men in the active components of the Army, Navy, Air Force, and Marine Corps. In addition, we were asked to include a smaller sample of National Guard and other reserve-component members sufficient to support comparisons of sexual assault, sexual harassment, and gender discrimination between the active and reserve components. The U.S. Coast Guard also asked that RAND include a sample of its active- and reserve-component members. In total, therefore, RAND invited close to 560,000 service members to participate in the study, making it the largest study of sexual assault and harassment ever conducted in the U.S. military.

Active-component respondents were randomly assigned to one of four different survey instruments:

1. A “long form,” consisting of a sexual assault module; a sex-based MEO violation module, which assessed sexual harassment and gender discrimination; and questions on respondent demographics, psychological state, command climate, attitudes and beliefs about sexual assault in the military and the nation, and other related issues.
2. A “medium form,” consisting of the sexual assault module, the sex-based MEO violation module, and demographic questions.
3. A “short form,” consisting of the sexual assault module, the screening items from the sex-based MEO violation module, and demographic questions. Thus, these respondents did not complete the full, sex-based MEO violation assessment.
4. The “prior form,” consisting of the unwanted sexual contact, sexual harassment, and gender discrimination assessments from the 2012 WGRA.

The long, medium, and short forms included the new questions developed at RAND to more reliably measure criminal sexual assault experiences as defined in the Uniform Code of Military Justice (UCMJ), and MEO violations of sexual harassment and gender discrimination as defined in DoD Directive 1350.2 (Under Secretary of Defense for Personnel and Readiness, 1995). Respondents who received the prior form saw questions nearly identical to those used in the 2012 WGRA survey, including questions on “unwanted sexual contact,” the construct used to measure sexual assaults,

and sexual harassment. Reserve-component members were randomly assigned to the medium or short forms, and members of the Coast Guard active component received only the long, medium, or short forms.

A total of 477,513 members of the DoD active component were randomly selected from a population of 1,317,561 active-component DoD service members and who met the study inclusion criteria, which required that they be age 18 or older, below the rank of a general or flag officer, and in service for at least six months. These are the same inclusion criteria used in prior WGRA surveys. The sample included 197,491 women and 280,022 men.

The web-based survey was fielded by Westat, a commercial research firm, in the summer of 2014. Of the 477,513 DoD active-component members invited to take the RMWS survey, 145,300 individuals participated, or just over 30 percent. The respondents included 34 percent of the women sampled (67,187) and 27.9 percent of the men (78,113). Service members in the Air Force had the highest response rate (43.5 percent), followed by Army (29.4 percent), Navy (23.3 percent), and Marine Corps (20.6 percent).

Organization of the Report

The focus of this volume is the technical performance of the RMWS study design, survey instrument, and sample weights, focusing on multiple sources of bias that could have undermined the accuracy of our survey results. Chapter Two examines non-response bias found in samples of survey nonrespondents whose sexual assault and harassment experiences we were later able to assess as part of a follow-up study of nonrespondents. Chapter Three examines the characteristics of the sample weights we developed using a new approach to survey weighting that permits adjustment on many more differences between respondents and nonrespondents than earlier methods. We then compared the survey estimates and variances using the new and older approaches to developing sample weights. Chapter Four assesses total survey error by comparing weighted survey estimates for the number of Victim Reporting Preference forms signed in the past year by active-component members with the true value for that number as revealed in SAPRO tracking data.

Chapter Five provides details on how men and women progressed through the sexual assault module in the RAND form, and on how follow-up items assessing the UCMJ criteria for such assaults filtered identified unwanted experiences down to just those qualifying as a sexual assault. Chapter Six examines possible sources of overcounting or undercounting of sexual assault attributable to sample frame inclusion criteria and scoring decision rules we implemented. Chapter Seven provides similar descriptive analyses for the sexual harassment and gender discrimination module.

Chapter Eight compares the people and events counted by the prior form as experiencing either unwanted sexual contact or sexual harassment with those counted by the RAND form as experiencing sexual assaults or sexual harassment. Chapter Nine describes patterns of survey breakoff found on the RAND forms and the prior form, and provides an assessment of whether breakoff differentially occurred among those with higher or lower risk of sexual assault. Chapter Ten examines the complaints RAND or others received about the language used in the sexual assault module.

Finally, Chapter Eleven draws crosscutting conclusions and makes a series of recommendations for future administrations of the WGRA.

Statistical Analysis and Reporting Conventions Used in This Report

The statistical analyses presented in this report and its appendixes employ statistical procedures designed to reduce the likelihood of drawing inappropriate conclusions or compromising the privacy of respondents.

First, we assured respondents in the survey *Privacy Statement* (part of the informed consent) that our reports would not include analyses conducted within subsets smaller than 15 respondents. Thus, to maintain participant privacy, the report and its appendixes do not include sample statistics (including confidence intervals) computed within groups smaller than 15 unweighted respondents. If such a cell appears in a table, the point estimates and its confidence intervals are replaced with “not reportable” (NR).

Second, the report contains estimated population percentages that vary dramatically in their statistical precision. Some estimates have a 95-percent confidence interval with a width of 0.3 percentage points, while others have a width of 30 percentage points. This occurs because some percentages are estimated using more than 100,000 respondents, while others are estimated on small subsamples (e.g., male airmen who experienced a sexual assault). To reduce the likelihood of misinterpretations, percentages with very low precision are not reported. Specifically, percentages estimated with a margin of error greater than 15 percentage points are replaced with NR (where the margin of error is defined as the larger half-width of the confidence interval). In such cases, the confidence intervals are still presented to communicate the range of percentages that is consistent with the data. Such imprecise estimates are better thought about as ranges rather than points.

The text and tables in this report do not use a constant level of numerical precision. Because the statistical precision of the estimates vary by more than two orders of magnitude, and because the purpose of numbers presented in the text and tables may be slightly different, we have tried to select a level of numerical precision that is appropriate for each situation.

Follow-Up Studies of Survey Nonrespondents

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Survey nonresponse has the potential to introduce bias in the estimates of key outcomes. To the extent that nonrespondents and respondents differ on observed characteristics, we can use weights to adjust the sample so that the weighted respondents match the full population on those observed characteristics. This can eliminate the portion of nonresponse bias associated with those observed variables. When all nonresponse bias can be eliminated in this manner, the “missingness” is called *ignorable* or *missing at random* (Little and Rubin, 2002). The more variables that are observed for the nonrespondents and incorporated into the weights, the more plausible it is to assume that the weights eliminate any nonresponse bias. This study had an unusually large amount of data on survey nonrespondents because DMDC provided RAND with a broad range of socio-demographic and workplace-related measures from sampled members’ personnel files. The survey weights incorporated this information in an attempt to make the weighted analytic sample representative of the full population in terms of their risk for sexual assault, sexual harassment, and gender discrimination. When the propensity to respond (or participate) in a survey is related to survey outcomes even after adjusting for all of the individual-level characteristics available, nonresponse weights will not fully eliminate nonresponse bias. Such an association could occur for several reasons. For example, individuals may choose to respond to the study precisely because they have experienced one of these violations and are more motivated to “tell their story” on the survey than unaffected service members. In that case, the respondents to the survey could have higher rates of sexual assault or harassment than nonrespondents, even when these groups are otherwise matched on demographic and military factors. Alternatively, individual respondents who have experienced these violations may avoid participating in the study. For example, post-traumatic cognitive avoidance is widely observed in traumatized samples and is part of the definition of posttraumatic stress disorder (American Psychiatric Association, 2013). Similarly, some victims may not trust the researchers to keep experiences reported on the survey private, potentially putting them at risk for embarrassment or retaliation. To the extent that those who experience such outcomes participate at lower rates than the rest of the sample, survey estimates may be lower than the underlying “true” population estimate.

These hypothesized mechanisms could each produce *non-ignorable* missingness (Little and Rubin, 2002), which refers to missing data that is uniquely associated with the survey measurements of interest, and thus its effects are not eliminated through standard methods to minimize nonresponse bias. We are primarily concerned about the net effect of all of these mechanisms. For example, if the number of people who experienced sexual assault and avoid the study because of the assault is the same as the number who participate specifically because of their assault, the aggregate pattern of nonresponse would not be associated with sexual assault, and these mechanisms would not produce a nonresponse bias in our estimates.

Assessing the existence of non-ignorable nonresponse is difficult or impossible within the primary study data set. Nor can it be determined by assessing the plausibility of various theories of how an individual's propensity for responding depends on their experiences with sexual assault or harassment; all of these theories could be correct and yet there may be no overall response bias.

The only direct way to assess this type of bias is to measure the association between the study outcomes and propensity for responding; however, that requires observing the study outcomes among survey nonrespondents. Because we considered nonresponse bias to be the largest potential source of survey error in the RMWS, the study was designed to include additional data collection on survey nonrespondents to directly assess this potential bias.

The purpose of our follow-up studies of nonrespondents is to go back into the field and empirically assess the primary study outcomes in groups of individuals who failed to respond to the main study. These data can then be used to determine if the respondents to the main survey have higher or lower rates of sexual assault, sexual harassment, or gender discrimination than the observed nonrespondents, even when these groups are otherwise matched on demographic and military factors. This comparison may provide information about the direction and approximate magnitude of any nonresponse bias.

Study Procedures

Overview

We conducted three follow-up studies of survey nonresponse, each using slightly different procedures to collect outcome data on sampled members who failed to respond within the survey field period. Although the sample was randomized into these three groups prior to fielding the survey, group assignment had no effect on any procedures used in the main RMWS survey. Assignment only affected study procedures for those members who were nonrespondents at the conclusion of the main study field period. The three follow-up groups correspond to (1) nonrespondents who were subsequently recruited by phone and mail for a phone interview (*phone follow-up* sample); (2) non-

respondents who were subsequently recruited by mail for a self-administered paper survey (*mail follow-up* sample); and (3) nonrespondents who were given additional time to complete the survey on the web, but were not subject to additional recruitment efforts beyond those of the main study (*late web* sample).

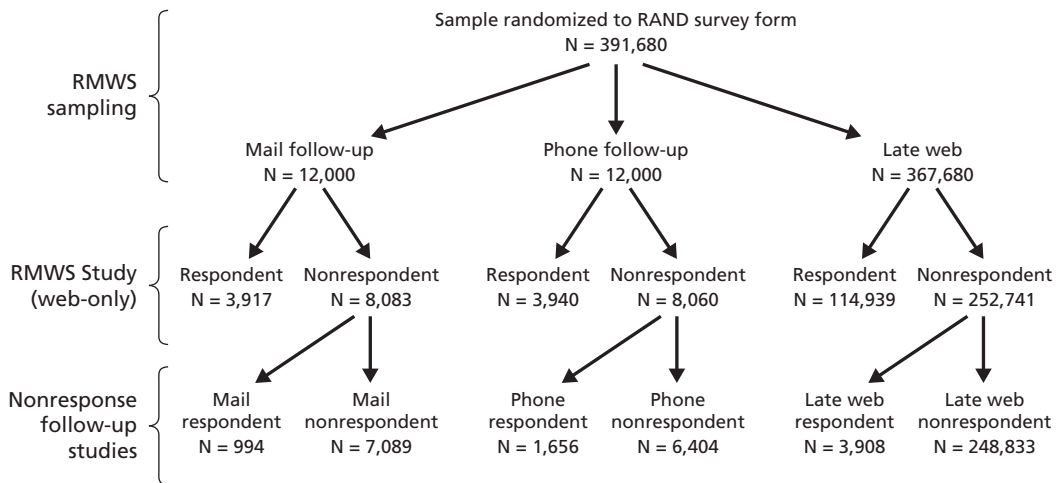
Participants

For the main RMWS, 391,680 active-component members were randomly assigned to receive the RAND form as a web-based survey. As shown in Figure 2.1, these service members were randomized into one of three mutually exclusive nonresponse follow-up groups: phone follow-up (N = 12,000), mail follow-up (N = 12,000), and late web (N = 367,680). The assignment to these groups were implemented as simple random samples of active-component DoD members who had been previously randomized to the short form (which itself is an equal-probability stratified random sample of the overall sample). However, active-component Coast Guard members (N = 14,167) were not randomized into either the phone or mail follow-up groups; these Coast Guard members are included in the late web group.

Active-component sample members who completed the survey during the RMWS study field period were counted as respondents, and their data were analyzed to produce the main RMWS survey results for active-component members, whether they had been assigned to the phone, mail, or late web samples or not. For all groups, approximately 30 percent of the active-component sample responded to the main survey during its field period.

Among those who had been assigned to the phone and mail follow-up samples, 8,060 and 8,083, respectively, did not participate in the RMWS study, and so became

Figure 2.1
Diagram of RMWS and Nonresponse Follow-Up Studies



the samples for the high-intensity nonresponse follow-up studies. An additional 252,741 active-component members who were nonrespondents to the main RMWS survey and who had not been assigned to the mail or phone contingency samples became the sample for late web follow-up study. Although no additional efforts were made to recruit this sample into the follow-up study (beyond those that had already taken place for the main study), these service members were allowed to take the survey after the RMWS field period was closed. Their responses were not included in the main RMWS analytic sample and findings because they did not complete before the end of the fielding period, but were instead treated as a third sample of RMWS main study nonrespondents who were subsequently followed up.

High-Intensity Phone Follow-Up Study: Measures and Procedures

The sample of service members randomized to phone follow-up who were nonrespondents in the main study was sent a prenotification letter approximately ten days after the close of the main survey's field period. This letter served several functions: (1) it alerted recipients to the fact that we would be calling them to request a phone interview for the RMWS; (2) it provided the consent form for the study so that participants would have a copy for their records and so the interviewer could forgo reading the full consent form in cases when the respondent had read it in advance; (3) it contained \$4 in cash as a preincentive to improve response rates; and (4) it provided a toll-free number to call if the member wished to conduct the survey at a time of their convenience rather than waiting for our interviewers to call them. When a prenotification letter was returned as postal nondeliverable, another letter was mailed to the next best address available.

In most cases, these letters were sent to the same address used for mail notifications in the main study (see Volume 1). However, we also used recent mailing addresses from a LexisNexis search and EU Services (which had U.S. Postal Service change of address information) to attempt to identify the best available mailing address. Similarly, recruitment telephone calls were made to a phone number provided by DMDC when it was available. When such a number was not available, not in service, or when interviewers were informed it was a wrong number, subsequent calls were made to additional numbers. These other phone numbers included some provided by DMDC, as well as those identified from a LexisNexis search and manual tracing through publicly available records.

Service members in the sample could be called up to nine times during the survey field period (October 20–November 25, 2014), and one message could be left on the answering machine at a given number alerting the sampled member of the study. Approximately 50 percent of all calls went to voicemail or an answering machine. Members sampled into the phone follow-up who requested removal from further communications received no further contacts. Those who were contacted but gave a “soft refusal” (for example, they hung up on the interviewer or said they did not have time

to complete the survey at that time) were recontacted if the study team determined that such a contact was not against the stated wishes of that member. Twenty-five percent of such cases eventually completed the survey.

The phone survey instrument was implemented as a computer-assisted telephone interview, using a trained, live interviewer reading questions from a computer. Thus the instrument could implement a complex skip pattern similar to the web-based survey used in the main study (see Appendix A). However, to maximize the response rate, the instrument was simplified substantially from the full web instrument. It included (1) the five questions designed to remind respondents of the past-12-month time frame to minimize response *telescoping*, or the tendency to regard past events as occurring more recently than they did; (2) the behavioral screening items from the assessment of sexual harassment; (3) the behavioral screening items from the assessment of gender discrimination; and (4) both the screening items and the follow-up items for the assessment of sexual assault in the past year. Relative to the full instrument, it excluded (1) the follow-up items designed to assess persistence or severity of each potentially harassing behavior; (2) the follow-up items designed to assess harm to career from potential gender discrimination; (3) the follow-up items that provide descriptive details about incidents of sexual assault, sexual harassment, and gender discrimination; and (4) the lifetime assessment of sexual assault and the general attitudes and beliefs questions. Sampled members were advised that this shortened survey would take between seven and 12 minutes to complete and took respondents an average of nine minutes to complete.

High-Intensity Mail Follow-up Study: Measures and Procedures

The full sample of service members randomized to mail follow-up who were nonrespondents in the main RMWS study was sent a mailing by first-class mail approximately ten days after the close of the fielding period for the RMWS survey. This mailing contained several documents: (1) a booklet containing the informed consent and a self-administered paper version of the survey; (2) \$4 in cash as a preincentive to improve response rates; (3) a postage-paid return envelope for the survey booklet; and (4) a cover letter that explained the survey, answered frequently asked questions, and provided directions for how to get any questions answered by phone or on the web. If this mailing was returned as postal nondeliverable, another letter was mailed to the next best address available.

In most cases, these letters were sent to the same address used for mail notifications in the main study. However, we also used recent mailing addresses from a Lexis-Nexis search and EU Services (which had U.S. Postal Service change of address information) to attempt to identify the best available mailing address.

A second survey was mailed using Federal Express (or USPS Priority Mail if the address was a post office box) on October 30, 2014, to 6,911 members who had not responded to the first mailing. Unlike the first mailing, which was sent via USPS first-class mail, the second mailing involved an attempt to hand-deliver the mailing to the

service member. The content of the second mailing was identical to the initial mailing, except that it did not include an additional monetary incentive. Surveys returned to Westat and entered before December 1, 2014, were included in the final data set.

The mail survey was implemented as a teleform instrument, i.e., one in which the respondent marks the appropriate box so that responses can be machine-read. It is difficult to implement a complex skip pattern in a paper instrument, because paper instruments rely on respondents carefully reading question instructions. Such skips can be facilitated with the careful design of the questionnaire's layout and formatting, but we determined that it would be best to limit the instrument to only two levels of question skips. This required some subtle changes in the way in which respondents navigated the sexual assault assessment module relative to both the main web survey and the phone survey (see Appendix B). In addition, the paper version could not be customized so that the date that represented one year ago would be exactly one year prior to when the respondent filled out the survey. Surveys mailed on October 10, 2014, used a date of "10/1/2013" when indicating the beginning of the period the survey questions concerned; surveys mailed on October 30, 2014, used a date of "11/3/2013" when indicating the beginning of the period the survey questions concerned.

Similar to the phone survey, the paper version of the instrument was substantially shortened relative to the main web survey to provide only the important information necessary for assessing key outcomes. The paper survey included (1) the five questions designed to remind respondents of the past-12-month time frame; (2) the behavioral screening items from the assessment of sexual harassment; (3) the behavioral screening items from the assessment of gender discrimination; and (4) both the screening items and the follow-up items for the assessment of sexual assault in the past year, but with a slightly simplified skip pattern for the follow-up questions. Relative to the full instrument, the paper survey excluded (1) the follow-up items designed to assess persistence or severity of each potentially harassing behavior; (2) the follow-up items designed to assess harm to career from potential gender discrimination; (3) the follow-up items that provide descriptive details about incidents of sexual assault, sexual harassment, and gender discrimination; and (4) the lifetime assessment of sexual assault and the general attitudes and beliefs questions. This shortened survey was advertised to sample members as taking five minutes.

Late Web Study: Measures and Procedures

In addition to the phone and mail high-intensity recruitment efforts, we also observed study outcomes in a sample of late responders through the main web portal. These participants were subject to no additional outreach or incentives except for the recruitment messages provided to all members sampled into the main study and completed the same web-based survey instrument as the main study respondents. The only difference in procedures was that the survey field period was extended to November 25, 2014. These respondents completed the survey after the official survey close date.

Neither late web respondents nor those in the phone or mail intensive follow-up studies were included in the analytic samples used to provide our prior published estimates of sexual offenses in the military, including those found in our top-line reports and our Volume 2 and 3 reports.

Response Rates

The primary purpose of this follow-up study of nonrespondents was to observe key outcomes among individuals who were nonrespondents for the main study, and thus were omitted from the main study estimates. A key measure of the success of the study is the extent to which these procedures succeeded in recruiting the nonrespondents. This goal of achieving a high response rate is tempered somewhat with the acknowledgement that the sample of individuals subject to these procedures had already demonstrated they were difficult to recruit. They had all failed to respond within the main study field period after receiving three mail invitations and nine email invitations.

For the purposes of sample accounting and outcome analyses, respondents were classified by the condition to which they had been randomized, rather than their mode of survey response. Specifically, a small number of individuals who received recruitment phone calls ($N = 66$) or were mailed a survey packet ($N = 128$) subsequently completed the survey on the web using the login credentials that had been emailed during the main study. These respondents are treated as part of the phone or mail follow-up groups because they were assigned to those groups and subject to those recruitment procedures, regardless of the mode of survey administration they ultimately selected.

Similar to the main study, sampled members were counted as respondents if they completed the sexual assault assessment module (see Volume 1 for details). There were 1,656 respondents in the phone follow-up sample, 994 in the mail follow-up sample, and 3,908 in the late web sample. This gives response rates of 20.5 percent, 12.3 percent, and 1.5 percent, for the phone, mail, and late web samples, respectively, using American Association of Public Opinion Research “RR1” definitions for response rates (American Association of Public Opinion Research, 2011). Thus, the high-intensity phone follow-up was most effective at recruiting the main study nonrespondents, with the mail recruitment method only about half as effective. However, both of these high-intensity follow-up methods were an order of magnitude more effective at converting nonrespondents relative to the late web group, which merely lengthened the field period with no additional recruitment efforts.

Sample Characteristics

All of these response rates are relatively low and raise concerns about the extent to which these follow-up study respondents should be seen as representative of the nonrespondents to the main study. It is possible that these additional recruitment methods capture essentially the same types of individuals as those who participated in the main study and thus would be expected to have the same nonresponse biases. To best iden-

tify nonresponse bias in the main study estimates, the changes in the survey methods (e.g., changing modes, adding incentives, lengthening the field period, using additional contact information, and making additional recruitment attempts) would yield a different pattern of nonresponse. That is, the follow-up studies would include individuals who are different from the main study respondent on those factors associated with nonresponse in the main study. Ideally, the follow-up studies capture different respondents, not just more respondents.

As discussed in Chapter Three, several groups were substantially underrepresented among respondents in the main RMWS study. These included men, sailors, Marines, and junior enlisted members. Table 2.1 presents the characteristics of the participants in the three nonrespondent follow-up studies relative to the respondents in the main RMWS study. Each estimate represents a group's proportion within one of the unweighted follow-up samples divided by that group's proportion in the unweighted main RMWS. For example, the proportion of the phone follow-up sample that was junior enlisted (E1–E4) was 1.90 times the proportion of the main study that was junior enlisted. Thus, while junior enlisted were substantially underrepresented in the unweighted RMWS respondent sample (see Chapter Three), the phone follow-up was better able to collect information on junior enlisted. In contrast, ratios near 1.0 indicate that the follow-up respondents have similar characteristics to the main RMWS respondents.

The pattern of respondent characteristics is generally similar across the three nonrespondent follow-up samples. Those specific groups of individuals who were substantially underrepresented among respondents to the main study (particularly junior enlisted, sailors, and Marines) are better represented in the nonresponse follow-up studies. These effects are most pronounced for the phone follow-up condition. For example, it yielded a sample in which the proportion of respondents who were junior enlisted was nearly twice as high as in the main study sample.

While the response rates in these follow-up studies are lower than ideal, these procedures did succeed in recruiting different types of service members than the main study. Whatever processes produced survey nonresponse appear to be meaningfully different in these follow-up studies relative to the main study. Thus, the follow-up studies may provide useful assessments of any nonresponse bias that exists in the main study estimates. However, our conclusions about nonresponse bias will be limited to the extent that characteristics exist that are (a) associated with nonresponse in both the main study and the follow-up studies, (b) associated with primary study outcomes, and (c) independent of the factors included in RMWS nonresponse weighting.

Analysis of Nonresponse Bias

The analysis of the nonresponse follow-up studies was designed to determine whether the main study estimates of prevalence rates for sexual assault, sexual harassment, and

Table 2.1
Characteristics of Respondents in Each Follow-up Sample,
Relative to Main Study Respondents (Ratios)

	Follow-Up Sample		
	Phone	Mail	Late Web
Sex			
Female	0.85	0.93	0.98
Male	1.13	1.06	1.02
Branch of service			
Air Force	0.60	0.61	0.68
Army	1.11	1.08	1.09
Coast Guard	NA	NA	0.60
Marine Corps	1.59	1.16	1.36
Navy	1.31	1.52	1.44
Pay grade			
E1–E4	1.90	1.26	1.31
E5–E9	0.76	0.91	0.88
O1–O3	0.72	0.98	0.90
O4–O6	0.37	0.82	0.94

NOTE: The ratio reflects the proportion of respondents in each nonrespondent follow-up sample divided by their proportion among respondents to the main study. Ratios for the late web sample include Coast Guard members, while they are excluded from both the numerator and denominator in the phone and mail columns because Coast Guard members were not randomized into those conditions (NA = not applicable).

gender discrimination were biased by survey nonresponse. In an attempt to mitigate nonresponse biases, those estimates incorporated nonresponse weights that accounted for the known differences in characteristics across respondents and nonrespondents in the main study. However, nonresponse bias would still occur whenever there is a difference in the prevalence of those outcomes between the study respondents and nonrespondents, even after conditioning on all of the factors included in the nonresponse weights. For example, if female Marines who responded to the study had higher rates of sexual assault than those who did not respond, weighting the respondent sample so that it matched the full population on the proportion of female Marines would not eliminate the nonresponse bias.

Therefore, measuring nonresponse bias requires determining whether respondents and observed nonrespondents with the same characteristics (i.e., “matched”) have the

same prevalence of these outcomes; specifically whether they have the same prevalence when they are matched to one another in the same way that the characteristics of the respondents were matched to the overall population by the main study nonresponse weights.

In our analysis of these nonresponse follow-up samples, we use the same nonresponse model that was used to derive weights in the main study,¹ that is, the model that was designed to adjust the main study respondents to match the characteristics of the full population. However, in the current analyses, we used that weighting model to generate three sets of weights that matched the main study respondents to the respondents in each of the three nonresponse studies separately: One set of weights matched the characteristics of the main study respondent to the characteristics of the nonrespondents observed in the late web condition; one set matched them to the nonrespondents observed in the phone follow-up condition; and one set matched them to the nonrespondents observed in the mail follow-up condition.

An estimate of nonresponse bias can then be made by comparing the prevalence of each outcome between the observed nonrespondents from a follow-up study and a matched group of respondents in the main study. If individuals in the nonresponse follow-up condition have *higher* prevalence than matched respondents from the main study, it suggests that the main study prevalence estimates *underestimate* the true prevalence. If individuals in the nonresponse follow-up condition have *lower* prevalence than matched respondents from the main study, it suggests that the main study prevalence estimates *overestimate* the true prevalence.

Comparing the prevalence of these outcomes between the observed nonrespondents in the follow-up studies relative to the matched respondents from the main study was done within a Poisson regression model.² This model used a log link-function, so that exponentiated model coefficients expressed incidence or prevalence ratios. These ratios compare the mean/proportion observed among a follow-up group divided by the mean/proportion observed among the matched respondents from the main study. For example, an incidence ratio of 2.0 on the sexual harassment measure would indicate that respondents in the nonresponse follow-up sample said “yes” to twice as many sexual harassment screening questions, on average, as similar respondents in the main study.³ Such a result would imply that the main study underestimated the true preva-

¹ The nonresponse model used to derive RMWS weights is described in Volume 1. The models used in the analyses of these follow-up studies of nonresponse used the same type of prediction model, the same predictors, and the same statistical criteria to identify the set of weights that provided optimal balance. As with the main study weights, the follow-up study weights were applied only to the main study respondents.

² The models used robust standard errors (i.e., General Estimating Equations), rather than inferring statistical significance directly from a Poisson distribution. All models were estimated within SAS PROC GENMOD.

³ As mentioned earlier, the follow-up instruments had abbreviated measures that did not include the follow-up questions that allow a dichotomous measure of sexual harassment in the past year. Thus, these analyses are based on counts of possible sexual harassment experiences.

lence of sexual harassment due to a nonresponse bias in which victims of sexual harassment were less likely to be respondents in the main survey in a manner that was not corrected by the study nonresponse weights.

As a practical matter, the inference of nonresponse bias is complicated by the fact that we have three studies of nonrespondents. Each used different methods, and produced substantially different response rates. As a result, the estimates of nonresponse bias may differ across these studies. The phone follow-up produced the highest response rate and yielded a sample with the highest proportions of groups that were underrepresented in the main study. That sample allowed for slightly stronger inferences than the mail follow-up and considerably stronger than the late web follow-up.

On the other hand, the phone follow-up used a live interviewer, unlike the main study or the other two follow-up studies. Respondents in the phone follow-up were asked to reveal to another person the details of any sexual assault or harassment experiences. There is long line of research in the field of survey methodology demonstrating that live-interviewer surveys can result in lower reports of embarrassing or stigmatizing experiences relative to the same survey administered without a live interviewer (e.g., Bradburn, 1983; Tourangeau and Yan, 2007). There is also some evidence that surveys using live interviewers produce less-accurate responses for sensitive questions (e.g., Kreuter, Presser and Tourangeau, 2008). These mode effects are regularly found in the measurement of experiences involving alcohol use or sexual behavior (Acree et al., 1999; Aquilino, 1994), and some have found lower rates of reported sexual victimization (Turner et al., 1998; Parks, Pardi and Bradizza, 2006) and intimate partner violence (Hussain et al., 2013) when using live interviewers. In contrast, studies that compare responses to sensitive questions between web and mail survey modes generally have not found substantial differences (e.g., McCabe et al., 2006). Thus, although the phone follow-up study yielded the best sample for studying nonrespondents, that mode of survey administration may underestimate the true rate of sexual assault, sexual harassment, and gender discrimination. Because of this possible reporting bias, if we found that the phone follow-up produced higher prevalence estimates than matched respondents from the main study, it would provide strong evidence of nonresponse bias in the main study estimates. However, a finding that the phone follow-up produced lower prevalence cannot be easily interpreted because that effect could result from either a nonresponse bias in the main study or a live-interviewer reporting bias in the phone follow-up.

One final analytic complication is that the instruments are not identical across the three follow-up studies. Both the mail and phone follow-up instruments (as well as the short form instrument on the web) omit the follow-up questions for sexual harassment and gender discrimination. Those instruments cannot create the primary measures of sexual harassment and gender discrimination reported in the main project reports. Similarly, the mail follow-up required simplification of the skip patterns within the

sexual assault assessment module, modifications that might affect the probability that someone who screens into the module is classified as having experienced sexual assault.

To address these differences in the surveys, the primary analyses will focus on those measures that were administered identically across all three instruments. These outcomes differ from the measures reported in the main project reports but are necessarily highly associated with those outcomes. For sexual harassment, we analyzed the number of sexual harassment screeners that were indicated (range: 0–13); these are behaviorally specific experiences where sexual behavior of a coworker made the respondent uncomfortable, angry, or upset. For gender discrimination, we used the number of discrimination screeners indicated (range: 0–2). For sexual assault, we used a dichotomous measure indicating whether any of the six behaviorally specific unwanted experiences occurred, but without the follow-up questions that assessed offender intent or method of coercion.

Results

Table 2.2 presents the risk ratios comparing the observed nonrespondents from the three nonresponse follow-up studies with their matched respondents from the main study. There were substantial differences across the three follow-up studies designed to assess prevalence among nonrespondents to the main study. The phone follow-up generally yielded risk ratios less than one, indicating that participants in the phone follow-up had lower rates of study outcomes relative to similar respondents to the main study with ratios significantly less than one for sexual harassment and gender discrimination.

In contrast, both the mail follow-up and late web studies yielded risk ratios greater than one. These ratios were significantly greater than one on all outcomes within the late web study, where we had the most statistical power to find such effects.⁴ Within the mail follow-up study, only gender discrimination yielded a risk ratio that was significantly greater than one.

Thus, the three follow-up studies imply different conclusions about possible nonresponse bias in the main study prevalence estimates presented in earlier volumes. The late web and mail nonresponse follow-up studies imply that main study findings slightly underestimated the prevalence of these outcomes among non-

⁴ As mentioned earlier, the outcomes presented in Table 2.2 are different than those used as primary outcomes in the main study because of differences in the instruments across survey modes. However, the primary sexual assault measure in the main study can be computed for both the phone follow-up and late web follow-up studies. Analyzing that sexual assault measure yielded nearly equivalent risk ratios to those presented in Table 2.2: 0.90 and 1.20, for the overall phone and late web, respectively. Similarly, the dichotomous primary study measures for sexual harassment and gender discrimination can be estimated for late web follow-up. Analyzing those measures within the overall late web follow-up study yielded similar risk ratios to those presented in Table 2.2: 1.14 and 1.10, for sexual harassment and gender discrimination, respectively. None of the inferential results are different if logistic regression is substituted for Poisson regression for measures that are dichotomous.

Table 2.2
Risk of Sexual Assault, Sexual Harassment, and Gender Discrimination
for Nonrespondent Follow-Up Studies Relative to Their Matched
Respondents from the Main Study

Follow-Up Study	Overall Risk Ratio	Women Risk Ratio	Men Risk Ratio
Phone Follow-Up			
Sexual Assault	0.81 (0.61–1.08)	0.83 (0.59–1.16)	0.78 (0.46–1.31)
Sexual Harassment	0.73*** (0.62–0.86)	0.78* (0.64–0.96)	0.64*** (0.49–0.83)
Gender Discrimination	0.68*** (0.59–0.80)	0.70*** (0.60–0.82)	0.59** (0.40–0.88)
Mail Follow-Up			
Sexual Assault	1.05 (0.76–1.47)	1.26 (0.88–1.80)	0.59 (0.26–1.32)
Sexual Harassment	1.05 (0.88–1.25)	1.15 (0.94–1.41)	0.84 (0.61–1.15)
Gender Discrimination	1.27*** (1.10–1.45)	1.25** (1.09–1.44)	1.33 (0.95–1.88)
Late Web Follow-Up			
Sexual Assault	1.19* (1.01–1.41)	1.20 (0.99–1.45)	1.31* (1.06–1.60)
Sexual Harassment	1.20*** (1.10–1.31)	1.19*** (1.08–1.33)	1.21* (1.03–1.43)
Gender Discrimination	1.14*** (1.06–1.23)	1.13** (1.05–1.22)	1.22 (0.99–1.49)

NOTE: These risk ratios represent the prevalence/incidence rate for an outcome among a given nonresponse follow-up study divided by the rate among the matched respondents from the main study. 95-percent confidence intervals are included in parenthesis. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$ testing the null hypothesis that the risk ratio = 1.

respondents to the main study. The estimates of this bias range from 5 percent to 27 percent depending on the outcome. These results suggest that the main study's overall population estimates may be slightly too low.⁵ On the other hand, the phone

⁵ These risk ratios do not correspond exactly to the magnitude of the bias in the overall population estimates. The bias in the population estimates depends on both the size of the prevalence difference between nonrespondents and respondents as well as the response rate. For example, if 30 percent of the sample responded to the survey and the study underestimated prevalence among nonresponders by a factor of 1.20, it would result in a total bias of 1.14 in the population estimates (e.g., the study would yield a 7-percent prevalence when the actual population prevalence was 8 percent). Second, the three follow-up samples may not be representative of the full

follow-up study implies that main study findings overestimated the prevalence of these outcomes among nonrespondents to the main study, an estimated bias of approximately the same magnitude but in the opposite direction.

Discussion and Conclusions

Overall, these findings do not show a consistent pattern of nonresponse bias. However, the interpretation of these results is complicated by strong evidence of a survey mode effect, as well as differences across the three follow-up studies in their ability to recruit a representative sample of nonrespondents. In short, the survey methodology literature supports a strong hypothesis that phone interviews result in underreporting of sexual assault, sexual harassment, and gender discrimination. Because the study found lower prevalence in the phone follow-up group relative to matched respondents from the main study, it is not possible to determine the extent to which those low risk ratios were due to nonresponse bias in the main study versus a response bias in the phone survey. However, the other two follow-up studies were less successful in recruiting study nonrespondents, so estimates based on those studies are harder to generalize to the broader set of study nonrespondents.

The mail follow-up study may provide the best overall estimates of nonresponse bias, because it yielded a response rate that was an order of magnitude better than the late web study but is not hypothesized to suffer from the types of live-interviewer response biases found in the phone survey. The mail follow-up study found descriptively small and statistically nonsignificant evidence of nonresponse bias when looking at sexual assault and sexual harassment outcomes. The confidence intervals suggest that the nonresponse bias on the main study could be in either direction for those outcomes. However, the mail follow-up study also suggests that the main study estimates of gender discrimination may have resulted in a slight underestimate of the true prevalence of gender discrimination in the population.

Although these follow-up studies of nonrespondents did not identify a consistent pattern of nonresponse bias, they do provide other useful information about nonresponse in the main study, as well as about the feasibility of improving response rates through modifications of the survey methods. The phone follow-up was successful at recruiting military service members who failed to respond to email invitations; the overall response rate achieved by combining the main web survey and the phone follow-up study was 45 percent. For studies that ask less-sensitive questions than the

population of nonrespondents. To the extent that the size of the risk ratio comparing nonrespondents and respondents varies meaningfully across subgroups in the population, the estimates from these follow-up studies may not precisely correspond to the risk ratio in the full population. The goal of these follow-up studies is to identify the direction and approximate magnitude of the remaining nonresponse bias; they are not well suited for estimating post hoc corrections to the population prevalence estimates contained in Volumes 2 and 3.

WGRA/RMWS, this mixed-mode design has the potential to substantially reduce nonresponse, although we would not suggest such an approach for topics that are likely to be underreported in live-interviewer surveys. Alternatively, interactive voice recognition phone interviews may produce less response biases than live-interviewer surveys (Kreuter, Presser, and Tourangeau, 2008) and may be a cost-effective means of increasing the overall response rate relative to a web-only administration.

The fact that so many junior enlisted personnel responded to phone calls (even after RAND had sent out nine email invitations) may suggest that phone outreach and recruitment is an effective way to reach hard-to-recruit subpopulations. That method of outreach may be effective even if survey administration is conducted on the web. It is plausible that some of the low-responding groups do not regularly use email communications as part of their military duties, which presents a problem for web surveys that recruit primarily through email sent to work addresses. It may be possible to motivate respondents to complete a web survey using automated phone messages or text messages to better capture service members who do not normally use email as part of their jobs. To the extent that the survey is well adapted for completion on a smartphone, it may also be more convenient to receive links to the web survey via text messages than via email for many service members.

Another observation from the follow-up studies of nonresponse is that a longer field period may improve the sample. The 2014 RMWS was conducted on a tight timeline with a field period substantially shorter than used in prior WGRA studies. However, even after the announced closing date, and without any additional recruitment messages, responses continued to accumulate by leaving the web site open for an additional eight weeks. Had we included these late respondents in the main study sample, they would have accounted for a 1-percent improvement in main study response rates. Perhaps more importantly, the service members who completed during this period were often from groups (junior enlisted, Marine Corps, and Navy) who were substantially underrepresented in the unweighted sample. This suggests that a longer field period might also be useful for improving the representativeness of the sample.

The Efficacy of Sampling Weights for Correcting Nonresponse Bias

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As a continuation of our investigation of nonresponse bias in Chapter Two, in this chapter we explore predictors of nonresponse and key survey outcomes, and the amount of nonresponse bias reduced using sample weights. Survey nonresponse reduces sample size and, thereby, the precision of estimates. More critically, it may introduce bias in estimates of survey outcomes when the outcomes of those who respond differ systematically from those who do not respond. While even a low rate of nonresponse could introduce bias into survey estimates (Groves, 2006), concerns about nonresponse bias grow as response rates decline. Unfortunately, for the past decade or more, response rates have declined dramatically for military and civilian surveys (Falk, 2012; Kohut et al., 2012; National Research Council, 2014).

One standard approach to address the threat of nonresponse bias is sample weighting that ensures that survey respondents are representative of the underlying population, at least in terms of characteristics that are known about the population (Heeringa, West, and Berglund, 2010; Little and Rubin, 2002; Schafer and Graham, 2002). For instance, if women participate in the survey at higher rates than men, sample or nonresponse weights can be used to ensure that the weighted proportion of women respondents matches the correct proportion of women in the population. Similarly, nonresponse weights can also be used to ensure other characteristics of the weighted respondent sample match the population—such as age distribution, marital status, and education level. By ensuring representativeness on all these characteristics, the sample weights ensure that survey estimates of, say, income are not biased by any systematic over- or underrepresentation among respondents of one gender or higher education levels, factors known to be associated with income.

In contrast to a majority of surveys in civilian settings, military surveys benefit from an extraordinary wealth of information about the population of service members. This information, in theory, could be used in the development of nonresponse weights, potentially reducing nonresponse bias in survey estimates. However, there is a cost to adding factors and complexity to nonresponse weight models. Specifically, adding factors that are only weakly associated with either nonresponse or the primary survey outcomes usually has the effect of driving up variance in survey estimates. Although the estimates may be less biased, on average, they may become unacceptably imprecise.

Hence, the efficacy of weighting adjustments may be conceptualized as a trade-off in bias and variance of survey estimates.

For the RMWS study, we developed a new approach to nonresponse weighting designed to permit inclusion of many more factors in our nonresponse weighting models than would be possible using traditional methods, without driving up variance to unacceptable levels. In addition to these “outcome-optimized RMWS weights” (hereafter called “RMWS weights”), we created a second set of weights using the same weighting approach as has been used in earlier administrations of the WGRA. We constructed these “WGRA weights” so that we could report results on questions from the 2014 version of the prior form using weights comparable to those used for estimates from past WGRAs. Generating both sets of weights also provides us an opportunity to compare the impact on survey estimates of nonresponse weighting using the new RMWS weighting methods with the more familiar weighting methods used in earlier WGRA studies.

If survey respondents and nonrespondents differ in terms of some characteristic, this characteristic is a potential source of nonresponse bias only if it is also associated with an outcome of interest. For instance, if respondents and nonrespondents have different proportions of women, the respondents will still offer an unbiased estimate of, say, population rates of sexual assault if men and women experience sexual assault at equal rates. If men and women do not experience assaults at equal rates, however, the gender differences between respondents and nonrespondents pose a risk of introducing nonresponse bias in sexual assault estimates for the population, unless we explicitly adjust for gender differences between the sample of respondents and the underlying population.

In this chapter, we examine a wide range of candidate factors available to us through the administrative data on service members maintained by DMDC. Specifically, we conducted analyses on the active-component population to identify factors associated with both nonresponse and primary outcome variables, and therefore factors that we would consider candidates for our nonresponse models. After constructing weights that included all of these factors in an efficient way, we examined the effect of using these weights in survey estimation on variance inflation (design effect), bias reduction, and mean squared error (MSE) or accuracy.

Participant Characteristics Associated with Survey Nonresponse

We identified four categories of characteristics that were known to be predictors of survey response in the DMDC population or that were hypothesized as predictors of nonresponse in the RMWS: (1) demographic, (2) military career, (3) military environment, and (4) survey fieldwork factors. This analysis includes the full sample of 197,491 women and 280,022 men because all characteristics, including the survey fieldwork

indicators, are available for both respondents and nonrespondents (see Chapter Two). Table 3.1 provides a complete list of variables we included in the models used to construct the RMWS weights.

We report *risk ratios* (RRs) as a measure of the association of these factors with nonresponse in the RMWS, an effect size metric that we have used across all volumes of this study. An RR is the probability of survey response in the designated category (for instance, Hispanics) divided by the probability of response in the designated comparison category (for instance, whites). In order to estimate risk ratios, we exponentiated the coefficients from a regression model that used a log link function—specifically, a Poisson regression with robust error variance (Zou, 2004). The dependent variable was

Table 3.1
Predictors in Outcome-Optimized RMWS Weights

Demographic Factors	Military Environment Factors
Gender (male/female)	Percentage male within members' specific occupation ^a
Date of birth	Number of people within members' specific occupation ^a
Race/ethnicity	Percentage male at military installation ^b
Marital status	Number of people at military installation ^b
Total number of dependents (spouse, children, or others)	Percentage male in military unit ^c
Education level	Number of people in military unit ^c
Armed Forces Qualification Test (AFQT) Score	
Military Career Factors	Survey Fieldwork Factors
Branch of service	Change-of-address entered in DMDC records after sampling (Y/N)
Pay grade (20 categories)	Change of station after sampling (Y/N)
Days of active-duty service, past year (reserves only)	Change of station, past year (Y/N)
Cumulative months of active federal military service	No mailing address at time of sampling (Y/N)
Projected end date for current term	No email address at time of sampling (Y/N)
Date of entry into military service	First letter returned as postal nondeliverable (Y/N)
Separated or retired after sampling (Y/N)	Email sent by Marines (Y/N) ^d
Months deployed since 9/11/2001	Percentage of sent emails that bounced back
Months deployed since 7/01/2013	
DoD occupational group (20 categories)	
Duty unit location (CONUS/OCONUS)	

NOTES: Categories containing fewer than 40 cases among survey respondents were combined. CONUS = continental United States; OCONUS = outside the continental United States.

^a Derived from 302 DoD occupational categories.

^b Derived as two separate variables for each member's assigned installation (N = 3,031) and their duty installation (N = 3,147).

^c Derived as two separate variables for each member's assigned unit (N = 24,496) and their duty unit (N = 24,517).

^d Email invitations were sent directly by the Marine Corps for those Marines who did not have a valid email address in the DMDC-provided contact information.

a binary indicator with response status for the study, where 0 indicates nonresponse and 1 indicates response. In a second step, the factor was examined in a similar regression model that also included main (or marginal) effects of three covariates that were included in previous WGRA nonresponse models: branch of service, pay grade, and race/ethnicity (we stratified on gender; thus, we do not explicitly control for gender in the models). These models allow us to assess whether the candidate factors were associated with nonresponse, over and above what the standard covariates explain. We refer to the risk ratios produced in models that include these covariates as “adjusted risk ratios.” Here, we present just these adjusted models stratified by gender (Table 3.2, for women, and Table 3.3, for men), and discuss just those adjusted risk ratios that were significant at the $p < 0.01$ level. Complete results, including unadjusted risk ratios, are found in Appendix C, Tables C.1 and C.2.

Characteristics Associated with Nonresponse Among Women

Demographics. The joint test for race/ethnicity was significant. Blacks and Hispanics had adjusted risk ratios indicating they were 9 percent and 5 percent less likely to

Table 3.2
Characteristics Associated with Nonresponse Among Women

Variable	Sample Size (197,491)	Full Sample Mean	Respondent Mean	Adjusted Risk Ratio ^a	P-Value	P-Value from Joint Test
Demographics						
Age in years as of August 1, 2014 ^b		28.6	30.8	1.16	<0.0001	
Race/ethnicity						<0.0001
Non-Hispanic white (ref)	96,730	49.0	53.5			
Non-Hispanic black	53,570	27.1	25.2	0.91	<0.0001	
Hispanic	25,187	12.8	10.9	0.95	<0.0001	
Asian	8,887	4.5	4.8	1.01	0.3998	
Other	13,117	6.6	5.6	0.95	<0.0001	
Marital status						<0.0001
Married (ref)	90,723	45.9	51.8			
Never married	88,135	44.6	37.3	0.93	<0.0001	
Divorced/ separated/other	18,633	9.4	10.9	0.92	<0.0001	
Number of dependents		0.9	1.0	1.01	<0.0001	

Table 3.2—Continued

Variable	Sample Size (197,491)	Full Sample Mean	Respondent Mean	Adjusted Risk Ratio ^a	P-Value	P-Value from Joint Test
Education level						<0.0001
High school or less (ref)	114,516	58.0	44.9			
Some college	32,169	16.3	19.2	1.11	<0.0001	
Bachelor's degree	30,823	15.6	20.2	1.25	<0.0001	
Graduate degree	19,984	10.1	15.6	1.27	<0.0001	
Military Career						
Service branch						<0.0001
Air Force (ref)	59,324	30.0	40.1			
Army	69,445	35.2	34.2	0.76	<0.0001	
Navy	54,946	27.8	20.3	0.61	<0.0001	
Marine Corps	13,776	7.0	5.4	0.68	<0.0001	
Pay grade						<0.0001
E1–E3 (ref)	46,634	23.6	14.9			
E4	40,711	20.6	16.1	1.19	<0.0001	
E5–E6	55,798	28.3	29.7	1.55	<0.0001	
E7–E9	15,853	8.0	11.8	2.14	<0.0001	
W1–W5	1,699	0.9	1.3	2.44	<0.0001	
O1–O3	24,755	12.5	16.4	1.91	<0.0001	
O4–O6	12,041	6.1	9.7	2.29	<0.0001	
AFQT percentile (enlisted only) ^b		60.1	62.0	1.12	<0.0001	
Years of active military service) ^b		7.1	8.7	1.08	<0.0001	
Deployment status						<0.0001
Never deployed (ref)	97,712	49.5	44.3			
Deployed before 08/01/2013	81,197	41.1	48.3	1.00	0.5059	
Deployed after 08/01/2013	18,582	9.4	7.5	0.81	<0.0001	

Table 3.2—Continued

Variable	Sample Size (197,491)	Full Sample Mean	Respondent Mean	Adjusted Risk Ratio ^a	P-Value	P-Value from Joint Test
Months deployed since 9/11/2001) ^b		11.6	12.5	1.05	<0.0001	
Months deployed since 7/1/2013) ^b		2.8	3.0	1.10	<0.0001	
Separated/retired	8,980	4.5	0.6	0.13	<0.0001	
DoD occupational area						<0.0001
Infantry, guncrews, and seamanship specialists	8,554	4.3	3.4	1.00	0.9038	
Electronic equipment repairers	11,251	5.7	5.0	1.20	<0.0001	
Communications and intelligence specialists	16,689	8.5	8.0	1.15	<0.0001	
Health care specialists	24,582	12.4	14.0	1.34	<0.0001	
Other technical and allied specialists	4,607	2.3	2.5	1.25	<0.0001	
Functional support and administration (ref)	40,621	20.6	21.9	1.26	<0.0001	
Electrical/mechanical equipment repairers	20,983	10.6	7.2			
Craftswomen	4,040	2.0	1.4	0.96	0.2282	
Service and supply handlers	21,955	11.1	7.8	0.92	<0.0001	
Non-occupational	5,714	2.9	1.5	0.83	<0.0001	
Tactical operations officers	4,818	2.4	3.2	2.32	<0.0001	
Intelligence officers	2,833	1.4	2.0	2.36	<0.0001	
Engineering and maintenance officers	3,441	1.7	2.6	2.55	<0.0001	

Table 3.2—Continued

Variable	Sample Size (197,491)	Full Sample Mean	Respondent Mean	Adjusted Risk Ratio ^a	P-Value	P-Value from Joint Test
Scientists and professionals	2,366	1.2	2.0	2.77	<0.0001	
Health care officers	15,293	7.7	11.0	2.48	<0.0001	
Administrators	4,263	2.2	3.3	2.64	<0.0001	
Supply, procurement, and allied officers	3,825	1.9	2.7	2.45	<0.0001	
Other officers (20, 21, 29)	1,656	0.8	0.7	1.73	<0.0001	
Unit location						
Continental United States (ref)	163,301	82.7	81.3			
Outside the continental United States	34,190	17.3	18.7	1.07	<0.0001	
Military Environment						
Percentage male in occupation group ^b		74.8	72.8	0.96	<0.0001	
Size ^c of occupation group ^b		31,083.4	28,031.0	0.98	<0.0001	
Percentage male in unit ^b		77.3	76.5	1.00	0.1151	
Size ^c of unit ^b		404.9	281.8	0.91	<0.0001	
Percentage male in installation (zip code) ^b		82.2	81.9	0.99	<0.0001	
Size ^c in installation (zip code) ^b		9,864.3	9,051.6	0.95	<0.0001	
Fieldwork Indicators						
Change in assigned unit zip since 8/1/2013	55,563	28.1	24.6	1.02	<0.0001	
Change in assigned unit zip since 4/1/2014	39,243	19.9	14.6	0.72	<0.0001	
Change of mailing address since 4/1/2014	64,350	32.6	27.1	0.84	<0.0001	

Table 3.2—Continued

Variable	Sample Size (197,491)	Full Sample Mean	Respondent Mean	Adjusted Risk Ratio ^a	P-Value	P-Value from Joint Test
No valid mailing address	3,858	2.0	1.4	0.84	<0.0001	
Mailing 1 is postal nondeliverable	29,696	15.0	8.9	0.71	<0.0001	
No valid email address	8,180	4.1	0.8	0.23	<0.0001	
Marine Corps sent email	939	0.5	0.2	0.57	<0.0001	
Percentage of emails bounced ^b		8.6	1.0	0.81	<0.0001	

NOTE: P-values from individual tests of significance are shown in column 5, while the p-values for a joint test come from a chi-square score test; p-values for both the joint and individual tests appear in column 6. Variables marked “ref” are the reference variables in their categories.

^a The adjusted risk ratio comes from a model that includes race/ethnicity (indicated levels), service branch, and pay grade.

^b Indicates variables entered as continuous, for which the risk ratio corresponds to one standard deviation change in the variable (standard deviations are listed in Appendix C).

^c Size measured by number of people.

respond, respectively, compared with whites. Those with a marital status of “divorced, separated, or other” or “never married” were 7–8 percent less likely to respond relative to those “married.” An increase in education resulted in an increase in probability of response: Those with a graduate degree were 27 percent more likely to respond than those with a high school education or less. Also, a one standard deviation (eight-year) increase in age was associated with a 16-percent increase in response rate, while each additional dependent resulted only in a 1-percent increase in probability of response.

Military career. Those in the Army, Marine Corps, and Navy had a lower probability of response of 24 percent, 32 percent, and 39 percent, respectively, compared with Air Force service members. Those at pay grades of E4 or E5–E6 had a 19 percent and 55 percent higher response rate, respectively, compared with service members at the E1–E3 level. Service members at other pay grades (E7–E9, W1–W5, O1–O3 and O4–O6) had substantially higher response rates (with relative risk of 2 or greater) compared with the E1–E3 reference category. Those deployed after August 2013 were 19 percent less likely to respond, relative to those never deployed. Those deployed since 2001 were more likely to respond than those not deployed since 2001. An additional 11 months of deployment since September 11, 2001, was associated with a 5-percent increase in response rate, while an additional three months of deployment since July 1, 2013, was associated with a 10-percent increase in response rate.

There was a big decrease (87 percent) in response rate for those service members who had separated or retired after the sample was drawn (which is not surprising due

to the difficulty of contacting those who have left the services). A one standard deviation (18-percent) increase in AFQT scores was associated with a 12-percent increase in response rate, while an additional seven years of active federal military service was associated with an 8-percent increase in response rate. Service members posted outside the continental United States were 7 percent more likely to respond, compared with those within the continental United States. Occupational group was a significant predictor of survey response, as indicated by the significant joint test. A majority of the 18 occupational groups we analyzed had significantly higher response rates than the reference group (electrical/mechanical equipment repairers).

Military environment. An additional 15 percentage points in the percentage of males in a service member's occupation was associated with a 4-percent reduction in response rate, while an additional 6.4 percent males in one's installation was associated with a 1-percent reduction in response rate. An increase in size (measured by number of service members) reduced propensity to respond—an additional 32,000 persons in the occupation, an additional 500 persons in the unit, or an additional 10,000 persons in the installation was associated with 2-percent, 9-percent, and 5-percent reductions in response rates, respectively.

Survey fieldwork indicators. A change in station or mailing address after sampling (both indicators of a move just prior to survey fielding) resulted in 28-percent and 16-percent decreases in response rate, respectively. However, a change in station in the past year was associated with a slight (2 percent) increase in response rate. Also, the lack of a valid mailing address or an incorrect mailing address in DMDC records (resulting in the mailing being returned as postal nondeliverable) resulted in 16-percent and 29-percent decreases in response rate, respectively. Service members without a valid email address reduced their likelihood of response by 77 percentage points. Service members who received an email from the Marine Corps (an indicator that their email address was missing from DMDC records, but that the Marine Corps was able to send them a survey invitation on RAND's behalf) had a 43-percent lower likelihood of responding. Finally, with each additional 10 percent of survey notification emails not delivered (another indicator of the lack of a working email address), we saw a 19-percent reduction in likelihood of response.

Characteristics Associated with Nonresponse Among Men

Demographics. The joint test for race/ethnicity was significant. Blacks were 3 percent less likely, while Asians were 18 percent more likely, to respond relative to whites. Those with a marital status of "divorced, separated, or other" or "never married" were less likely to respond (21 percent and 17 percent, respectively) compared with those "married." An increase in education level was associated with an increase in the probability of response. Compared with those with high school education or less, having an education level of some college, a bachelor's degree, or a graduate degree increased the probability of response by 20 percent, 29 percent, and 34 percent, respectively. Also,

Table 3.3
Characteristics Associated with Nonresponse Among Men

Variable	Sample Size (280,022)	Full Sample Mean	Respondent Mean	Adjusted Risk Ratio ^a	P-Value	P-Value from Joint Test
Demographics						
Age in years as of August 1, 2014 ^b		29.1	32.9	1.26	<0.0001	
Race/ethnicity						<0.0001
Non-Hispanic white (ref)	183,534	65.5	68.9			
Non-Hispanic black	41,019	14.6	13.4	0.97	0.0031	
Hispanic	32,627	11.7	10.1	1.03	0.0119	
Asian	10,289	3.7	4.2	1.18	<0.0001	
Other	12,553	4.5	3.5	0.96	0.0131	
Marital status						<0.0001
Married (ref)	162,077	57.9	72.6			
Never married	107,985	38.6	23.3	0.79	<0.0001	
Divorced/ separated/other	9,960	3.6	4.1	0.83	<0.0001	
Number of dependents		1.5	2.0	1.05	<0.0001	
Education level						<0.0001
High school or less (ref)	189,582	67.7	48.7			
Some college	33,933	12.1	17.3	1.20	<0.0001	
Bachelor's degree	34,346	12.3	18.3	1.29	<0.0001	
Graduate degree	22,161	7.9	15.7	1.34	<0.0001	
Military Career						
Service branch						<0.0001
Air Force (ref)	63,865	22.8	34.1			
Army	108,411	38.7	37.4	0.67	<0.0001	
Navy	64,461	23.1	18.1	0.55	<0.0001	
Marine Corps	43,185	15.4	10.4	0.57	<0.0001	

Table 3.3—Continued

Variable	Sample Size (280,022)	Full Sample Mean	Respondent Mean	Adjusted Risk Ratio ^a	P-Value	P-Value from Joint Test
Pay grade						<0.0001
E1–E3 (ref)	64,409	23.0	9.5			
E4	54,450	19.4	11.6	1.39	<0.0001	
E5–E6	83,845	29.9	31.6	2.43	<0.0001	
E7–E9	28,613	10.2	17.6	3.96	<0.0001	
W1–W5	4,401	1.6	2.7	4.43	<0.0001	
O1–O3	25,658	9.2	13.6	3.37	<0.0001	
O4–O6	18,646	6.7	13.4	4.47	<0.0001	
AFQT percentile (enlisted only) ^b		63.9	65.5	1.09	<0.0001	
Years of active military service ^b		7.8	11.0	1.16	<0.0001	
Deployment status						<0.0001
Never deployed (ref)	112,101	40.0	28.2			
Deployed before 8/1/2013	135,307	48.3	61.5	1.04	<0.0001	
Deployed after 8/1/2013	32,614	11.6	10.3	0.87	<0.0001	
Months deployed since 9/11/2001 ^b		14.6	16.3	1.06	<0.0001	
Months deployed since 7/1/2013 ^b		3.0	3.1	1.10	<0.0001	
Separated/retired	12,243	4.4	0.7	0.17	<0.0001	
DoD occupational area						<0.0001
Infantry, guncrews, and seamanship specialists	44,050	15.7	8.6	0.71	<0.0001	
Electronic equipment repairers	22,553	8.1	7.5	1.10	<0.0001	
Communications and intelligence specialists	24,374	8.7	7.6	0.99	0.4665	
Health care specialists	14,523	5.2	5.5	1.33	<0.0001	

Table 3.3—Continued

Variable	Sample Size (280,022)	Full Sample Mean	Respondent Mean	Adjusted Risk Ratio ^a	P-Value	P-Value from Joint Test
Other technical and allied specialists	7,218	2.6	3.0	1.16	<0.0001	
Functional support and administration (ref)	25,060	8.9	11.0	1.27	<0.0001	
Electrical/mechanical equipment repairers	50,038	17.9	15.6			
Craftworkers	8,565	3.1	2.7	1.01	0.4596	
Service and supply handlers	26,135	9.3	7.4	0.92	<0.0001	
Non-occupational	8,801	3.1	1.3	0.89	<0.0001	
Tactical operations officers	19,990	7.1	11.6	4.08	<0.0001	
Intelligence officers	3,117	1.1	1.9	4.16	<0.0001	
Engineering and maintenance officers	6,983	2.5	4.7	4.71	<0.0001	
Scientists and professionals	2,980	1.1	2.2	4.75	<0.0001	
Health care officers	5,690	2.0	3.6	4.30	<0.0001	
Administrators	2,839	1.0	2.0	4.93	<0.0001	
Supply, procurement and allied officers	4,012	1.4	2.5	4.46	<0.0001	
Other officers (20, 21, 29)	3,087	1.1	1.3	3.19	<0.0001	
Unit location						
Continental United States (ref)	230,820	82.4	80.5			
Outside the continental United States	49,202	17.6	19.5	1.10	<0.0001	

Table 3.3—Continued

Variable	Sample Size (280,022)	Full Sample Mean	Respondent Mean	Adjusted Risk Ratio ^a	P-Value	P-Value from Joint Test
Military Environment						
Percentage male in occupation group ^b		86.1	84.8	0.88	<0.0001	
Size ^c of occupation group ^b		39,198.9	31,088.0	0.93	<0.0001	
Percentage male in unit ^b		86.4	84.6	0.90	<0.0001	
Size ^c of unit ^b		393.7	281.8	0.94	<0.0001	
Percentage male in installation (zip code) ^b		85.2	84.1	0.94	<0.0001	
Size ^c of installation (zip code) ^b		11,847.4	9,852.0	0.92	<0.0001	
Fieldwork Indicators						
Change in assigned unit zip since 8/1/2013	73,552	26.3	21.8	1.05	<0.0001	
Change in assigned unit zip since 4/1/2014	54,879	19.6	15.6	0.79	<0.0001	
Change of mailing address since 4/1/2014	83,832	29.9	24.6	0.84	<0.0001	
No valid mailing address	6,205	2.2	1.3	0.89	<0.0001	
Mailing 1 is postal nondeliverable	46,246	16.5	7.6	0.62	<0.0001	
No valid email address	17,440	6.2	1.0	0.24	<0.0001	
Marine Corps sent email	4,980	1.8	0.4	0.44	<0.0001	
Percentage of emails that bounced ^b		10.8	1.2	0.83	<0.0001	

NOTE: P-values from individual tests of significance are indicated in column 5, while the p-values for a joint test come from a chi-square score test and shown in column 6. Variables marked "ref" are the reference variables in their categories.

^a The adjusted risk ratio comes from a model that includes race/ethnicity (indicated levels), service branch, and pay grade.

^b Indicates variables entered as continuous, for which the risk ratio corresponds to one standard deviation change in the variable (standard deviations are listed in Appendix C).

^c Size measured by number of people.

a one standard deviation (eight-year) increase in age was associated with a 26-percent increase in response, while each additional dependent led to a 5-percent increase in response rate.

Military career. Those in the Army, Marine Corps, and Navy had a reduction in probability of response of 33 percent, 43 percent, and 45 percent, respectively, compared with those in the Air Force. A service member at a pay grade of E4 or E5–E6 had a 39 percent and 143 percent higher response rate, respectively, compared with E1–E3 service members. Service members at other pay grades (E7–E9, W1–W5, O1–O3, O4–O6) had response rates three to four times that of E1–E3 service members. Those deployed after August 2013 were 13 percent less likely, while those deployed after September 2001 were 4 percent more likely, to respond compared with those never deployed. For an additional three months of deployment in the past year, or 11 months of deployment since September 11, 2001, there were 10-percent and 6-percent increases in response rates, respectively. We also saw an 83-percent decrease in response rates among those who had separated or retired since the sample was drawn.

A one standard deviation (18-percent) increase in AFQT scores was associated with a 9-percent increase in response rate, while an additional seven years of active federal military service was associated with a 16-percent increase in response rate. Being outside the continental United States was associated with a 10-percent increase in response rate, compared with those in the continental United States. As was true for women, occupational group was significantly associated with response, with most occupational categories demonstrating response rates that were significantly higher than the reference group (electrical/mechanical equipment repairers). Indeed, several of the occupation groups had response rates that were three to five times greater than that of the reference group.

Military environment. We explored two types of environmental factors. One set looked at the percentage of males in the military environment. An additional 15 percentage points in the percentage of males in one's occupation, an additional 11.2 percentage points of males in one's unit, and an additional 6.4 percentage points of males in one's installation were associated with 12-percent, 10-percent and 6-percent reductions in response rates, respectively. We also explored the impact of size (measured by number of service members) of one's unit, installation, or occupation as another set of environmental factors. An additional 32,000 persons in the same occupation, an additional 500 persons in the unit, or an additional 10,000 persons in the installation were associated with 7-percent, 6-percent and 8-percent reductions in response rates, respectively.

Survey fieldwork indicators. A change in station or mailing address after sampling (both indicators of a move just prior to survey fielding) resulted in 21-percent and 16-percent decreases in response rates, respectively. However, a change in station in the past year was associated with a small (5-percent) increase in response rate. Also, the lack of a valid mailing address or an incorrect mailing address in DMDC records

(returned as postal nondeliverable) resulted in 11-percent and 38-percent decreases in response rates, respectively. Service members without a valid email address reduced their likelihood of response by 76 percentage points. Service members to whom the Marine Corps sent an email were 56 percent less likely to respond. Also, with each additional 10 percent of survey notification emails not delivered (another indicator of a bad or non-working email address), we saw a 17-percent reduction in likelihood of response.

Implications for Nonresponse Bias Adjustment

Many of the variables that we had hypothesized as potential correlates of response were confirmed to have significant associations with response status. In some cases, covariates were strong predictors of nonresponse, such as the more than fourfold differences in response rates among those with and without valid email addresses, among members of different occupational groups, and among men at different pay grades. Notably, many of these large associations remained even after controlling for gender, branch of service, pay grade, and race/ethnicity, covariates traditionally used in the industry standard for nonresponse weighting in military surveys. If the additional variables that we have identified as strong predictors of nonresponse are also correlated with our survey outcomes, their omission from nonresponse weighting presents a risk for nonresponse bias in survey estimates. In the following section, we examine the association of these same characteristics with the primary RMWS outcome measures.

Association of Participant Characteristics with Survey Outcomes

In this section, we describe the association of the demographic and other factors with three primary outcomes from the RMWS survey: (1) any sexual assault, (2) any sexual harassment, and (3) any gender discrimination. We selected these three outcomes because they are our primary outcomes. In a few analyses, we sub-divide sexual assault into three subtypes and sexual harassment into two subtypes, but those subtypes generally had similar predictors. The sample is restricted to survey respondents with non-missing responses for these three survey outcomes (67,187 women and 78,113 men). Here, again, we report adjusted risk ratios computed for males and females separately using models that control for the same key covariates: service branch, pay grade, and race/ethnicity. Findings for women and men are presented in Tables 3.4 and 3.5, respectively. In the sections that follow, we discuss only those risk ratios that are significant at the $p < 0.01$ level.

Characteristics Associated with Primary Outcomes Among Women

Demographics. Race/ethnicity and marital status were significant predictors of the three outcomes. Whites had the highest risk for sexual assault, sexual harassment, or

Table 3.4
Association of Participant Characteristics with Survey Outcomes Among Women

Variable	Sample Size (67,187)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Demographics										
Age in years (as of August 1, 2014) ^b	67,187	0.62	<0.0001		0.83	<0.0001		1.07	0.0040	
Race/ethnicity				<0.0001			<0.0001			<0.0001
Non-Hispanic white (ref)	35,938									
Non-Hispanic black	16,941	0.70	<0.0001		0.71	<0.0001		0.69	<0.0001	
Hispanic	7,345	0.69	<0.0001		0.95	0.2089		0.90	0.0468	
Asian	3,210	0.61	<0.0001		0.66	<0.0001		0.64	<0.0001	
Other	3,753	0.88	0.1684		0.99	0.8094		0.90	0.1713	
Marital status				<0.0001			<0.0001			<0.0001
Married (ref)	34,817									
Never married	25,049	1.89	<0.0001		1.20	<0.0001		0.93	0.0501	
Divorced/separated/other	7,321	2.17	<0.0001		1.40	<0.0001		1.26	<0.0001	
Number of dependents	67,187	0.86	<0.0001		0.95	<0.0001		1.03	0.0209	
Education				<0.0001			0.0016			0.3718

Table 3.4—Continued

Variable	Sample Size (67,187)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
High school or less (ref)	29,362									
Some college	12,572	0.77	0.0003		0.94	0.0815		1.02	0.6298	
Bachelor's degree	13,216	0.68	<0.0001		0.94	0.1349		0.96	0.4323	
Graduate degree	10,220	0.41	<0.0001		0.79	0.0003		1.06	0.4792	
Military Career										
Service				<0.0001			<0.0001			<0.0001
Air Force (ref)	26,940									
Army	23,010	1.58	<0.0001		1.89	<0.0001		2.28	<0.0001	
Navy	13,630	1.72	<0.0001		1.93	<0.0001		2.07	<0.0001	
Marine Corps	3,607	2.17	<0.0001		2.04	<0.0001		2.51	<0.0001	
Pay grade				<0.0001			<0.0001			<0.0001
E1–E3 (ref)	10,004									
E4	10,849	0.82	0.0012		1.22	<0.0001		1.50	<0.0001	
E5–E6	19,930	0.49	<0.0001		0.96	0.2640		1.50	<0.0001	
E7–E9	7,956	0.25	<0.0001		0.63	<0.0001		1.30	0.0002	
W1–W5	880	0.38	<0.0001		0.54	<0.0001		1.25	0.1147	
O1–O3	11,040	0.47	<0.0001		0.87	0.0013		1.29	<0.0001	

Table 3.4—Continued

Variable	Sample Size (67,187)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
O4–O6	6,538	0.16	<0.0001		0.44	<0.0001		1.42	<0.0001	
AFQT percentile (enlisted only) ^b	48,031	1.29	<0.0001		1.19	<0.0001		1.14	<0.0001	
Years of active military service ^b	67,134	0.69	<0.0001		0.82	<0.0001		1.05	0.1045	
Deployment status				<0.0001			<0.0001			<0.0001
Never deployed (ref)	29,742									
Deployed before 8/1/2013	32,432	0.93	0.1937		1.01	0.7564		1.12	0.0007	
Deployed after 8/1/2013	5,013	1.38	< .0001		1.28	< .0001		1.28	< .0001	
Months deployed since 9/11/01 ^b	37,445	0.90	0.0304		0.97	0.1846		1.01	0.6364	
Months deployed since 7/1/13 ^b	5,013	0.97	0.7745		0.95	0.2768		0.90	0.1137	
Separated/retired	421	1.94	0.0018		1.36	0.0228		1.73	0.0003	
DoD occupational area				<0.0001			<0.0001			<0.0001
Infantry, guncrews, and seamanship specialists	2,265	0.97	0.8231		0.71	<0.0001		0.64	0.0001	

Table 3.4—Continued

Variable	Sample Size (67,187)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Electronic equipment repairers	3,343	0.97	0.7599		0.99	0.9009		0.82	0.0144	
Communications and intelligence specialists	5,375	0.89	0.2398		0.93	0.1591		0.76	<0.0001	
Health care specialists	9,382	0.76	0.0020		0.71	<0.0001		0.56	<0.0001	
Other technical and allied specialists	1,659	0.90	0.4871		0.79	0.0035		0.66	0.0002	
Functional support and administration	14,683	0.73	0.0002		0.68	<0.0001		0.52	<0.0001	
Electrical/mechanical equipment repairers (ref)	4,855									
Craftswomen	913	0.80	0.2449		0.93	0.4310		1.11	0.3792	
Service and supply handlers	5,232	0.87	0.1471		0.88	0.0172		0.70	<0.0001	
Non-occupational	1,022	0.49	0.0005		0.41	<0.0001		0.27	<0.0001	
Tactical operations officers	2,165	0.19	<0.0001		0.49	<0.0001		1.37	0.0030	

Table 3.4—Continued

Variable	Sample Size (67,187)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Intelligence officers	1,330	0.23	<0.0001		0.46	<0.0001		1.17	0.2031	
Engineering and maintenance officers	1,714	0.18	<0.0001		0.45	<0.0001		1.25	0.0559	
Scientists and professionals	1,327	0.17	<0.0001		0.32	<0.0001		0.91	0.4717	
Health care officers	7,400	0.08	<0.0001		0.26	<0.0001		0.71	0.0003	
Administrators	2,206	0.18	<0.0001		0.40	<0.0001		0.96	0.7491	
Supply, procurement and allied officers	1,817	0.14	<0.0001		0.37	<0.0001		0.99	0.9063	
Other officers	499	0.21	<0.0001		0.52	<0.0001		1.19	0.3425	
Unit location										
Continental United States (ref)	54,555									
Outside the continental United States	12,585	1.21	0.0003		1.04	0.2690		1.03	0.4507	
Military Environment										
Percentage male in occupation group ^b	67,187	1.21	<0.0001		1.21	<0.0001		1.35	<0.0001	

Table 3.4—Continued

Variable	Sample Size (67,187)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Size ^c of occupation group ^b	67,187	0.99	0.5846		1.01	0.6245		1.03	0.1598	
Percentage male in unit ^b	66,973	1.15	<0.0001		1.16	<0.0001		1.23	<0.0001	
Size ^c of unit ^b	67,187	1.02	0.2735		1.05	<0.0001		1.02	0.2248	
Percentage male in installation (zip code) ^b	67,125	1.12	<0.0001		1.09	<0.0001		1.14	<0.0001	
Size ^c of installation (zip code) ^b	67,140	0.97	0.2812		1.02	0.2269		1.03	0.0458	
Fieldwork Indicators										
Change in assigned unit zip since 8/1/2013	16,499	1.06	0.2548		0.97	0.3738		0.92	0.0486	
Change in assigned unit zip since 4/1/2014	9,806	1.07	0.2990		0.90	0.0059		0.96	0.3598	
Change of mailing address since 4/1/2014	18,239	1.13	0.0131		1.01	0.6796		1.03	0.4648	
No valid mailing address	927	0.92	0.6370		0.70	0.0091		0.51	0.0040	
No valid email address	549	1.44	0.0515		1.41	0.0007		1.07	0.6621	

Table 3.4—Continued

Variable	Sample Size (67,187)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Mailing 1 postal nondeliverable	5,992	1.22	0.0027		0.98	0.6981		0.94	0.3273	
Marines sent email	125	0.91	0.7754		0.98	0.9433		1.17	0.6015	
Percentage of emails bounced ^b	67,187	1.03	0.0721		1.05	<0.0001		1.02	0.1019	

NOTE: P-values from individual tests of significance are indicated in the “P-Value” columns, while the p-values for a joint test come from a chi-square score test and shown in the “P-Value from Joint Test” columns. Variables marked “ref” are the reference variables in their categories.

^a The adjusted risk ratio comes from a model that includes race/ethnicity (indicated levels), service branch, and pay grade.

^b Indicates variables entered as continuous, for which the risk ratio corresponds to one standard deviation change in the variable (standard deviations are listed in Appendix C).

^cSize measured by number of people.

gender discrimination. Blacks and Asians had a lower risk of sexual assault or harassment, while Hispanics had a (31 percent) lower risk of sexual assault only, compared with whites. Those in the “never married” category had an elevated risk for sexual assault (89 percent) or sexual harassment (20 percent), while those in the “divorced, separated, or other” category had an increased risk across all three outcomes (26–117 percent), compared with those “married.” A one standard deviation (eight-year) increase in age was associated with a reduction in risk for both sexual assault (38 percent) and sexual harassment (17 percent), and a small increase (7 percent) in gender discrimination. Having an additional dependent was associated with a decrease in risk for sexual assault (14 percent) and sexual harassment (5 percent). An increase in education level was associated with a decrease in risk for sexual assault, across all levels, relative to those at an education level of “high school or less.” Having a graduate degree resulted in a significant decrease in risk of sexual harassment only (21 percent); education was not significantly associated with gender discrimination.

Military career. Service, pay grade, and occupational area were significantly associated with risk for one or more of the survey outcomes. Being a woman in the Army, Navy, and Marine Corps was associated with an increase in risk for sexual assault (58–117 percent), sexual harassment (89–104 percent), and gender discrimination (107–151 percent), compared with women in the Air Force. The relationship of the survey outcomes with pay grade was varied. While service members at the E4 level had an 18-percent lower risk of sexual assault, they had a 22-percent higher risk of sexual harassment and a 50-percent higher risk of discrimination, compared with women at the level of E1–E3. Service women at the E5–E6 level had a 51-percent lower risk of sexual assault, but similar risk (about 1.0) of sexual harassment, compared with E1–E3 women. Service women at other pay grade levels (E7–E9, W1–W5, O1–O3, and O4–O6) had a substantially lower risk of sexual assault and sexual harassment, and an increased risk of discrimination, compared with women at the E1–E3 level. Those deployed after August 2013 were (28–38 percent) more likely to experience sexual assault, sexual harassment, or discrimination, relative to those never deployed. Also, those deployed before August 2013 were 12 percent more likely to experience gender discrimination compared with those never deployed.

Among enlisted service women, a one standard deviation (18-percent) increase in AFQT scores was associated with increased risk across all outcomes: sexual assault (29 percent), sexual harassment (19 percent), and gender discrimination (14 percent). An additional seven years of active federal military service was associated with 31-percent and 18-percent decreases in risk of sexual assault and harassment, respectively. If the sampled woman had separated or retired since the sample was drawn, we saw a 94-percent increase in risk for sexual assault and a 73-percent increase in gender discrimination. Being outside the continental United States was associated with a 21-percent increase in sexual assault relative to being in the continental United States.

Among occupational groups, the reference group (electrical/mechanical equipment repairers) experienced the highest rates of sexual assault and sexual harassment, but not gender discrimination. Whereas occupational group was a significant predictor of sexual assault and sexual harassment, in many cases the differentiation appears to be one between occupations held by officers (who are exposed to lower risks) and those held by others. Exceptions to this rule include enlisted women in health care and administrative roles, who are exposed to lower rates of sexual assault and harassment, and women who are infantry, guncrew, or seamanship specialists, who experience lower rates of sexual harassment, compared with the reference group.

Military environment. The percentage of the work environment composed of men was significantly associated with risk of sexual assault, sexual harassment, and gender discrimination among women. An increase of one standard deviation (15 percentage points) in the percentage of males in the occupation group was associated with an increased risk of 21 percent for both sexual assault and sexual harassment, and 35 percent for gender discrimination. An increase of one standard deviation (11.2 percentage points) in the percentage of men in the unit was also associated with an increased risk for all three outcomes: sexual assault (15 percent), sexual harassment (16 percent), and gender discrimination (23 percent). Also, an increase in the size of the unit was associated with an increase in risk of sexual harassment among women. Finally, an increase of one standard deviation (6.4 percentage points) in the percentage men in one's installation was associated with an increased risk for sexual assault (12 percent), sexual harassment (9 percent), and gender discrimination (14 percent).

Survey fieldwork indicators. The associations between fieldwork indicators and the three survey outcomes were not consistent. The lack of a valid mailing address in DMDC records was associated with large reductions in women's risk of sexual assault (30 percent) and gender discrimination (49 percent), while the lack of a valid email address in DMDC records had a similar but opposite effect (41-percent increase) on the risk of sexual harassment. Service women whose first postal mailing was returned as nondeliverable were at 22-percent higher risk of sexual assault. Similarly, an additional 10 percent of the survey notification emails not delivered was associated with a 5-percent increase in risk of harassment.

Characteristics Associated with Primary Outcomes Among Men

Demographics. We found fewer significant associations between demographic characteristics and survey outcomes among men compared with women. Age, education level, and number of dependents were not significantly associated with the three survey outcomes. Blacks had a 27-percent reduction in risk for sexual harassment compared with whites. Those "never married" had an elevated risk of sexual assault (50 percent) and sexual harassment (24 percent), while those in the "divorced, separated, or other" category had an increased risk (39 percent) of sexual harassment, compared with those who are "married."

Table 3.5
Association of Participant Characteristics with Survey Outcomes for Men

Variable	Sample Size (78,113)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Demographics										
Age in years (as of August 1, 2014) ^b	78,113	0.88	0.1922		0.92	0.0459		1.11	0.1204	
Race/ethnicity				0.4839			0.0002			0.2618
Non-Hispanic white (ref)	53,812									
Non-Hispanic black	10,442	1.25	0.1566		0.73	0.0002		0.81	0.1164	
Hispanic	7,886	0.84	0.3643		1.10	0.2411		0.82	0.1812	
Asian	3,278	0.84	0.5498		0.96	0.7473		0.82	0.3616	
Other	2,695	1.02	0.9335		0.87	0.3225		0.76	0.2745	
Marital status				0.0186			0.0004			0.3832
Married (ref)	56,719									
Never married	18,198	1.50	0.0030		1.24	0.0005		1.03	0.7633	
Divorced/separated/other	3,196	1.21	0.5371		1.39	0.0078		1.37	0.1172	
Number of dependents	78,113	0.95	0.2652		0.96	0.0211		1.00	0.9043	
Education				0.1442			0.1980			0.4940
High school or less (ref)	37,207									

Table 3.5—Continued

Variable	Sample Size (78,113)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Some college	13,205	0.71	0.0746		1.01	0.9190		1.08	0.5464	
Bachelor's degree	14,032	1.07	0.7710		1.15	0.1409		1.10	0.5699	
Graduate degree	12,028	0.76	0.4389		1.36	0.0273		1.39	0.1310	
Military Career										
Service				<0.0001			<0.0001			<0.0001
Air Force (ref)	26,610									
Army	29,226	2.67	<0.0001		2.08	<0.0001		2.41	<0.0001	
Navy	14,157	3.61	<0.0001		2.19	<0.0001		2.52	<0.0001	
Marine Corps	8,120	2.43	<0.0001		1.43	0.0002		1.35	0.0963	
Pay grade				<0.0001			<0.0001			<0.0001
E1–E3 (ref)	7,383									
E4	9,066	1.37	0.1225		1.52	<0.0001		2.04	0.0003	
E5–E6	24,694	0.76	0.1546		0.77	0.0034		1.34	0.1218	
E7–E9	13,728	0.34	<0.0001		0.36	<0.0001		0.77	0.2285	
W1–W5	2,148	0.26	0.0118		0.31	<0.0001		0.90	0.7542	
O1–O3	10,606	0.44	0.0015		0.66	<0.0001		1.04	0.8658	
O4–O6	10,488	0.33	<0.0001		0.31	<0.0001		1.05	0.8216	

Table 3.5—Continued

Variable	Sample Size (78,113)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
AFQT percentile (enlisted only) ^b	54,160	1.22	0.0030		1.22	<0.0001		1.05	0.3482	
Years of active military service ^b	78,080	0.80	0.0396		0.80	<0.0001		0.97	0.6537	
Deployment status				0.1733			0.0648			0.611
Never deployed (ref)	22,039									
Deployed before 8/1/2013	48,027	0.76	0.0786		0.92	0.2473		1.02	0.8891	
Deployed after 8/1/2013	8,047	0.99	0.9405		1.13	0.1745		0.88	0.4632	
Months deployed since 9/11/2001 ^b	56,074	0.87	0.0864		0.85	<0.0001		0.81	0.0007	
Months deployed since 7/1/2013 ^b	8,047	0.95	0.8139		1.04	0.6828		1.03	0.8434	
Separated/retired	584	4.48	<0.0001		1.94	0.0027		2.53	0.0031	
DoD occupational area				0.1441			<0.0001			<0.0001
Infantry, guncrews, and seamanship specialists	6,745	0.97	0.8985		0.85	0.1425		0.64	0.0707	
Electronic equipment repairers	5,833	0.87	0.5668		1.31	0.0058		1.61	0.0128	

Table 3.5—Continued

Variable	Sample Size (78,113)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Communications and intelligence specialists	5,969	0.72	0.1986		1.02	0.8290		1.49	0.0417	
Health care specialists	4,320	0.83	0.4555		0.91	0.3918		2.06	0.0002	
Other technical and allied specialists	2,323	0.75	0.4458		1.18	0.2463		1.65	0.0516	
Functional support and administration	8,614	0.98	0.9334		0.95	0.6072		1.66	0.0045	
Electrical/mechanical equipment repairers (ref)	12,219									
Craftsworkers	2,094	0.74	0.4494		1.16	0.3308		0.81	0.5700	
Service and supply handlers	5,753	1.03	0.9150		1.07	0.5462		1.73	0.0047	
Non-occupational	1,001	0.79	0.6970		0.73	0.2805		0.74	0.6797	
Tactical operations officers	9,074	0.37	0.0042		0.30	<0.0001		0.94	0.8302	
Intelligence officers	1,450	0.37	0.0946		0.32	<0.0001		1.26	0.5639	

Table 3.5—Continued

Variable	Sample Size (78,113)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Engineering and maintenance officers	3,654	0.29	0.0054		0.28	<0.0001		1.23	0.5339	
Scientists and professionals	1,688	0.17	0.0159		0.28	<0.0001		2.36	0.0095	
Health care officers	2,823	0.18	0.0022		0.35	<0.0001		2.13	0.0131	
Administrators	1,575	0.24	0.0295		0.31	<0.0001		2.17	0.0214	
Supply, procurement and allied officers	1,991	0.39	0.0397		0.19	<0.0001		1.24	0.5726	
Other officers	987	0.13	0.0521		0.52	0.0084		1.65	0.2770	
Unit location										
Continental United States(ref)	62,835									
Outside the continental United States	15,201	1.33	0.0368		1.14	0.0332		1.20	0.0841	
Military Environment										
Percentage male in occupation group ^b	78,113	1.08	0.3537		0.98	0.5654		0.70	<0.0001	
Size ^c of occupation group ^b	78,113	1.03	0.6276		0.93	0.0064		0.94	0.1728	

Table 3.5—Continued

Variable	Sample Size (78,113)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Percentage male in unit ^b	77,741	0.99	0.8294		1.02	0.5879		0.68	<0.0001	
Size ^c of unit ^b	78,113	1.08	0.1069		1.04	0.1001		1.06	0.1838	
Percentage male in installation (zip code) ^b	78,008	0.94	0.3062		1.06	0.0706		0.86	0.0006	
Size ^c of installation (zip code) ^b	78,036	0.90	0.0678		1.02	0.4763		0.97	0.4553	
Fieldwork Indicators										
Change in assigned unit zip since 8/1/2013	17,030	0.95	0.7275		1.04	0.4975		1.05	0.6418	
Change in assigned unit zip since 4/1/2014	12,172	1.44	0.0164		0.96	0.5637		1.04	0.7698	
Change of mailing address since 4/1/2014	19,207	1.11	0.4164		1.06	0.2740		1.06	0.5567	
No valid postal address	1,018	0.88	0.8295		0.81	0.4282		0.38	0.1766	
No valid email address	743	2.98	0.0010		1.41	0.1037		1.79	0.1061	
Mailing 1 is postal nondeliverable	5,901	0.87	0.4965		0.82	0.0293		1.15	0.3565	
Marines sent email	285	4.13	0.0011		1.06	0.8707		0.71	0.7346	

Table 3.5—Continued

Variable	Sample Size (78,113)	Sexual Assault			Sexual Harassment			Gender Discrimination		
		Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test	Adjusted RR ^a	P-Value	P-Value from Joint Test
Percentage of emails that bounced ^b	78,113	1.09	0.0039		1.04	0.0216		1.03	0.4327	

NOTE: P-values from individual tests of significance are indicated in the “P-Value” columns, while the p-values for a joint test come from a chi-square score test and are shown in the “P-Value from Joint Test” columns. Variables marked “ref” are the reference variables in their categories.

^a The adjusted risk ratio comes from a model that includes race/ethnicity (indicated levels), service branch, and pay grade.

^b Indicates variables entered as continuous, for which the risk ratio corresponds to one standard deviation change in the variable (standard deviations are listed in Appendix C).

^c Size measured by number of people.

Military career. Service, pay grade, and occupational area were significantly associated with risk of one or more of men's survey outcomes. Being a man in the Army, Navy, and Marine Corps was associated with an increased risk of sexual assault (143–261 percent), sexual harassment (43–119 percent), and discrimination (35–152 percent), compared with those in the Air Force. Service members at a pay grade of E4 had a 52-percent higher risk of sexual harassment, and two times the risk of discrimination, as service members at the E1–E3 level. Service members at all other pay grades (E5–E6, E7–E9, W1–W5, O1–O3, O4–O6) had a reduced risk of sexual harassment compared with men at the ranks of E1–E3, while those at pay grades of E7–E9, W1–W5, O1–O3, O4–O6 had substantially lower risk of sexual assault than E1–E3 service men.

Among enlisted service men, an 18-percent increase in AFQT scores was associated with a 22-percent increase in risk of both sexual assault and sexual harassment. An additional seven years of active federal military service was associated with a 20-percent decrease in risk for both sexual assault and harassment. An additional 11 months of deployment since September 11, 2001, reduced the risk for sexual harassment and gender discrimination by 15 percent and 19 percent, respectively. If the sampled person had separated or retired since the sample was drawn, the risk for sexual assault, sexual harassment, and gender discrimination was 4.5 times, two times, and 2.5 times greater than those not separated or retired.

Occupational area was not significantly associated with the risk of sexual assault for men, but it was associated with sexual harassment and gender discrimination. For sexual harassment, the primary distinction was between occupations held by officers, in which men were exposed to lower risk of past-year sexual harassment than the reference group, and those held by others who experienced higher rates of sexual harassment. Men's gender discrimination experiences appear to be associated with such occupations as health care specialists, functional and administrative support, and scientists and professionals.

Military environment. Although an increase in the size of an occupational group by 32,000 people is associated with a small (7-percent) reduction in the risk of sexual harassment, for the most part military environment variables are not associated with men's risk of sexual assault or sexual harassment. In contrast, men's experience with gender discrimination is strongly associated with the percentage of men in their occupational group, unit, and facility. As the percentage of men in an occupational group increases, men's risk of past-year gender discrimination declines—an additional 15 percent males in one's occupational group corresponds to a 30-percent reduction in risk of discrimination. An additional 11.2 percent males in the occupation or an additional 6.4 percent males in the facility was associated with a reduction of 32 percent and 14 percent, respectively, in risk of gender discrimination.

Survey fieldwork indicators. A change in assigned unit since the sample was drawn was associated with a 44-percent increase in risk of sexual assault. Those without a valid email address in DMDC records were at three times the risk of sexual assault

compared with that of those with one. Service members to whom the Marine Corps sent an email (an indicator that their email address was missing in DMDC records, but that the Marine Corps was able to send them a survey invitation on RAND's behalf) had more than four times the risk of sexual assault compared with those who did not have an email sent. Also, those with an additional 10 percent emails returned (or bounced) had a 9-percent higher risk for sexual assault.

Observations on Association of Predictors with Primary RMWS Outcomes

We found fewer factors significantly associated with sexual assault and sexual harassment of men compared with women, but otherwise the associations were remarkably consistent in direction and magnitude with those found for women (likely due to smaller sample sizes for men). For sexual assault, all of the factors associated with assaults against men ($p < 0.01$) were similarly related to assaults against women, with the exception of three fieldwork factors that were predictive of risk for men but non-significant predictors for women. For sexual harassment, all but three factors associated with harassment of men were also associated with harassment of women, and the three exceptions (being of rank E5–E6, having an electronic equipment repair job, and the number of people in the member's occupational group) were all nonsignificant predictors for women.

The same is not true for gender discrimination, however, where only branch of service, pay grade, and recent retirement/separation are significant predictors of discrimination for both men and women. Instead, we see strikingly divergent effects of some occupational areas and military environment factors on gender discrimination for men and women. Whereas for women, the occupations of health care specialist, functional support and administration, and service and supply handlers are all significant predictors of lower risk of gender discrimination, men with those occupations are at higher risk of past-year discrimination. Similarly, whereas a higher percentage of men in a service member's occupational area, unit, or installation is associated with an increase in the risk of gender discrimination for women, it decreases that risk for men.

Several of the associations are surprising and bear further investigation. One is the strong relationship between recent separations from the military and past-year sexual assault. Specifically, women who recently separated are almost twice as likely as those who remain in the active component to have been sexually assaulted in the past year. Men who recently separated are more than four times as likely to have been sexually assaulted compared with those remaining in the military. Similar but smaller effects are also observed for gender discrimination. The mechanism underlying these effects is not yet clear, but one potential hypothesis is that exposure to sexual assault, sexual harassment, or gender discrimination causes people to leave the military. (Note that a hypothesis of causation is impossible to test with these cross-sectional data.)

Another finding that is surprising is that higher AFQT scores are associated with increased risk of both sexual assault and sexual harassment for men and women. The

AFQT score measures reading comprehension, vocabulary, and math and reasoning skills, but the observed association with sexual assault and harassment is in the opposite direction of factors (like education level) related to AFQT score. AFQT scores are a key determinant of occupations for which enlisted members might qualify. Therefore, it is plausible that jobs requiring higher AFQT scores are also associated with elevated risk of sexual assault or harassment. Here again, additional investigations are required to better understand the relationship between AFQT scores and our outcomes.

Finally, we cannot explain the mechanism that would lead to our finding that men without a valid email address have nearly three times the risk of a past-year sexual assault compared with those with a valid email address. While it is true that some E1–E3 service men may not yet have been assigned a military email address and junior enlisted personnel have higher rates of sexual assault, the association we have detected included controls for pay grade. To understand this finding, it would be necessary to learn more about how email addresses are assigned to service members, when they are eliminated or missing, and circumstances that lead to their omission from DMDC records.

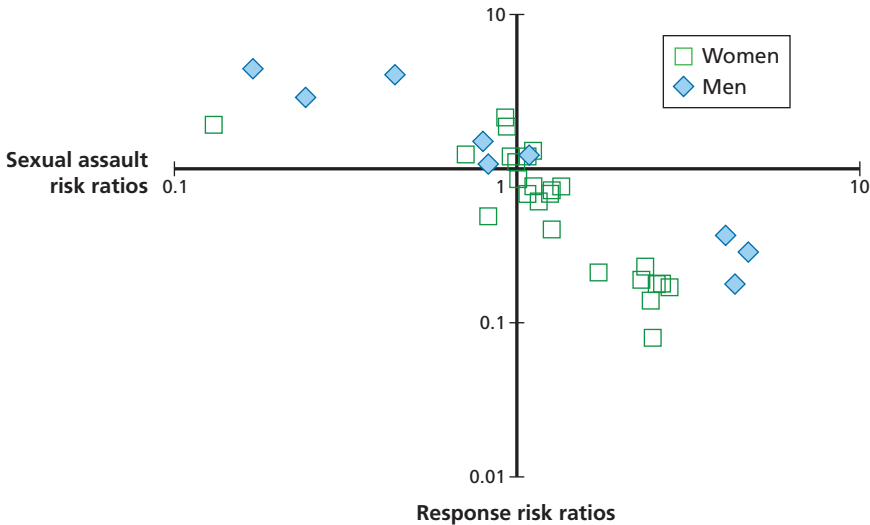
Characteristics That Could Lead to Nonresponse Bias

Many of the characteristics considered in the last section were associated with both survey nonresponse and primary survey outcomes in (adjusted) models that already included members' pay grades, branch of service, gender, and race/ethnicity. As such, the WGRA nonresponse weights that relied just on pay grade, branch of service, gender, and race/ethnicity would present a considerable risk of failing to correct for important sources of nonresponse bias. Whether such bias actually occurred, however, would depend on whether these factors work together to increase bias or whether they work at odds, canceling each other's effects.

We can examine this by looking at the direction of all those effects we found to be significantly associated with both survey response and with our primary outcomes. Figure 3.1 presents all such adjusted risk ratios significantly associated with survey response and past-year sexual assault at $p < 0.01$ (as indicated in Tables 3.4 and 3.5), controlling for gender, service, pay grade, and race/ethnicity, in order to show how much of the variance in response and sexual assault not explained by the conventional weighting approach can be explained with these additional variables. Because the effects are ratios, we use axes that are logarithmically scaled so that ratios above and below the value of 1 scale symmetrically.

What Figure 3.1 reveals is a strong log-linear correspondence between adjusted risk ratios for most of the factors. In particular, most points in this plot for men and women fall in the upper-left or lower-right quadrants. That is, the effects not accounted for by the standard weighting covariates are associated with higher risk of sexual assault

Figure 3.1
Adjusted Risk Ratios for Factors Significantly Associated with Survey
Response and Sexual Assault



NOTE: Risk ratios are adjusted for service, pay grade, and race/ethnicity. Axes use logarithmic scales. Ratios significant at the $p < 0.01$ level are included.

RAND RR87016-3.1

among those least likely to participate in the survey, or lower risk of sexual assault among those most likely to participate in the survey. In both cases, and for men and women, the effects of these factors would be to contribute to the underestimation of sexual assault.

This analysis provides compelling evidence of the importance of adjusting for as many of these factors as possible in the construction of sample weights for purposes of the RMWS survey, in order to eliminate the threat of underestimation bias they present. Further, given that large sample sizes reduce variance but not bias, even small bias reductions are a good trade-off in surveys, such as ours, with very large sample sizes and great precision (Elliott and Haviland, 2007). Adjusted risk ratios for other primary outcomes (sexual harassment and gender discrimination) exhibit the same pattern, and lead to similar conclusions.

The Development and Performance of RMWS Weights

When producing results from the prior form, we used the same weighting approach that was used in 2012 (called *WGRA weights*). When presenting results for the new RAND forms, we used a weighting approach designed to make the analytic sample representative of the population of active-component service members on a broader

range of factors (called *RMWS weights*).¹ In this section, we summarize the development of these two weights, and then compare them on several performance measures.

The WGRA and RMWS weights for the 2014 data are each a product of three component weights: (1) design weights, to account for disproportionate sampling of women and men; (2) nonresponse weights, to make the weighted participant sample comparable to the population on a set of characteristics known in the population; and (3) poststratification weights, to make proportions of weighted respondents identical to those in the population for key reporting categories. Where the two sets of weights differed was in the characteristics used to construct the nonresponse weights. In constructing the WGRA weights, we used a logistic regression model with predictors similar to those used previously by DMDC for past WGRA surveys.² For the RMWS weights, we sought to include all of the additional factors described in the prior section (listed in Tables 3.2 through 3.5).

Weighting on variables that are not associated with any survey outcomes cannot remove any nonresponse bias in those outcomes, but can increase the variance of the weights, with resulting reductions in the precision of estimates (Little and Vartivarian, 2005). Thus, the ideal weights are based on a nonresponse model that includes only those factors that are associated with the key outcomes and nonresponse. Such a model would remove the maximum amount of nonresponse bias, while limiting the variance in the weights to just the amount needed to eliminate nonresponse bias. Thus, the best weighting approach is one that has been optimized for the specific outcomes that the study is designed to measure.

To construct outcome-optimized RMWS weights, we first identified those factors statistically associated with one or more of six primary outcomes for our study (three types of sexual assault and three types of sex-based MEO violations). Specifically, a separate regression model was estimated for each of the six primary outcomes among survey respondents using the full range of administrative and survey paradata as predictors (see Table 3.1). The six resulting models are used to estimate service members' risk of each outcome in the full sample (both respondents and nonrespondents) as a weighted combination of the available predictors. Second, we estimated a model predicting survey nonresponse from gender, service branch, pay grade, and the predicted risk values from the six outcome models estimated in the first step. In this way, the large number of factors considered in the first step enter into the nonresponse model only to the extent that they are predictive of one or more of the primary outcomes.

¹ See Chapter Five of Volume 1 for a detailed discussion of the two sample weighting methods (Morral, Gore, and Schell, 2014).

² WGRA nonresponse/poststratification weights fully balanced respondents to the sample frame on the factors of gender, pay grade, service branch, and minority status. They partially balanced on deployment status, combat occupations, and marital status.

The regression models that predict each of the six primary outcomes were run separately for men and women because the relationship between risk factors and outcomes were hypothesized to differ across gender. These models were estimated using a machine-learning algorithm, Generalized Boosted Models (GBM; Ridgeway, 2012), to best capture the relationship between the predictors and the outcomes. GBM is a general, automated, data-adaptive modeling algorithm that can estimate the relationship between a variable of interest and a large number of covariates of mixed type, while also allowing for flexible nonlinear relationships between the covariates and the response propensity (Friedman, 2001; Ridgeway, 1999). These routines were run in the R software package. During the GBM estimation, the complexity of each model was optimized using tenfold cross validation—i.e., parameters were added to the model until the model maximized out-of-training-sample prediction. This procedure prevents overfitting the data.

The resulting models were used to create predicted values for each person in the full sample of both respondents and nonrespondents on each of our primary outcomes (e.g., the predicted probability of penetrative sexual assault). By definition, these predicted values are weighted combinations of the variables contained in Table 3.1. The original variables are no longer associated with the outcomes when controlling for these particular weighted combinations.

In the second-stage model, we derived the nonresponse weights using a response propensity model. Specifically, the response propensity model was estimated with a binary indicator of survey response (respondent versus nonrespondent) as the dependent variable with several independent variables: (1) the six predicted outcome variables from the initial step, (2) a 40-category indicator of the reporting categories (service x pay grade x gender), (3) form type (long, medium, or short), and (4) all two-way interactions among these predictors. Including the reporting categories and form types in this model insured that any nonresponse bias identified in this process was removed from both the aggregate DoD estimate and from estimates within various reporting categories.

The nonresponse model was also estimated using GBM (Ridgeway, 2012) to best capture the relationship between the various predictors and survey responding. This approach allowed for flexible modeling and has been shown to improve the performance of logistic regression (McCaffrey, Ridgeway, and Morral, 2004; Ridgeway and McCaffrey, 2007). Unlike ordinary GBM, in which parameters are added to the model until out-of-training-sample prediction is maximized, we wished to optimize the model to achieve the best weights. Specifically, we added parameters to the model until the resulting weights maximized the similarity between the respondents and the full sample. The similarity was assessed using maximum Kolmogorov–Smirnov statistic among all predictors in the model. Thus, the GBM stopped when the weights achieved the best balance between the cumulative distributions of respondents and nonrespondents on all of the predictor variables in the model.

Table 3.6 shows the balance achieved in these six predicted risk variables using the RMWS and WGRA weights, relative to the design-weighted estimate in the full sample (because the full sample included an oversampling of women, these design-weighted values provide an estimate of population values). The results in Table 3.6 demonstrate that the RMWS weights are more representative of the full sample estimate. The balance on the other predictors included in the nonresponse model—gender, pay grade, and service—was nearly perfect for both the RMWS and WGRA weights due to poststratification. More-detailed tables of predicted risk within each reporting category (cross-classified gender, service, and rank) are found in Appendix C, Tables C.1 and C.2.

Table 3.6 illustrates that, for every outcome except attempted sexual assault, the WGRA weights underestimated the level of risk in the population—by as much as 12 percent (in the case of non-penetrative sexual assaults). In contrast, the RMWS weights correctly matched respondents to population risk levels with good accuracy, the greatest discrepancy being for hostile work environment, where the RWMS weights yielded an estimate of population risk that was 1 percent over the true value.

Predictors of Discrepancy Between RMWS and WGRA Weights

The RMWS and WGRA weights yielded different prevalence estimates for our main outcomes. We were interested in identifying which of the factors used in the non-response weighting models contributed to these differences. As described above, the RMWS weights were derived using a two-step process to ensure that variables were included only if they were associated with both (a) one or more primary study outcome(s) and (b) propensity for nonresponse. Because of this two-step procedure, it is difficult to discern directly from the earlier tables (Tables 3.2 through 3.5) which specific administrative variables are responsible for differences across the two weights.

To identify significant predictors of differences, we estimated the associations between (a) the administrative variables used in weighting and (b) the difference

Table 3.6
Comparison of Predicted Risk in Full Sample and Nonresponse-Adjusted Respondents

Predicted risk of:	Design-Weighted Estimate in Full Sample	WGRA-Weighted Estimate Among Respondents	RMWS-Weighted Estimate Among Respondents
Gender discrimination	3.15%	3.11%	3.14%
Quid pro quo	0.39%	0.37%	0.39%
Hostile work environment	8.21%	7.83%	8.30%
Penetrative sexual assault	0.46%	0.43%	0.46%
Non-penetrative sexual assault	0.78%	0.69%	0.77%
Attempted sexual assault	0.02%	0.02%	0.02%

between the RMWS and WGRA weight for each individual. Specifically, the RMWS and WGRA weights were separately normalized so that the average respondent had a weight equal to 1, and then each individual's WGRA weight was subtracted from their RMWS weight. For any subgroup of respondents that represented the same proportion of the population under both weighting systems, the mean of this difference is zero. For example, if the proportions of the sample that were in the Marine Corps were identical under both weighting systems, the mean differences between these weights within the Marine Corps is zero. In contrast, a subgroup with a mean difference in weights equal to +1 would be one where the RMWS weights gave those individuals more weight than the WGRA weights. Specifically, if the WGRA weights gave individuals in that group the same weight as overall average respondent (i.e., 1), the RMWS weights gave them twice the weight of the average respondent ($1 + 1 = 2$). Similarly, a linear slope can be computed for continuous administrative variables to assess the extent to which each factor explains the difference between these weights. Table 3.7 provides the subgroup means or slopes and R^2 statistics from a model of the difference between weights to indicate the proportion of variance that can be attributed to each specific effect. For

Table 3.7
Association of Participant Characteristics with the Difference Between RMWS and WGRA Weights

Variable	Mean/Slope	R^2
Demographics		
Age in years (as of August 1, 2014) ^a	0.036	0.0002
Race/ethnicity		0.0003
Non-Hispanic white	0.001	0.0000
Non-Hispanic black	-0.017	0.0001
Hispanic	0.027	0.0001
Asian	-0.053	0.0001
Other	0.053	0.0001
Marital status		0.0030
Married	0.045	0.0029
Never married	-0.085	0.0025
Divorced/separated/other	-0.047	0.0001
Number of dependents ^a	-0.006	0.0003
Education		
High school or less	0.005	0.0000

Table 3.7—Continued

Variable	Mean/Slope	R²
Some college	-0.017	0.0000
Bachelor's degree	0.005	0.0000
Graduate degree	-0.006	0.0000
Military Career		
Service		0.0000
Air Force	0.000	0.0000
Army	0.000	0.0000
Navy	0.000	0.0000
Marine Corps	0.000	0.0000
Pay grade		0.0040
E1–E3	-0.146	0.0024
E4	0.128	0.0021
E5–E6	-0.001	0.0000
E7–E9	0.004	0.0000
W1–W5	-0.011	0.0000
O1–O3	0.000	0.0000
O4–O6	0.000	0.0000
AFQT percentile (enlisted only) ^a	-0.028	0.0000
Years of active military service ^a	0.031	0.0005
Months deployed since 9/11/01 ^a	0.008	0.0001
Months deployed since 7/1/13 ^a	-0.016	0.0003
Separated/retired	1.328	0.0101
DoD occupational area		0.0096
Infantry, guncrews, and seamanship	0.056	0.0002
Electronic equipment repairers	-0.029	0.0000
Communications and intelligence specialists	0.047	0.0002
Health care specialists	-0.247	0.0052
Other technical and allied specialists	-0.097	0.0002
Functional support and administration	-0.041	0.0003
Electrical/mechanical equipment repairers	0.181	0.0036

Table 3.7—Continued

Variable	Mean/Slope	R²
Craftworkers	0.067	0.0001
Service and supply handlers	0.031	0.0001
Nonoccupational	0.174	0.0004
Tactical operations officers	-0.004	0.0000
Intelligence officers	0.031	0.0000
Engineering and maintenance officers	-0.031	0.0000
Scientists and professionals	-0.017	0.0000
Health care officers	-0.015	0.0000
Administrators	-0.014	0.0000
Supply, procurement and allied	0.016	0.0000
Other officers	0.198	0.0003
Unit outside the continental United States	-0.031	0.0002
Military Environment		
Percentage male in occupation group ^a	0.083	0.0029
Number of people in occupation group ^a	-0.022	0.0004
Percentage male in unit ^a	0.125	0.0067
Number of people in unit ^a	-0.041	0.0036
Percentage male in installation (zip code) ^a	0.181	0.0041
Number of people in installation (zip code) ^a	-0.028	0.0008
Fieldwork Indicators		
Change in assigned unit zip since 8/1/2013	-0.043	0.0005
Change in assigned unit zip since 4/1/2014	0.019	0.0001
Change of mailing address since 4/1/2014	0.006	0.0000
No valid mailing address	0.046	0.0000
No valid email address	2.576	0.0491
Mailing 1 is postal nondeliverable	-0.124	0.0011
Marines sent email	3.349	0.0262
Percentage of emails bounced ^a	3.249	0.0964

^a Indicates variables entered as continuous, for which the parameter indicates the expected difference in weights per unit change in the variable.

categorical factors with multiple levels, such as pay grade, with 6 degrees of freedom, or occupation code, with 17 degrees of freedom, this table indicates the group effect.

In general, the fieldwork indicators were the most important factors explaining the difference between weights. Having a bad email address was positively associated with all primary outcomes for both men and women, and was strongly and negatively associated with propensity for survey participation. As a result, respondents who had a bad email address (indicated by either having no address or having email bounced back as undeliverable) were given substantially more weight using the RMWS weights. The mean difference of 3.2 suggests that the RMWS gave those individuals four times the weight as the WGRA weights, if the WGRA weights gave them the same weight as the average respondent.

Several other administrative variables also influenced the difference between weights. Specifically, the RMWS system gave greater weight to members who retired or separated after the sample frame was drawn, members from units or zip codes that were predominantly male, members from occupational categories that were predominately male, and individuals in specific occupational codes. In all of these cases, RMWS gave additional weight to individuals whose characteristics were underrepresented among survey participants (see Tables 3.2 and 3.3) and who were at elevated risk for sexual assault or sexual harassment (see Tables 3.4 and 3.5). Note that the linear effects summarized in Table 3.6 describe the marginal effects of these factors on the RMWS weights relative to the WGRA weights. However, the underlying models used nonlinear effects, including interactions of these characteristics across the 40 reporting categories, as well as nonlinear effects of the continuous variables. Therefore, Table 3.6, which only includes the simple linear effects of continuous variables, underrepresents the importance of those variables in explaining the difference between the RMWS and WGRA weights.

Statistical Characteristics of the WGRA and RMWS Weights

The WGRA and RMWS weights have a correlation of 0.78; the standard deviation of the RMWS weights is 1.38 times that of the WGRA weights, and both weights were normalized to have a mean of 1. Weighted estimates of the prevalence of primary survey outcomes were higher when using the RMWS weights, compared with when using the WGRA weights. To describe the practical effect of the higher standard deviation on the precision of survey estimates, we computed design effects. Design effects describe the loss in precision of survey estimates that can be attributed to sampling weights. For example, a design effect of 2 means that twice as many respondents are required to achieve the level of precision as a simple random sample design that does not require weighting would produce. The overall design effect associated with each set of weights was approximated with $(1 + CV_w^2)$, where CV_w is the coefficient of variation (Kish, 1965). Kish's formula is only an approximation, and may misrepresent the variance impact of the weights for any particular statistic of interest. However, these

approximations are still informative for relative comparisons, particularly when the impact of the weight components on the design effect is small, as in our case.

Tables 3.8 and 3.9 present the design effects associated with the three components of the sampling weights. The design effect associated with the design weights was 1.33 across both weights because the study design is the same. The oversampling of women in the design phase meant we had to sample 33 percent more people to achieve the same level of precision as would have been possible without any oversampling. The design effect of the nonresponse weights derived using the RMWS and WGRA approaches were 2.14 and 1.59, respectively. Also, the design effect associated with post-stratification in both weights was negligible. The overall design effect of the RMWS weights (3.69) was larger than that of the WGRA weights (2.62). In comparison, the design effect for the much smaller 2012 WGRA survey was approximately 2.53. Although the design effect associated with the RMWS weights was larger than that with the WGRA weights, the large sample sizes included in the RMWS study ensured ample precision for the outcomes of interest. Design effects for key reporting strata (pay grade, service) for men and women are provided in Appendix C, Tables C.3 and C.4.

The RMWS weights are more variable than the WGRA weights because the RMWS approach included many more factors in its adjustment. However, the RMWS weights should also reduce nonresponse bias in the population estimates of sexual assault, sexual harassment, and gender discrimination to a greater extent than the WGRA weights, because the RMWS weights include additional variables that are

Table 3.8
Design Effect of Components of RMWS Weights

	Mean	Standard Deviation	Design Effect
Design weights	1.00	0.57	1.33
Model-based nonresponse weight	1.00	1.07	2.14
Post-stratification weight	1.00	0.12	1.01
Overall WGRA weight	1.00	1.64	3.69

Table 3.9
Design Effect of Components of WGRA Weights

	Mean	Standard Deviation	Design Effect
Design weights	1.00	0.57	1.33
Model-based nonresponse weight	1.00	0.77	1.59
Poststratification weight	1.00	0.14	1.02
Overall WGRA weight	1.00	1.27	2.62

significantly associated with survey outcomes. In surveys with large sample sizes, however, even small reductions in bias may offer a good trade-off for variance inflation.

Evaluation of the Accuracy of Estimates using RMWS and WGRA Weights

In Table 3.10, we illustrate the effect of using the two sets of nonresponse weights on overall accuracy, as assessed by the MSE of prevalence estimates for primary survey outcomes. Weights can reduce MSE of a specific estimate to the extent that they reduce bias in that estimate, but can increase MSE to the extent that they increase the standard error of that estimate. Effective weights reduce MSE by eliminating more squared bias than they add in sampling variance (i.e., the square of the standard error of the estimate). This is called the *bias-variance trade-off*. A single set of analytic weights can cause different amounts of bias reduction and variance inflation for every variable in the analysis, so the effect of the weights on MSE is outcome-specific. Thus, estimates of the overall “design effects” of weights (e.g., Tables 3.8 and 3.9) do not tell us much about the performance of the weights on the key outcomes of the study. Individual outcomes may show more or less variance inflation than implied by the overall design effect, and even when there is substantial variance inflation, the weights may still be associated with reduced MSE on a specific outcome if accompanied by a reduction in bias.

Table 3.10 contains additional information about the performance of the weights across all of the primary outcomes of the study. It includes the primary outcomes from both the RAND form and the prior form of the 2014 survey. For each outcome, the table provides the prevalence estimate and the standard error for that estimate under three sets of weights: (1) design weights that account for the intentional oversample of women, but do not account for nonresponse; (2) WGRA weights that are derived similarly to the weights DMDC has used in prior WGRA studies and that were used when reporting estimates derived from the prior form; and (3) RMWS weights that were used for reporting results from the RAND form of the survey.

Table 3.10 demonstrates that (1) the RMWS and the WGRA nonresponse weights both result in an upward adjustment to the design-weighted estimate of prevalence for all survey outcomes, meaning that nonrespondents appear to be at greater risk of experiencing sexual crimes and violations than respondents; (2) the WGRA weights, which include an important but small set of predictors, produce lower prevalence estimates than the RMWS weights across all outcomes except for gender discrimination among men, where the two are almost equal; and (3) the standard errors associated with the RMWS estimates are larger than for WGRA estimates. Both sets of weights imply that survey nonresponse introduces a net downward bias in estimates of prevalence. However, the inclusion of additional variables in the development of RMWS nonresponse weights yields higher prevalence estimates than the reduced set of factors included in the WGRA weights.

Table 3.10
Evaluation of Survey Estimates with RMWS Weights Compared to WGRA Weights

Outcome	Design-Weighted Estimates ^a		WGRA-Weighted Estimates ^b		RMWS-Weighted Estimates ^c		RMWS weights have lower MSE if true prevalence is greater [less] than:
	Prevalence	Std. Error	Prevalence	Std. Error	Prevalence	Std. Error	
Overall							
Prior Form							
Unwanted sexual contact	1.11	0.06	1.43	0.11	1.68	0.17	1.59
Sexual harassment	5.51	0.12	6.00	0.20	6.23	0.24	6.16
RAND Form							
Sexual assault	1.03	0.03	1.30	0.05	1.54	0.08	1.43
Sexual harassment	6.62	0.10	8.06	0.17	8.85	0.23	8.47
Gender discrimination	3.19	0.06	3.23	0.09	3.33	0.10	3.29
Men							
Prior Form							
Unwanted sexual contact	0.63	0.06	0.93	0.12	1.16	0.19	1.08
Sexual harassment	2.73	0.13	3.50	0.22	3.64	0.27	3.65
RAND Form							
Sexual assault	0.47	0.03	0.74	0.06	0.95	0.09	0.85
Sexual harassment	4.24	0.11	5.92	0.20	6.61	0.27	6.29
Gender discrimination	1.50	0.06	1.74	0.10	1.73	0.11	[1.58]

Table 3.10—Continued

Outcome	Design-Weighted Estimates ^a		WGRA-Weighted Estimates ^b		RMWS-Weighted Estimates ^c		RMWS weights have lower MSE if true prevalence is greater [less] than:
	Prevalence	Std. Error	Prevalence	Std. Error	Prevalence	Std. Error	
Women							
Prior Form							
Unwanted sexual contact	3.37	0.15	4.31	0.22	4.61	0.26	4.48
Sexual harassment	18.57	0.33	20.23	0.40	20.94	0.44	20.60
RAND Form							
Sexual assault	3.60	0.08	4.51	0.11	4.87	0.14	4.70
Sexual harassment	17.82	0.22	20.35	0.28	21.57	0.31	20.97
Gender discrimination	11.11	0.18	11.77	0.21	12.40	0.24	12.09

^a The survey estimate was adjusted for oversampling of women, with no adjustment for nonresponse.

^b The prevalence estimate was computed using the WGRA nonresponse weights, in addition to design weights.

^c The prevalence estimate was computed using the RMWS nonresponse weights, in addition to design weights.

The differences between the various columns within Table 3.10 give direct information about the possible bias-variance trade-off for WGRA and RMWS weights. The difference between the standard errors indicates the relative sampling error associated with the two sets of weights. The difference between prevalence estimates provides information about the possible bias reduction associated with the different weights. For example, when two weights yield the same prevalence estimates, they necessarily offer the same bias reduction. In such a case, the weight that yields the smaller standard error provides the lower MSE, or greatest accuracy. When the estimates diverge, however, the estimate closer to the true population value offers greater bias reduction. However, that bias reduction needs to be considered in light of the standard error of the estimates to determine if it yields better accuracy overall, i.e., a lower MSE.

A direct calculation of MSE requires knowing the true population prevalence of these outcomes. Rather than estimating the MSE under specific assumptions about the true population prevalences for all outcomes, the final column of Table 3.10 presents the specific population prevalence for which the RMWS and WGRA weights would have the same MSE. This column integrates information about the difference between the two prevalence estimates and their relative standard errors into a single number.³ Whenever the true population prevalence is on the same side of that number as the RMWS weighted estimate, the RMWS weights provide lower MSE. Conversely, when the true population value is on the other side, the WGRA weights provide lower MSE.

Across the key outcomes, the population prevalence under which both weights would yield the same MSE typically falls between the WGRA and RMWS estimates. The two exceptions are sexual harassment of men (assessed on the prior form), and gender discrimination of men (assessed on the RAND form). For gender discrimination of men, the RMWS estimate yields higher accuracy only if the true population prevalence was lower than 1.58 percent. This appears unlikely, because both estimates are greater than that value. For this measure, the two weights have almost identical estimates and thus the slightly lower standard errors achieved by the WGRA weights

³ The population prevalence at which the two weights have the same MSE was calculated as

$$\mu_{equal_MSE} = \frac{\bar{X}_r^2 - \bar{X}_w^2 + V_{diff}}{2(\bar{X}_r - \bar{X}_w)}$$

where \bar{X}_r and \bar{X}_w are the prevalence estimates using the RMWS and WGRA weights, respectively, and V_{diff} is the difference in the squared standard errors of the estimates attributable to weighting. The computation of V_{diff} for these binary variables adjusts for the dependence of the standard error on the mean under the binomial distribution. Specifically, we have adjusted for this dependence by retaining the change in variance from the WGRA to the RMWS weights if the WGRA weights had resulted in the estimate obtained by the RMWS weights. The formula for μ_{equal_MSE} reduces to the mean of the two estimates when V_{diff} is zero, i.e., when the two weights have the same variance inflation, the estimate closer to the true value has lower MSE. When the two weights yield nearly same prevalence estimates, the formula takes on arbitrarily high or low values, indicating that the estimate with the lower standard error is preferred for all plausible values of the true population prevalence.

(0.01-percent lower) make it likely to be the more accurate measure, although the difference in standard errors is too small to be of any practical significance.

For all of the other variables, identifying which weights yield lower MSE depends on whether the variables added to the RMWS nonresponse model (relative to the WGRA model) resulted in (a) an exact correction of the true nonresponse bias; (b) an undercorrection for the true nonresponse bias, meaning that the true rate of sexual assault, for instance, falls above the RMWS estimate; or (c) an overcorrection for the true nonresponse bias, meaning the true value falls below the RMWS estimate. The RMWS weighting process identified substantial differences in risk between nonrespondents and respondents across the outcomes (as indicated by such characteristics as age, occupation, and the gender distribution of the unit). Because the RMWS weights achieved better balance between respondents and the full population on these risk indicators than the WGRA weights, it may be that the true prevalence on these outcomes is very close to the RMWS-weighted estimates. If one assumes that the RMWS weights provide an estimate very close to the true value, the RMWS weights yield more accurate results in all cases except sexual harassment of men (assessed on the prior form) and gender discrimination of men (assessed on the RAND form). In fact, for most variables, the RMWS weights appear to yield more-accurate estimates if one assumes that the nonresponse bias remaining with the RMWS weights is only slightly better than with the WGRA weights. For instance, the true value at which sexual assaults measured on the RAND form are equally accurately measured (a true prevalence of 1.43) falls less than 1 percent above the midpoint between the WGRA and RMWS weighted estimates (1.42). Therefore, if the RMWS weights yield estimates even moderately closer to the true value than the WGRA weights, we can conclude that the RMWS weights result in greater accuracy.

As noted elsewhere in this report (Chapters Two, Four, and Six), there are multiple indications that prevalence estimates using the RMWS weights may still underestimate the true prevalences. If the RMWS estimates are, in fact, underestimates of the true values, the RMWS weights yield more-accurate estimates than the WGRA estimates for all outcomes other than gender discrimination among men.

Finally, it is worth noting some limitations of selecting weights based on an MSE criterion, or designing weights to minimize MSE. The MSE metric treats errors due to biased estimation and errors due to sampling variability as equivalent. However, these are not always considered to be equal threats to the validity of a study. This is because errors caused by variance inflation can be easily assessed and are indicated by the confidence intervals presented alongside every prevalence estimate. The magnitude of this type of error is fully reflected in the population inference we draw from the results (i.e., in all tests of statistical significance). In contrast, error due to bias is invisible in the analyses and represents a direct threat to the validity of all study conclusions regardless of statistical significance. Thus, our analysis arguing that RMWS weights offer improved MSE relative to the WGRA weights underestimates the full advantage of the

RMWS weights if they result in lower bias, as appears likely. One might reasonably prefer the lower-bias estimate even if it were substantially less accurate, so long as the confidence intervals around estimates were sufficiently small to draw useful population inferences.

Conclusion

In estimating prevalence rates with survey data, individuals with the characteristic of interest (e.g., experienced past-year sexual assault) may be more or less likely to participate in the survey than people without it. The sample prevalence, based on respondents' data, is then a biased estimator of the true prevalence. The standard statistical approach to adjustment for this potential bias is nonresponse weighting. The nonresponse weighting approach developed for the RMWS employed novel methods necessary for modeling a rich and large set of individual characteristics drawn from demographic, career, and military environment characteristics available in DMDC administrative data, as well as indicators from the survey fieldwork. This new approach has allowed us to account for differences between respondents and nonrespondents on a much wider range of known characteristics than has previously been possible, without unacceptably elevating the variance of survey estimates.

In analyses comparing sample weights generated using the RMWS approach to those of the earlier WGRA approach, we found that the new weights reduced differences between the analytic sample and the population on a wide range of factors associated with both nonresponse and key outcomes. These differences between the respondents and the population were not satisfactorily addressed using the earlier methods. Moreover, nearly all of these factors drove bias in the same direction—specifically, service members with characteristics associated with a higher risk of sexual assault or harassment also had the lowest likelihood of responding to the survey. Therefore, the upward adjustment of estimates due to the RMWS weights, compared with the estimates produced by applying the WGRA weights, reflects a reduction in nonresponse bias and provides more accurate estimates of prevalence.

Examination of the factors leading to the greatest differences between the RMWS and WGRA weights revealed several fieldwork metadata factors that contributed substantially to explaining risk and survey nonresponse, including having no valid email address, having email addresses that bounced, and having an email address known only to the Marine Corps. It is obvious why the validity of a service member's email address would affect the likelihood of participation in the survey. Much less clear, and worthy of further investigation, is the question of why those at higher risk of sexual assault and sexual harassment would be disproportionately likely to have poor email address information in the DMDC data set.

Another important predictor of the differences between the WGRA and RMWS weights was whether the service member recently left the military, as those who recently separated appear to be at substantially elevated risk of having experienced past-year sexual assault, sexual harassment, or gender discrimination. This finding has important implications for how the survey is conducted in the future. If those who are sexually assaulted or harassed are more likely to leave the military, failure to count past-year sexual assaults among those who left the military by the time the survey is administered will bias prevalence downward. Therefore, our finding suggests the importance of estimating rates of sexual assault and harassment among all who served in the military in the past year, even those who separated prior to survey fielding. At a minimum, those who separated from the military after the sample was drawn and start the survey should not be counted as ineligible to participate when their service overlapped with the period in which prevalence is estimated. This, however, requires a change from past practice, as in earlier WGRA administrations all such recent separations were excluded from the sample of eligible respondents on the basis of the first survey question.

A common disadvantage of weighting is an increase in the variance of the estimates. We found that the RMWS weights led to only a modest increase in the sampling variability of the prevalence estimates. Whereas the overall design effect associated with the traditional WGRA weights was 2.62, the RMWS weights produced a design effect about 40 percent larger (3.69). An assessment of the trade-off between bias and variance for primary survey outcomes suggested that, under reasonable assumptions, the increases in variance were more than offset by reductions in bias for almost every outcome examined (the one exception being gender discrimination against men). Finally, given that large sample sizes reduce variance but not bias, even small bias reductions are a good trade-off in large sample sizes. We conclude, therefore, that there are several reasons to believe that the nonresponse weighting approach employed by the RMWS corrected for important sources of nonresponse bias without unacceptably driving up variability in prevalence estimates.

Investigation of Total Survey Error Using Official Records of Reported Sexual Assaults

Terry L. Schell and Andrew R. Morral

Most of the efforts to assess or quantify error in RAND's estimates of the prevalence of sexual assault have been focused on specific sources of potential error. Our nonresponse weights, and the various analyses of nonresponse, are focused on potential nonresponse bias; analysis of the instrument content, wording, and complexity is focused on minimizing potential classification error; efforts to reduce telescoping or maintain confidentiality are designed to reduce response biases; the large sample and attention to the variance of the weights are designed to minimize sampling error, while the inclusion of confidence intervals on our population estimates are intended to quantify sampling error.

What we would most like to know, however, is the net effect of all of sources of error, including potential coverage errors in our sample frame or processing errors in our handling of collected data. Do these various sources of error counteract each another, or are the errors compounded? In some cases, it may be possible to quantify total survey error regardless of the source. For example, a survey designed to predict population voting behavior can be compared with the subsequent voting behavior. The challenge in empirically estimating total survey error is finding alignment between a known value in the population of interest and a survey-based estimate of the same value. The current study was designed to estimate the prevalence of sexual assault in the past year within the military, and if the true prevalence over that period were known, there would have been no reason to conduct the survey.

The military does collect official statistics on the subset of sexual assaults that are reported (in either a restricted or unrestricted manner). OSD's SAPRO maintains records of these reports. While the number of official reports is just a subset of all sexual assaults, it is a number that is closely tracked in SAPRO annual reports. Moreover, the number of such reports provides a real-world benchmark against which we can compare survey estimates.

To facilitate this comparison, the RAND form included questions that were designed to align as closely as possible to official records of sexual assault. Specifically, respondents who were classified as having experienced a sexual assault in the past 12 months were asked:

Since [Date one year prior to survey administration], did you initial and sign a form labeled VICTIM REPORTING PREFERENCE STATEMENT (DD Form 2910 or CG Form 6095)? This form allows you to decide whether to make a restricted or unrestricted report of sexual assault. A Sexual Assault Response Coordinator (SARC) or Victim Advocate (VA) would have assisted you with completing this form. To see a version of this form, click here. [The final phrase served as a hyperlink to an image of DD Form 2910; Respondents were given three response options: “yes,” “no,” and “not sure.”]

DD Form 2910 serves as the basis for official records of sexual assaults. That is, to be included in the administrative records SAPRO uses to tabulate the number of official reports, service members who were sexually assaulted had to have completed this form. Therefore, to the extent that (a) the weighted sample was representative of the full population in terms of risk for sexual assault, (b) respondents were correctly classified as experiencing a sexual assault in the past year, and (c) they answered the questions in an unbiased manner, a survey-based estimate of the number of signed DD Form 2910s should closely correspond to recorded number of signed forms.

To align our survey estimates with numbers from the administrative data, we do not use the overall number of reported sexual assaults from the SAPRO report for FY 2014, but look at the subset of those reports that match the scope and timeframe of the survey estimates. Specifically, we requested from SAPRO the number of official reports where (a) the victim was in the active component of a DoD service, (b) the incident being reported occurred in a one-year period (FY 2014)¹ and (c) the report itself was signed in the same one-year period. SAPRO identified 2,997 reports that met these criteria.

As discussed in Volume 2 of this series, the RMWS survey estimated that 11.2 percent of those who experienced a sexual assault in the past year also indicated that they signed/initialed a DD Form 2910 in the past year. In addition, 10.7 percent indicated that they were “not sure” if they signed a DD Form 2910. Table 4.1 presents the population count of individuals that corresponds to those percentages.

Assessing the correspondence between survey estimates and official records of reported sexual assaults requires making assumptions about those respondents who answered “not sure” when asked about signing a DD Form 2910 and those who experienced a sexual assault but did not answer this question at all. In Table 4.1, we present two assumptions about those respondents:

- **Method 1.** One could estimate the total number of official reports based just on the proportion of the entire sample who answered “yes” when asked if they signed

¹ The period assessed by the survey was always one year prior to date of administration (for both the assessment of any sexual assault itself and the reporting of an assault). Thus, the dates used in the survey do not exactly correspond to FY 2014, but both estimates are for a one-year period and the periods almost entirely overlap.

Table 4.1
Comparison of Survey-Estimated Counts of Reported Sexual Assaults to Official Reports of Sexual Assault

Type of Count	Total	Men	Women
Official record of DD Form 2910	2,997	640	2,357
Survey estimates of DD Form 2910			
“Yes” survey response (Method 1 estimate)	2,177	374	1,803
“Not sure” survey response	2,165	1,150	1,015
“Yes” distributing “not sure” proportionately to Yes/No ^a (Method 2 estimate)	2,435	420	2,015

NOTE: Official records of signed DD Form 2910 for FY 2014 provided by SAPRO.

^a Estimates assume that the proportion of respondents indicating “Not Sure” who actually signed the form is the same as the proportion observed among those who answered with a definite response of “Yes” or “No.”

a DD Form 2910. The counts in the row labeled “Yes” *survey response* assume that none of the respondents who answered “not sure” actually signed form DD2910. This is computed based on the percentage of nonmissing responses to this question among those who experienced a sexual assault in the past 12 months. Thus, this approach assumes the true distribution across yes/no/not sure among victims who failed to answer this question is the same as the distribution among those who answered. However, this method assumes that none of those who said they were “not sure” if they completed the form actually did complete it. This does not appear plausible. There are many reasons why victims who signed DoD paperwork shortly after a traumatic experience may be unsure as to whether they signed a particular form. However, we include the Method 1 estimate here because the proportion of respondents who indicated “not sure” is substantial and we want to be explicit about the importance of assumptions about the true rate of reporting sexual assaults among that group.

- **Method 2.** In this approach, we estimated the number of official reports based on the number of survey respondents who say “yes” to the Form DD 2910 question, plus a portion of those who said “not sure.” Specifically, we made the assumption that among those who experienced a sexual assault in the past 12 months but who were either missing the item or who indicated “not sure,” the proportion who actually did sign the form is the same as the proportion as victims who gave a definite response of “yes” among all those who answered either “yes” or “no” to the question.² That is, “not sure” responses are treated in the same manner as

² More specifically, this method is equivalent to mean imputation of both item missing and “not sure” responses, conditioning those imputed values on both (a) experiencing a sexual assault in the past 12 months and (b) respondent gender.

missing data in Method 1. This is a more plausible assumption about the true rate of reporting among the “not sure” respondents, and is a relatively standard approach to handling ambiguous survey responses, but may still be inaccurate.

Using either method, the estimated number of reported sexual assaults in the population extrapolated from the survey is lower than the true rate of reported sexual assaults. For our primary estimate (Method 2), the survey estimate is 81 percent of the true population value (86 percent among women, 66 percent among men). This suggests that the survey is undercounting the number of individuals who experienced a sexual assault in the past year and reported it.

It may be, however, that our assumption that victims who say they are “not sure” they completed the form, actually completed it at higher rates than the proportion who said they did complete it among all who gave a definitive response. For example, if one assumes that 33 percent of those who responded “not sure” actually filed an official report, we would get a survey estimate that is almost identical to true value, suggesting no survey error.

Overall, we interpreted these analyses as demonstrating low levels of total survey error for the portion of sexual assaults in the past year that have been officially reported. Under relatively common assumptions, the survey estimates for the DoD population appear well aligned with, but smaller than, the true values. The precise causes of the identified total survey error are not known. This error could be a result of errors in our assumptions about the true reporting behavior of individuals who responded “not sure,” errors that apply only to the current analyses. Alternatively, the error could originate in some features of the broader survey that would affect our primary survey estimates. Possible sources of error include some combination of a measure of sexual assault that misses some true victims, undersampling of members at risk, nonresponse biases that are not fully mitigated by the RMWS weights, random/sampling error, or data processing errors. Although we cannot precisely identify the source of survey error, we can investigate two of these candidates, random sampling error and coverage error (i.e., undersampling some groups at risk).

Sampling error alone appears unlikely to fully account for the discrepancy. The 95-percent margin of error for these estimated total population counts is 370 individuals (251 among women, 365 among men). Thus, the true value is slightly greater than the upper confidence limit on the “Method 2” survey estimate for both the estimate among women and overall. (However, among men the true value is within the confidence interval of the survey estimate.) It is unlikely that the survey estimates (total and for women) would be this low if the only source of error were from random sampling variability.

Similarly, we know that the sample frame of the survey excluded some individuals who were included within the population covered by the SAPRO records of reported sexual assaults, suggesting some coverage error in the survey sample frame. For a range

of logistical and policy reasons discussed in Volume 1, the survey excluded (1) members who entered the service fewer than six months before the survey field period; (2) members who left the military early in the year, prior to survey sampling; and (3) general or flag officers (O7 and higher). In addition, the official statistics of reported sexual assaults may include more than one case filed by a single individual, whereas our survey estimates assume just one Form DD2910 per person making an official report.

As will be discussed in Chapter Six, the available evidence suggests the sample frame error is in the direction of undercounting members who were sexually assaulted, and that the primary source of this undercount comes from excluding individuals who separated from the military before our sample was drawn. We estimate that this coverage error could result in an undercount of 1,000 to 3,000 people that were sexually assaulted in the past year, of which perhaps 100 to 300 filed a sexual assault report (see Chapter Six). Therefore, coverage errors in the sample frame could account for much of the total survey error found using the Method 2 estimates above, although it cannot account for all of the identified error. However, the identified total survey error is small enough that the estimated sample frame coverage error, along with the known random sampling error, could explain the estimated total survey error without positing additional biases due to measurement, nonresponse adjustment, or data processing.

A substantial limitation of this analysis of total survey error is that it does not directly estimate error for the primary outcomes of interest. While the overall survey was designed to estimate the population prevalence of sexual assault, sexual harassment, and gender discrimination, we were only able to investigate total survey error for the small proportion of sexual assaults that were officially reported. It is possible that the low levels of survey error found on reported sexual assaults do not generalize to the survey error that occurs on the much larger number of unreported sexual assaults. For example, those victims who avoided telling military authorities about their assault may also avoid revealing the assault on a DoD survey. Alternatively, such individuals could conceivably be more interested than other service members in completing the survey and be overrepresented in the survey estimates. Our investigation of total survey error ruled out substantial error in one class of sexual assaults, but this investigation does not definitively rule out the possibility of substantial error in our overall study estimates of sexual assault, sexual harassment, or gender discrimination.

Performance of the Sexual Assault Survey Module

Lisa H. Jaycox, Andrew R. Morral, Terry L. Schell, and Coreen Farris

The construction of a new survey to assess incidence and prevalence of sexual assault raises questions about how the survey items function and whether the classification scheme is valid. A full description of the way in which the sexual assault questions map to the UCMJ can be found in Volume 1 (specifically, Appendix B) of this series. But even if the questions themselves are well aligned with the law, it is possible that the way survey items are used to classify individuals could result in misclassification of those who did or did not experience a sexual assault. In this chapter, we explain how the classification scheme works and how respondents answered each of the classification items. This information shows clearly which individuals were classified as having experienced a sexual assault and why others were ruled out.

RAND sought to pattern its survey questions on criteria in military law defining sexual assaults (Article 120 of the UCMJ). The law, however, is complex. Multiple criteria must be met to establish a crime, and these criteria specify events that themselves require complex definitions. To simplify for purposes of a self-administered survey, complex concepts are broken down into a series of sequential questions, and a skip logic is used to present relevant follow-up questions to determine whether unwanted events (the six sexual assault screening items) meet the intentionality and offender behavior criteria that define a UCMJ sexual assault in the past year.

Intentionality

For all assaults other than rape, the UCMJ requires that the unwanted contact must not have occurred accidentally or for some legitimate purpose, but instead must either have been for a sexual purpose or to abuse, degrade, harass, or humiliate the victim. The RAND form includes questions about offender intent from the perspective of the victim, so as to exclude events that were unwanted, but were, for instance, accidental contacts or contacts that were legitimate. There are many legitimate reasons that someone may touch a service member in a private area of his or her body that should not be counted as sexual assault. Unwanted contact with private areas can occur with some frequency during combat training, working in close quarters, uniform adjustments,

required medical exams, and the like. Although these contacts may be unwanted—and may occur without consent—they are not sexual assaults in either military or civilian jurisdictions if they are accidental or legitimate.

Indeed, because intent is such a core part of the law and of common-sense understandings of sexual assault, it figured prominently in the 2012 WGRA unwanted sexual contact question, which tried to exclude legal contacts where there was no sexual intent, in two ways: (1) It asked only about “intentional sexual contacts” and (2) it described the events covered by the question in a way that implied they occurred for a sexual purpose, using descriptions such as “sexual intercourse,” “oral sex,” or “anal sex.” These criteria were part of a single complex question that included additional conditions as well. The RAND form separates questions concerning intent, so as to focus respondents on one condition at a time. In addition, we expanded the intent criteria to include events that were not considered “sexual” and yet are classified as sexual assaults in the UCMJ. An example of this might be contact with genitals as part of a hazing ritual that is not viewed as a sexual experience, but rather as one intended to abuse, humiliate, or degrade.

Thus, the RAND form assesses intentionality with two questions asked of respondents who indicate an unwanted event other than penetration by a penis (under the UCMJ, penetration by a penis does not require that a sexual or abusive intent be established):

1. The first question asks if the unwanted experience was “abusive or humiliating, or intended to be abusive or humiliating?” Respondents who indicated that the unwanted event was abusive or humiliating, or was intended as such, met our “intent” criterion. While this is slightly different than the criteria laid out in 10 U.S. Code §920, Article 120(g)(1)(B), it is far easier for the respondent to answer. Moreover, it expands the domain of sexual assaults counted in our instrument to include hazing, abuse, and harassment incidents that some victims may not consider “sexual.”
2. Only if respondents answered “no” to the first question were they asked “Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused.” This is effectively equivalent to the approach used in the WGRA assessment of unwanted sexual contact to establish intent, though it is split off as a separate question rather than bundled with the other clauses making up the unwanted sexual contact question.

Offender Behavior/Lack of Consent

The second criterion that all sexual assaults must satisfy requires that the offender used one of several mechanisms specified in the UCMJ to compel the act or contact

(see Volume 1, Appendix B, for exact correspondence with the UCMJ). To assess this, respondents were asked to indicate which of the following occurred during the incident:

1. They [the assailant(s)] did not stop even when you told them or showed them that you were unwilling.
2. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.
3. They physically injured you.
4. They threatened to physically hurt you (or someone else).
5. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.
6. They did it when you were passed out, asleep, or unconscious.
7. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.
8. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).

If a respondent indicated that none of the offender behaviors listed above was present in the incident, he or she was asked about three additional situations that might describe the incident. The first two of these incidents are likely to meet the criteria of a crime under Article 120, but are less clear-cut than the criteria embodied by questions 1–8 above: (1) They made you so afraid that you froze and could not tell them or show them that you were unwilling; and (2) They did it after you had consumed so much alcohol that the next day you could not remember what happened.

The final item (“It happened without your consent”) was delivered to respondents who did not endorse any of the eight primary types of offender coercive behavior. It was placed last in the survey to catch any instances of nonconsent that were not captured in the earlier items. It was explicitly included to capture instances where an event happened so suddenly that explicit refusals were not possible and threats or force were not used. This could occur, for example, with a sudden groping of genitals that would not be well described by the offender behaviors listed earlier. The UCMJ also includes such a blanket “without consent” criterion to account for the same types of crimes.

Confirming Past-Year Time Frame

Finally, toward the end of the sexual assault section on the survey, respondents are asked a few questions to verify that the assault they just described occurred in the past year, and if not, when the most recent event of this type occurred (if there had been

other such events). Thus, there are several steps to the classification process. Every respondent is asked all six past-year sexual assault screener items in order from most-serious crime to least-serious crime, as indicated by the UCMJ.

- For the first item, penile penetration, a “yes” answer is followed up with just those questions establishing offender behaviors defined in the UCMJ. If they indicate that any of the qualifying offender behaviors occurred, they are classified as having experienced a sexual assault.
- For the next five screener items, a “yes” answer is followed by up to three steps if the respondent has not yet been classified as experiencing a sexual assault on one of the prior screening items.
 - Respondents are asked if the offender intent was abusive, humiliating, or intended to be abusive or humiliating.
 - Only if respondents answer “no” to the question on abusive intent are they asked if the intent was sexual.
 - Respondents who answered “yes” to either intent question are then asked which, if any, coercive offender behaviors occurred. If any of the specified behaviors occurred, the respondent is classified as having experienced a sexual assault.
- Later in the survey, respondents are asked to confirm that the event they identified happened in the past 12 months. If they confirm this, or that some other such event did occur in the past 12 months, they are classified as having experienced a sexual assault in the past year.

To illustrate how questions were presented, consider two scenarios:

1. Person A did not experience any unwanted contacts of these types in the past year, and thus answered “no” to each of the six unwanted event sexual assault screener items. He was not presented with any questions other than those six screener items. He was not classified as having experienced a sexual assault in the prior year.
2. Person B experienced unwanted kissing with tongue penetration and, in a separate event, unwanted touching of private areas in a hazing-type event. She answered “yes” to the second sexual assault screener item (i.e., “Since X date, did you have any unwanted experiences in which someone put any object or any body part other than a penis into your vagina, anus, or mouth? The body part could include a finger, tongue, or testicles”), but then answered “no” to the two questions inquiring about the offenders’ abusive, humiliating, or sexual intent, because she believed it to be the result of joking among a group of coworkers. By answering “no” to both items, she was skipped to the next screener item. She indicated a “no” for being made to penetrate another person, and was skipped to the next screener item. She indicated here that she experienced unwanted touching of her private areas. In this case, she did experience it as abusive or humiliat-

ing, and answered “yes” to this item. She then received questions related to non-consent or coercion, and indicated there was use of physical force, injury, and that the event continued when she showed she was unwilling. Accordingly, this respondent is classified as having experienced a non-penetrative sexual assault. She was skipped to the last two screener items, but was not asked follow-up questions regardless of her answers.

Table 5.1 shows the results of classification of past-year sexual assault for female and male respondents for each of the six screener items.

Next, we review the flow of individuals through the categorization process in order to understand Table 5.1 and the discussion that follows. Figure 5.1 illustrates the flow of the screening logic captured in Table 5.1.

In the first column of Table 5.1 (column A), we present the proportion of all active-component service members who endorsed any of the six unwanted event screening items. More members indicated that at least one of these events occurred than were classified as having experienced a sexual assault. Indeed, as reported in Volume 2 of this series, the percentage endorsing one or more unwanted events is approximately 1 percentage point higher than the percentage classified as experiencing a sexual assault for women, men, and overall. Respondents could answer “yes” to more than one of the six screening questions, so percentages in this column are overlapping. Note that this column provides confidence intervals, as does the last (Column F), because these are estimates that can be applied to the population of service members under study. The columns in the middle (B–E), however, do not have confidence intervals because they reflect the actual flow through this particular survey among the survey respondents and are not meant to be generalized to the broader population.

Some of the types of unwanted events were much more rare than others. For example, among women, being forced to penetrate someone else (0.31 percent) was less common than being penetrated (1.79 percent indicated penile penetration and 1.03 percent indicated penetration by some other body part or object). Penile penetration was more common among women (1.79 percent) than among men (0.23 percent). This pattern of results is similar to what would be expected given gender differences and known prevalence of the different types of crimes.

Column B shows the percentage of service members who indicated that they experienced each type of unwanted event, and who were not already classified as having experienced a sexual assault based on an earlier screening item (those who were already counted as experiencing a sexual assault were not asked the follow-up questions for subsequent screening items to reduce response burden).

Percentages in Column B are higher for men than women for three of the five remaining several assault types. This indicates that women were somewhat more likely to be classified as experiencing a sexual assault on the more-severe screener items that occur earlier in the questionnaire. For instance, about one-third of women who indicated they were subjected to unwanted touching of their private parts had already been

Table 5.1
Classification of Past-Year Sexual Assault Among Female and Male Respondents

	A	B	C	D	E	F
Sexual Assault Screener Since [X Date], . . .	Percent who had the unwanted experience (Steps 1–6 are overlapping categories) ^a	Of those in column A, percent who did not qualify for sexual assault on a previous question and, therefore, received follow-up questions	Of those in column B, percent who indicated offender’s intent was abusive, humiliating, or sexual.	Of those in column C, percent who indicated that at least one offender coercive behavior occurred	Of those in column D, percent who confirmed that the event occurred in the prior 12 months	Percent classified as experiencing this type of sexual assault (Steps 1–6 are mutually exclusive categories) ^a
FEMALES						
Step 1. . . did you have any unwanted experiences in which someone put his penis into your vagina, anus, or mouth?	1.79% (1.63–1.96)	(100%) ^b	(100%) ^b	94.18%	95.55%	1.61% (1.46–1.77)
Step 2. . . did you have any unwanted experiences in which someone put any object or any body part other than a penis into your vagina, anus, or mouth? The body part could include a finger, tongue or testicles.	1.08% (0.95–1.23)	49.02% (50.98% already classified in Step 1)	92.07%	97.7%	92.52%	0.44% (0.36–0.54)
Step 3. . . did anyone make you put any part of your body or any object into someone’s mouth, vagina, or anus when you did not want to?	0.31% (0.25–0.39)	16.71% (83.29% already classified in Step 1-2)	76.70%	75.09%	87.41%	0.03% (0.01–0.05)
Step 4. . . did you have any unwanted experiences in which someone intentionally touched private areas of your body (either directly or through clothing)?	4.66% (4.41–4.93)	66.65% (33.35% already classified in Step 1-3)	85.35%	95.49%	96.41%	2.44% (2.25–2.64)

Table 5.1—Continued

	A	B	C	D	E	F
	Percent who had the unwanted experience (Steps 1–6 are overlapping categories) ^a	Of those in column A, percent who did not qualify for sexual assault on a previous question and, therefore, received follow-up questions	Of those in column B, percent who indicated offender's intent was abusive, humiliating, or sexual.	Of those in column C, percent who indicated that at least one offender coercive behavior occurred	Of those in column D, percent who confirmed that the event occurred in the prior 12 months	Percent classified as experiencing this type of sexual assault (Steps 1–6 are mutually exclusive categories) ^a
Sexual Assault Screener Since [X Date], . . .						
Step 5. . . did you have any unwanted experiences in which someone made you touch private areas of their body or someone else's body (either directly or through clothing)? This could involve the person putting their private areas on you. (Private areas include buttocks, inner thigh, breasts, groin, anus, vagina, penis, or testicles.)	1.43% (1.28–1.6)	11.15% (88.85% already classified in Step 1-4)	73.39%	89.50%	97.82%	0.10% (0.07–0.15)
Step 6. . . did you have any unwanted experiences in which someone attempted to put a penis, an object, or any body part into your vagina, anus or mouth, but no penetration actually occurred?	1.23% (1.09–1.39)	17.18% (82.82% already classified in Step 1-5)	92.70%	92.21%	94.55%	0.17% (0.12–0.24)

Table 5.1—Continued

	A	B	C	D	E	F
Sexual Assault Screener Since [X Date], . . .	Percent who had the unwanted experience (Steps 1–6 are overlapping categories) ^a	Of those in column A, percent who did not qualify for sexual assault on a previous question and, therefore, received follow-up questions	Of those in column B, percent who indicated offender's intent was abusive, humiliating, or sexual.	Of those in column C, percent who indicated that at least one offender coercive behavior occurred	Of those in column D, percent who confirmed that the event occurred in the prior 12 months	Percent classified as experiencing this type of sexual assault (Steps 1–6 are mutually exclusive categories) ^a
MALES						
Step 1. . . did you have any unwanted experiences in which someone put his penis into your anus or mouth?	0.23% (0.15–0.34)	(100%) ^b	(100%) ^b	88.06%	97.90%	0.20% (0.12–0.31)
Step 2. . . did you have any unwanted experiences in which someone put any object or any body part other than a penis into your anus or mouth? The body part could include a finger, tongue, or testicles.	0.26% (0.16–0.39)	65.37% (34.63% already classified in Step 1)	53.08%	97.76%	99.10%	0.09% (0.03–0.21)
Step 3. . . did anyone make you put any part of your body or any object into someone's mouth, vagina, or anus when you did not want to?	0.18% (0.12–0.26)	49.82% (50.18% already classified in Step 1-2)	55.80%	88.79%	96.89%	0.04% (0.02–0.07)
Step 4. . . did you have any unwanted experiences in which someone intentionally touched private areas of your body (either directly or through clothing)?	1.41% (1.20–1.64)	83.78% (16.22% already classified in Step 1-3)	50.91%	95.35%	93.58%	0.54% (0.43–0.66)

Table 5.1—Continued

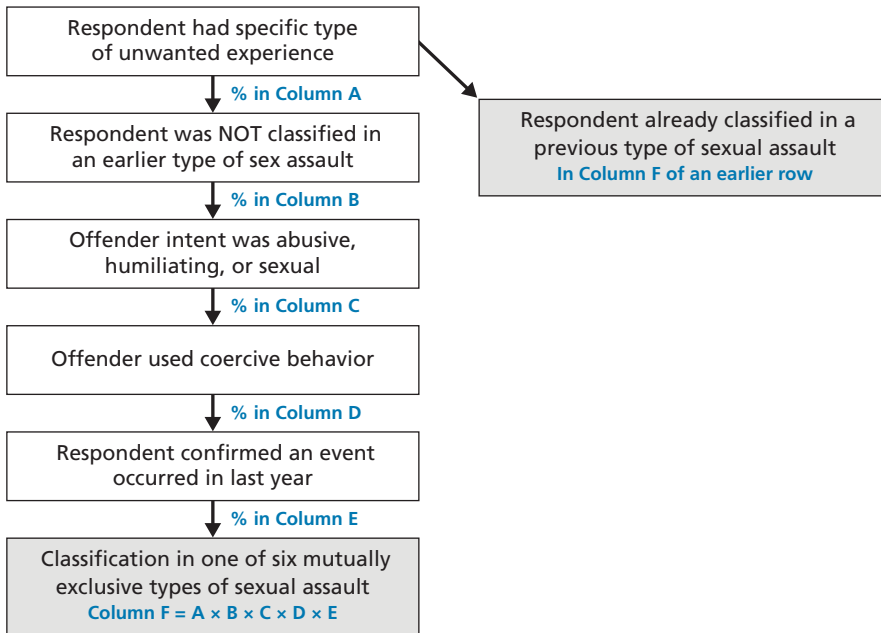
	A	B	C	D	E	F
	Percent who had the unwanted experience (Steps 1–6 are overlapping categories) ^a	Of those in column A, percent who did not qualify for sexual assault on a previous question and, therefore, received follow-up questions	Of those in column B, percent who indicated offender's intent was abusive, humiliating, or sexual.	Of those in column C, percent who indicated that at least one offender coercive behavior occurred	Of those in column D, percent who confirmed that the event occurred in the prior 12 months	Percent classified as experiencing this type of sexual assault (Steps 1–6 are mutually exclusive categories) ^a
Sexual Assault Screener Since [X Date], . . .						
Step 5. . . did you have any unwanted experiences in which someone made you touch private areas of their body or someone else's body (either directly or through clothing)? This could involve the person putting their private areas on you. (Private areas include buttocks, inner thigh, breasts, groin, anus, vagina, penis, or testicles.)	0.45% (0.32–0.61)	30.89% (69.11% already classified in Step 1-4)	67.11%	95.28%	90.78%	0.08% (0.03–0.18)
Step 6. . . did you have any unwanted experiences in which someone attempted to put a penis, an object, or any body part into your anus or mouth, but no penetration actually occurred?	0.24% (0.14–0.39)	20.83% (79.17 already classified in Step 1-5)	12.61%	65.69%	100.00%	0.00% (0–0.01)

NOTE: There are minor differences between the percentages reported in column F and related data presented in earlier reports. The data in this table handle missingness slightly differently than for primary study estimates. In this table, item missing data are treated as “no” responses because they had the same effect on the respondents’ skip pattern. In primary survey estimates, individuals with item missingness on one-half or more of the required criteria for a given measure are treated as missing at random, and population estimates are based on percentages among nonmissing.

^a Numbers in parentheses in columns A and F are confidence intervals.

^b Respondents automatically pass through these two steps for penile penetration. It is the first question on sexual assault, so they cannot have qualified for a sexual assault in an earlier item (column B). Respondents were not asked about the intent of the offender for penile penetration to align with UCMJ code (column C).

Figure 5.1
Flowchart of Survey Logic Underlying Categorization of Past-Year Sexual Assault



RAND RR870/6-5.1

classified as having experienced a penetrative sexual assault. In contrast, 15 percent of men who indicated they had been subjected to unwanted touching of their private parts had been already classified as experiencing a penetrative sexual assault.

Men were more likely than women to indicate that the unwanted event was neither sexual nor intended to abuse or humiliate them. Whereas women indicated these intentions occurred 73 percent to 93 percent of the time (across screener items), men noted these intentions 13 percent to 67 percent of the time (Column C in Table 5.1). These rates of endorsement were lower for men in three of the five assault types. Thus, men were less likely to have their unwanted events classified as sexual assaults. We break down the responses to these two intent items further in Table 5.2, but note that small numbers make these estimates imprecise. As can be seen in Table 5.2, there is variation in responses depending on the type of event. Fewer than one-half of respondents indicated that the event was intended to be abusive or humiliating. On the other hand, among those who indicated no abusive or humiliating intent, men indicated less often that the event was done for a sexual reason, as compared with women for three of the five types of events. Together, these items work to classify men and women on somewhat different grounds. As reported in Volume 2 of this series, men were more likely than women to have qualified as having experienced a sexual assault by indicating that the intent was to abuse or humiliate them (as opposed to a sexual purpose):

Table 5.2
Affirmative Responses to Questions About Assault Intent, Among Those Presented with the Question

	Affirmed Intent Was Abusive or Humiliating		(If No.) Affirmed Intent Was Sexual		Affirmed Intent Was Either Abusive, Humiliating, or Sexual ^a	
	Male	Female	Male	Female	Male	Female
Penile penetration	NA	NA	NA	NA	NA	NA
Penetration by other body part of object	48.14% (21.68–75.40)	43.74% (35.17–52.59)	9.00% (1.68–25.22)	86.09% ^b (77.90–92.11)	52.81% (25.63–78.80)	92.17% (87.49–95.52)
Forced to penetrate another person	21.71% (7.11–44.48)	43.39% (22.47–66.20)	41.73% (19.09–67.26)	NR (NR)	54.38% (30.81–76.59)	72.17% (48.61–89.20)
Touched in private areas	34.97% (27.71–42.77)	35.78% (32.47–39.18)	23.84% (17.03–31.82)	76.80% ^b (73.15–80.17)	50.47% (42.42–58.51)	85.10% (82.63–87.34)
Forced to touch another person	34.33% (9.17–68.54)	22.30% (12.32–35.32)	51.20% (31.60–70.54)	64.03% (47.55–78.40)	67.95% (46.53–84.97)	72.05% (57.94–83.56)
Attempted penetration	7.36% (0.30–31.75)	29.37% ^b (18.35–42.48)	NR (NR)	90.35% ^b (79.82–96.50)	11.20% (1.15–36.73)	93.19% (85.64–97.50)

^a The numbers in these columns differ slightly from those in Table 5.1, column C. Table 5.1 only includes individuals who were classified as study respondents, which required nonmissing responses to a minimum set of sexual assault questions. To better understand the pattern of exclusions caused by nonaffirmative responses to these questions, this table includes everyone presented with the question, regardless of whether they are considered to be an overall study respondent.

^b Indicates significant gender difference.

70.0 percent of men (95% CI: 60.2–77.8) compared with 41.7 percent of women (95% CI: 38.3–45.2). These estimates exclude sexual assaults involving penile penetration because intent questions were not asked in those cases.

Across screening items, 66 percent to 98 percent of respondents who indicated that their unwanted contact involved a criminal intent also indicated that there was offender coercion or lack of consent (Table 5.1, Column D). As such, most unwanted events for which criminal intent was present were classified as sexual assaults with the RAND form. As reported in Volume 2, penetrative assaults were more likely to have involved physical force, injuries, and threats than non-penetrative assaults, particularly among men, and also more likely to involve drug and alcohol incapacitation (for men and women) than non-penetrative assaults.

Column E of Table 5.1 indicates the rates at which individuals confirmed that the unwanted event classified as a sexual assault, or some other such assault, occurred in the previous year. In general, these rates were very high, with 87 percent to 100 percent of the assault types confirmed as occurring in the past year. Across sexual assault types, 4.7 percent of those classified as experiencing a sexual assault were found to have had no such experience in the past year (which is 0.08 percent [95% CI: 0.05–0.11] of the active-component population), with no differences apparent across service, gender, pay grade, or type of assault.

Finally, Column F of Table 5.1 shows the population estimates for individuals experiencing each type of assault. These values represent the product of values in Columns A through E. As can be seen, classification within the different events show different patterns. For example, very few individuals were excluded from being classified as having experienced a sexual assault after having experienced unwanted penetration, whereas more are excluded from being classified as having experienced a sexual assault after having experienced unwanted touching of private parts. This pattern makes sense, because it is more plausible that unwanted touching could occur by accident or for a legitimate purpose, and without explicit coercion, than could penile penetration. Thus, the pattern of classification has some clear face validity.

Columns C, D, and E together represent the filters that cause individuals who have not yet been classified as experiencing a sexual assault to have their unwanted event excluded from classification as a sexual assault. Their combined effect is fairly small for events involving penetration with a penis, for which 10 percent of women and 14 percent of men with such an unwanted experience end up not having this event classified as a past-year sexual assault. For women, these events are not counted as past-year sexual assaults either because they indicated there was no coercive offender behavior or failure of consent, or because they indicated the event took place more than a year ago, with both such conditions occurring more or less equally often. For men, in contrast, events involving penile penetration failed to be counted as past-year sexual assaults almost exclusively because the event did not involve coercion or failure of consent.

For other screening items, and for men in particular, the primary reason unwanted events fail to be classified as a past-year sexual assault is because of responses to the intent questions. That is, many respondents indicated that their unwanted event involved neither abuse or humiliation, an intention to abuse or humiliate, nor a sexual intention. Indeed, in a separate analysis, we have calculated that 99 percent of those active-component members who had an unwanted event but were not classified as experiencing a sexual assault were excluded from this classification based on their responses to the intent questions (the remaining 1 percent were excluded because their unwanted events did not involve coercion or the circumvention of consent).

Given the large number of unwanted events excluded from sexual assault classification due to the intent questions, we have considered whether the intent items may be causing the misclassification of true sexual assaults, with a resulting undercount of sexual assaults.

It could be argued, for instance, that victims are reluctant to speculate about the offender's intent, and that the intent questions present a difficult or impossible task for some victims. If this were the case, however, we might expect large numbers skipping the intent questions. Among those who indicated an unwanted event but who failed to be classified as having a sexual assault, just 1 percent did not meet the intent criterion because they skipped these questions. Thus, these respondents did not indicate that they could not speculate on the intention behind the contact; rather, they indicated that the intentions were not criminal. It is the victim's interpretation of that intent that determines whether *they* experience it as a violation or sexual assault as opposed to, for instance, an accident or a misunderstanding.

A second reason to suspect that the intent questions are not unduly restricting the experiences classified as sexual assaults is the fact that the RAND survey questions actually capture more instances of sexual assault than the WGRA's older method, which did not separately inquire about intent, but included intent as a part of a lengthy definition of unwanted sexual contact. As we discuss in Chapter Eight, after excluding those unwanted sexual contacts that occurred more than a year ago, the WGRA question produced a population estimate of about 18,400, in comparison with the RAND sexual assault assessment that produced an estimate of 20,300. These estimates are not statistically significantly different, but neither do they provide any suggestion that the RAND sexual assault assessment is leading to an undercount of sexual assaults. Indeed, we suspect that the intent questions contributed to our identification of sexual assaults not counted by the prior form, because our first intent question captures sexual assaults that are designed to abuse or humiliate, an intention identified by many respondents who also described the assault as "hazing." In contrast, the prior-form question emphasizes just sexual intentions, and could thereby fail to identify hazing and other abusive sexual assaults.

Finally, the large majority (81 percent) of those who experienced one of the sexual assault screening behaviors but who were subsequently not classified as experiencing

a sexual assault because of their responses on the intent questions occurred among those indicating an unwanted contact that did not involve nonpenile penetration or attempted penetration. This, too, may provide indirect support for the use of intention screening items. Specifically, our approach was to cast a broad net around instances of unwanted contact with private areas, and then to winnow out those unwanted contacts that were accidental, legitimate (such as medical exams), or other events that were not perceived as abusive, degrading, or sexual. In most of these cases of unwanted contacts that would not be considered sexual assaults, we would expect the contacts not to involve penetration. Therefore, the fact that non-penetrative assaults make up the large majority of cases that are discounted as sexual assaults due to the intention questions is consistent with our expectations.

The fact that our offender-behaviors questions resulted in just 1 percent of those with unwanted contacts being classified as not sexually assaulted raises the possibility that those questions are too broad, effectively counting all offender behaviors as evidence of a crime. Whereas most of these questions describe unambiguous forms of unlawful coercion, such as use of force, threats, or deception, there are a few offender behavior questions that are arguably more ambiguous. This is particularly true for the last three questions, which are seen only by respondents who have not selected any of the eight offender behaviors that are unambiguous. These final three items concern contacts that occurred when (1) “they made you feel so afraid that you froze and could not tell them or show them that you were unwilling”; (2) “they did it after you had consumed so much alcohol that the next day you could not remember what happened”; or (3) “it happened without your consent.”

As a practical matter, only the last of these potentially ambiguous questions is important, because, as discussed in Chapter Six, less than one-quarter of 1 percent of those we classified as experiencing a sexual assault indicated alcohol blackout or frozen in fear were the only means by which the unwanted contact was coerced. In contrast, approximately 18 percent of unwanted contacts were described as happening without the respondent’s consent.

The rationale for including a “without your consent” option is to account for a range of types of sexual assault that do not involve threats of violence or deception, but where the victim never had a chance to say or show that they did not consent to the contact. The most obvious example is unexpected groping that occurs before the victim has a chance to refuse the contact. This appears to be the reason that an all-purpose nonconsensual contact mechanism is included in the Article 120 definition of sexual assault. Specifically, in addition to the mechanisms involving force, threats, drugs, or deception, Article 120 explicitly includes “offensive touching of another, however slight, including any nonconsensual sexual act or nonconsensual touching.”

Here, again, the most conspicuous examples of sexual assaults that involve no other listed coercive mechanism, yet the respondent did not provide consent, would seem to be non-penetrative contacts that occur too quickly for the victim to indicate

they do not consent. This is, in fact, consistent with what we see in our results: whereas less than 1 percent of penetrative sexual assaults were classified as such on the basis of the “did not provide consent” option, closer to 30 percent of non-penetrative sexual assaults qualified as such on the basis of respondents indicating they did not provide consent (see detailed results in Volume 2, Chapter Three). This does not prove that the “did not provide consent” option is not counting too many unwanted contacts as sexual assaults. However, it does suggest that the pattern of events classified as assaults on the basis of this question is plausible, and it does not strain credulity to imagine that 30 percent of non-penetrative assaults, or 18 percent of all sexual assaults, could involve contacts like groping where coercive force is not used, and where the victim does not have the opportunity to say or show that they do not consent to the contact.

Given the very slight effect the offender-behavior items have on the classification of sexual assault, it might be reasonable in some contexts to omit those questions altogether from future administrations of the survey. Still, the contextual information they provide about the circumstances and nature of the assaults is valuable, and including these items may be important for demonstrating strict adherence to the definitions of sexual assault provided in the law.

Conclusions

Results of this analysis show a few key aspects of the performance of the sexual assault module of the survey, and lend some support to face validity—in as much as the pattern of results resemble known rates of offenses and fit logical and consistent patterns based on gender and type of crime:

- Use of specific behavioral screener items alone, without qualifiers as to intentionality or lack of consent, results in higher estimates of prevalence than when these criteria are applied. That is, some individuals are not classified as having experienced a sexual assault despite having experienced unwanted events in the prior year.
- Use of specific behavioral screener items may pick up individuals who would not use the word “sexual” to describe their experience, but who indicate the intent was to abuse or humiliate. Items used to capture intention of the event show that men and women indicate that the intent was abusive or humiliating at equal rates, but when they indicate the event was not abusive, women are more likely to indicate it was sexual intent than men. As such, among those classified as having experienced a sexual assault, a higher proportion of men than women are classified on the basis of saying the unwanted event was done to humiliate or abuse. Thus, this classification scheme may detect some assaults against men in particular that would not be detected with screener items used in the past.

- Items used to capture intention of the event cause some non-penetrative contacts to not be classified as assaults, but few nonpenile penetrative contacts are excluded on the basis of the intention items. This is consistent with the expectation that some unwanted contacts identified with the screener items could include accidental or legitimate contacts that correctly would not be classified as sexual assaults.

For the most part, the specific behavioral screener items with intentionality confirmed capture the types of events that are described in the UCMJ. That is, the vast majority of respondents indicate that the event included some type of offender behavior or lack of consent that conforms to UCMJ definitions.

Undercounting and Overcounting of Service Members Exposed to Sexual Assault

Andrew R. Morral, Terry L. Schell, and Lisa H. Jaycox

Our methods for estimating the proportions of service members exposed to sexual assault in the past year and the total number of past-year assault victims are subject to several sources of possible error. Here, we consider six:

- Inclusion of preservice assaults
- Exclusion of assaults against members with fewer than six months of service
- Inclusion or exclusion of tonic immobility and alcohol blackouts
- Exclusion of members who recently separated from the military
- Inclusion of nonpenile oral penetration in the penetration counts
- Possible exclusion of civilian sexual assaults among reserve-component members.

Inclusion of Preservice Sexual Assaults

The study sample frame included service members who joined the service six months ago or longer. Therefore, some sampled members had only been in the military for six to 12 months. Possibly, therefore, any past-year sexual assault reported by this group could have occurred during the portion of the past year that preceded their service entry, in which case including these respondents would inflate the apparent risk to members of the military. That is, if a member with eight months of active-component experience describes an event that occurred 11 months earlier (when they were not a service member), this assault should not be counted as one against a service member.

A total of 2.4 percent of the active-component sample had served for six to 12 months, and 1.3 percent of this subgroup reported a past-year sexual assault. If all of their sexual assaults were excluded from the estimate of the proportion of active-component men and women who experienced one or more sexual assaults, it would have the effect of reducing that proportion from 1.54 percent to 1.51 percent, or it would reduce the total number of active-duty members projected to have experienced a past-year sexual assault from about 20,300 to about 19,900. Therefore, the maximum possible overcount due to those with six to 12 months of service is relatively small.

We can get a better estimate of the possible effect of preservice assaults on our service estimates by examining what respondents said about who assaulted them and the circumstances of the assault. Table 6.1 describes the proportion of all new service members who provided details of the “worst” past-year assault that links it to their time in the military, as opposed to during the time before they joined. The results suggest that more than 98 percent of the assaults reported by those with six to 12 months of active-duty service occurred at a military installation or on a ship, during basic training or other military training, or was in some other way linked to their military service. Indeed, approximately 91 percent of the assaults were committed by another service member. While it is possible that the remaining 2 percent of assaults against new service members occurred before joining the military, the effect of excluding these cases from the overall estimates is small: the overall proportion of active-duty members estimated to have experienced a past-year sexual assault declines from 1.537 percent to 1.536 percent, meaning the population estimate of 20,300 members with past-year sexual assaults could be an overestimate by fewer than ten members.

Together, these results suggest that the influence of preservice past-year assaults on our estimates of the proportion of members who experienced past-year assaults is negli-

Table 6.1
Proportion of Sexual Assaults Linked to Military Service, All Sexually Assaulted Active-Duty Respondents with Six to 12 Months of Service

Question	Proportion
At the time of the event, was the person who did this to you someone in the military?	90.67 (76.97–100)
Did the unwanted event occur at a military installation/ship, armory, or reserve unit site?	NR (67.38–97.92)
Did the unwanted event occur while you were on temporary duty/temporary additional duty, at sea, or during field exercises/alerts?	NR (0–66.18)
Which of the following best describe the situation when this unwanted event occurred? You were at a military function.	NR (31.24–100)
Offender was a civilian employee or contractor working for the military?	6.75 (0–20.51)
Did the unwanted event occur while you were completing military occupational specialty school/technical training [etc.]?	NR (31.43–88.96)
Did the unwanted event occur while you were in recruit training/basic training?	NR (8.98–78.6)
Did the unwanted event occur while you were in Officer Candidate or Training School/Basic or Advanced Officer Course?	1.11 (0–3.47)
Did the unwanted event occur while you were in any kind of military combat training?	NR (0–68.17)
Any of the above indicators that crime related to military service or military personnel	97.68 (93.93–100)

gible. The shift in the estimate is not practically significant—smaller than our rounding error—and would not be statistically significant even within our large sample.

Exclusion of Assaults Against Members With Fewer Than Six Months of Service

Following procedures established by DMDC for the WGRA surveys, the 12,469 officers and enlisted members with fewer than six months of service as of August 1, 2014, were excluded from the sample frame because they are hard to contact—they have often moved since their address was pulled for the sample frame—and much of their past year was spent before entering the military. An additional 72 active-component members were excluded because they had not reached the age of 18 by August 1, 2014.

Assuming these 12,541 active-component members experienced any assaults during their short careers in the service, these assaults will have gone uncounted in our study. However, we can estimate bounds for the extent of the resulting undercounting. Because RAND sampled service members more than a month before August 1, 2014, it is reasonable to assume that most who were excluded for having less than six months of service had either completed basic training/recruit training, or were nearing its completion. Therefore, as a lower bound for the proportion of this group exposed to sexual assaults, we used the proportion of active-component members with between six and 12 months of service who indicated they were assaulted during basic or recruit training (0.21 percent). For an upper bound, we took the proportion of those with six to 12 months of service who were assaulted by another member of the military or in a military setting (1.5 percent). These bounds suggest that the exclusion of active-component members with fewer than six months of service or who were not yet 18 resulted in an undercount of between 25 and 190 people who experienced a past-year assault during their time in the military. That is, if we attempted to adjust for this, our estimate of 20,300 active-duty members exposed to past-year sexual assault(s) should be increased by 25 to 190 people.

Exclusion of Members Who Recently Left the Service

Our estimate that 20,300 service members experienced at least one sexual assault in the past year excludes those past-year sexual assaults that occurred against members who left the service before our sample frame was drawn. We do not have statistics on the number of service members who separated from the service sometime during the year prior to the survey, but we can make a rough guess that it is close to the number who separated from the service in 2013, or about 206,000 separations (Office of the Deputy Secretary of Defense [Military Community and Family Policy], 2014). If we

assume that the rate of these losses is consistent over the year, nine of the 12 months of the losses would have been excluded from our sample frame (three months of losses would have been included in our sample frame, because we fielded the survey about three months after the population data was current). We can additionally assume that, among those who separated in the nine months preceding our sample frame, on average they spent 4.5 months, or one-half of the nine-month period, in the military.

With these assumptions, the true prevalence of sexual assaults against service members in the past year, SA_t , could be calculated as

$$SA_t = R_m P + (1/2)(9/12)R_n L = R_m P + (9/24)R_n L$$

where R_m is the rate of past-year sexual assault measured in the sample frame, R_n is the annualized rate of sexual assault among the past-year service members lost to the sample frame, P is the size of the sample frame (1,317,561), and L is the number of losses or separations over the year (206,000, by assumption).

Using this calculation, if those who separated in the past year have no higher or lower monthly risk of having a past-year sexual assault than those who were included in our survey (i.e., $R_m = R_n$), we can estimate that, in addition to the 20,300 service members in the sample frame who experienced past-year sexual assaults, there were 900 more who were sexually assaulted but left the service before they could be represented in the survey. Thus, the true number of service members who experienced an assault in the past year would be closer to 21,200.

Of course, if those who have been sexually assaulted are more likely to separate from the service than those who have not been sexually assaulted, as evidence in Chapter Three suggests, the true number who experienced a sexual assault would be higher yet. For instance, if those who separated in the past year had an annualized sexual assault prevalence comparable to that of women in the military (i.e., $R_m = 4.87\%$), we would generate a corrected estimate for the number of service members who were sexually assaulted of 23,100, which is 14-percent higher than the 20,300 estimate that takes no account of past-year assaults against members that have since left the service.

In sum, of all the sources of possible over- or underestimation we have considered here, the underestimation due to separations in the past year had the largest effect. A more-accurate assessment of the effects of differential loss rates on sexual assault estimates would require research evaluating the past-year sexual assault experiences of members who are separating from the service.

Inclusion or Exclusion of Alcohol Blackouts and Fear Responses That Immobilize

UCMJ crimes require that one of several types of coercion occur that deprive the assaulted person of the opportunity to freely decline the unwanted contact. Most of these coercion methods, such as the use of force, threats, or drugs are reasonably straightforward to assess. As discussed in Volume 1 of this series (Morral, Gore, and Schell, 2014, Appendix A), there are two forms of coercion that require more information to establish clear evidence of a crime than can reasonably be collected from a self-report survey instrument. These concern cases where victims do not indicate their unwillingness because they are frozen with fear, and cases in which the victim is sufficiently intoxicated by alcohol at the time of the assault as to sustain a subsequent alcohol-induced blackout state.¹ For both of these more-complex circumstances, the UCMJ has sections that can be interpreted as defining such events as crimes, but more detail would be needed to verify that the event qualifies as a crime under Articles 80 or 120.

As part of the sexual assault screening questions, all respondents classified as having a past-year sexual assault indicate some form of coercion. Most indicate that one of eight primary forms of coercion occurred. However, those who say none of the primary forms of coercion applied to their unwanted sexual experience are asked whether they could not object to the contact because they were frozen in fear, or whether they could not remember what happened because of an alcoholic blackout.

Few service members were classified as experiencing a past-year sexual assault solely on the basis of either the frozen-in-fear or blackout methods of coercion. Specifically, 0.23 percent (95% CI: 0.08–0.53) of all whom we classified as experiencing a past-year sexual assault fit into this category. As such, if we were to exclude all such service members from our estimates of the prevalence of past-year sexual assaults, that rate would fall from 1.537 percent to 1.533 percent. This would mean that our estimate of 20,300 sexually assaulted service members in the past year was too high by fewer than 50 members.

¹ Alcohol-induced blackouts (i.e., memory loss for events that occurred during intoxication) typically occur after approximately ten alcoholic drinks, and with blood alcohol concentrations of 0.16 percent or greater (Goodwin et al., 1970; White, Simpson, and Best, 1997). In addition to the potential for alcohol-induced blackout, individuals with blood alcohol concentrations at this level experience gross motor impairment, loss of balance, and may require assistance to walk. Vomiting is common, and the gag reflex is impaired, raising the risk for asphyxiation. Judgment, reaction time, vision, and hearing are impaired. Speech is slurred. The individual may be disoriented to time and place.

Inclusion of Nonpenile Oral Penetration in the Penetration Counts

In contrast to prior WGRA surveys, we included the mouth as one of the orifices that, when violated, would be counted as a penetrative assault. We included the mouth because Article 120 of the UCMJ is unambiguous that penetration of the mouth by any object or body part is a “sexual act” subject to Article 120. However, some reviewers have suggested that such offenses may involve unwanted kissing with the tongue, or even just putting a grape in the person’s mouth. Such incidents could clearly qualify as sexual assaults as defined in Article 120, even if in practice they might not be prosecuted as such. Nevertheless, to address concerns that have been raised about our inclusion of penetration of the mouth, we analyze here how this category of offenses influences our estimated rates of sexual assault in the military.

Few service members were categorized as experiencing a past-year sexual assault solely on the basis of a penetration of their mouth by something other than a penis. Across the active component, just 0.02 percent (95% CI: 0.01–0.03) were classified as sexually assaulted on the basis of this question. If all such cases were excluded from our counts, the estimated rate of past-year sexual assault in the military would fall from 1.54 percent to 1.52 percent. This would have the effect of lowering our population estimate of 20,300 service members experiencing sexual assault in the past year by about 250 cases.

Possible Exclusion of Civilian Sexual Assaults Among Reserve Component Members

An unexpected finding reported in Volume 2 of this series was that a high percentage of assaults against reserve-component members involved an offender in the military or the assault took place in a military setting (86 percent). When we considered only “part-time” members of the reserve component (those with administrative records indicating they were not full time and who indicated on the survey that they spent 180 days or less drilling or working for the military), their assaults disproportionately involved military members or settings as well (85 percent).

A reasonable question these findings raise is whether reserve-component members understood that this DoD survey with a title referencing the “military workplace” was asking about all unwanted sexual experiences in the past year, not just those associated in some way with their military duties. If, instead, they underreported their unwanted nonmilitary experiences, but correctly reported their military experiences, this would result in an apparent overrepresentation of assaults that involve military members or settings.

While such a bias is possible, the instrument was written to minimize this misunderstanding. The instructions to the sexual assault module state, “please include

experiences *no matter who did it to you or where it happened*. It could be done to you by a male or female, service member or civilian, someone you knew or a stranger” [emphasis in original]. In addition, each behaviorally specific screening question was worded to be broadly inclusive without any reference to military context, e.g., “Since 8/1/2013, did you have any unwanted experiences in which someone put his penis into your anus or mouth?” Thus, while it is possible that the entire study underestimates the rate at which service members are sexually assaulted by civilians, it would require that the respondents ignore the instructions and add restrictions to the questions that were not present in the text.

There are other reasons to doubt that reserve-component members were systematically underreporting nonmilitary sexual assaults, at least to an extent that would produce the pattern of results we observed. Such a bias would have to be very large to explain the high rate of military sexual assaults among reservists. If, for instance, the true proportion of sexual assaults against part-time reserve-component members involving military personnel or settings was 11 percent (the average portion of the year this group indicated they spent in compensated military duties) rather than the 85 percent that we observe, it would require that 98 percent of reserve-component respondents who experienced a nonmilitary sexual assault failed to indicate that on the survey.

Of course, the proportion of time that reserve-component members spend with other members or in military settings may be substantially greater than the time they spend in compensated duties or drilling. However, even if we assumed the true proportion was twice the proportion of compensated time (22 percent rather than 11 percent), that would still imply that 95 percent of all nonmilitary sexual assaults went unreported. If we quadruple the estimate of the amount of time these part-time reservists spent with other members of the military or in military settings (to 44 percent), this would still imply that 86 percent of all nonmilitary sexual assaults went unreported. Such high rates of underreporting would imply that virtually all reserve respondents misunderstood the instructions and questions.

An undercount of non-military sexual assaults by such a large fraction would imply that the true risk of past-year sexual assault among reservists is much higher than estimated. For example, a bias this large would imply that 7.4 percent experienced a sexual assault in the past year, rather than the currently estimated value of 0.9 percent (0.38 percent for men, 3.13 percent for women). For reference, averaged over the decade 2003–2013, the annual rate of sexual assault in the total U.S. population was estimated to be 0.25 percent for men and 2.03 percent for women by the National Crime Victimization Survey (Bureau of Justice Statistics, undated).

In addition, the pattern of results we observe using the RAND survey is generally similar to results from previous WGR and WGRA studies. Our estimates of sexual assault rates in the reserve component (0.38 percent for men, 3.13 percent for women) are not statistically significantly different than the 2012 WGR estimates of unwanted

sexual contact (0.5 percent for men, 2.8 percent for women). We know from our comparison of the WGRA and RMWS questions that unwanted sexual contact and sexual assault rates from the two methods arrive at similar estimates. If our RMWS estimates of sexual assault were substantially underreported due to confusion over question wording or scope, it suggests that either (a) the 2012 WGRR also suffered from the same systematic response bias, despite the fact that it used substantially different question wording and instructions, or (b) the true rate of sexual assault in the reserve component jumped markedly since 2012, but this jump was obscured by the offsetting response bias created by confusion over the RAND questions. Finally, our finding that reserve-component men and women experienced lower rates of past-year sexual assaults than active-component members agrees with the parallel finding for unwanted sexual contact that has been found in all previous WGRR surveys. Indeed, the difference in risk between the active and reserve components observed in the 2012 WGRA and WGRR were very similar to the differences observed in the RMWS.

For these reasons, we doubt that our findings of elevated risk of sexual assault among active-component members compared to reserve, and our finding that sexual assaults against reserve-component members disproportionately involve military personnel or settings, can be explained entirely by a systematic response bias on the part of reserve-component members. Nevertheless, we note that in 2015 DMDC conducted a new WGRR survey, and could use this new survey to further examine whether reserve-component members understand the scope of the unwanted sexual contact and sexual assault questions.

Conclusions

This chapter considers a range of possible sources of bias on our survey estimates, such as exclusions from the sample frame (e.g., when service members exposed to sexual assault risk were nevertheless excluded from the sample frame) and specification errors (when people are incorrectly categorized as experiencing a sexual assault). Table 6.2 summarizes the estimates presented in this chapter for the likely direction and approximate magnitude of these possible sources of bias on our estimate of the number of service members who experienced a sexual assault in the past year.

By far the largest source of potential bias is the exclusion from the sample frame of members who served in the military during the past year, but separated before the sample frame was drawn. Using a range of estimates for their sexual assault risk, from rates that are almost certainly too low to almost certainly too high, this source of error contributes to an underestimate of 4 percent to 14 percent of the true number.

This finding, in conjunction with the effect of excluding members in their first six months of service, suggests that our survey estimates are almost certainly slightly biased in the direction of providing underestimates of sexual assault prevalence. That

Table 6.2
Summary of Possible Biases in the Estimated Number of Active-Component Members Who Experienced a Sexual Assault Due to Sample Frame and Specification Errors

Rank	Source of Bias	Possible Size of Overestimate	Possible Size of Underestimate
1	Exclusion of recent separations		900–2,800
2	Inclusion of nonpenile oral penetration	250	
3	Exclusion of assaults in first six months of service		25–190
4	Inclusion of frozen in fear and blackouts	50	
5	Inclusion of preservice assaults	<10	

NOTE: The overall RMWS estimate of the number of active-component members who experienced a sexual assault in the past year was 20,300 individuals. Thus, an underestimate of 203 would correspond to the RMWS estimate omitting 1 percent of all true cases.

is, the sources of bias that plausibly lead to underestimates are considerably larger than the countervailing sources of bias that may lead to overestimates. Moreover, the two largest potential sources of overestimation (our inclusion of blackouts, frozen in fear, and oral penetration) are not actually errors in our opinion. The decision to include these members in our population estimate was consistent with the definitions of these crimes within the UCMJ. We list them in this chapter only for the benefit of those who question how large an effect these choices had on our overall estimates. Finally, we also conclude that, although some members of the reserve component may have misconstrued the survey instructions, and therefore reported only those sexual assaults they experienced that were linked in some way to the military, such errors are unlikely to be large enough to explain the high proportion of sexual assaults against reservists that are linked to their military service.

Performance of the Sexual Harassment and Gender Discrimination Module

Coreen Farris, Lisa H. Jaycox, and Terry L. Schell

The survey measures of sexual harassment and gender discrimination in the military were designed to assess the extent to which negative workplace experiences rose to the level of a DoD-defined MEO violation. As such, the measures differ from other measures in the literature, which do not require respondents to meet such a high threshold for inclusion in the group of individuals who have experienced sexual harassment or gender discrimination. This chapter provides a thorough description of the survey construction and derived variable definitions. By comparing the percentage of service members who endorsed a screening item (e.g., repeated sexual “jokes”) with those who also met the follow-up criteria that the behavior was persistent or met a “reasonable person” standard, the reader can examine how these additional definitional requirements affected the final estimates of sexual harassment. In addition, this chapter describes a programming error that was discovered by DMDC in the definition of *sexually hostile workplace harassment* and quantifies the very small effect it had on the previously reported estimates of sexual harassment.

Sexual Harassment and Gender Discrimination Screening Items

The section of the survey that assessed respondents’ experiences with sexual harassment or gender discrimination began with a brief set of instructions that informed them that the next set of questions would query “several things that someone from work might have done to you that were upsetting or offensive, and that happened AFTER [date exactly one year prior to survey completion date].” The instructions also provided a detailed definition of “someone from work”:

[A]ny person you have contact with as part of your military duties. “Someone from work” could be a supervisor, someone above or below you in rank, or a civilian employee/contractor. They could be in your unit or in other units.

The instructions also specified that incidents could have occurred on-duty or off-duty, on-base or off-base, and should be included provided the person who did them was someone from work.

The module began with 15 screening questions that assessed inappropriate workplace behaviors (see Table 7.1, items SH1–SH15). Respondents who denied having experienced any of the 15 inappropriate workplace behaviors in the past year were categorized as service members who were not sexually harassed or discriminated against; these respondents received no further questions in this module.

Table 7.1
Fifteen Inappropriate Workplace Behaviors and the Percentage of Men and Women Who Indicated They Experienced Each Behavior in the Past Year

Inappropriate Workplace Behavior	Men (95% CI)	Women (95% CI)
Any Sexually Hostile Workplace Behaviors (SH1–SH11)	11.86 (11.20–12.54)	25.76 (25.13–26.41)
SH1: Since [X Date], did someone from work repeatedly tell sexual “jokes” that made you uncomfortable, angry, or upset?	5.17 (4.69–5.68)	13.08 (12.57–13.61)
SH2: Since [X Date], did someone from work embarrass, anger, or upset you by repeatedly suggesting that you do not act like a [man/ woman] is supposed to?	6.28 (5.75–6.84)	7.65 (7.24–8.08)
SH3: Since [X Date], did someone from work repeatedly make sexual gestures or sexual body movements (for example, thrusting their pelvis or grabbing their crotch) that made you uncomfortable, angry, or upset?	2.65 (2.28–3.07)	5.13 (4.77–5.51)
SH4: Since [X Date], did someone from work display, show, or send sexually explicit materials like pictures or videos that made you uncomfortable, angry, or upset?	1.58 (1.34–1.85)	3.59 (3.31–3.90)
SH5: Since [X Date], did someone from work repeatedly tell you about their sexual activities in a way that made you uncomfortable, angry, or upset?	3.53 (3.15–3.95)	7.55 (7.14–7.97)
SH6: Since [X Date], did someone from work repeatedly ask you questions about your sex life or sexual interests that made you uncomfortable, angry, or upset?	2.86 (2.49–3.27)	8.22 (7.79–8.68)
SH7: Since [X Date], did someone from work make repeated sexual comments about your appearance or body that made you uncomfortable, angry, or upset?	2.01 (1.70–2.35)	8.70 (8.26–9.15)
SH8 and SH8a: Since [X Date], did someone from work either <u>take</u> or <u>share</u> sexually suggestive pictures or videos of you when you did not want them to? AND Did this make you uncomfortable, angry, or upset?	0.45 (0.31–0.62)	1.03 (0.88–1.19)
SH9 and SH9a: Since [X Date], did someone from work make <u>repeated</u> attempts to establish an <u>unwanted</u> romantic or sexual relationship with you? These could range from repeatedly asking you out for coffee to asking you for sex or a “hook-up.” AND Did these attempts make you uncomfortable, angry, or upset?	0.61 (0.44–0.83)	9.02 (8.58–9.48)

Table 7.1—Continued

Inappropriate Workplace Behavior	Men (95% CI)	Women (95% CI)
SH10: Since [X Date], did someone from work intentionally touch you in a sexual way when you did not want them to? This could include touching your genitals, breasts, buttocks, or touching you with their genitals anywhere on your body.	1.18 (0.95–1.45)	3.06 (2.77–3.36)
SH11: Since [X Date], did someone from work repeatedly touch you in any other way that made you uncomfortable, angry, or upset? This could include almost any <u>unnecessary</u> physical contact including hugs, shoulder rubs, or touching your hair, but would not usually include handshakes or routine uniform adjustments.	1.38 (1.16–1.64)	5.31 (4.97–5.68)
Any <i>Quid Pro Quo</i> Behaviors (SH12–SH13)	0.55 (0.39–0.75)	2.35 (2.10–2.61)
SH12: Since [X Date], has someone from work made you feel as if you would get some [If reserve, insert “military”] workplace benefit in exchange for doing something sexual? For example, they might hint that they would give you a good evaluation/fitness report, a better assignment, or better treatment at work in exchange for doing something sexual. Something sexual could include talking about sex, undressing, sharing sexual pictures, or having some type of sexual contact.	0.41 (0.27–0.61)	1.81 (1.60–2.05)
SH13: Since [X Date], has someone from work made you feel like you would get <u>punished or treated unfairly in the</u> [If reserve, insert “military”] workplace if you did not do something sexual? For example, they hinted that they would give you a bad evaluation/fitness report, a bad assignment, or bad treatment at work if you were not willing to do something sexual. This could include being unwilling to talk about sex, undress, share sexual pictures, or have some type of sexual contact.	0.33 (0.22–0.47)	1.38 (1.20–1.57)
Any Gender Discrimination Behaviors (SH14–SH15)	4.11 (3.77–4.46)	29.73 (29.09–30.38)
SH14: Since [X Date], did you hear someone from work say that [men/women] are <u>not</u> as good as [women/men] at your particular [If reserve, insert “military”] job, or that [men/women] should be prevented from having your job?	1.84 (1.62–2.08)	19.56 (18.96–20.16)
SH15: Since [X Date], do you think someone from work mistreated, ignored, excluded, or insulted you because you are a [man/woman]?	3.10 (2.82–3.41)	24.36 (23.76–24.97)

Classification of Sexual Harassment of the Sexually Hostile Work Environment Type

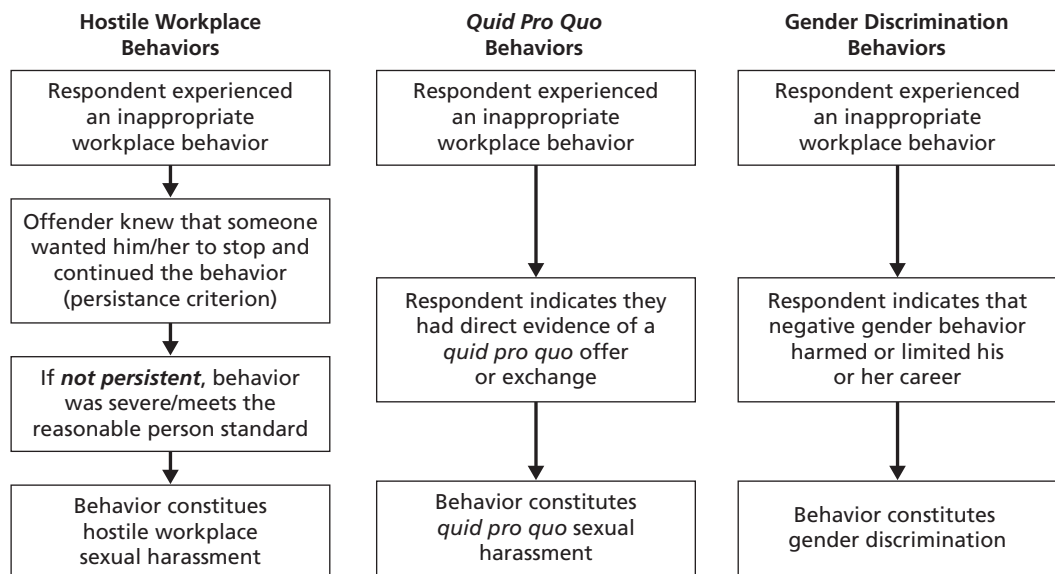
Respondents who indicated that they experienced an inappropriate workplace behavior received follow-up questions specific to the item(s) that they endorsed. The first 11 items (SH1–SH11) correspond to workplace behaviors that could be categorized as the *sexually hostile workplace* form of sexual harassment if the offensive behavior was either (1) persistent (i.e., the respondent indicated the behavior continued even after the coworker knew that it was upsetting to others) or was described by the respondent as

(2) severe (i.e., the behavior was so severe that most service members of the respondent’s gender would find it offensive). A flow chart describing the flow of follow-up questions and the logic for final categorization as an MEO violation is illustrated in Figure 7.1.

For nine of the 11 items that assessed behaviors that might meet the criteria for sexually hostile work environment harassment (SH1–SH7, SH9, SH11), respondents received up to three follow-up questions to assess whether the behavior was persistent or severe. For ease of responding, persistence was measured with a series of two questions. First, we assessed whether the offender was aware that his or her behavior was offensive. The item text was: “Do you think they knew that you or someone else wanted them to stop? If it happened more than once or by more than one person, do you think any of them ever knew?” For those who believed the offender was aware that others were offended by his or her behavior, the second question asked: “Did they continue this unwanted behavior even after they knew that you or someone else wanted them to stop?” If the respondent indicated that the behavior persisted even after the offender was aware that someone wanted them to stop, their experience met the persistence criterion, and the respondent was classified as having experienced sexual harassment of the sexually hostile work environment type.

For respondents who had experienced an inappropriate workplace behavior that was not persistent, their experience might still constitute sexual harassment of the sexually hostile work environment type if the behavior they experienced was suffi-

Figure 7.1
Flowchart of the Assessment of Sexually Hostile Workplace Harassment, *Quid Pro Quo* Sexual Harassment, and Gender Discrimination



ciently severe as to meet the reasonable person standard. Consistent with DoD policy, severity was assessed via the item “Do you think that this was ever severe enough that [most men/women] in the military would have been offended by [these jokes] if they had heard them?” The phrase in braces was specific to the screening question assessed. Service members who responded “yes” to this follow-up item were classified as having experienced sexual harassment of the sexually hostile work environment type.

For one of the 11 items corresponding to sexually hostile work environment harassment (SH10), no follow-up questions to assess persistence or severity were required (or administered). This item assessed whether “someone from work intentionally touched you in a sexual way when you did not want them to” which could include touching “genitals, breasts, buttocks, or touching you with their genitals anywhere on your body.” If this behavior occurred in a work environment (as assessed), it is automatically categorized as an incident of sexual harassment of the sexually hostile workplace environment type. According to case law and DoD directives, unwanted sexual contact need not be persistent (i.e., a single occurrence is sufficient to establish an MEO violation) and is, by definition, severe, as it is also classified as a criminal offense under UCMJ Article 120.

Finally, for one of the 11 items corresponding to sexually hostile work environment harassment (SH8), the follow-up question assessing severity was administered, but questions assessing persistence were not. This screening question assessed whether someone from work took or shared “sexually suggestive pictures or videos of you when you did not want them to.” If this behavior occurs in a work setting, it need not be persistent to rise to the level of an MEO violation—a single occurrence is adequate. However, it does have to be sufficiently severe that a reasonable person would be offended by the incident. For example, while some service members might be personally offended by an image of themselves in a form-fitting shirt, circulating the photo would not be considered sexual harassment if most service members would not be offended by a similar photograph of themselves. Respondents who endorsed the screening item, and verified that they believed that most service members of the same gender would be offended if it had happened to them, were categorized as having experienced sexual harassment of the sexually hostile work environment type.

Results

As seen in Table 7.1, 25.8 percent of service women and 11.9 percent of service men indicated that they had experienced at least one of the 11 inappropriate workplace behaviors that could indicate a sexually hostile work environment in the past year. Some of the 11 behaviors, such as a coworker taking or sharing sexually suggestive pictures (SH8 and SH8a), were relatively rare (1.0 percent of women and 0.45 percent of men endorsed the item). Whereas other hostile workplace behaviors, such as coworkers repeatedly telling jokes that the service person found offensive (SH1), were experienced by many women (13.1 percent) and men (5.2 percent). Across the 11 behaviors that

could indicate a sexually hostile workplace, women were more likely than men to have experienced each (Table 7.1). In the most extreme differentiation between the genders, women (9.02 percent) were nearly 15 times more likely than men (0.61 percent) to indicate that someone from work had made repeated attempts to establish an unwanted romantic or sexual relationship that the respondent found offensive (SH9 and SH9a).

Tables 7.2 and 7.3 show the flow of service women and men (respectively) through the categorization process by which inappropriate workplace behaviors were categorized as sexual harassment of the sexually hostile work environment type. Considering first the persistence criterion for establishing that a sexually hostile work environment is present, there was variation between men and women. For service women who endorsed inappropriate hostile workplace behavior, 39 percent to 59 percent indicated that the person knew that someone was offended and continued the behavior anyway (Table 7.2). In general, fewer men indicated that the inappropriate behavior that they experienced was known to be offensive and persisted (32 percent to 47 percent across behaviors; Table 7.3).

Even when an inappropriate hostile workplace behavior is not persistent, DoD directives categorize events that were so severe that a reasonable person would be offended as MEO violations. Among women endorsing a hostile workplace behavior and who denied that it was persistent, 59 percent to 85 percent indicated that it was severe enough that most women in the military would be offended (Table 7.2). For men, the proportion who believed the nonpersistent, inappropriate workplace behavior was severe enough to meet a reasonable-person standard varied from 20 percent to 54 percent across behaviors (Table 7.3).

Combined across the two criteria used to categorize inappropriate workplace behaviors as sexual harassment of the sexually hostile work environment type, at least three-quarters of women who experienced the inappropriate behaviors were ultimately categorized as experiencing an MEO violation (see column 3 in Table 7.2). At the lower end, 74 percent of women who indicated that someone at work had touched them in a way that made them uncomfortable or upset (SH11) were categorized as experiencing an MEO violation (i.e., the behavior was persistent or severe). At the upper end, 88 percent of women who indicated that someone at work had repeatedly made sexual gestures or sexual body movements (SH3) were categorized as experiencing an MEO violation. This range excludes an item assessing unwanted sexual touching (SH10), which is automatically categorized as an MEO violation.

The percentages of men who experienced inappropriate workplace behaviors that were ultimately categorized as having experienced an MEO violation tended to be lower than for women (49–66 percent). For men, the behavior least likely to meet one of the criteria for classification as sexual harassment of the sexually hostile work environment type was screening item SH5 (someone from work repeatedly discussing their sexual experiences in a way that offended the respondent). Of the men who indicated that they had this experience, 49 percent indicated that it was either persistent or met

Table 7.2

For Women, Questionnaire Flow from Experiencing an Inappropriate Workplace Behavior to Being Categorized as Having Experienced Sexual Harassment of the Sexually Hostile Work Environment Type

Hostile Workplace Inappropriate Workplace Behaviors	Percentage of female respondents who experienced the inappropriate workplace behavior	Of those, percentage who indicated the behavior was persistent	If not persistent, percentage who indicated the behavior was severe/meets reasonable person standard	Of those who experienced the inappropriate workplace behavior, percentage categorized as experiencing a sexually hostile work environment	Percentage of all female respondents categorized as experiencing a sexually hostile work environment
SH1: Repeatedly tell sexual “jokes” that made you uncomfortable, angry, or upset?	13.1 (12.57–13.61)	49%	70%	84%	11.0 (10.52–11.49)
SH2: Embarrass, anger, or upset you by repeatedly suggesting that you do not act like a [man/woman] is supposed to?	7.7 (7.24–8.08)	48%	69%	83%	6.3 (5.96–6.75)
SH3: Repeatedly make sexual gestures or sexual body movements that made you uncomfortable, angry, or upset?	5.1 (4.77–5.51)	59%	72%	88%	4.5 (4.16–4.85)
SH4: Display, show, or send sexually explicit materials like pictures or videos that made you uncomfortable, angry, or upset?	3.6 (3.31–3.90)	51%	67%	82%	3.0 (2.71–3.26)
SH5: Repeatedly tell you about their sexual activities in a way that made you uncomfortable, angry, or upset?	7.6 (7.14–7.97)	50%	71%	85%	6.4 (6.04–6.81)
SH6: Repeatedly ask you questions about your sex life or sexual interests that made you uncomfortable, angry, or upset?	8.2 (7.79–8.68)	52%	66%	83%	6.8 (6.44–7.26)
SH7: Make repeated sexual comments about your appearance or body that made you uncomfortable, angry, or upset?	8.7 (8.26–9.15)	50%	71%	84%	7.3 (6.94–7.77)

Table 7.2—Continued

Hostile Workplace Inappropriate Workplace Behaviors	Percentage of female respondents who experienced the inappropriate workplace behavior	Of those, percentage who indicated the behavior was persistent	If not persistent, percentage who indicated the behavior was severe/meets reasonable person standard	Of those who experienced the inappropriate workplace behavior, percentage categorized as experiencing a sexually hostile work environment	Percentage of all female respondents categorized as experiencing a sexually hostile work environment
SH8 and SH8a: Either <u>take</u> or <u>share</u> sexually suggestive pictures or videos of you when you did not want them to? AND Did this make you uncomfortable, angry, or upset	1.0 (0.88–1.19)	NA ^a	86%	86%	0.9 (0.74–1.04)
SH9 and SH9a: Make <u>repeated</u> attempts to establish an <u>unwanted</u> romantic or sexual relationship with you? AND Did these attempts make you uncomfortable, angry, or upset?	9.0 (8.58–9.48)	53%	67%	83%	7.6 (7.14–7.98)
SH10: Intentionally touch you in a sexual way when you did not want them to?	3.1 (2.77–3.36)	NA ^a	NA ^b	100%	3.1 (2.77–3.36)
SH11: Repeatedly touch you in any other way that made you uncomfortable, angry, or upset? ^c	5.3 (4.97–5.68)	39%	59%	74%	3.9 ^d (3.64–4.24)

NOTE: 95-percent confidence intervals for each population estimate are indicated in parentheses. Confidence intervals are not provided for non-population estimates.

^a Criterion was not assessed, because the behavior need not be persistent to rise to the level of an MEO violation.

^b Criterion was not assessed, because unwanted sexual touching in a workplace—a criminal behavior—is considered severe without requiring respondent verification.

^c Item SH11 was asked only of respondents who answered “no” to SH10. Respondents who answered “yes” to SH10 were automatically coded (for final estimates) as having experienced the broader category represented in SH11.

^d This value (3.9 percent) represents the proportion of respondents who were presented with SH11 who were ultimately categorized as having experienced a sexually hostile workplace environment. When respondents who experienced unwanted sexual touching (SH10) are included in this category, the percentage is 7.0.

Table 7.3

For Men, Questionnaire Flow from Experiencing an Inappropriate Workplace Behavior to Being Categorized as Having Experienced Sexual Harassment of the Sexually Hostile Work Environment Type

Hostile Workplace Inappropriate Workplace Behaviors	Percentage of male respondents who experienced the inappropriate workplace behavior	Of those, percentage who indicated the behavior was persistent	If not persistent, percentage who indicated the behavior was severe/meets reasonable person standard	Of those who experienced the inappropriate workplace behavior, percentage categorized as experiencing a sexually hostile work environment	Percentage of all male respondents categorized as experiencing a sexually hostile work environment
SH1: Repeatedly tell sexual “jokes” that made you uncomfortable, angry, or upset?	5.2 (4.69–5.68)	33%	24%	49%	2.5 (2.19–2.89)
SH2: Embarrass, anger, or upset you by repeatedly suggesting that you do not act like a [man/woman] is supposed to?	6.3 (5.75–6.84)	35%	34%	57%	3.6 (3.14–4.02)
SH3: Repeatedly make sexual gestures or sexual body movements that made you uncomfortable, angry, or upset?	2.7 (2.28–3.07)	40%	30%	58%	1.5 (1.25–1.85)
SH4: Display, show, or send sexually explicit materials like pictures or videos that made you uncomfortable, angry, or upset?	1.6 (1.34–1.85)	32%	28%	51%	0.8 (0.63–1.02)
SH5: Repeatedly tell you about their sexual activities in a way that made you uncomfortable, angry, or upset?	3.5 (3.15–3.95)	36%	20%	49%	1.7 (1.48–1.99)
SH6: Repeatedly ask you questions about your sex life or sexual interests that made you uncomfortable, angry, or upset?	2.9(2.49–3.27)	35%	26%	52%	1.5 (1.23–1.79)
SH7: Make repeated sexual comments about your appearance or body that made you uncomfortable, angry, or upset?	2.0 (1.70–2.35)	47%	38%	66%	1.3 (1.07–1.65)

Table 7.2—Continued

Hostile Workplace Inappropriate Workplace Behaviors	Percentage of male respondents who experienced the inappropriate workplace behavior	Of those, percentage who indicated the behavior was persistent	If not persistent, percentage who indicated the behavior was severe/meets reasonable person standard	Of those who experienced the inappropriate workplace behavior, percentage categorized as experiencing a sexually hostile work environment	Percentage of all male respondents categorized as experiencing a sexually hostile work environment
SH8 and SH8a: Either <u>take</u> or <u>share</u> sexually suggestive pictures or videos of you when you did not want them to? AND Did this make you uncomfortable, angry, or upset	0.4 (0.31–0.62)	NA ^a	54%	54%	0.2 (0.14–0.38)
SH9 and SH9a: Make <u>repeated</u> attempts to establish an <u>unwanted</u> romantic or sexual relationship with you? AND Did these attempts make you uncomfortable, angry, or upset?	0.6 (0.44–0.83)	46%	38%	66%	0.4 (0.26–0.59)
SH10: Intentionally touch you in a sexual way when you did not want them to?	1.2 (0.95–1.45)	NA ^a	NA ^b	100%	1.2 (0.95–1.45)
SH11: Repeatedly touch you in any other way that made you uncomfortable, angry, or upset? ^c	1.4 (1.16–1.64)	35%	32%	55%	0.8 ^d (0.58–0.99)

NOTE: 95-percent confidence intervals for each population estimate are indicated in parentheses. Confidence intervals are not provided for non-population estimates.

^a Criterion was not assessed, because the behavior need not be persistent to rise to the level of an MEO violation.

^b Criterion was not assessed, because unwanted sexual touching in a workplace—a criminal behavior—is considered severe without requiring respondent verification.

^c Item SH11 was asked only of respondents who answered “no” to SH10. Respondents who answered “yes” to SH10 were automatically coded (for final estimates) as having experienced the broader category represented in SH11.

^d This value (0.8 percent) represents the proportion of respondents who were presented with SH11 who were ultimately categorized as having experienced a sexually hostile workplace environment. When respondents who experienced unwanted sexual touching (SH10) are included in this category, the percentage is 1.9.

the reasonable person standard for severity. The behaviors most likely to meet classification criteria among men were SH7 and SH9; 66 percent of men who said that someone from work made repeated sexual comments about their appearance and 66 percent who had repeated and unwanted attempts to establish a romantic relationship were categorized as experiencing a MEO violation. Again, this range excludes unwanted sexual touching, which is automatically categorized as an MEO violation.

Classification of Sexual Harassment of the *Quid Pro Quo* Type

Two screening items assessed inappropriate workplace behaviors that could indicate that a *quid pro quo* violation had occurred, provided the level of evidence was sufficient to meet DoD criteria for an MEO violation (SH12 and SH13). As shown in Table 7.1, 2 percent of women and less than 1 percent of men endorsed one of the two screening items indicating that someone from work had either offered a workplace benefit (SH12) or threatened a workplace punishment (SH13) in exchange for sexual behavior.

For those respondents who indicated an inappropriate workplace experience that could constitute sexual harassment of the *quid pro quo* type, we assessed the level of evidence they had for such an offer with five follow-up questions (see Table 7.4 for exact wording). The first three follow-up items are defined in DoD directives as sufficient evidence to indicate that a *quid pro quo* violation occurred; thus, respondents who endorsed one of the screening items (SH12 or SH13) and who indicated that they had adequate evidence for an exchange (e.g., SH12a, SH12b, or SH12c) were classified as having experienced sexual harassment of the *quid pro quo* type. Individuals who did not endorse any of the first three follow-up items (e.g., SH12a, SH12b, or SH12c), and instead indicated that they believed a *quid pro quo* exchange was offered based on rumor, hearsay, or inference based on the person's personality (e.g., SH12d or SH12e) were not classified as having experienced an MEO violation.

As shown in Table 7.5, among women who indicated an inappropriate workplace behavior suggestive of a *quid pro quo* exchange, 74.3 percent of the women who were offered a workplace benefit and 67.2 percent of women who were threatened with a workplace punishment had adequate evidence of an exchange as to suggest that an MEO violation had occurred and were subsequently classified as having experienced sexual harassment of the *quid pro quo* type. The percentages among men were 77.2 percent and 56.9 percent, respectively (Table 7.6).

Classification of Gender Discrimination

Two screening items assessed inappropriate workplace behaviors that could indicate that gender discrimination may have occurred (provided subsequent questions

Table 7.4
Follow-Up Items Assessing the Level of Evidence for a Possible *Quid Pro Quo* Offer

You indicated that, after [date exactly one year prior], someone from work made you feel as if you would get some workplace benefit in exchange for doing something sexual. What led you to believe that you would get a workplace benefit if you agreed to do something sexual? Select “Yes” or “No” for each item.

SH12a. They told you that they would give you a reward or benefit for doing something sexual.	Yes	No
SH12b. They hinted that you would get a reward or benefit for doing something sexual. For example they reminded you about your evaluation/fitness report about the same time that they expressed sexual interest.	Yes	No
SH12c. Someone else told you they got benefits from this person by doing sexual things.	Yes	No
SH12d. You heard rumors from other people that this person treated others better in exchange for doing sexual things.	Yes	No
SH12e. Based on what you knew about their personality, you thought you could get a benefit.	Yes	No

verified harm to career, as is necessary to meet DoD criteria for an MEO violation; SH14 and SH15). As shown in Table 7.1, 30 percent of women and 4 percent of men endorsed one of the two screening items indicating that someone from work had either indicated that someone of their gender should not be in their work position (SH14) or believed that someone from work had mistreated them due to their gender (SH15). These experiences were more common among women than men (Table 7.1). One out of every five female service members indicated that someone from work had said that women were not as good as men at their military occupation or that women should be prevented from having the respondent’s job. One out of every four women said that someone from work had mistreated, ignored, excluded, or insulted them because of gender.

For those respondents who indicated an inappropriate workplace experience that could constitute gender discrimination, we assessed whether the behavior “ever harmed or limited [the respondent’s] career.” To clarify the meaning of harm to career, we provided examples including harm to an evaluation or fitness report and impact on promotion or the respondent’s next assignment. Service members who responded “yes” to the follow-up question were categorized as having experienced a gender discrimination MEO violation.

Among women who had experienced an inappropriate workplace behavior suggestive of gender discrimination, 41–43 percent indicated that the behaviors had risen to a level that harmed or limited the respondent’s career (an MEO violation; see Table 7.7). Among men who had these inappropriate workplace experiences, 35–50 percent were categorized as experiencing an MEO violation (Table 7.8).

Table 7.5

For Women, Questionnaire Flow from Experiencing an Inappropriate Workplace Behavior to Being Categorized as Having Experienced Sexual Harassment of the *Quid Pro Quo* Type

<i>Quid Pro Quo</i> Inappropriate Workplace Behaviors	Percentage of female respondents who experienced the inappropriate workplace behavior	Of those who experienced the inappropriate workplace behavior, percentage who indicated they had <u>direct evidence</u> of an offer or exchange	Of those who experienced inappropriate workplace behavior, percentage categorized as experiencing a <i>quid pro quo</i> violation	Percentage of female respondents categorized as experiencing a <i>quid pro quo</i> violation
Since [X Date], has someone from work made you feel as if you would get . . .				
SH12: . . . some workplace benefit in exchange for doing something sexual?	1.8 (1.60–2.05)	74%	74%	1.3 (1.16–1.56)
SH13: . . . <u>punished or treated unfairly</u> in the workplace if you did not do something sexual?	1.4 (1.20–1.57)	67%	67%	0.9 (0.77–1.10)

Table 7.6

For Men, Questionnaire Flow from Experiencing an Inappropriate Workplace Behavior to Being Categorized as Having Experienced Sexual Harassment of the *Quid Pro Quo* Type

<i>Quid Pro Quo</i> Inappropriate Workplace Behaviors	Percentage of male respondents who experienced the inappropriate workplace behavior	Of those who experienced the inappropriate workplace behavior, percentage who indicated they had <u>direct evidence</u> of an offer or exchange	Of those who experienced inappropriate workplace behavior, percentage categorized as experiencing a <i>quid pro quo</i> violation	Percentage of male respondents categorized as experiencing a <i>quid pro quo</i> violation
Since [X Date], has someone from work made you feel as if you would get . . .				
SH12: . . . some workplace benefit in exchange for doing something sexual?	0.4 (0.27–0.61)	77%	77%	0.3 (0.18–0.52)
SH13: . . . <u>punished or treated unfairly</u> in the workplace if you did not do something sexual?	0.3 (0.22–0.47)	57%	57%	0.2 (0.10–0.31)

Table 7.7

For Women, Questionnaire Flow from Experiencing an Inappropriate Workplace Behavior to Being Categorized as Having Experienced Gender Discrimination

Gender Discrimination Inappropriate Workplace Behaviors	Percentage of female respondents who experienced the inappropriate workplace behavior	Of those who experienced the inappropriate workplace behavior, percentage who indicate that it harmed their career	Of those who experienced inappropriate workplace behavior, percentage categorized as experiencing a probable MEO violation	Percentage of female respondents categorized as experiencing an MEO violation
Since [X Date], . . .				
SH14: . . . did you hear someone from work say that [men/women] are <u>not</u> as good as [women/men] at your particular job, or that [men/women] should be prevented from having your job?	19.6 (18.98–20.16)	41%	41%	8.1 (7.69–8.51)
SH15: . . . do you think someone from work mistreated, ignored, excluded, or insulted you because you are a [man/woman]?	24.4 (23.76–24.97)	43%	43%	10.6 (10.15–11.03)

Table 7.8

For Men, Questionnaire Flow from Experiencing an Inappropriate Workplace Behavior to Being Categorized as Having Experienced Gender Discrimination

Gender Discrimination Inappropriate Workplace Behaviors	Percentage of male respondents who experienced the inappropriate workplace behavior	Of those who experienced the inappropriate workplace behavior, percentage who indicate that it harmed their career	Of those who experienced inappropriate workplace behavior, percentage categorized as experiencing a probable MEO violation	Percentage of male respondents categorized as experiencing an MEO violation
Since [X Date], . . .				
SH14: . . . did you hear someone from work say that [men/women] are <u>not</u> as good as [women/men] at your particular job, or that [men/women] should be prevented from having your job?	1.8 (1.62–2.08)	35%	35%	0.6 (0.52–0.77)
SH15: . . . do you think someone from work mistreated, ignored, excluded, or insulted you because you are a [man/woman]?	3.1 (2.82–3.41)	50%	50%	1.6 (1.35–1.78)

Error in Categorizing Hostile Workplace Experiences

A programming error caused a small number of respondents to be mischaracterized as missing on key sexual harassment variables, when, in fact, they had answered in a way that met criteria for having experienced an MEO violation. This occurred within the series of questions that assessed hostile work environment harassment. The coding error affected the categorization of respondents who indicated that the inappropriate behavior occurred, indicated that the offender(s) knew that someone wanted them to stop, skipped the question assessing whether the offender(s) continued after they knew, and, finally, indicated that the behavior was serious enough that most service members would have been offended. This series of answers is adequate to indicate that an MEO violation occurred, but respondents with this pattern of responses were incorrectly categorized as missing too many responses to characterize their experiences.

To investigate the possible effect of the error on the previously reported estimates, we calculated the number of respondents who were affected and how correcting the error would change the percentage estimates for MEO violations. Table 7.9 shows the number of respondents in each question series who met criteria for having experienced the type of sexual harassment assessed, but who were misclassified as missing. For example, in the second row, showing MEO violations attributable to being told one is not acting according to one's gender role, four women were incorrectly categorized as missing when they should have been categorized as experiencing hostile workplace sexual harassment. These respondents went on to answer the remaining questions that assessed other types of hostile workplace behaviors. Two of the four women described other types of hostile workplace violations, such that they were ultimately correctly categorized as belonging in the group of respondents who experienced a sexually hostile workplace environment despite the misclassification on one set of questions. Finally, the remaining two women were categorized as having experienced another type of MEO violation, and, thus, were ultimately correctly categorized as having experienced an MEO violation.

The last column of Table 7.9 provides the number of respondents who were not counted as having experienced an MEO violation. This error occurred for, at most, five women, and sometimes no one, for each type of hostile work environment harassment. Across all types, a total of eight women and one man were not ultimately categorized as experiencing hostile workplace harassment when they should have been, and seven women were not ultimately characterized as experiencing an MEO violation when they should have been. The total number of people is lower than the sum across rows, because in some cases the same person was mischaracterized in more than one row.

Finally, we calculated the change in the top-line estimates of sexually hostile work environment, sexual harassment, and any MEO violation that occurred as a result of the coding error. At most, the estimates increased only by three one-hundredths of a percentage and, in some cases, not at all (see Table 7.10). These discrepancies are too

Table 7.9
Number of Active-Component Respondents Who Were Mischaracterized as “Missing” When They Should Have Been Coded as Experiencing a Hostile Work Environment

Sexually Hostile Work Environment Violations	Number of respondents mischaracterized as “missing,” when their responses to follow-up items qualified them as having experienced a hostile work environment (Men/Women)	Number of respondents who were not subsequently categorized as experiencing a sexually hostile work environment on the basis of responses on another hostile workplace behavior (Men/Women)	Number of respondents who were not subsequently categorized as experiencing an MEO violation on the basis of responses to any other series of MEO questions (Men/Women)
SH1: Repeatedly tell sexual “jokes” that made you uncomfortable, angry, or upset?	1 / 10	1 / 5	0 / 5
SH2: Embarrass, anger, or upset you by repeatedly suggesting that you do not act like a [man/woman] is supposed to?	0 / 4	0 / 2	0 / 0
SH3: Repeatedly make sexual gestures or sexual body movements that made you uncomfortable, angry, or upset?	2 / 1	0 / 0	0 / 0
SH4: Display, show, or send sexually explicit materials like pictures or videos that made you uncomfortable, angry, or upset?	1 / 6	0 / 0	0 / 0
SH5: Repeatedly tell you about their sexual activities in a way that made you uncomfortable, angry, or upset?	1 / 8	0 / 0	0 / 0
SH6: Repeatedly ask you questions about your sex life or sexual interests that made you uncomfortable, angry, or upset?	0 / 6	0 / 0	0 / 0
SH7: Make repeated sexual comments about your appearance or body that made you uncomfortable, angry, or upset?	1 / 6	0 / 0	0 / 0
SH8 and SH8a: Either take or share sexually suggestive pictures or videos of you when you did not want them to? AND Did this make you uncomfortable, angry, or upset?	0 / 0	0 / 0	0 / 0

Table 7.9—Continued

Sexually Hostile Work Environment Violations	Number of respondents mischaracterized as “missing,” when their responses to follow-up items qualified them as having experienced a hostile work environment (Men/Women)	Number of respondents who were not subsequently categorized as experiencing a sexually hostile work environment on the basis of responses on another hostile workplace behavior (Men/Women)	Number of respondents who were not subsequently categorized as experiencing an MEO violation on the basis of responses to any other series of MEO questions (Men/Women)
SH9 and SH9a: Make repeated attempts to establish an unwanted romantic or sexual relationship with you? AND Did these attempts make you uncomfortable, angry, or upset?	0 / 7	0 / 2	0 / 2
SH10: Intentionally touch you in a sexual way when you did not want them to?	0 / 0	0 / 0	0 / 0
SH11: Repeatedly touch you in any other way that made you uncomfortable, angry, or upset?	0 / 6	0 / 2	0 / 2
Total People	–	1 / 8	0 / 6

Table 7.10
Changes in Top-Line MEO Violation Estimates as a Result of Programming Error

MEO Violation	With Coding Error	Error Corrected	Change
Sexually Hostile Work Environment			
Men	6.58% (6.07–7.12)	6.58% (6.07–7.13)	+ 0.0023%
Women	21.41% (20.81–22.03)	21.44% (20.84–22.06)	+ 0.0292%
Sexual Harassment			
Men	6.61% (6.09–7.15)	6.61% (6.09–7.16)	+0.0023%
Women	21.57% (20.96–22.19)	21.60% (20.99–22.22)	+ 0.0293%
Any MEO Violation			
Men	7.43% (6.91–7.99)	7.43% (6.91–7.99)	0.0000%
Women	25.97% (25.34–26.61)	26.00% (25.36–26.64)	+ 0.024%

small to have any practical or policy implications, so we record the corrected values in this volume but will not revise and republish Volumes 2 and 3 of *Sexual Assault and Sexual Harassment in the U.S. Military*, nor the two top-line reports where the erroneous values were previously published (National Defense Research Institute, 2014a; 2014b). Appendix D provides complete summary tables of the changes in top-line estimates by service branch and gender (Tables D.1 and D.2), by pay grade and gender (Tables D.3 and D.4), and for the reserve component by gender (D.5). All of the analyses presented earlier in this chapter use the corrected variable derivation for *sexually hostile work environment*, *sexual harassment*, and *any MEO violation*.

Conclusion

In the RAND form, the measures of sexual harassment and gender discrimination first assess whether the respondent had a series of inappropriate workplace experiences. For respondents who have had these negative events occur, additional follow-up items assess characteristics of the events that are required in order for the experiences to be categorized as an MEO violation. By definition, a higher percentage of service members had inappropriate workplace experiences in the past year than had an MEO violation. Depending on the needs of the leader or decisionmaker accessing these numbers, he or she may be more interested in the higher percentage of service members

who experienced inappropriate workplace behaviors (whether those experiences rose to the level of an MEO violation or not), or he or she may be more interested in the smaller percentage of service members whose experiences met DoD policy definitions of an MEO violation. This chapter provides clarity about the nature of each of these estimates. In addition, we described a programming error in our definition of *sexually hostile work environment*, which affected a very small number of respondents and shifted prevalence estimates of hostile workplace experiences by less than three one-hundredths of a percentage.

Comparison of Events Identified by the Prior Form and RAND Forms

Andrew R. Morral, Terry L. Schell, and Coreen Farris

Whereas most RMWS survey respondents received a version of the new RAND form, 29,541 were randomly assigned to a questionnaire that included the sexual harassment and unwanted sexual contact questions used in earlier administrations of the WGRA survey (the prior form). This survey design allowed us to establish whether rates of sexual harassment and unwanted sexual contact were different in 2014 than in prior years, using survey questions and methods that were comparable to those used in the past. It also allowed us to compare estimates derived from the RAND forms with those from the prior form. That is, because respondents were randomly assigned to one or the other form, there should be no systematic differences in the respondents to each form, or their true rates of exposure to criminal or MEO violations. Therefore, in this chapter, we compare estimates from each form to draw inferences about differences in the types of events captured.

Although top-line rates of exposure to sexual assault (or, under the WGRA, unwanted sexual contact) and sexual harassment as measured by the prior form and RAND forms are similar, this apparent similarity conceals substantial differences in the people counted and the types of crimes they experienced. The RMWS was designed to capture sex crimes as defined in the UCMJ and MEO violations as defined in DoD policy. In contrast, the WGRA measures a climate of unwanted sexual experiences associated with illegal behavior, but was not designed as a precise crime or MEO violation measure.

As summarized below, comparisons between the results of the prior form and those of the RAND form suggest that the WGRA counts among those with past-year “unwanted sexual contacts” and sexual harassment some people who have not experienced sex crimes or MEO violations in the past year, while at the same time missing others who have had such experiences. We summarize here some of the key differences in the offenses counted by the two methods.

All comparisons in this chapter are for the members of the DoD active-component population. To ensure that differences noted in this section are attributable to the questionnaires themselves, and not the sample weighting system used, in this chapter all estimates use the RMWS weights. This includes estimates for outcomes assessed on the prior form survey. In earlier volumes, prevalence estimates for outcomes assessed

on the prior form used the WGRA weights to be consistent with existing time trends, while results based on the RAND-designed survey questions use the RMWS weights, which generally yield higher estimates of sexual assault, sexual harassment, and gender discrimination (see Table 3.10). As such, estimates reported in this chapter based on data from the prior form survey differ slightly from those reported in other volumes of *Sexual Assault and Harassment in the U.S. Military*. For example, the estimated number of members who experienced an *unwanted sexual contact* in Volume 2 was 18,900; in this chapter, using the RMWS weights, this is estimated to be 22,100 members.

Some Past-Year Unwanted Sexual Contacts Counted with the Prior Form Occurred More Than a Year Ago

Both the prior form and RAND forms asked about events occurring in the past year. Prior research shows that many respondents report crimes as having taken place in the past year when they actually experienced them more than a year ago. This kind of timeframe “telescoping” can lead to substantially overestimated crime rates (Ander- sen, Kasper, and Frankel, 1979; Cantor, 1989; Lehnen and Skogan, 1984). To mini- mize this bias, the RMWS incorporated many techniques designed to reduce or limit response telescoping (see Volume 1). This appears to have been effective. At the end of the sexual assault section on the RAND form and the end of the unwanted sexual contact section on the prior form, we asked respondents to confirm that the event they were describing occurred in the past year.¹ Whereas 6.8 percent taking the RAND form said they were sure the event actually occurred more than a year ago (i.e., should not be counted as a past-year event), 23.0 percent of prior-form respondents said they were sure the event occurred more than a year ago.²

Moreover, respondents who confirmed that their sexual assaults occurred more than one year ago were excluded from the past-year estimates derived from the RAND form. Using the standard WGRA procedures, the much-larger portion who acknowl- edged that their “one event” occurred more than a year ago were nevertheless included in estimates for the rate of past-year unwanted sexual contacts, which results in over- counting. Had they been excluded, rates on the RAND and prior forms would have

¹ Such a question had not previously been included in the WGRA survey, and it represented the only item added to the prior form used in the 2014 RMWS.

² Our estimate here differs from the preliminary results offered in the top-line report (National Defense Research Institute, 2014a). In that report, we said 25 percent of those counted as having a past-year unwanted sexual contact later said they were sure the event did not occur in the past year. Whereas, in the top-line report, our prior form analyses all used sample weights that followed the methods previously used by DMDC, in this report we applied the new weights developed by RAND to both the RAND form results and the prior form results, so that differences observed between results from the two forms are due exclusively to the form, not to differences in the weights applied to each form.

looked substantially different from each other, with the RAND form identifying more cases of past-year sexual assault.

Both the RAND and prior forms asked respondents for details on one of the possibly multiple unwanted events that occurred in the past year, and it is this “one event” that respondents were asked to confirm occurred within the past year. It could be argued, however, that even though the “one event” selected by prior-form respondents was more often found to have occurred more than a year ago, they nevertheless may still have experienced another unwanted sexual contact that did occur in the past year. That is, perhaps the respondent had several events occur, and even though the one they described happened more than a year ago, others could have occurred within the past year resulting in their proper classification as having a past-year unwanted sexual contact.

This possibility can be tested indirectly. On the prior form, the item immediately following the unwanted sexual contact question asked how many such events occurred in the past year. The next item asked which of those events had the “greatest effect” on the person, and this became the “one event” that respondents were asked to describe in greater detail. If respondents said that only one such event occurred to them, it is presumably only this one that qualified them as having experienced a past-year unwanted sexual contact and which they selected as their “one event.” Therefore, among those with just one unwanted sexual contact, all who later said their one event actually occurred more than a year ago were considered as not having had a past-year unwanted sexual contact.

More than 40 percent of respondents classified as having a past-year unwanted sexual contact on the prior form said that only one such event occurred. Among this 40 percent, 23 percent (or 9.3 percent of all indicating one or more unwanted sexual contacts) later said the event definitely occurred more than a year ago, and so should be excluded from estimated rates of past-year unwanted sexual contacts. Eliminating these cases would decrease the population estimate for past-year unwanted sexual contact by 9.3 percent, from about 22,100 to 20,300.

Undoubtedly, however, other respondents who said their one event occurred more than a year ago but said they had more than one unwanted sexual contact in the past year should also be excluded from the estimate. That is, it would turn out that all of their “past-year” unwanted sexual contacts actually occurred more than a year ago. On the prior form, for those who said their one event actually occurred more than a year ago, we did not follow up to establish whether their other “past year” events did occur in the past year. We did such a follow-up on the RAND form, however, finding that 73.9 percent of those with more than one past-year sexual assault and who said their worst event occurred more than a year ago did not have any sexual assault in the past year. If we assumed that a comparable rate would be found for prior-form respondents with more than one past-year unwanted sexual contact and who confirmed their one event occurred more than a year ago, this would further reduce the population estimate for past-year unwanted sexual contact to 18,400.

Together, these analyses suggest that the WGRA population estimates (as well as the prior form estimate in 2014) of past-year unwanted sexual contact could be inflated by 20 percent, an overestimate attributable to telescoping.

It is possible that the question wording we used to confirm that the “one event” took place in the past year was confusing to respondents, meaning they erroneously indicated that the event occurred more than a year ago. However, this could not account for most of the 22 percent of all who were counted as having a past-year unwanted sexual contact, but who indicated their one event took place more than a year ago. This is because we used the same question wording on the parallel item in the RAND form, where just 6.8 percent of those initially classified as experiencing a past-year sexual assault confirmed the assault occurred more than a year ago. This indicates that the maximum respondent error rate on this item is unlikely to be substantially above 7 percent, which still leaves 15 percent of WGRA cases correctly confirming their one event occurred more than a year ago.

The Prior Form Identifies Fewer Penetrative Sexual Assaults Than the RAND Form

Whereas all types of sexual assaults can be traumatizing, laws treat penetrative crimes as most severe, so they represent an important measure of the severity of sex crimes against service members. Comparison of the number of penetrative sexual assaults and penetrative unwanted sexual contacts derived from the two forms reveals large differences, however.³

Estimates for 2014 using the prior form suggest there were approximately 4,200 service members (95% CI: 3,200–5,300) who experienced a penetrative unwanted sexual contact in the past year (including those that were improperly included due to the telescoping problem described above).⁴ In contrast, the RAND measure assesses whether any of the sexual assaults experienced by the service member in the past year could be counted as a penetrative assault. This estimate suggests the number of penetrative assaults is almost twice as large as was measured using the prior form (7,800 on the RAND form [95% CI: 6,500–9,400]). This effect is most pronounced among men, with the prior form yielding estimates that are less than one-third the rate found using the RAND measures (1,200 versus 3,700, with 95-percent confidence intervals of 600–2,100 and 2,400–5,300, respectively).

³ We count as “penetrative” unwanted sexual contacts all those listed in the prior form as involving completed sexual intercourse, oral sex, anal sex, or penetration by an object or finger.

⁴ The estimates described in this paragraph differ from those from a similar analysis presented in the DoD top-line report (National Defense Research Institute, 2014a). In the DoD Top Line report, point estimates were rounded to the nearest 500. Here all population estimates have been rounded to the nearest 100.

The WGRA was not designed to count the number of people who experienced penetrative unwanted sexual contacts. The only description detailing types of assault occurs for the “one event” selected by the respondent. If respondents experienced multiple unwanted sexual contacts, it is conceivable that they would select an event that involved touching only as their “one event,” even though they also experienced a past-year penetrative contact, for instance. In that case, the WGRA would collect no information that revealed the respondent had also experienced a penetrative assault.

Evidence from the RAND form, however, suggests respondents almost always select as their “worst event” a sexual assault that matches the severity of the most severe sexual assault. On the RAND form, respondents are first asked questions that determined the most severe sexual assault type they experienced in the past year. Respondents were then asked to describe the “worst” such assault. When we compared the “worst” event to the most severe, they nearly always matched:

- 99.2 percent of those whose “worst event” type was penetrative had been classified as having a penetrative assault as their most severe past-year assault
- 97.1 percent of those whose “worst event” was a non-penetrative (contact only) assault were classed as having a non-penetrative assault as their most severe past-year event
- 74.6 percent of those with a “worst event” of attempted assault were classed as having attempted assault as their most severe past-year sexual assault.

The overall concordance rate across the three categories is 97.0 percent.

This suggests that it is very unlikely that many people who experienced penetrative assaults in the past year chose some other less-serious crime as the one that had the greatest effect on them when completing the prior form. Instead, it suggests the RAND form identified nearly twice as many people who experienced a serious penetrative assault in the past year.

There may be several reasons for the difference in estimates of penetrative sexual assaults produced by the two forms. The RAND form asked three behaviorally specific and detailed questions about penetrative sexual assault that align closely with the definitions used in the UCMJ; those three questions are asked of everyone in the survey. In contrast, the prior form first filters out most respondents on the basis of a single complex gating question. Research on survey design, however, shows omnibus questions about rape do not cue memories of relevant experiences as effectively as do a series of behaviorally specific questions (Cook et al., 2011; National Research Council, 2014; Koss, 1993). Therefore, one factor contributing to the different rates produced by the two forms may be that the RAND form is more likely to cue memories of unwanted sexual contacts.

A second potentially important difference is that the prior form’s screening question emphasized that the events under consideration were “sexual” events. The ques-

tion makes reference to “sexual contacts,” “sexual intercourse,” “oral sex,” “anal sex,” and “sexual touching.” However, many sexual assaults may not feel sexual. Indeed, the UCMJ does not require that the assault be perceived as “sexual” by the victim or the perpetrator. Penetration or contact with genitalia, mouth, or buttocks that is abusive, harassing, or demeaning can qualify as sexual assaults in Article 120 of the UCMJ, even if they are not done with a sexual intent. As such, some instances of bullying, hazing, or harassment that victims do not experience as in any way “sexual,” but which are sexual assaults under the law, may well be inadvertently omitted by the focus on sexuality in the prior-form items. The RAND form, like the UCMJ code, describes behavioral events without imputing to them an experience of “sexuality,” and therefore is less likely to exclude sexual assaults that are not experienced as sexual contacts. This could also explain why the difference in rates of penetrative sexual assaults between the prior form and RAND forms are disproportionately higher for men than women, since, as noted in Volume 2, men are far more likely to describe the sexual assaults they experienced as designed to humiliate or abuse, or as forms of hazing, rather than as sexual encounters.

Unwanted Sexual Contacts on the Prior Form May Include Events That Are Not UCMJ Crimes

A large percentage of respondents on the prior form indicated that their unwanted sexual contact was not described by any of the options meant to classify sexual assaults. For instance, 14.6 percent of those classified as having experienced an unwanted sexual contact say the “one event” did not involve another person doing any of the behaviors defining unwanted sexual contact: sexually touching; attempting unsuccessfully to have sexual intercourse; making the respondent have sexual intercourse; attempting unsuccessfully to make the respondent perform or receive oral sex, anal sex, or penetration by a finger or object; or making the respondent perform or receive oral sex, anal sex, or penetration by a finger or object.⁵ In other words, more than one out of every seven respondents classified as experiencing an unwanted sexual contact selected as their “one event” an incident that did not match any of the criteria defining an unwanted sexual contact.

The 2014 prior-form estimates suggest there were approximately 18,000 service members who described “one event” involving an unwanted sexual contact that was not classed as penetrative (it was contact only, attempted, or unspecified). In contrast, the

⁵ Although 19.8 percent of those counted as experiencing unwanted sexual contact in the past year cannot be counted as experiencing a penetrative, non-penetrative, or attempted sexual contact as their “one event,” 14.6 percent of this number indicated “did not do this” for every type of sexual contact listed for establishing the unwanted sexual contact type categorization. The remaining 5.2 percent skipped one or more items, and did not mark “yes” on any item they did not skip.

RAND form identified 12,400 members with non-penetrative crimes in the past year. Thus, it may be that the estimate generated using the RAND form excluded some 5,600 service members who would have been counted as having experienced an unwanted sexual contact on the prior form, but whose experience does not meet the legal threshold for a sexual assault. An alternative explanation—that many penetrative crimes are counted as non-penetrative or unclassified unwanted sexual contacts on the prior form—seems unlikely: if respondents frequently misclassified or failed to classify their sexual assaults, we would expect to see the same behavior on the RAND form. As discussed above, however, the concordance between the most serious past-year sexual assault and the classification offered by respondents on their worst past-year assault is 97.0 percent.

This finding that 5,600 of the estimated 22,100 service members classed as experiencing an unwanted sexual contact using the prior form, or 25 percent, appear not to have experienced criminal events is consistent with one of the criticisms of the survey discussed in Volume 1. That is, the wording of the unwanted sexual contact question could be interpreted to include sexual encounters that were unwanted but not criminal (Schenck, 2014). While identification of strictly criminal acts was, according to the WGRA survey developers, never the goal of the unwanted sexual contact measure, considerable confusion arose over how the unwanted sexual contact results should be interpreted. By applying the definitional criteria contained in UCMJ Article 120 to rule out noncriminal events, the RMWS survey results should offer greater clarity on the significance and severity of the sexual assaults it identifies.

Differences Between the WGRA and RAND Sexual Harassment Definitions

Data collected with the prior form produced an estimated prevalence of past-year sexual harassment among men and the total active-duty population that is lower than the RAND form estimates (Table 8.1). (As with other analyses in this chapter, the prior-form estimates reported here use the new RMWS weights so any differences between the RAND form and the prior form are attributable to the surveys themselves, not differences in the weights applied to them.)

This difference likely reflects important dissimilarities in the way the two instruments define sexual harassment. The section that follows explores several ways in which the two instruments differ in their categorization of sexual harassment, and the changes in population estimates that occur when the RMWS classification criteria for sexual harassment are altered to more closely match the classification criteria of the prior form. Although these exercises provide a general sense of how each of the measurement differences may affect the overall estimate of sexual harassment, in each case, other significant differences between the instruments remain and no perfect one-to-one correspondence is possible between the two.

Table 8.1
Estimated Percentage of Active-Component Service Members Who Experienced Sexual Harassment in the Past Year, as Assessed with the Prior Form and RAND Form

Form	Total	Men	Women
Prior	6.23% (5.76–6.73)	3.64% (3.12–4.21)	20.94% (20.08–21.81)
RAND	8.85% (8.40–9.31)	6.61% (6.09–7.15)	21.57% (20.96–22.19)

NOTES: 95-percent confidence intervals for each estimate are indicated in parentheses. RMWS weights were applied to both forms for comparability.

Sexual Contact Crimes Occurring in the Workplace

The modified version of the SEQ-DoD-Short (Stark et al., 2002) that is used in the WGRA surveys and the prior form includes two items that assessed attempted or completed sexual assaults by coworkers. The RAND measure of sexual harassment included a single item assessing unwanted sexual touching, which is potentially classifiable as a sexual assault under Article 120. Although these workplace events may be classified as criminal actions, they may also be sexual harassment if the perpetrator was someone with whom the victim works. For example, a worker who was sexually assaulted by a supervisor may have grounds to bring both a criminal case for the sexual assault and a sexual harassment case for the hostile work environment. The scoring conventions for the WGRA and RAND sexual harassment measures differ in their treatment of these sexual contact items. The WGRA excluded them from the estimate of sexual harassment; for example, respondents who indicated that a coworker had sex with them against their will are not counted as having experienced sexual harassment unless they also experienced some other form of sexual harassment. The RAND sexual harassment measure included unwanted sexual touching in the workplace as a possible instance of sexual harassment. Even though the event may rise to the level of a sexual assault (i.e., a crime), respondents who indicated on the RAND form that someone they worked with touched them sexually without their consent were categorized as having been sexually harassed whether they had additional sexual harassment experiences or not.

To assess whether inclusion of types of sexual harassment incidents that may also be sexual crimes may have inflated the RAND rate of sexual harassment relative to the prior form, we recalculated our sexual harassment estimates excluding all respondents for whom their only sexual harassment experience was unwanted sexual touching by a coworker. This alignment in scoring strategy with the WGRA did not significantly reduce the estimated rate of sexual harassment among women or men as measured by the RAND form. For women, the rate of sexual harassment with sexual touching at work excluded (21.48 percent, 95% CI: 20.87–22.10) was not significantly different than the rate of sexual harassment when incidents of unwanted sexual touching at

work were included (21.57 percent, 95% CI: 20.96–22.19). For men, the rate of sexual harassment with sexual touching at work excluded (6.58, 95% CI: 6.07–7.13) was not significantly different than the rate of sexual harassment when incidents of unwanted sexual touching at work were included (6.61 percent, 95% CI: 6.09–7.15). That is, this measurement difference does not account for the difference in men’s sexual harassment prevalence as measured by the prior form versus the RAND form.

This analysis was conducted for the sole purpose of determining whether alignment with WGRA scoring criteria for sexual harassment would change the RAND estimates of sexual harassment. It is not an endorsement of excluding workplace sexual assaults from the measure of sexual harassment. In agreement with the original developers of the SEQ (Stark et al., 2002) and legal and DoD directives, we encourage continued scoring of the RMWS sexual harassment measure as including unwanted workplace sexual contact as one form of sexual harassment.

Respondent Classifies Events as Sexual Harassment

The RAND measure of sexual harassment does not require respondents to correctly label their workplace experiences as “sexual harassment” in order to be categorized as having experienced sexual harassment in the past year. Because most people are not familiar with the details of equal employment opportunity law and MEO regulations, many who experience sexual harassment do not recognize it as such, and are unable to correctly label the events (Fitzgerald, Swan, and Fischer, 1995). Instead, the RAND form walks respondents through a series of questions assessing the criteria to establish that an MEO violation had occurred. In contrast, the prior form requires respondents to indicate that they had an inappropriate workplace experience in the past year *and* that they consider it to have been “sexual harassment.” All respondents who do not consider the events to have been sexual harassment are excluded from the prior form estimates of sexual harassment in the services, using the scoring criteria of previous WGRA surveys.

This difference in measurement could have dramatic effects on the estimated rate of sexual harassment. To explore this possibility, we assessed the degree to which the estimated rate of sexual harassment in the past year, as measured by the RAND form, would decline if we were to also require, as the WGRA surveys have, that the respondent label their experiences sexual harassment. We are able to assess this on the RAND form because, although we did not require respondents to label an event as harassment to count it as such, we did ask them if they considered it to be “sexual harassment.” If self-labeling were required, the estimated rate of sexual harassment in the past year among female service members would drop from 21.57 percent (95% CI: 20.96–22.19) to 15.16 percent (95% CI: 14.62–15.71). That is, requiring women to label their experiences sexual harassment drops the RAND form’s estimated annual prevalence of sexual harassment below the estimate from the prior form (20.23 percent; 95% CI: 19.45–21.03). For men, requiring potential victims to label their experiences “sexual harassment” drops the RAND form estimate from 6.61 percent to 3.31 percent (95%

CI: 2.92–3.73), which more closely aligns with the prior-form estimate (3.50 percent; 95% CI: 3.07–3.97). Again, these modified estimates are provided only to allow an assessment of comparability between the two forms and are not intended to indicate support for the survey requirement that respondents have thorough knowledge of sexual harassment law and policy.

Persistence, Severity, or Direct Evidence of *Quid Pro Quo*

Unlike the modified version of the SEQ-DoD-Short used in the prior form, the RAND sexual harassment measure included multiple criteria to assess whether the inappropriate workplace behavior the respondent experienced rose to the level of an MEO violation, as specified in DoD directives. Possible indicators of a hostile workplace environment (e.g., repeated sexual jokes that the respondent found offensive) were followed by additional questions to ascertain whether the offender(s) ever knew that someone in the workplace was offended and, if so, whether their behavior persisted, or failing that, whether the behavior was so severe that a reasonable person would find it offensive. Only after meeting one of these follow-up conditions was an inappropriate workplace behavior categorized as sexual harassment.

The SEQ-DoD-Short takes a different approach to classification. Rather than seeking to classify events that likely would rise to the standards set in legal precedent and DoD directives, the instrument sought to classify “psychological” sexual harassment (Fitzgerald, Swan, and Magley, 1997). In short, it assessed whether the respondent experienced workplace events that he or she found offensive (a construct we referred to as “inappropriate workplace behavior”), but did not follow with an assessment of persistence, severity, or of direct evidence in the case of *quid pro quo* exchanges.

To assess whether the added classification criteria used in the RAND form may have suppressed the estimate of sexual harassment relative to the prior form, we calculated the percentage of male and female service members who experienced any of our initial screening questions assessing inappropriate workplace behavior that could indicate sexual harassment, without requiring them also to specify that the events were persistent or severe (in the case of hostile workplace events) or that they had direct evidence of an exchange (in the case of *quid pro quo* events). This alignment in scoring strategy increased our past-year estimates among women (26.12 percent; 95% CI: 25.48–26.77) and men (11.98 percent; 95% CI: 11.32–12.67), and increased the discrepancy with the WGRA estimates for women (20.94 percent; 95% CI: 20.08–21.81) and men (3.64 percent; 95% CI: 3.12–4.21). One potential explanation is that, while the prior form did not require the events to meet DoD criteria for sexual harassment, it did require the respondent to label the event “sexual harassment,” which would also have the effect of reducing the estimates overall.

This analysis was conducted for the sole purpose of determining whether alignment with WGRA scoring criteria for sexual harassment would change the RAND estimates of sexual harassment. We believe it will be important to continue to report

both experiences of inappropriate workplace behaviors (i.e., negative workplace experiences that do not necessarily rise to the level of an MEO violation, but nonetheless represent an unprofessional work environment), as well as sexual harassment as defined by DoD directives.

With All Possible Alignment

To provide the closest match to WGRA criteria for sexual harassment, we calculated the rate of sexual harassment estimated using the RAND measure with the following changes: (1) excluding possible sexual contact crimes from the estimate; (2) requiring the respondent to label his or her experiences “sexual harassment”; and (3) dropping the requirement that inappropriate workplace behaviors be either persistent, severe, or that there be direct evidence of a *quid pro quo* exchange. With these changes to the RAND classification requirements, the estimated percentage of military women who would be classified as sexually harassed shifts from 21.57 percent (95% CI: 20.96–22.19) to 14.91 percent (95% CI: 14.38–15.46), and the estimated percentage of military men who would be classified as sexually harassed shifts from 6.61 percent (95% CI: 6.09–7.15) to 3.25 percent (95% CI: 2.86–3.67). In relation to the prior form, it reduces alignment with the prior form estimate of sexual harassment among service women (20.23 percent; 95% CI: 19.45–21.03) and creates alignment with estimate among service men (3.50 percent; 95% CI: 3.07–3.97).

Conclusions

In this chapter, we presented analyses comparing responses on the newly designed RAND form survey to responses on the prior-form survey, which administered the questions used in the 2012 WGRA to assess sexual harassment and unwanted sexual contact. These comparisons suggest that the prevalence of unwanted sexual contact in the past year generated by the prior form is likely overestimated by 20 percent because of the inclusion of service members whose most-recent unwanted sexual contact occurred more than a year earlier. Similarly, we demonstrate that the prior form identified only about one-half as many service members who experienced penetrative sexual assaults as the RAND form (4,200 versus 7,800). This effect is larger for male service members, with the RAND form identifying three times as many experiencing penetrative assaults (1,200 versus 3,700). This effect may be partially attributable to the RAND form identifying more sexual assaults that occur in the context of hazing or that are not perceived as sexual by the service member relative to the prior form.

On the other hand, the prevalence of unwanted sexual contacts that are not penetrative (assessed on the prior form) was substantially higher than the prevalence of non-penetrative sexual assault (assessed on the RAND form). Indeed, the prior form counted 5,600 more individuals in this category than the RAND form. If the experi-

ences of these individuals does not, in fact, meet the criteria for a UCMJ sexual assault, as suggested by the fact that the RAND form did not identify a similar proportion on non-penetrative assault victims, this would suggest that 25 percent of all unwanted sexual contacts counted using the prior form were not, in fact, crimes. Many may not have met even the criteria for an unwanted sexual contact, as we found that 18 percent of all those with a “one event” on the prior form that was not penetrative positively affirmed that their unwanted sexual contact experience met none of the behavioral descriptions defining unwanted sexual contact.

The fact that the over- and undercounts described here for the prior form approximately cancel each other should not be taken as evidence that the WGRA questionnaire offers a satisfactory measure of sexual offenses for the purposes of tracking the effectiveness of DoD policies or for estimating the total number of offenses occurring against service men and women. Measures that do not accurately and precisely count those people or events that are the target of training, prevention, or other policies or programs are unlikely to be sensitive to changes brought about by these programs. For example, the implementation of policies that effectively reduce sexual assaults may not result in a detectable corresponding change in this measure of unwanted sexual contact.

A comparison of responses across forms also revealed differences in how the two sexual harassment measures identified instances of harassment. One substantial difference between measures was that the prior form measure only counted as cases of sexual harassment those participants who labeled their inappropriate work experiences as “sexual harassment.” We found this “labeling” requirement significantly reduced prevalence estimates for sexual harassment. Indeed, if the RAND form required respondents to label instances as sexual harassment, our overall prevalence rate for past-year sexual harassment would have fallen by 30 percent. The effect is more pronounced among men, where rates would have fallen by 50 percent. This largely explains the different prevalence estimates produced by the two methods. When we adjust the RAND form’s past-year sexual harassment prevalence rates to match the criteria used in the prior form (implementing the “labeling” requirement, excluding sexual touching and other adjustments), we found that the RAND form identified fewer cases of sexual harassment against women and comparable numbers for men compared with the prior form.

The pattern of differences across survey forms suggests that the new form, designed by RAND to address several concerns about the WGRA instrument, offers improved validity and interpretability relative to the prior form. We recommend that DMDC use the questions and scoring rules developed for the RAND form in future WGRA surveys.

Analysis of Survey Nonconsent and Breakoff

Terry L. Schell

Documenting the factors that lead to survey nonresponse can be informative about the possibility of nonresponse bias. One way to investigate these factors is to look separately at different types of nonresponse: sampled individuals who never navigate to the survey website; individuals who go to the website but did not consent to participate in the study; and individuals who begin to participate but quit, or break off, at some point before the end of the survey.

In the current study, the latter two categories are particularly interesting because detailed information about the topic of the survey was not presented to service members until after they navigated to the survey web portal. The recruitment materials did not emphasize that the survey was about sexual assault, sexual harassment, or gender discrimination. This was a deliberate decision designed to reduce the extent to which decisions about survey participation were based on the individual's personal experience with these outcomes—which could create a type of nonresponse bias that cannot be removed by weighting or other common methods.

Once a sampled service member navigated to the website, however, the informed consent notice explained that “The survey asks about whether or not you have experienced harassment, discrimination, or inappropriate sexual behavior.” Decisions about whether to participate made at that point may be directly influenced by the respondent's personal experiences with those topics and may be the source of non-ignorable nonresponse bias. Similarly, some respondents started the survey but stopped responding at some point. That type of breakoff may be a reaction to the particular content of the survey and may result in respondents and nonrespondents being meaningfully different in their underlying experiences with sexual assault, sexual harassment, and gender discrimination.

In order to better understand how survey nonconsent and breakoff may have contributed to nonresponse bias, this chapter documents the number of individuals who stopped participating at each point in the process, and provides information about the characteristics of the individuals who stopped at that point (“breakoffs”).

Survey Nonconsent Rate

For the purpose of this analysis, all respondents whose unique study identification number was logged on the survey landing page, but failed to answer any survey questions, were considered survey nonconsent. This included some people who accidentally clicked on the link in the email and did not read the consent form before closing their browser, as well as some people who clicked the “continue” button on the informed consent page (indicating an agreement to do the survey), but then chose not to answer any questions.

A total of 156,130 individuals who had been assigned to the RAND instrument (long, medium, or short form; active or reserve component; DoD service or Coast Guard) hit the survey starting page, and 94.1 percent of those answered at least one survey question (146,986). Thus, 9,144 individuals met our definition of survey nonconsent. This rate of nonconsent represents 2.0 percent of the total sample frame (i.e., a 2-percent reduction in the overall response rate), and 5.9 percent of the individuals who hit the survey start page.

Rates of nonconsent were similar on the prior form, which used an abbreviated set of questions from past WGRA studies and was administered to a random sample of active-component DoD service members. Prior-form respondents saw a consent form that was very similar to that used with the RAND form. The primary difference was that prior-form respondents were told that the survey took 12 minutes to complete, while almost all RAND form respondents (87 percent, those randomized to short and medium forms) were told it would take eight minutes to complete, while the long-form participants (13 percent) were told it would take 20 minutes. A total of 2,539 service members assigned to the prior form met our definition of nonconsent. This rate of nonconsent represents 2.5 percent of the total sample frame (i.e., a 2.5-percent reduction in the overall response rate), and 7.4 percent of the individuals who hit the survey start page.

Survey Breakoff Rates

Survey breakoff was defined by identifying the last question for which the respondent selected a valid response option. For most respondents, this was the final question on their version of the survey. A respondent may have clicked the “next item” button to investigate the subsequent questions, but would be counted as “breakoff” after the point where they last entered a response. For items that included explicit “don’t know” response options, those options were considered valid responses.

RAND Instrument

Due to the complex skip pattern and multiple instrument forms, the number of individuals who were presented a given question varied substantially across questions on the RAND instrument. However, when a question was presented, it was always placed in the same ordinal position within the instrument. Thus, we can compute the cumulative rate of breakoff as respondents moved through the survey. Table 9.1 presents the number of individuals for whom a particular item was their final complete item. It combines respondents across the short, medium, and long forms. For example, 559 respondents' final complete item was "Intro1," indicating that some respondents broke off immediately after completing the first item ("Are you male or female?"). Demo3 was the final complete item for the majority of all respondents (80 percent) because it was the final item presented to participants in the short and medium forms.

As expected, the 35 *base* questions that were included in all forms of the instrument and were not imbedded within any item skip pattern occurred as respondents' final response more frequently than items shown to fewer participants. These base questions are listed in boldface in Table 9.1. On average, 0.17 percent broke off after each base question. Within these base questions, there is a clear pattern with the six items that were followed by survey instructions or a new topic of questions showing higher rates of breakoff; on average, each such questions were the final response for 0.41 percent. The six sexual assault screening questions were base questions and averaged 0.24 percent breakoff, which is close to the average for other base items.

Because of the complex skip pattern and separate forms, Table 9.1 does not always identify which item was the first item that respondents failed to answer. We have included Table 9.2, which presents the same data on the subset of respondents who were assigned to the short form, which has fewer questions and a simplified skip pattern.

Across all three forms, the total rate of breakoff in the core portion of the RAND instrument (the portion administered to all participants) was 6.4 percent. That is, 93.6 percent of those who answered at least one survey question also answered the last question administered to all participants (Demo3). We can also compute the breakoff within the long form questions for those respondents who were randomized to the long form instrument and answered at least one question on the survey. Of the 22,164 individuals in this group, 1,261 (5.7 percent) gave their final survey response after the first item of the long form but before the last item administered to all long-form participants (Longform29). The breakoff rate across the entire long form (combining the core and the long-form-specific items) was 12.8 percent.

To better understand where in the core instrument that 6.4 percent broke off, as well as how breakoff affected unit nonresponse, we computed breakoff rates within each survey module in Table 9.3, rather than for each question.

All breakoff prior to the sexual assault classification module was counted as unit nonresponse and handled through the nonresponse weights. This type of breakoff represented 3.9 percent of the sample who started the survey and contributed to a reduction in the overall response rate of 1.2 percent.

Table 9.1
Final Participant Response by Survey Item, All RAND Form Types

Item	Count	Percentage	Cumulative Percentage	Item	Count	Percentage	Cumulative Percentage
Intro1	559	0.38%	0.38%	SH6c	5	0.00%	3.37%
P1	247	0.17%	0.55%	SH6d	8	0.01%	3.37%
P2	91	0.06%	0.61%	SH7b	7	0.00%	3.38%
P3	120	0.08%	0.69%	SH7c	8	0.01%	3.38%
P4	140	0.10%	0.79%	SH7d	7	0.00%	3.39%
P5	1,054	0.72%	1.50%	SH8d	1	0.00%	3.39%
SH1	196	0.13%	1.64%	SH9b	3	0.00%	3.39%
SH2	159	0.11%	1.75%	SH9c	8	0.01%	3.40%
SH3	131	0.09%	1.83%	SH9d	7	0.00%	3.40%
SH4	105	0.07%	1.91%	SH11b	2	0.00%	3.40%
SH5	111	0.08%	1.98%	SH11c	1	0.00%	3.40%
SH6	106	0.07%	2.05%	SH11d	6	0.00%	3.41%
SH7	95	0.06%	2.12%	SH12e	2	0.00%	3.41%
SH8	129	0.09%	2.21%	SH13e	1	0.00%	3.41%
SH8a	1	0.00%	2.21%	SH14a	13	0.01%	3.42%
SH9	111	0.08%	2.28%	SH15a	83	0.06%	3.48%
SH9a	4	0.00%	2.29%	SHFU1	105	0.07%	3.55%
SH10	109	0.07%	2.36%	SHFU2	19	0.01%	3.56%
SH11	137	0.09%	2.45%	SHFU2_1	2	0.00%	3.56%
SH12	146	0.10%	2.55%	SHFU3	23	0.02%	3.58%
SH13	131	0.09%	2.64%	SHFU4	12	0.01%	3.59%
SH14	101	0.07%	2.71%	SHFU4c	12	0.01%	3.59%
SH15	873	0.59%	3.30%	SHFU5	14	0.01%	3.60%
SH1b	6	0.00%	3.31%	SHFU5a	1	0.00%	3.60%
SH1c	6	0.00%	3.31%	SHFU5d	8	0.01%	3.61%
SH1d	8	0.01%	3.32%	SHFU6	32	0.02%	3.63%
SH2b	10	0.01%	3.32%	SHFU7a	3	0.00%	3.63%
SH2c	2	0.00%	3.33%	SHFU7b	3	0.00%	3.64%
SH2d	8	0.01%	3.33%	SHFU7e	66	0.04%	3.68%
SH3b	6	0.00%	3.34%	SHFU8a	2	0.00%	3.68%
SH3c	1	0.00%	3.34%	SHFU8b	2	0.00%	3.68%
SH3d	10	0.01%	3.34%	SHFU8d	2	0.00%	3.68%
SH4c	5	0.00%	3.35%	SHFU8e	3	0.00%	3.69%
SH4d	5	0.00%	3.35%	SHFU8f	2	0.00%	3.69%
SH5b	1	0.00%	3.35%	SHFU8g	1	0.00%	3.69%
SH5c	6	0.00%	3.35%	SHFU8i	26	0.02%	3.71%
SH5d	12	0.01%	3.36%	SHFU9a	2	0.00%	3.71%
SH6b	5	0.00%	3.37%	SHFU9c	2	0.00%	3.71%

Table 9.1—Continued

Item	Count	Percentage	Cumulative Percentage	Item	Count	Percentage	Cumulative Percentage
SHFU9d	51	0.03%	3.74%	OB4b	2	0.00%	4.64%
SHFU10a	5	0.00%	3.75%	OB4c	1	0.00%	4.64%
SHFU10c	1	0.00%	3.75%	OB4d	3	0.00%	4.65%
SHFU10e	1	0.00%	3.75%	OB4f	2	0.00%	4.65%
SHFU10g	47	0.03%	3.78%	OB4g	1	0.00%	4.65%
SHFU10h	2	0.00%	3.78%	OB4h	2	0.00%	4.65%
SHFU10i	1	0.00%	3.78%	OB4j	1	0.00%	4.65%
SHFU10n	9	0.01%	3.79%	OB4k	1	0.00%	4.65%
SHFU11f	44	0.03%	3.82%	SA5	179	0.12%	4.77%
SHFU12a	4	0.00%	3.82%	PF5a	1	0.00%	4.77%
SHFU12c	7	0.00%	3.83%	OB5d	2	0.00%	4.77%
SHFU12f	1	0.00%	3.83%	SA6	777	0.53%	5.30%
SHFU12g	14	0.01%	3.84%	OB6d	1	0.00%	5.30%
SHFU12h	2	0.00%	3.84%	OB6h	1	0.00%	5.30%
SHFU12i	1	0.00%	3.84%	OB6j	1	0.00%	5.31%
SHFU12j	3	0.00%	3.84%	SAFU1	4	0.00%	5.31%
SHFU12k	2	0.00%	3.84%	SAFU2	11	0.01%	5.32%
SHFU12l	5	0.00%	3.84%	SAFU3f	1	0.00%	5.32%
SHFU12m	1	0.00%	3.85%	SAFU5	3	0.00%	5.32%
SHFU12n	3	0.00%	3.85%	SAFU6	3	0.00%	5.32%
SHFU12p	6	0.00%	3.85%	SAFU7	4	0.00%	5.32%
SHFU12r	1	0.00%	3.85%	SAFU8d	1	0.00%	5.32%
SHFU12s	4	0.00%	3.85%	SAFU8l	2	0.00%	5.32%
SHFU12t	3	0.00%	3.86%	SAFU8g	1	0.00%	5.33%
SHFU12u	3	0.00%	3.86%	SAFU8h	1	0.00%	5.33%
SHFU12v	3	0.00%	3.86%	SAFU8j	2	0.00%	5.33%
SHFU12w	6	0.00%	3.86%	SAFU9b	1	0.00%	5.33%
SHFU12x	14	0.01%	3.87%	SAFU9d	4	0.00%	5.33%
SA1	401	0.27%	4.15%	SAFU9e	5	0.00%	5.33%
OB1a	2	0.00%	4.15%	SAFU10h	1	0.00%	5.34%
OB1c	1	0.00%	4.15%	SAFU10i	9	0.01%	5.34%
OB1h	6	0.00%	4.15%	SAFU11a	1	0.00%	5.34%
SA2	283	0.19%	4.35%	SAFU11f	1	0.00%	5.34%
OB2g	1	0.00%	4.35%	SAFU11k	1	0.00%	5.34%
SA3	223	0.15%	4.50%	SAFU12	4	0.00%	5.35%
SA4	203	0.14%	4.64%	SAFU13a	1	0.00%	5.35%
PF4a	5	0.00%	4.64%	SAFU13b	1	0.00%	5.35%
PF4b	1	0.00%	4.64%	SAFU13d	5	0.00%	5.35%
OB4a	2	0.00%	4.64%	SAFU14	3	0.00%	5.35%

Table 9.1—Continued

Item	Count	Percentage	Cumulative Percentage	Item	Count	Percentage	Cumulative Percentage
SAFU15	1	0.00%	5.35%	Longform5	33	0.02%	86.05%
SAFU16	3	0.00%	5.36%	Longform6	18	0.01%	86.06%
SAFU17	7	0.00%	5.36%	Longform7	15	0.01%	86.07%
SAFU18e	4	0.00%	5.36%	Longform8	20	0.01%	86.09%
SAFU19	3	0.00%	5.37%	Longform9	45	0.03%	86.12%
SAFU20n	1	0.00%	5.37%	Longform10b	1	0.00%	86.12%
SAFU21	2	0.00%	5.37%	Longform10e	17	0.01%	86.13%
SAFU22l	1	0.00%	5.37%	USCG3c	1	0.00%	86.13%
SAFU23	3	0.00%	5.37%	USCG3d	2	0.00%	86.13%
SAFU24	2	0.00%	5.37%	Longform11	79	0.05%	86.19%
SAFU28a	1	0.00%	5.37%	Longform11_1	4	0.00%	86.19%
SAFU30d	1	0.00%	5.37%	Longform12a	2	0.00%	86.19%
SAFU30g	3	0.00%	5.37%	Longform12e	8	0.01%	86.20%
SAFU30i	1	0.00%	5.38%	Longform13	73	0.05%	86.24%
SAFU30x	1	0.00%	5.38%	Longform14d	1	0.00%	86.25%
SAFU32	7	0.00%	5.38%	Longform14e	186	0.13%	86.37%
SAFU33a	1	0.00%	5.38%	Longform15a	10	0.01%	86.38%
SAFU33b	1	0.00%	5.38%	Longform15b	1	0.00%	86.38%
SAFU33d	3	0.00%	5.38%	Longform15e	2	0.00%	86.38%
SAFU33b_1	1	0.00%	5.38%	Longform15f	1	0.00%	86.38%
SAFU34	2	0.00%	5.39%	Longform15g	2	0.00%	86.38%
SAFU36	28	0.02%	5.41%	Longform15i	37	0.03%	86.41%
SAFU37a	1	0.00%	5.41%	Longform16a	1	0.00%	86.41%
SAFU37e	3	0.00%	5.41%	Longform16b	1	0.00%	86.41%
SAFU38a	20	0.01%	5.42%	Longform16e	19	0.01%	86.42%
SAFU38b	8	0.01%	5.43%	Longform17	6	0.00%	86.43%
SAFU38c	6	0.00%	5.43%	Longform18	27	0.02%	86.44%
SAFU38d	5	0.00%	5.43%	Longform19	62	0.04%	86.49%
SAFU38e	318	0.22%	5.65%	Longform19e	27	0.02%	86.51%
SAFU39	7	0.00%	5.66%	Longform20a	1	0.00%	86.51%
SAFU40	12	0.01%	5.66%	Longform20b	1	0.00%	86.51%
DEMO1	206	0.14%	5.80%	Longform20d	2	0.00%	86.51%
RGSF1	28	0.02%	5.82%	Longform20h	24	0.02%	86.52%
DEMO2	917	0.62%	6.45%	USCG2	1	0.00%	86.53%
DEMO3	116,907	79.54%	85.98%	Longform21b	6	0.00%	86.53%
Longform1	25	0.02%	86.00%	Longform22	147	0.10%	86.63%
Longform2	12	0.01%	86.01%	Longform23a	2	0.00%	86.63%
Longform3	10	0.01%	86.01%	Longform23b	3	0.00%	86.63%
Longform4	20	0.01%	86.03%	Longform23c	1	0.00%	86.63%

Table 9.1—Continued

Item	Count	Percentage	Cumulative Percentage
Longform23d	1	0.00%	86.63%
Longform23i	1	0.00%	86.63%
Longform23j	22	0.01%	86.65%
USCG1e	10	0.01%	86.66%
USCG4a	1	0.00%	86.66%
USCG4h	2	0.00%	86.66%
USCG5e	1	0.00%	86.66%
Longform24	72	0.05%	86.71%
Longform25d	22	0.01%	86.72%
Longform26	24	0.02%	86.74%
Longform27	19	0.01%	86.75%
Longform28	130	0.09%	86.84%
Longform29	11,729	7.98%	94.82%
USMC2	2	0.00%	94.82%
USMC3	125	0.09%	94.91%
USMC4a	95	0.06%	94.97%
USMC4b	81	0.06%	95.03%
USMC4c	109	0.07%	95.10%
USMC4d	88	0.06%	95.16%
USMC4e	151	0.10%	95.26%
USMC4f	727	0.49%	95.76%
USAF1	6,235	4.24%	100.00%

NOTE: Percentages are given among the proportion of the sample that answered at least one question (N = 146,986), including the reserve components and Coast Guard. Item labels in bold were administered to all respondents in all forms. Some survey items are not presented in this table because they were not the final survey item for any participant. Questions are listed in the order they appear in the instrument. The instrument is included in Volume 1 of this report series.

Table 9.2
Final Participant Response by Survey Item, RAND Short Form

Item	Count	Percentage	Cumulative Percentage	Item	Count	Percentage	Cumulative Percentage
Intro1	240	0.38%	0.38%	OB4g	1	0.00%	4.05%
P1	105	0.17%	0.55%	SA5	89	0.14%	4.19%
P2	40	0.06%	0.62%	SA6	330	0.53%	4.72%
P3	42	0.07%	0.68%	OB6d	1	0.00%	4.72%
P4	66	0.11%	0.79%	SAFU1	1	0.00%	4.72%
P5	432	0.69%	1.48%	SAFU2	1	0.00%	4.72%
SH1	93	0.15%	1.63%	SAFU5	1	0.00%	4.73%
SH2	66	0.11%	1.74%	SAFU6	2	0.00%	4.73%
SH3	55	0.09%	1.82%	SAFU8I	1	0.00%	4.73%
SH4	44	0.07%	1.89%	SAFU8h	1	0.00%	4.73%
SH5	36	0.06%	1.95%	SAFU8j	2	0.00%	4.74%
SH6	46	0.07%	2.03%	SAFU9d	1	0.00%	4.74%
SH7	43	0.07%	2.09%	SAFU9e	2	0.00%	4.74%
SH8	48	0.08%	2.17%	SAFU10h	1	0.00%	4.74%
SH8a	1	0.00%	2.17%	SAFU10i	2	0.00%	4.75%
SH9	43	0.07%	2.24%	SAFU12	2	0.00%	4.75%
SH9a	1	0.00%	2.24%	SAFU13d	2	0.00%	4.75%
SH10	41	0.07%	2.31%	SAFU14	1	0.00%	4.75%
SH11	61	0.10%	2.41%	SAFU16	2	0.00%	4.76%
SH12	67	0.11%	2.51%	SAFU17	3	0.00%	4.76%
SH13	55	0.09%	2.60%	SAFU18e	2	0.00%	4.76%
SH14	35	0.06%	2.66%	SAFU19	2	0.00%	4.77%
SH15	390	0.62%	3.28%	SAFU23	1	0.00%	4.77%
SA1	174	0.28%	3.56%	SAFU24	1	0.00%	4.77%
OB1h	1	0.00%	3.56%	SAFU30g	1	0.00%	4.77%
SA2	105	0.17%	3.73%	SAFU32	3	0.00%	4.78%
OB2g	1	0.00%	3.73%	SAFU33a	1	0.00%	4.78%
SA3	102	0.16%	3.90%	SAFU33d	1	0.00%	4.78%
SA4	87	0.14%	4.04%	SAFU33b_1	1	0.00%	4.78%
PF4a	4	0.01%	4.04%	SAFU34	2	0.00%	4.79%
OB4c	1	0.00%	4.04%	SAFU36	16	0.03%	4.81%
OB4d	1	0.00%	4.05%	SAFU37e	1	0.00%	4.81%
OB4f	1	0.00%	4.05%	SAFU38a	7	0.01%	4.82%

Table 9.2—Continued

Item	Count	Percentage	Cumulative Percentage
SAFU38b	3	0.00%	4.83%
SAFU38d	1	0.00%	4.83%
SAFU38e	126	0.20%	5.03%
SAFU39	2	0.00%	5.04%
SAFU40	2	0.00%	5.04%
DEMO1	85	0.14%	5.17%
RGSF1	14	0.02%	5.20%
DEMO2	442	0.71%	5.91%
DEMO3	58,750	94.09%	100.00%

NOTE: Percentages are given among the proportion of the sample that answered at least one question (N = 62,437), including the reserve components and Coast Guard. Item labels in bold were administered to all respondents. Some survey items are not presented in this table because they were not the final survey item for any participant. The instrument is included in Volume 1 of this report series.

Table 9.3
Survey Breakoff by Module, RAND Combined and RAND Short Form

Module	Final Response at or After:	But Final Response Before:	Combined	Short Only
Time-setting module	INTRO1	P5	0.79%	0.79%
MEO screeners	P5	SH15	1.92%	1.87%
MEO follow-up	SH15	SHFU12x	1.16%	NA
Sexual assault classification ^a	SHFU12x	SA6	0.91%	1.53%
Sexual assault follow-up	SA6	SAFU37e	0.63%	0.62%
Lifetime sexual assault	SAFU37e	SAFU38e	0.25%	0.22%
Demographic	SAFU38e	DEMO3	0.79%	0.87%
Total	INTRO1	DEMO3	6.45%	5.91%

NOTE: Percentages are given among the proportion of the entire sample that answered at least one survey question, including the reserve components and Coast Guard. The combined column aggregates across the short, medium, and long forms. Breakoffs for the MEO follow-up module are not applicable (NA) for the short form, because that module does not appear in the short form.

^a The short form goes directly from SH15 to SA1, so breakoff in the sexual assault classification module is defined for the short form as having a final response on or after SH15 but before SA6.

Table 9.3 also presents breakoff analysis within the short form respondents to better estimate the number of individuals who broke off at the beginning of the sexual assault classification module. In the medium and long forms, there are many different items that may immediately precede the instructions for the sexual assault module. In the short form, all respondents were presented with SH15 immediately before the instructions for the sexual assault module. For the short form, breakoff within the sexual assault classification model is defined by those respondents with a final survey response occurring on or after SH15 but before SA6. The rate of breakoff associated with the sexual assault classification module was 1.5 percent of the short form sample who started the survey. This rate of breakoff is generally similar to other modules in the survey. The sexual assault classification module contained between six and 70 questions, depending on skips, but had a slightly lower rate of breakoff than the MEO screening module (15–17 questions). On the other hand, the breakoff rate in the sexual assault classification module was slightly higher than the two shorter modules that contained items presented to everyone, the five time-setting questions, and the three to four demographic questions. As seen in Table 9.2, however, SH15—the last item in the short form’s MEO screener module—was the single item most likely to be the final response for short-form respondents. There were two screens of survey instructions after SH15 and before respondents get to the next question (SA1). An inspection of web

server logs indicated that a large proportion of those who dropped out after completing SH15 but before completing SA1 do so on those instruction screens. Such breakoff may be a response to the subject matter, which is revealed in the instructions, rather than the content of SA1 itself.

Prior Form Instrument

The 2012 WGRA instrument had substantial break off (DMDC, 2014, Appendix B). In 2012, 13.9 percent of those who started the survey gave their final survey response before reaching the critical unwanted sexual contact assessment, and 18.5 percent broke off at some point prior to the final question presented to all participants. In particular, survey items that had several response options or were presented in a grid format were associated with substantial survey breakoff in 2012. For example, 3.7 percent of the respondents broke off during the posttraumatic stress disorder (PTSD) checklist (Weathers et al., 1993), 1.0 percent broke off during the Cohen Perceived Stress scale. As discussed in Volume 1 of this series, the prior form developed by RAND substantially shortened the WGRA instrument used in previous surveys to minimize breakoff. When items that presented a substantial response burden were not necessary for our research goals, we eliminated them from the instrument.

Table 9.4 presents the number of individuals on the prior form for whom a particular item was their final completed item. RAND's prior form had substantially lower rates of breakoff than the 2012 WGRA instrument, with 6.5 percent cumulative breakoff prior to the last item presented to all participants (the unwanted sexual contact assessment, PF32). The biggest single dropping source of breakoff was the gender discrimination scale (PF27) with 1.5 percent dropping out in that module. The rate of breakoff within the gender discrimination scale in the 2012 WGRA was also 1.5 percent (DMDC, 2014, Appendix B).

Effect of Survey Breakoff on Sample Characteristics

While survey breakoff represents a very small portion of the overall survey nonresponse, because this type of nonresponse is directly informed by—or is a reaction to—the content of the survey, it can represent a source of bias that is not well mitigated by survey weights or other nonresponse corrections. Even though the proportion of the sample that broke off the survey was substantially lower for the RMWS than for prior administrations of the WGRA, it is still possible that the breakoff represents a meaningful source of bias in study estimates. This is particularly true when estimating rare outcomes, such as sexual assault, that occur in less than 2 percent of the population being studied.

To better understand the role played by this type of nonresponse, it is useful to investigate how the sample characteristics were affected by survey breakoff. For exam-

Table 9.4
Final Participant Response by Survey Item, Prior Form

Item	Count	Percentage	Cumulative Percentage	Item	Count	Percentage	Cumulative Percentage
PF2	125	0.40%	0.40%	PF27_10	2	0.01%	4.40%
PF3	65	0.21%	0.60%	PF27_12	1	0.00%	4.40%
PF4_1	14	0.04%	0.65%	PF27_13	139	0.44%	4.84%
PF4_2	14	0.04%	0.69%	PF28	2	0.01%	4.85%
PF4_4	4	0.01%	0.70%	PF29_4	10	0.03%	4.88%
PF6	67	0.21%	0.91%	PF29_5	172	0.54%	5.42%
PF7	46	0.15%	1.06%	PF30_1	2	0.01%	5.43%
PF8	27	0.09%	1.14%	PF30_2	9	0.03%	5.46%
PF9	13	0.04%	1.19%	PF30_3	4	0.01%	5.47%
PF10	181	0.57%	1.76%	PF30_4	2	0.01%	5.48%
PF11_1	3	0.01%	1.77%	PF30_6	5	0.02%	5.49%
PF11_2	5	0.02%	1.78%	PF30_8	1	0.00%	5.49%
PF11_3	1	0.00%	1.79%	PF30_9	1	0.00%	5.50%
PF11_4	3	0.01%	1.80%	PF30_10	136	0.43%	5.93%
PF11_5	1	0.00%	1.80%	PF30_11	3	0.01%	5.94%
PF11_6	68	0.22%	2.01%	PF30_13	2	0.01%	5.94%
PF17	20	0.06%	2.08%	PF30_14	2	0.01%	5.95%
PF18	226	0.71%	2.79%	PF30_19	141	0.45%	6.40%
PF19_1	6	0.02%	2.81%	PF31	33	0.10%	6.50%
PF19_2	9	0.03%	2.84%	PF32	28,952	91.57%	98.07%
PF19_3	3	0.01%	2.85%	PF33	23	0.07%	98.15%
PF19_4	1	0.00%	2.85%	PF34_3	1	0.00%	98.15%
PF19_7	2	0.01%	2.86%	PF34_5	8	0.03%	98.17%
PF19_9	194	0.61%	3.47%	PF35_7	1	0.00%	98.18%
PF27_1	5	0.02%	3.49%	PF35_9	12	0.04%	98.22%
PF27_2	7	0.02%	3.51%	PF36	1	0.00%	98.22%
PF27_3	4	0.01%	3.52%	PF37	4	0.01%	98.23%
PF27_4	6	0.02%	3.54%	PF38_10	8	0.03%	98.26%
PF27_5	1	0.00%	3.55%	PF40	2	0.01%	98.26%
PF27_6	5	0.02%	3.56%	PF41	4	0.01%	98.28%
PF27_7	256	0.81%	4.37%	PF42_3	1	0.00%	98.28%
PF27_8	4	0.01%	4.38%	PF44_3	7	0.02%	98.30%
PF27_9	2	0.01%	4.39%	PF46	9	0.03%	98.33%

Table 9.4—Continued

Item	Count	Percentage	Cumulative Percentage
PF47	1	0.00%	98.33%
PF48_10	3	0.01%	98.34%
PF59_4	3	0.01%	98.35%
PF60_7	2	0.01%	98.36%
PF69_14	2	0.01%	98.36%
PF72_15	7	0.02%	98.39%
PF73	6	0.02%	98.41%
SAFU25	504	1.59%	100.00%

NOTE: Percentages are given among the proportion of the sample that answered at least one question (N = 31,616), all are active-component DoD service members. Item labels in bold were administered to all respondents. Some survey items are not presented in this table because they were not the final survey item for any participant. The instrument is included in Volume 1 of this series.

ple, if the participants who break off early are at unusually low risk for sexual assault (e.g., male, Air Force, senior officers), it supports a theory suggesting that nonresponse occurs because the topic of the survey is not personally relevant to respondents, it is not seen as important, or it is considered objectionable. On the other hand, if survey break-off is associated with characteristics that put the respondent at risk for sexual assault (e.g., female, Marine Corps, junior enlisted), it supports a theory that nonresponse occurs because those individuals who have personal experiences with, or knowledge of, sexual assaults do not want to disclose that information on the survey. These scenarios have opposite implications for the likelihood of non-ignorable missingness—i.e., missingness attributable to respondents' reactions to the specific topic of the survey—that is not well captured in our nonresponse weights. Alternatively, breakoff may occur in a way that is unassociated with sexual assault risk, which would suggest relatively minimal nonresponse bias.

To investigate this issue, we estimated the risk for sexual assault for every service member in the sample based on all available administrative data available. Specifically, we used the “composite variables” that had been derived as part of RMWS weights. These are predicted risks for sexual assault based on a regression model that predicted sexual assault from a large list of variables (see Volume 1, Table 5.3) among survey respondents. These predicted values were computed on both respondents and nonrespondents, including those who started the survey but dropped out before answering the sexual assault questions.

As discussed in Chapter Three, individuals whose characteristics put them at a low propensity to respond to the survey also put them at high risk for sexual assault. The current analyses investigate if that association occurs because high-risk individuals never hit the survey web page (and possibly never got the invitation), refused to participate when informed about the content of the survey, or began to participate but broke off when they got to detailed questions about sexual assault and harassment. The data in Table 9.5 track the average predicted risk for sexual assault for different subsets of the sample, beginning with the full sample, moving to the portion of the sample who hit the web page, to those who started the survey, and finally to those who finished the sexual assault classification module (and were counted as respondents). This predicted risk estimate is based on the full range of sample characteristics investigated during derivation of RMWS sample weights. It is based on the sum of the three subtypes of sexual assault risk that were derived as part of those weights (see Chapter Three).

The predicted risk of sexual assault for the sample shifted substantially between the full sample invited to participate and the subsample that hit the survey start page. This step accounted for the bulk of nonresponse, more than 300,000 cases, and nearly the entire shift in predicted risk. The full, unweighted sample had a mean predicted

Table 9.5
Predicted Risk of Sexual Assault by the Type of Nonresponse or Breakoff

Subsample	Sample Size	Mean Risk	Loss of N	Change in Mean Risk	Mean Risk for Dropouts
The full sample invited to RAND form	459,279	2.13%			
... who hit the survey start page	156,130	1.81%	303,149	0.32%	2.30%
... who answered at least one survey question	146,986	1.81%	9,144	0.01%	1.94%
... who started MEO screeners	144,775	1.80%	2,211	0.00%	1.98%
... who finished MEO screeners	143,003	1.80%	1,772	0.00%	2.05%
... who started sexual assault module	141,291	1.79%	1,712	0.01%	2.45%
... who finished sexual assault classification	139,968	1.79%	1,323	0.00%	1.66%

NOTES: Samples including the active and reserve components, DoD, and Coast Guard. Risk of sexual assault is based on a regression model including all predictors in Volume 1, Table 5.3. Data are unweighted.

risk of sexual assault of 2.1 percent,¹ but those who hit the survey start page had a risk of 1.8 percent, a rate that is 15 percent lower. The sampled service members who dropped out of the sample at this stage had a mean risk of 2.3 percent, i.e., they were at higher risk for sexual assault than the overall sample average.

The change in predicted risk for sexual assault after that point was minimal. This was both because fewer individuals dropped out at those stages, and those who did leave tended to be similar in their risk of sexual assault to the subsample who hit the survey web page. Nonconsent (i.e., arriving at the start page but not answering any questions) shifted the sample to have slightly lower risk, but only by 0.01 percentage points. This was because the predicted risk for those who did not consent (1.9 percent) was slightly higher than the subsample that hit the survey start page. On the other hand, those who did not consent were actually at slightly lower risk for sexual assault than the overall sample. This pattern, in which those who broke off the survey were at slightly higher risk than those who started the survey but were at slightly lower risk than the full population, continued until the sexual harassment follow-up module (between the end of the MEO screeners and the beginning of the sexual assault module). These follow-up questions were only given to those respondents who indicated experiencing one of the sexual harassment or gender discrimination screeners. Such individuals have high risk of sexual assault, so the individuals who broke off in the MEO follow-up module had characteristics that put them at higher risk than either the overall sample or the

¹ This value differs slightly from the overall study estimate of sexual assault risk because those estimates were for the full population, while this estimate is for the full sample. Because the sampling plan included an oversample of women, these estimates of sexual assault risk are slightly higher than the study estimates for the population.

subsample that completed the MEO screeners. This resulted in another small shift in the sample of 0.01 percentage points.²

Breakoff within the sexual assault classification module also had no meaningful effect on the predicted risk of the remaining sample. Those who broke off in the module had a predicted sexual assault risk of 1.7 percent, which is actually lower than the full sample (2.1 percent) and very slightly lower than the subsample that started the module (1.8 percent). In other words, the service members who quit the survey during the sexual assault assessment were slightly more likely to come from low-risk groups (men, officers, etc.) than high-risk groups (women, enlisted, etc.), a response bias that is in the opposite direction of the overall effect on nonresponse.

In summary, the shift in predicted risk of sexual assault between the overall sample being targeted and the survey respondents occurred almost entirely before the service members arrived at the study web page, and before they had been presented with detailed information about the content of the survey. Once they got to the survey start page, the remaining nonresponse (caused by either survey nonconsent or breakoff) was not meaningfully associated with those respondent characteristics that put them at risk for sexual assault. Combing across survey nonconsent and survey breakoff, the predicted risk for these nonrespondents is quite similar to the original sample, with a mean risk of sexual assault of 2.0 percent. Thus, there is little evidence that the net nonresponse bias we observe overall was produced as a reaction to the content of the survey.

Conclusions

During the survey design phase, the study team attempted to design an instrument that minimized nonresponse due to survey breakoff. This included moving *base* survey items to the front of the instrument, reducing instrument length (for as many respondents as possible), avoiding questions with complex response options, and avoiding complex question wording. This resulted in generally low rates of survey breakoff, less than 4 percent broke off before the mandatory sexual assault items. This compares favorably with the 2012 WGRA instrument in which 13.9 percent broke off before the mandatory item assessing unwanted sexual contact.

² The decision to include the detailed sexual harassment and gender discrimination follow-up questions before the sexual assault assessment involved several trade-offs. Because those follow-up questions provided more opportunities for respondents who experienced these MEO violations to drop out of the survey before the required sexual assault questions, we were concerned that it could result in meaningful survey bias. To mitigate this, we randomized a large portion of the sample, the RAND short form, to not get those follow-up questions. We can empirically investigate whether including the MEO follow-up module resulted in downward bias in the sexual assault prevalence estimates by comparing estimates across forms. The rates of sexual assault estimated in the forms that included the MEO follow-up questions were actually slightly higher than, but not significantly different from, the rates estimated in the short form. There does not appear to be a net bias introduced by inclusion of the MEO follow-up questions before the sexual assault questions.

Overall, our analysis of the rates of survey nonconsent and survey breakoff, as well as their effects on the sample characteristics, suggests that there is little evidence that overall nonresponse was a reaction to the survey content. Only a small proportion of study nonrespondents dropped out after being directly informed about survey content or seeing the survey questions, and those who dropped out after that point did not, on average, have a higher or lower predicted risk for sexual assault. The difference in predicted sexual assault risk between the respondents and the intended sample was driven by the sampled service members who either did not receive or did not respond to the mail and emailed invitations.

Service Member Tolerance of the RAND Form

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The RAND form used behavioral descriptions of forms of physical contact that can qualify as sexual assault under UCMJ Article 120. Some of these descriptions were more explicit than had been used in earlier WGRA surveys, though they are comparable to the behaviorally specific language found in many surveys of the general population (e.g., the National Intimate Partner and Sexual Violence Survey conducted by the Centers for Disease Control and Prevention), and surveys of special populations, such as college students.¹

The use of behaviorally specific language poses two known risks: some who take the survey may be offended by its content, and some may find that the language triggers upsetting memories and feelings. Because these are known risks, respondents were advised during the informed-consent process to take them into consideration in deciding whether to participate in the study. Nevertheless, some participants were offended or upset by the survey, either because they regarded the language used in the sexual assault screening items as unnecessarily graphic, too intrusive, or because it triggered disturbing memories or could trigger such memories in others.²

Given the unprecedented size of the sample (more than one-half of a million service members were invited to participate, and close to 200,000 accessed the online survey), the complaints about the survey raised three key questions. First, was the rate of respondents who complained about the survey language unusually or unacceptably high? Second, did the survey language harm some victims of sexual assault or discourage their participation? Third, were risks to participants sufficiently great to outweigh the value of scientific knowledge gained by using questions with clear, explicit language? In this chapter, we examine these questions using data RAND collected on all survey complaints filed with RAND, Westat (the organization that fielded the survey and supplied survey helpdesk operators), or to an office at DoD when we were informed about those complaints.

¹ A good discussion of methods for measuring sexual assault that reviews many survey instruments is found in National Research Council, 2014.

² RAND also received other types of complaints that are common to most survey efforts, such as complaints about being contacted at home or at work, being contacted too many times, wasting government money, etc. In this chapter, we focus just on those complaints that concerned the survey language.

Complaint Rates

There is little objective or standardized evidence on the rates of complaints in other surveys. Moreover, definitions of what constitutes a complaint differ from survey to survey. According to DMDC, which administered the 2012 WGRA, there were six complaints about the survey instrument itself: five that it was too long and one that it was too intrusive (DMDC, 2015). Out of 22,792 completed WGRA surveys in 2012, these six complaints represented a complaint rate of 27 per 100,000 completed surveys.

In 2014, RAND fielded a shortened version of the 2012 WGRA form, the prior form, to a sample of respondents, and three versions of the RAND form to others. Although it was not always possible to determine which form complaints referred to, we know with certainty that two of 149 survey-language complaints concerned the prior form, for a complaint rate of seven per 100,000 completes (Table 10.1). This is significantly lower than the 141 complaints attributable to the RAND form, which corresponds to a complaint rate of 122 per 100,000 completes.

Table 10.1
Complaints Received About Survey Language in the RAND
Survey, by Respondent and Survey Characteristics (When Known)

	Complaints per 100,000 Completes	95% CI
By form		
Prior form	7	(1–24)
RAND form	122	(103–144)
By gender		
Men	125	(102–153)
Women	83	(63–108)
By service		
Army	119	(91–152)
Navy	65	(38–102)
Air Force	90	(66–119)
Marine Corps	51	(19–111)
By pay grade		
E1–E4	43	(25–70)
E5–E9	131	(106–161)
O1–O3	42	(19–79)
O4–O6	200	(138–279)

Men had a statistically significantly higher complaint rate than women, and junior enlisted and junior officers had significantly lower complaint rates than senior enlisted and senior officers. Interestingly, therefore, those at the highest risk of sexual assault tended to complain least, whereas those at the lowest risk of sexual assault were most likely to find the survey language objectionable:

- Women have five times the risk of sexual assault as men, but their complaint rate was one-third lower than men's.
- Service members in ranks E1–E4 have more than twice the risk of sexual assault as those in ranks E5–E9, but their complaint rate was one-third that of E5–E9 members.
- Junior officers (O1–O3) have twice the risk of sexual assault as more-senior officers, but senior officers' complaint rate was nearly five times that of junior officers.

Although the propensity for complaint appears associated with risk for sexual assault, this may not represent a causal association. It may be, for example, that more-senior personnel were more likely to lodge complaints because they were passing along concerns that had been brought to their attention by the individuals they lead.

Harm to Victims

Behaviorally specific questions can trigger painful memories of attacks, leading to distress among victims. While research on such reactions is not extensive, several studies suggest that survey-induced distress is usually short-lived. Across several studies, the number of respondents who reported being upset during administration of surveys on sexual assault and other traumatic experiences is 4.5 percent to 13 percent (Finkelhor et al., 2014; Galea et al., 2005; Kilpatrick et al., 2007; Zajac et al., 2011). Moreover, most who experience distress while answering survey questions no longer feel distressed after completing the survey (Galea et al., 2005; Kilpatrick et al., 2007; Zajac et al., 2011). For instance, the National Women's Study–Replication (Kilpatrick et al., 2007), funded by the National Institute of Justice, conducted a victimization survey of 3,001 adult women from the U.S. general population and a national sample of 2,000 U.S. college women. In addition to screening for rape experiences using questions similar to those used in the RAND survey, women were asked about exposure to other traumatic events, alcohol and drug use, PTSD, and depression. In both samples, 7.6 percent of women said they were upset by any of the survey questions. Just one-half of 1 percent, or 26 out of 5,001 respondents in both samples, said they were still upset at the end of the interview, ten of whom were sufficiently upset to accept an offer to speak with a counselor. None felt they needed to talk to a counselor immediately.

The same pattern of findings was obtained in the National Survey of Adolescents—Replication project, funded by the National Institute of Child Health and Development (Zajac et al., 2011). This project surveyed 12- to 17-year-old adolescent males and females about their experiences with physical and sexual assault, witnessed violence, exposure to other traumatic events, alcohol and drug use, PTSD, depression, and suicidal ideation. Out of 3,614 adolescents, only eight remained upset at the end of the interview, and only two wished to speak to a counselor. Both of these studies found that individuals with traumatic-event histories were slightly more likely to experience distress, but the overwhelming majority was not distressed by the end of the survey interview. In summary, these findings are consistent with other research that demonstrates asking behaviorally specific questions about rape, sexual assault, and other traumatic events produces distress in only a small percentage of respondents and that even this transitory distress has dissipated by the end of the interview, except in a fraction of a percent out of thousands of respondents surveyed.

Twelve respondents contacted RAND or others to express the concern that victims of sexual assault would find the survey too painful to complete, making the survey invalid for estimating the prevalence of sexual assault and possibly harmful to victims of sexual assault. Of these 12 contacts, three indicated that they themselves had been assaulted, and discontinued their participation in the survey because the questions brought up painful feelings or memories. None of these self-identified victims indicated that they experienced any lasting distress from the survey.³ Clearly, if their experiences were typical of sexual assault victims, this would provide compelling evidence that the survey was too distressing to achieve its objectives. However, many others with sexual assault histories were able to complete the survey. Indeed, 12,210 service members with sexual assault histories completed the survey.

Some who complained about the survey language may not have mentioned that they were sexual assault victims. Nevertheless, considering just the three known sexual assault victims who complained out of the 12,210 who completed the survey, their complaint rate of 25 per 100,000 would be substantially lower than the overall complaint rate. Again, this may reflect a greater tolerance of questions concerning the details of sexual assaults among groups of service members with the highest exposure to this crime.

Others with sexual assault histories appreciate the opportunity to share their experiences and have them considered when DoD investigates the prevalence of sexual assault in the military. Indeed, sometimes questions about crime victimization can be distressing and appreciated. Notably, more self-identified victims (four) contacted RAND to express appreciation for the survey or in order to provide additional information on their experiences than to object to the language used in the survey.

³ Each of these reports was reviewed by RAND's and DoD's human subjects protection committees, which determined they represented expected risks of survey participation.

Benefits of the New RAND Survey Using Explicit Questions to Measure Sexual Assault

When research poses any risk to participants, it is critically important that the expected benefits of the research outweigh the potential risks. This raises the question of whether there were any benefits to individual research participants as well as to society of using the new RAND survey and its more-explicit questions that appear to produce more distress among participants than the earlier WGRA questions.

One potential benefit to service members, particularly those with sexual assault histories, is the validation that comes from DoD taking the issue sufficiently seriously to measure sexual assault in an unambiguous way, so that their experiences cannot be discounted. The public discounting of the seriousness of the unwanted sexual contacts reported on the prior measure (due to the ambiguities in that item) contributes to a perception that victims' reports of their own experiences on a confidential survey are not reliable. Relatedly, some with sexual assault histories will appreciate the opportunity to disclose their experiences to DoD through a mechanism that preserves their anonymity. Others without self-disclosed sexual assault experiences contacted us to express gratitude that the survey was being conducted, suggesting that some service members recognized the benefits to their own and their colleagues' work lives that could result from DoD gathering better data on sexual assault and sexual harassment in the military.

Conceivably, however, all of the benefits described above could have been achieved using survey language that offended or upset fewer service members. To what extent, therefore, was the use of questions with more-explicit language necessary to improve the quality of sexual assault prevalence research? As noted above, there is scientific consensus that using clear, explicit questions is essential for good sexual assault prevalence research (National Research Council, 2014). Past surveys using less-precise language produced results that raised questions among members of Congress and others charged with creating and implementing sexual assault policies that distracted from efforts to improve prevention and treatment programs, rather than supported it (see discussion in Volume 1, Chapter 1). Data cited elsewhere in this report provide evidence that the new RAND questions yield more-specific and sensitive sexual assault prevalence data than was obtained using the prior form (Chapters Five and Eight). Therefore, the research using more-explicit questions had a clear benefit to DoD because the sexual assault data obtained were much more accurate.

Conclusions

The RAND form was more likely to trigger survey language complaints than the prior form. Recognizing that many who were offended or upset by the language in the survey will not file a complaint, the true rate at which respondents were offended or disturbed is undoubtedly higher than the calculated rate of 122 complaints per

100,000 completes. Unfortunately, we know of no comparable data from civilian surveys of sexual assault—most of which use similarly specific behavioral and anatomical language—with which to evaluate whether this rate of complaints is especially high. We can, however, say that the offense was not sufficiently severe or widespread to cause a surge of breakoffs during the sexual assault screening module where the language drawing complaints occurred. As shown in Chapter Nine, more respondents broke off in the uncontroversial sexual harassment screening module than in the sexual assault screening module, and breakoffs overall were similar for the combined RAND forms and prior form (which drew few language complaints). Therefore, we believe that the fact that one out of every 820 who completed the survey complained about the language indicates an overall level of distress or offense was not excessive for a survey addressing such sensitive and uncomfortable topics.

Human research participant protection regulations do not require research to be entirely risk-free, as that would be impossible. Instead, these regulations require an analysis of whether risks posed by the research are outweighed by potential benefits. As described above, the RAND form had potential benefits both to individual research participants as well as to scientific knowledge about the prevalence and nature of sexual assault in the military. Ultimately, however, the decision as to whether the complaint rate—or distress generally—is too high rests on a judgment about the benefits of asking questions in the manner that triggers more complaints relative to the approach that triggers fewer. Our view is that obtaining the most-accurate data possible on the number and proportion of service members who are sexually assaulted each year is critical for sound policy. This enables DoD to determine how well DoD policy initiatives are preventing sexual assault and whether the existing military justice system and health care service response systems are adequate.

The fact that participants at the lowest risk for sexual assault tended to object to the survey language at the highest rates is a surprising finding worthy of further investigation. Possibly, the risk of assault feels so remote to these respondents that the minor inconvenience of being asked questions about whether they themselves have experienced such violations outweighs any benefits they can imagine the survey producing. Alternatively, perhaps men, more-senior pay grades, or officers are more likely than others to express their complaints about any topic, in which case their higher complaint rates would have nothing to do with their lower risks of sexual assault or harassment.

As has been recommended by the White House, fears of victims being harmed by survey questions should not deter efforts to understand and enumerate sexual assault crimes (White House Task Force to Protect Students from Sexual Assault, 2014). In the RMWS study, as in others, there is no evidence that the offense or distress caused any lasting harm to survey respondents. Moreover, the offense some take when reading descriptions of unwanted sexual encounters represents a tolerable risk when respondents are notified of the risk during their informed consent to participate in the research.

Conclusions and Recommendations for Future Administrations of the WGRA

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The 2014 RMWS was the largest study ever conducted to examine sexual assault and sexual harassment in the U.S. military. With nearly 170,000 survey respondents, the prevalence estimates generated from the study frequently had 95-percent confidence intervals that spanned less than half a percentage point, suggesting extraordinary precision in our estimates. But these confidence intervals, which only assess the uncertainty due to random sampling variability, could be misleading. Sampling variability is unlikely to be the primary source of error for our estimates. Instead, larger errors could result from several factors, including specification errors if, for instance, our sexual assault screening module misclassifies individuals; coverage errors due to the inclusion criteria used in the sample frame; and survey nonresponse, if our sample weights failed to fully adjust for important differences between those who chose to participate in the study and those who did not.

This volume examined the influence and magnitude of these less-easily quantified sources of error. Our investigations across all of these sources found no conclusive evidence of substantial bias or error in the primary RMWS estimates. However, there was a general pattern across these investigations, suggesting that our primary RMWS study estimates of sexual assault, sexual harassment, and gender discrimination are more likely to be underestimates than to be overestimates of the true values. In particular, three types of evidence suggest that the survey estimates could underestimate the true values, but any such underestimate is likely to be relatively small: (1) the non-response follow-up studies, (2) the analysis of individuals excluded from the sample frame, and (3) the comparison between survey estimates of officially reported sexual assaults to the number of actual reports.

In contrast, we found little evidence that the study was overcounting these outcomes. For example, although we conclude that a small number of pre-service sexual assaults may be captured in our estimates, this number is almost certainly lower than the larger number of assaults that go uncounted because we excluded members with fewer than six months of service, and those that left the military shortly before the survey fielded. Similarly, our analyses of the performance of the sexual assault and sexual harassment items provide no indication that more incidents were counted as crimes or violations than should have been.

Our conclusion that the study is more likely to underestimate than to overestimate the true values is stronger for the estimated counts of individuals who experienced these violations (e.g., 20,300 service members experienced a sexual assault in the past year) than for the estimated prevalence of these crimes (e.g., 1.5 percent of service members experienced a sexual assault in the past year). This is because the strongest evidence for bias comes from the fact that the survey sample frame clearly excluded some individuals who served in the military in the past year and who may have experienced these outcomes (e.g., members who separated before the sample was drawn). This source of bias may explain a substantial proportion of the total survey error identified by our comparison of survey-estimated counts of reported sexual assault to official reports of sexual assault (see Chapter Four). In contrast, evidence of bias in the estimated prevalence of sexual assault, sexual harassment, and sexual discrimination is weaker; the incomplete coverage of the sample frame necessarily has smaller effects on prevalence rates than on population counts. The three nonresponse follow-up studies (Chapter Two) provide some limited evidence the reported prevalence underestimates the true value. However, those effects were descriptively small, were not consistent across follow-up methods, and were not uniformly statistically significant.

In addition to the primary RMWS estimates, the RAND study also replicated the methods used in previous WGRA studies to produce time-trend data using the same measurement and weighting methods. The current investigations of bias provide stronger evidence that the WGRA methods underestimate the true rate of sexual assault. In particular, the analysis of nonresponse weights found that the WGRA system of weights resulted in the underrepresentation of a number of groups of service members who have a high risk for sexual assault and harassment. In addition, the prior form identified substantially fewer penetrative sexual assaults than the RAND form, particularly among men. However, this classification error was partially offset by telescoping errors, resulting in a substantial proportion of respondents being counted as experiencing an unwanted sexual contact in the past year, when in fact their last such experience occurred more than 12 months prior to the survey.

In the sections that follow, we discuss specific findings from this volume, and make recommendations for how future administrations of the WGRA might benefit from what we learned from the 2014 RMWS experience. Whereas most of these recommendations derive from findings reported in this volume, we also offer recommendations based on our experience conducting the 2014 RMWS.

Measurement Approach

Evidence provided in this volume demonstrates that the RAND instrument more accurately counted sexual assault crimes and MEO violations than the method previously used in the WGRA. For the measurement of sexual assault, the RAND form

counted as past-year crimes fewer events that actually occurred more than a year ago; it identified a large number of sexual assaults that were abusive, demeaning, or acts of hazing that appear to be missed by the earlier approach; the language and criteria used in the RAND form were more interpretable as crimes under the UCMJ; and the RAND questions used more-descriptive and unambiguous anatomical and behavioral language, which has been identified as a survey best practice for the measurement of sexual assault crimes (National Research Council, 2014).

The principal trade-off in implementing this best practice is that the more-precise language used to describe sexual assaults offends more people than the language previously used to describe “unwanted sexual contacts” (see Chapter Ten). Unfortunately, although the “unwanted sexual contacts” question offended fewer people, it was not specific enough to ensure that respondents understood the range of events that counted as criminal offenses. Moreover, as we have argued in Chapters Five and Eight, some evidence suggests that its reliance on undefined phrases like “oral sex” and “anal sex,” may have led to an undercount of sexual assaults that were experienced by the respondent as abusive or humiliating rather than as sexual acts. Because terms like these can be confusing or misleading, the RAND survey defined sexual assaults using the same kind of specific behavioral and anatomical language used in the UCMJ. This is also similar to most civilian surveys of sexual assault that follow the guidance recently offered by the National Research Council recommending the use of specific behavioral and anatomical language for the measure of sexual assault (National Research Council, 2014). Although there is evidence that the RAND questions generated more complaints, the actual rate of complaints was quite low, and did not appear to be a significant cause of survey breakoff (Chapter Nine). More people broke off from the RAND form during the uncontroversial sexual harassment screening than during the sexual assault screening, and breakoffs before completing the sexual assault/unwanted sexual contact measure were generally lower for the RAND form compared with the prior form.

For sexual harassment, the prior approach required service members to understand the nuances of MEO regulations and correctly apply them by labeling their unwanted workplace experiences “sexual harassment.” As shown in Chapter Eight, this labeling requirement can lead to a large undercount of sexual harassment. Had we required respondents to correctly label their sexual harassment experiences on the RMWS, we would have undercounted the prevalence of sexual harassment among women by 30 percent, and among men by 50 percent. Finally, the RMWS measurement of hostile work environment is more closely aligned to the language and criteria found in military (and civilian) equal opportunity regulations.

Gender discrimination is more difficult to assess, and the RMWS has some limitations that may be addressed with further revisions. To classify as discriminatory, comments or experiences must result in damage to the service member’s career, a causal attribution that respondents may not have sufficient information to be able to

make accurately. Moreover, at the time we conducted the survey, there were careers that women were officially barred from entering in the military. Some respondents may have been told that women were unqualified for those jobs, and may have felt the individual who expressed that opinion harmed their careers. The RMWS measure of gender discrimination may not fully distinguish between this type of legal gender discrimination and the unlawful form.

For these reasons, we believe the RMWS measurement approach more accurately captures UCMJ crimes and MEO violations than earlier WGRA measures. In addition, as we indicate in Chapter Ten, the offense or distress experienced by some service members asked to consider the RAND questions is a tolerable risk given the benefits of increased precision in prevalence estimates that are highly policy relevant. However, additional questions may be helpful to clarify events classified as gender discrimination.

Recommendation: Future WGRA surveys should use the RMWS measurement approach, or comparable survey questions that use behaviorally and anatomically specific language to clearly define criminal sexual assault and violations of equal opportunity law and policy.

Recommendation: In future WGRA surveys, DMDC should consider supplementing the RMWS measure of gender discrimination with additional questions to establish (a) whether the discrimination was legally mandated by the service, (b) the specific nature of the career harm suffered, and (c) the evidence that gender biases harmed the service member's career.

Sample Frame

The sample frame used in the 2014 RMWS and earlier WGRA surveys excluded service members with fewer than six months of service and included some whose past-year sexual assaults could have occurred before they joined the military (service members with six to 12 months of service). Although these inclusion criteria could lead to the exclusion of sexual assaults that should have been included in our prevalence estimates, or the inclusion of sexual assaults that should not have been counted, we showed in Chapter Eight that the magnitude of these errors is small, so they have minimal effects on our sexual assault prevalence estimates.

In contrast, we found that the exclusion of some service members who left the military in the year prior to drawing the sample could indeed have a significant effect on prevalence estimates. Under plausible assumptions, we believe that this exclusion suggests that the true rate of sexual assault could be 14 percent higher than reported. This is based on evidence in Chapter Three that members who separated from the military after the sample was drawn have higher rates of sexual assault than those who did not. Active-component women who recently separated from the military at the time of

the survey were almost twice as likely as other women to have been sexually assaulted, and men who separated were more than four times as likely as other men to have been sexually assaulted. Recent separations also experienced higher rates of sexual harassment and gender discrimination. Excluding all members who recently separated from the study creates a downward bias on the estimated number of members who experienced a sexual assault in the past year as well as the rate of such assaults.

Recommendation: Because the omission of recent separations could lead to significant bias in estimates of past-year sexual assaults, sexual harassment, and gender discrimination, we recommend including past-year separations in the sample frame of future WGRA surveys, or developing analytic approaches for estimating the number of crimes and violations those who separated experienced in the past year. Minimally, separations that occur after the WGRA sample frame is drawn should not be counted as ineligible, as has been the practice in earlier versions of the WGRA.¹

Recommendation: Because recent separations appear to have elevated risk of past year sexual assaults, sexual harassment, and gender discrimination, evaluate what effect such violations have on military careers and retention, and whether making an official report or receipt of available services reduces the separation rates of service members who have been sexually assaulted, harassed, or discriminated against.

Sampling Plan

Women face especially elevated risks of sexual assault and sexual harassment. However, the military is composed of many more men than women. As a result, more men than women experience past-year sexual assaults, and 60 percent of the sexual assault incidents in the past year occurred against men. With our large sample size of men, we were able to see for the first time that the experiences of men are quite different than those of women, and they suggest different prevention and intervention responses. Under the current sample design, the error in key estimates was much larger for men (and for the overall military) than for women, suggesting that a more efficient design would have oversampled women by a smaller factor than the current study.

Recommendation: DMDC should design future surveys to include sufficient numbers of men in the sample to ensure ongoing assessment of the nature of sexual assaults against them. In practice, this means large sample surveys that

¹ Our understanding is that any intentional sampling of fully separated personnel would require approval by the Office of Management and Budget. This regulatory requirement should not be seen as a significant barrier to implementing an improved sampling plan.

may not oversample women at rates as great as in the RMWS or previous WGRA studies. This can be done without reducing the precision of women's estimates below those of men.

Sample Weighting

The novel approach to nonresponse weighting that we developed for the 2014 RMWS solved a longstanding challenge in sample weighting for military surveys. Although the U.S. military maintains rich data on member demographics, test scores, service experiences, work environments, and other characteristics that could be associated with both survey nonresponse and risk for sexual assault or harassment, in practice relatively few of these factors have been directly accounted for in the nonresponse weighting methods used for earlier WGRA and other military surveys.

The rationale for limiting the number of such factors in weighting models is a good one: Including many variables in such models can drive up variance in the weights undermining the precision of even the largest surveys. Variance increases when factors are included in the model that have little or no association with the outcomes of interest and so can offer little or no nonresponse bias reduction. Our innovation was to use a large set of population characteristics to construct a small number of derived variables that captured the portion of variance from the larger set that was associated with the outcomes of interest, and then to use just these derived variables to supplement the factors traditionally used in WGRA nonresponse weighting models (for details on this method see Volume 1, Chapter Five).

As demonstrated in Chapter Three, this approach succeeded in reducing differences between the analytic sample and the population on a wide range of factors associated with both nonresponse and key outcomes that were not fully addressed using the conventional methods. Moreover, nearly all of the factors associated with a higher risk of sexual assault were also associated with propensity for survey nonresponse, so the exclusion of these factors resulted in the WGRA weights underestimating the true rate of sexual assault. In contrast, the RMWS weights result in an analytic sample that appears to have overall levels of risk for sexual assault, sexual harassment, and gender discrimination that are similar to the population of interest.

Finally, these reductions in bias were achieved with only modest inflation of variance in the survey estimates. Whereas the overall design effect associated with the traditional WGRA weights was 2.62, the RMWS weights produced a design effect about 40 percent larger (3.69). We would argue, however, that for a survey this large—with its already extraordinary precision—the small loss of precision associated with the increased variance is well worth the bias reduction we achieved.

Recommendation: DMDC should build on approaches developed for the RMWS to include a wider set of factors in future nonresponse weighting models than has previously been possible for military surveys like the WGRA.

Improving Response Rates

With average response rates around 30 percent, nonresponse bias poses perhaps the greatest threat to the validity of RMWS survey findings. While we present evidence that the RMWS weights do a better job of mitigating nonresponse bias than the prior weighting methods, it would be far better to have less nonresponse bias in the first place. This requires improving recruitment for underrepresented groups.

We recruited sample members using both letters and emails. However, email invitations were the key recruitment tool. Sampled members with missing email addresses had extremely low response rates while those missing mailing addresses did not. However, more than 10 percent of emails either could not be sent or were bounced back as undeliverable, and we know that the true proportion of emails that failed to reach sampled service members was higher, because many military email systems do not provide delivery failure notices for emails with incorrect addresses. This suggests that up-to-date email addresses are not available in the OSD personnel systems we used to collect contact information, even though most service members with at least six months of active-duty service have email addresses issued to them by DoD. It would be ideal if DMDC personnel records were automatically and promptly updated whenever email accounts are created or deleted from any military email system. However, military email systems are not administered by DMDC, so such a change would require the cooperation of the services and other DoD organizations that maintain email systems. Improving the coverage and reliability of email contact information in the personnel systems used for survey recruitment offers a promising approach for increasing response rates.

As has been true of other surveys of the military, our response rates were lowest among junior enlisted members (pay grades E1–E4), the members at highest risk of sexual assault and sexual harassment. These members may be disproportionately assigned to duties that do not require routine use of computers, so they may check their military email accounts only infrequently, and thereby encounter survey invitations less frequently. Many or most of these members do have smartphones on which they can access personal email accounts, and some services maintain databases where members can update their personal email addresses so as to receive work-related emails on these devices. Sending survey invitations to these personal email addresses—assuming members have volunteered their personal email information for such work-related purposes—offers another promising approach to reaching and recruiting members who are hard to recruit through DoD email accounts. Doing this, however, would require

the services to change the rules governing appropriate use of their database information (covered in their System of Record Notices) to explicitly permit sharing of contact information for official government survey invitations.

As part of our follow-up studies of nonresponse, RAND learned that phone outreach was particularly effective for recruiting groups that were underrepresented in the main survey. Although the sensitive nature of this survey makes it a poor choice for live phone administration, it may be possible to use the phone to motivate member participation in an automated interactive voice-recognition interview over the phone, or to use phone or text messages to motivate participation in the web-based survey. Such methods may improve response rates for those groups that are chronically underrepresented in DoD surveys (e.g., junior enlisted, infantry) and who do not use email as a regular part of their military duties. The feasibility of nonemail outreach methods should be investigated, as well as the most effective way to construct such messages (for example, research to determine whether a recorded voice message from the service chief is more effective than an unnamed caller).

Recommendation: OSD, the Defense Information Systems Agency, and the services should collaborate to improve the coverage and reliability of email contact information in the personnel systems used for survey recruitment.

Recommendation: DMDC should investigate additional modes of recruitment (phone or text message) that improve outreach to members who do not routinely use email as part of their military duties.

Further Study of Nonresponse Bias and Survey Error

While the contents of this volume may be highly technical, some of these investigations are critical to addressing legitimate concerns about the validity of the study. So long as future versions of this survey continue to have relatively low response rates (and so long as study estimates are shifted meaningfully by nonresponse weighting) ongoing research is needed to investigate the validity of assumptions used in creating nonresponse weights. The critical assumption is that there is no association between survey response propensity and the outcomes of interest while controlling for the factors included in the nonresponse weights. The best way to assess this critical assumption is to attempt to assess the outcomes among individuals who were treated as survey nonresponse in the primary survey estimates. Thus, consistent with Office of Management and Budget (2006) guidelines, we recommend that future administrations conduct nonresponse follow-up studies. Given the results presented in Chapter Two, we would suggest several changes relative to the way that RAND conducted the follow-up studies. First, it may be preferable to avoid using live interviewers due to evidence of response biases with that mode. Second, it would be helpful to test a web-based survey

(same mode and instrument as the main study) that used high-intensity recruitment methods.

Similarly, there is value in investigating whether the survey results correspond to the known population census of reported sexual assaults (Chapter Four). While reported sexual assaults are a small fraction of all sexual assaults, they are critical for assessing DoD policy, and it is good to validate the survey estimates against the true value. Such a comparison offers a test of total survey error, including error due to sampling variability, incomplete sample coverage, measurement specification error, nonresponse bias, and computational errors. This investigation of total survey error should be continued in future administrations. To facilitate this investigation, as well as to reduce overall bias due to a sample frame that excludes some individuals who have service in the military in the prior year, we recommend expanding the sample frame of the survey to include recently separated/retired members, or assessing this group through some other data collection effort.

Recommendation: In future administrations of the WGRA, DMDC should continue to compare survey estimates with actual numbers of filed Victim Reporting Preference forms as a measure of nonresponse bias and total survey error more generally. The procedure we used could be further refined to better match the survey's sample frame with the Victim Reporting Preference statements counted in the SAPRO database.

Frequency of WGRA Administration

Sexual assault rates are unlikely to change rapidly from year to year. As such, without enormous sample sizes, annual testing is likely to detect no significant changes in year-to-year rates, which is likely to be interpreted by many observers as “no improvement,” even though improvement may in fact be occurring. Annual surveys pose other problems too: The WGRA requires a large sample to characterize the sexual assault experiences of roughly 1 percent of the service. This means significant portions of the male and female service member population will need to be surveyed each year, driving up survey fatigue, and sensitizing the population to the survey's content and focus. Survey fatigue and sensitization risk encouraging selective survey participation and possible nonresponse bias.

Finally, the survey assesses experiences over a full year period. As such, estimates are largely insensitive to policy or programmatic changes made during the past year, because the estimates include incidents both before and after the changes. This problem is compounded by the time required to analyze and disseminate results. For example, the top-line RAND report, published in December 2014, included assaults from August 2013. Essentially, this survey is sensitive to policy interventions that were initiated approxi-

mately two years prior to the survey results being made public. Conducting the survey annually would not reduce this lag between the policies being implemented and their effects showing up in research, but it would make that lag less obvious to the reader. As such, conducting annual assessments may result in reports that are out of sync with the policy cycle, and may hamper—rather than facilitate—effective policy decisions.

Recommendation: OSD should conduct the survey no more frequently than once every two years.

Phone Survey Script

Annotated CATI Survey

[START OF SCREENER QUESTIONS]

INTRODUCTION

May I please speak to [SALUTE / NAME]?

[IF ASKED: My name is (INTERVIEWER'S NAME)]

1. SUBJECT SPEAKING/COMING TO PHONE
2. SUBJECT LIVES HERE – NEEDS APPOINTMENT
3. SUBJECT KNOWN, CANNOT BE REACHED AT THIS NUMBER
4. NEVER HEARD OF SUBJECT

[If Subject was not the person who initially answered the phone, verify identity]

Am I speaking to [SALUTE / NAME]?

- YES
- NO

[If “NO” AND SUBJECT IS NOT AVAILABLE, CLICK GO TO RESULT]

[IF YES, CLICK NEXT]

[Hello, my name is (INTERVIEWER'S NAME)].

I am calling on behalf of the 2014 RAND Military Workplace Study. We recently sent a letter saying we would be calling to conduct a short survey for the Department of Defense. This letter included an informed consent statement explaining the study and \$4 in cash. The survey takes about 7 to 12 minutes and asks whether or not you have experienced harassment, discrimination, or inappropriate sexual behavior in your military work environment.

Your participation in this survey is completely voluntary. Your decision to participate, or not, will have no effect on the benefits you receive. There is no penalty if you choose not to participate. You can withdraw from the interview at any time, and you may skip individual questions during the interview. Everyone is encouraged to participate so that the information we provide to the DoD and Congress is an accurate assessment of the military workplace environment.

RAND and Westat take many steps to keep your responses confidential. Your responses will be combined with other survey responses so that you cannot be identified in any reports.

Because some of the questions in the survey are sensitive, I want to suggest you take this call where no one else can overhear the questions. OK?

- YES
- NO
- DON'T KNOW
- REFUSED

[IF "NO", ASK SUBJECT IF HE OR SHE CAN TAKE THE CALL WHERE NO ONE WILL HEAR THE QUESTIONS. IF "NO" AGAIN, ASK IF THERE IS ANOTHER TIME WE COULD CALL WHERE THAT WOULD BE POSSIBLE]

Have you received the letter containing \$4 in cash and an informed consent statement explaining the study? [INTRO1_Letter](#)

- a. YES [GO TO SURVEY START] 1
- b. NO [CONTINUE] 2
- c. DON'T KNOW [CONTINUE] 97

I would be happy to send that to you. May I please confirm your mailing address?

Street 1:

Street 2:

City:

State:

Zip Code:

Country:

[Here, you should verify the address on file.]

- YES [Address confirmed]
- NO [Address needs to be updated]

[Please make any necessary updates then continue with the interview. Please leave the Country field blank if it's a US address.]

I need to give you a few more details about the study before we begin:

The DoD Privacy Advisory states that the Defense Manpower Data Center has provided certain information about you to allow RAND to conduct this survey. Your name and contact information have been used to send you notifications and information about this survey. The Defense Manpower Data Center has provided certain demographic information to reduce the number of questions in the survey and minimize the burden on your time. Your response and demographic data are linked by RAND to allow for a thorough analysis of the responses by demographics. RAND has not been authorized by DoD to identify or link survey response and demographic information with your name and contact information. The resulting reports will not include analysis of groupings of less than 15.

I also need to share the following information about the study. RAND is a private, nonprofit organization that conducts research and analysis to help improve public policy and decision making. RAND's research partner is Westat, an internationally known research and statistical survey organization. The DoD has funded RAND to conduct an independent assessment of the military work environment during the past year. You and other Service members, including all women and approximately 25 percent of men, are being urged to participate in order to ensure that DoD and Congress have a full understanding of Service members' experiences. The survey results will have a direct impact on training, military justice, and services that affect you and other service members.

RAND and Westat will not give the DoD information about who participated in the study, nor will RAND link your individual responses on this survey with your name or identity. RAND has also received a federal "Certificate of Confidentiality" that provides RAND with additional protection against any attempt to subpoena confidential survey records. However, the protections of a Certificate of Confidentiality are not absolute. If you tell us that a child or elderly person is being abused, or that you intend to harm yourself or someone else, the researchers may report it to the authorities. The Certificate of Confidentiality is not an endorsement of the project by the U.S. Department of Health and Human Services.

For most respondents, the survey involves no risks of participation. However, if you have ever experienced sexual harassment or assault, some questions may cause discomfort or distress. Some questions may be explicit. Therefore, you may prefer to take the survey in a private setting.

It is important to note that this survey is not a means of making a formal complaint or report that you wish to have DoD act upon. The survey will not collect the identity of any perpetrators of assault or harassment. Instead, we provide information at the end of the survey about how you can make a formal report of harassment or assault.

[Survey Start]

Do you have any questions about the study before we begin? S3

- a. YES [IF QUESTIONS ASKED, CONSULT INFORMED CONSENT STATEMENT AND FAQs; THEN CONTINUE IF THE SUBJECT AGREES TO BEGIN THE SURVEY.] 1
- b. NO [CONTINUE IF THE SUBJECT AGREES TO BEGIN THE SURVEY.] 2
- c. NOT A CONVENIENT TIME: **When would it be convenient for me to call back?** [GO TO RESULT AND FILL OUT CALLBACK FORM] 3
- d. REFUSED TO TAKE SURVEY [GO TO RESULT AND ENTER DISPOSITION CODE FOR TYPE OF REFUSAL] 98

Just to make sure you are the person I am supposed to interview, can you tell me your year of birth?

[YEAR:]

- EXACT MATCH [CLICK NEXT]
- NOT A MATCH [Thank you. We will check our records again. GO TO RESULT]
- REFUSED [GO TO RESULT]

[END OF SCREENER QUESTIONS]

Let's begin. Please answer each question thoughtfully and truthfully. This will allow us to provide an accurate picture of the different experiences of today's military members. If you prefer not to answer a specific question for any reason, just let me know.

During this interview, if you are feeling distressed, please let me know and I will provide contact information for crisis counselors who will provide you with confidential support and consultation.

[START OF CATI SURVEY]

Are you male or female? Intro1

- MALE 1
- FEMALE 2
- DON'T KNOW 97
- REFUSED 98

[Intro1 will determine wording in items—[brackets] indicate alternative forms. If Respondent does not provide gender then grab sample gender.]

Thank you. Most of this survey asks about experiences that have happened within the past 12 months. When answering these questions, please do NOT include any events that occurred before [Day_of_Week, X date].

Please try to think of any important events in your life that occurred near [X date] such as birthdays, weddings, or family activities. These events can help you remember which things happened before [X date] and which happened after as you answer the rest of the survey questions.

[PAUSE TO GIVE RESPONDENT A MOMENT TO RECALL EVENTS ONE YEAR AGO]

The following questions will help you think about your life one year ago. Please answer Yes or No to each.

[IF SERVICE MEMBER HESITATES, SAY: Let me know if you do not remember.]

1. **Do you currently live in the same house or building that you did on [X Date]? P1**
 - a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98

2. **Are you the same rank today that you were on [X Date]? P2**
 - a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98

3. **Are you in the same military occupation today as you were on [X Date]? P3**
 - a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98

4. **Were you on vacation or leave on [X Date]? P4**
 - a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98

5. **Were you married or dating someone on [X Date]? P5**
 - a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98

For the next questions, I will ask you about several things that someone from work might have done to you that were upsetting or offensive, and that happened AFTER [X date].

When I say “someone from work,” please include any person you have contact with as part of your military duties. “Someone from work” could be a supervisor, someone above or below you in rank, or a civilian employee or contractor. They could be in your unit or in other units.

These things may have occurred on-duty or off-duty, on-base or off-base. Please include them as long as the person who did them to you was someone from work.

Remember, all the information you share will be kept confidential. Please answer Yes or No for each question.

[Programming note: Use gender questions asked at the beginning of the survey to branch into parallel forms. Brackets within items show which words will be used by gender of respondent.]

6. **Since [X Date], did someone from work repeatedly tell sexual “jokes” that made you uncomfortable, angry, or upset? SH1**
 - a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98

[Programming note: Same sex as respondent]

7. **Since [X Date], did someone from work embarrass, anger, or upset you by repeatedly suggesting that you do not act like a [man/woman] is supposed**

- to?** (For example, by calling you [male respondents: “a woman, a fag, or gay”; female respondents: “a dyke, or butch”]). SH2
- a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98
8. **Since [X Date], did someone from work repeatedly make sexual gestures or sexual body movements (for example, thrusting their pelvis or grabbing their crotch) that made you uncomfortable, angry, or upset?** SH3
- a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98
9. **Since [X Date], did someone from work display, show, or send sexually explicit materials like pictures or videos that made you uncomfortable, angry, or upset?** SH4
- a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98
10. **Since [X Date], did someone from work repeatedly tell you about their sexual activities in a way that made you uncomfortable, angry, or upset?** SH5
- a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98
11. **Since [X Date], did someone from work repeatedly ask you questions about your sex life or sexual interests that made you uncomfortable, angry, or upset?** SH6
- a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97

- d. REFUSED 98
12. **Since [X Date], did someone from work make repeated sexual comments about your appearance or body that made you uncomfortable, angry, or upset?** SH7
- a. YES 1
b. NO 2
c. DO NOT REMEMBER 97
d. REFUSED 98
13. **Since [X Date], did someone from work either take or share sexually suggestive pictures or videos of you when you did not want them to?** SH8
- a. YES 1
b. NO 2
c. DO NOT REMEMBER 97
d. REFUSED 98

[If SH8=2, 97, or 98 (No, Don't Know, or Refused) then skip to SH9]

14. **Did this make you uncomfortable, angry, or upset?** SH8a
- a. YES 1
b. NO 2
c. DO NOT REMEMBER 97
d. REFUSED 98

[As a reminder, when I ask about “Someone from work” I want you to include any person you have contact with as part of your military duties. “Someone from work” could be a supervisor, someone above or below you in rank, or a civilian contractor. They could be in your unit or in other units. These things may have occurred off-duty or off-base. Please include them as long as the person who did them to you was someone from work.

Continue to answer Yes or No.]

15. **Since [X Date], did someone from work make repeated attempts to establish an unwanted romantic or sexual relationship with you?** [These could range from repeatedly asking you out for coffee to asking you for sex or a ‘hook-up.’] SH9
- a. YES 1

- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If SH9=2, 97, 98 (No, Don't Know, or Refused) then skip to SH10]

16. **Did these attempts make you uncomfortable, angry, or upset?** SH9a

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

17. **Since [X Date], did someone from work intentionally touch you in a sexual way when you did not want them to?** [This could include touching your genitals, breasts, buttocks, or touching you with their genitals anywhere on your body.] SH10

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If SH10=1 (Yes) then Skip to SH12 and PerceivedHostileWorkEnvironment = TRUE]

18. **Since [X Date], did someone from work repeatedly touch you in any other way that made you uncomfortable, angry, or upset?** [This could include almost any unnecessary physical contact including hugs, shoulder rubs, or touching your hair, but would not usually include handshakes or routine uniform adjustments.] SH11

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

19. **Since [X Date], has someone from work made you feel as if you would get some workplace benefit in exchange for doing something sexual?** [For example, they might hint that they would give you a good evaluation or fitness report, a better assignment, or better treatment at work in exchange for doing something sexual. Something sexual could include talking about sex, undressing, sharing sexual pictures, or having some type of sexual contact.] SH12

- a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98
20. **Since [X Date], has someone from work made you feel like you would get punished or treated unfairly in the workplace if you did not do something **sexual**?** [For example, they hinted that they would give you a bad evaluation or fitness report, a bad assignment, or bad treatment at work if you were not willing to do something sexual. This could include being unwilling to talk about sex, undress, share sexual pictures, or have some type of sexual contact.] SH13
- a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98
21. **Since [X Date], did you hear someone from work say that [men/women] are not as good as [women/men] at your particular job, or that [men/women] should be prevented from having your job?** SH14
- a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98
22. **Since [X Date], do you think someone from work mistreated, ignored, excluded, or insulted you because you are a [man/woman]?** SH15
- a. YES 1
 - b. NO 2
 - c. DO NOT REMEMBER 97
 - d. REFUSED 98

INTRODUCTION TO SA SECTION

[STARTING AT THIS POINT THROUGH THE REST OF THE SURVEY, ANY REFUSAL (INCLUDING HANG-UPS AND MILD REFUSALS) MUST BE CODED AS A FINAL REFUSAL.]

Please listen carefully to the following instructions about the next section. The questions will ask about unwanted experiences of an abusive, humiliating, or sexual nature. These types of unwanted experiences vary in severity. Some of them could be viewed as an assault. Others could be viewed as hazing or some other type of unwanted experience.

They can happen to both women and men.

I want to apologize for some of the graphic words in the next section. I will be describing things that DoD regulations define with graphic, anatomical language. It is important that I use the same names for body parts that the DoD uses. This is the best way to determine whether or not people have had these types of experiences.

When answering these questions, please include experiences no matter who did it to you or where it happened. It could be done to you by a male or female, Service member or civilian, someone you knew or a stranger.

Please include experiences even if you or others had been drinking alcohol, using drugs, or were intoxicated.

The following questions will ask you about events that happened AFTER [X date].

You do not have to answer any question that you don't want to answer. Remember, all the information you share will be kept confidential. We will not give your identifiable answers to the DoD.

Please answer Yes or No to the following questions.

117. **Since [X Date], did you have any unwanted experiences in which someone put his penis into your [If Intro1=2 (Female), display: "vagina,"] anus or mouth? SA1**
- YES 1
 - NO 2
 - DO NOT REMEMBER 97
 - REFUSED 98

[If SA1=1 (Yes) ask "OB1a", else continue]

118. **Since [X Date], did you have any unwanted experiences in which someone put any object or any body part other than a penis into your [If Intro1=2 (Female), display: "vagina,"] anus or mouth? [The body part could include a finger, tongue or testicles.] SA2**
- YES 1
 - NO 2
 - DO NOT REMEMBER 97

d. REFUSED 98

[If SA2=1 (Yes) and sexualAssault_12m ≠ “True”, then ask “PF2a”, else continue]

119. **Since [X Date], did anyone make you put any part of your body or any object into someone’s mouth, vagina, or anus when you did not want to?** [A part of the body could include your [If Intro1=1 (Male) display: “penis, testicles,”] tongue or fingers.] SA3

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If SA3=1 (Yes) and sexualAssault_12m ≠ “True”, then ask “PF3a”, else continue]

[Programming note: If sexualAssault_12m = “TRUE” on the basis of follow-ups to SA1-SA3 then penetrativeSA_12m = “TRUE” else penetrativeSA_12m = “FALSE”]

120. **Since [X Date], did you have any unwanted experiences in which someone intentionally touched private areas of your body (either directly or through clothing)?** [Private areas include buttocks, inner thigh, breasts, groin, anus, vagina, penis, or testicles.] SA4

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If SA4=1 (Yes) and sexualAssault_12m ≠ “True”, then ask “PF4a”, else continue]

121. **Since [X Date], did you have any unwanted experiences in which someone made you touch private areas of their body or someone else’s body (either directly or through clothing)?** [This could involve the person putting their private areas on you. Private areas include buttocks, inner thigh, breasts, groin, anus, vagina, penis or testicles.] SA5

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If SA5=1 (Yes) and *sexualAssault_12m* ≠ “True”, then ask “PF5a”, else continue]

[Programming note: If *SexualAssault_12m* = “TRUE” on the basis of follow-ups to SA4-SA5 then *contactSA_12m* = “TRUE” else *contactSA_12m* = “FALSE”]

122. **Since [X Date], did you have any unwanted experiences in which someone attempted to put a penis, an object, or any body part into your [If Intro1=2 (Female), display: “vagina,”] anus or mouth, but no penetration actually occurred?** SA6

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If SA6=2, 97, or 98 (No, Don’t know, or Refused) skip to End of Survey]

123. **As part of this attempt, did the person touch you anywhere on your body?** [This includes grabbing your arm, hair or clothes, or pushing their body against yours.] SA6a

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If *sexualAssault_12m* ≠ “True”, then ask “PF6a”, else continue]

[Programming note: If *sexualAssault_12m* = “TRUE” on the basis of follow-ups to SA6 then *attemptedSA_12m* = “TRUE” else *attemptedSA_12m* = “FALSE”]

[Purpose Follow Up Module START]

[Purpose Follow Up module: “X” in the question number refers to appropriate SA screener number (2-6)]

124. **Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating?** [If you aren’t sure, choose the best answer.] PFXa

- a. YES 1
- b. NO 2

- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If PFXa=1 (Yes) skip to OBX item]

125. Do you believe the person did it for a sexual reason? [For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.] PFXb

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If PFXb=1 (Yes) continue to OBX item]

[If PFXb=2, 97, 98 (No, Don't Know, or Refused) skip to next SA_screener question (SA3 –SA6)]

[Purpose Follow-Up Module END]

[Offender Behavior Module START]

[Offender Behavior Module: "X" in the question number refers to appropriate screener number (1-6)]

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened by answering Yes or No.

126. They continued even when you told them or showed them that you were unwilling. OBXa

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXa=1 (Yes) sexualAssault_12m= "TRUE"]

127. **They used physical force to make you comply.** [For example, they grabbed your arm or used their body weight to hold you down.] **OBXb**

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXb=1 (Yes) *sexualAssault_12m* = "TRUE"]

128. **They physically injured you.** **OBXc**

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXc=1 (Yes) *sexualAssault_12m* = "TRUE"]

129. **They threatened to physically hurt you (or someone else).** **OBXd**

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXd=1 (Yes) *sexualAssault_12m* = "TRUE"]

[IF OBXd=1 (Yes) then ask]

130. **Did they threaten you (or someone else) with a weapon?** **OBXd_1**

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[IF OBXd=1 (Yes) then ask]

131. **Did they threaten to seriously injure, kill, or kidnap you (or someone else)?**
OBXd_2

- a. YES 1

- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

132. **They threatened you (or someone else) in some other way.** [For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.] **OBXe**

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXe=1 (Yes) *sexualAssault_12m* = "TRUE"]

[Please continue to indicate which of the following happened.]

133. **They did it when you were passed out, asleep, or unconscious.** **OBXf**

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXf=1 (Yes) *sexualAssault_12m* = "TRUE"]

134. **They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.** **OBXg**

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXg=1 (Yes) *sexualAssault_12m* = "TRUE"]

135. **They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).** **OBXh**

- a. YES 1

- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXh=1 (Yes) *sexualAssault_12m* = "TRUE"]

[If *sexualAssault_12m* = TRUE, then skip to next screening item SA2-SA6, else continue.]

136. They made you so afraid that you froze and could not tell them or show them that you were unwilling. OBXi

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXi=1 (Yes) *sexualAssault_12m* = "TRUE"]

137. They did it after you had consumed so much alcohol that the next day you could not remember what happened. OBXj

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXj=1 (Yes) *sexualAssault_12m* = "TRUE"]

138. It happened without your consent. OBXk

- a. YES 1
- b. NO 2
- c. DO NOT REMEMBER 97
- d. REFUSED 98

[If OBXk=1 (Yes) *sexualAssault_12m* = "TRUE"]

[Offender Behavior Module END]

[After OB1-OB5, continue to next screening item SA2-SA6. After OB6 series questions, continue to END.]

Thank you for completing the survey with me today. This information will help improve the climate and safety of the U.S. military.

I understand that answering questions like the ones in this survey can be upsetting. If you feel you need support or would like to talk to someone, you can call:

the DoD Safe Helpline number [877-995-5247],

the Military Crisis Line [1-800-273-8255],

or the Rape, Abuse and Incest National Network [1-800-656-HOPE].

A DoD Safe Helpline counselor can also explain how to report a sexual assault and how to find out the current status of a sexual assault report.

Would you like the telephone numbers for any of those organizations?

Mail Survey (Male and Female Respondent Versions)

This appendix contains the male and female versions of the mailed RMWS survey.



2014 RAND Military Workplace Study

PRIVACY ADVISORY

The Defense Manpower Data Center has provided certain information about you to allow RAND to conduct this survey. Your name and contact information have been used to send you notifications and information about this survey. The Defense Manpower Data Center has provided certain demographic information to reduce the number of questions in the survey and minimize the burden on your time. Your response and demographic data are linked by RAND to allow for a thorough analysis of the responses by the demographics. RAND has not been authorized by DoD to identify or link survey response and demographic information with your name and contact information. The resulting reports will not include analysis of groupings of less than 15.

RCS#: DD-P&R(QD)1947
Expires: 7/25/2015

Before you begin this survey, please read the informed consent statement that follows.

INFORMED CONSENT STATEMENT

Introduction: The RAND Corporation and Westat are conducting a survey that asks about whether or not you have experienced harassment, discrimination, or inappropriate sexual behavior. We need your responses whether or not you have had these experiences. RAND is a private, nonprofit organization that conducts research and analysis to help improve public policy and decision making. RAND's research partner is Westat, an internationally known research and statistical survey organization.

Purpose: The Department of Defense (DoD) and Congress are working to understand the full extent of harassment and assault in the military and whether current efforts to reduce them are helping. The DoD has funded RAND to conduct an independent assessment of the military work environment during the past year. You and other Service members, including all women and approximately 25% of men, are being urged to participate in order to ensure that DoD and Congress have a full understanding of Service member experiences. The survey results will have a direct impact on training, military justice, and services that affect you and other Service members.

Survey Length: This survey will take about 5 minutes to complete.

Incentive: To thank you in advance for participating in our mail survey, we enclosed \$4.00 in cash with the first survey packet we mailed you. This is a small token of our appreciation for your time and support.

Voluntary Participation: Your participation is completely voluntary, and you may stop at any time. You can skip any question you don't want to answer.

Privacy: RAND will not give the DoD information about who participated in the study, nor will RAND link your individual responses on this survey with your name or identity. DoD has agreed to this condition to protect your privacy. RAND also received a federal "Certificate of Confidentiality" that provides RAND with additional protection against any attempt to subpoena confidential survey records. This Certificate helps ensure the confidentiality of your information by protecting the researchers from being forced to release information that might identify you, even under a court order or subpoena. However, the protections of a Certificate of Confidentiality are not absolute. If you tell us that a child or elderly person is being abused, or that you intend to harm yourself or someone else, the researchers may report it to the authorities. The Certificate of Confidentiality is not an endorsement of the project by the U.S. Department of Health and Human Services.

Added Protection Procedures: Only members of the RAND-Westat study team will have access to your individual responses, and we will take great care to protect your privacy and data. For example, RAND will collapse some categories or ranges of potentially identifying variables to prevent identification by inference. Study staff members have been trained to deidentify data to protect your identity and are subject to civil penalties for violating your confidentiality. Our research team has a number of safeguarding procedures in place to ensure that survey data are protected from accidental disclosure.

Risks of Participation: For most respondents, the survey involves no risks of participation. However, if you have ever experienced sexual harassment or assault, some questions may cause discomfort or distress. Some questions may be explicit; therefore, you may prefer to take the survey in a private setting.

Reporting Harassment or Assault: It is important to note that this survey is not a means of making a formal complaint or report that you wish to have DoD act upon. The survey will not collect the identity of any perpetrators of assault or harassment. Instead, we provide information below and at the end of the survey about how you can make a formal report of harassment or assault.

Resources Available to You: If you need resources or assistance, the DoD Safe Helpline (<https://www.safehelpline.org/>) provides worldwide live, confidential support, 24/7. You can initiate a report or search for your nearest Sexual Assault Response Coordinator (SARC). You can find links to Service-specific reporting resources and access information about the prevention of and response to sexual assault on their website or by calling the hotline at 1-877-995-5247.

Some questions in the survey may ask about upsetting experiences. If you feel distressed, for confidential support and consultation you can contact the Military Crisis Line (<http://veteranscrisisline.net/ActiveDuty.aspx>) or call them at 1-800-273-8255 (then press 1).

Who do you contact if you have questions or concerns about the survey?

- **RAND for questions about the overall study:** Contact the RAND team by email at WGRS2014@rand.org or go to the RAND website www.WGRS2014.rand.org
- **Westat Survey Helpdesk for computer, technical, or survey questions:** By telephone toll free at 1-855-365-5914 (OCONUS call collect 240-453-2620) or by email at support@randmilitarystudy.org.
- **Questions about your rights as a participant in this study:** Contact the RAND Human Subjects Protection Committee at 310-393-0411, ext. 6369, in Santa Monica, California.
- **Questions about the licensing of the survey:** Information about DoD surveys can be found at http://www.dtic.mil/whs/directives/corres/intinfocollections/iic_search.html. This survey's RCS is DD-P&R(QD)1947, Expiration July 25, 2015.

If you would like to participate in this survey, fill out the survey booklet and mail it to us in the enclosed pre-addressed privacy envelope. No postage is needed if you use the return envelope provided.

2014 RAND Military Workplace Study

Survey Introduction: Some of the questions in this survey will be personal. Please answer each question thoughtfully and truthfully. This will allow us to provide an accurate picture of the different experiences of today's military members. If you prefer not to answer a specific question for any reason, just leave it blank. For your privacy, you may want to take this survey where other people won't see your answers. Thank you for agreeing to participate in this important study.

Marking Instructions:

- ▶ Please use a black or blue pen to complete this form.
- ▶ Mark to indicate your answer. If you want to change your answer, darken the box and mark the correct answer.

Start Here

Please try to think of any important events in your life that occurred near 9/1/2013 such as birthdays, weddings, or family activities. These events can help you remember which things happened before 9/1/2013 and which happened after as you answer the rest of the survey questions.

The following questions will help you think about your life one year ago.

1. Do you currently live in the same house or building that you did on 9/1/2013?
 - Yes
 - No
 - Do not remember
2. Are you the same rank today that you were on 9/1/2013?
 - Yes
 - No
 - Do not remember
3. Are you in the same military occupation today as you were on 9/1/2013?
 - Yes
 - No
 - Do not remember
4. Were you on vacation or leave on 9/1/2013?
 - Yes
 - No
 - Do not remember
5. Were you married or dating someone on 9/1/2013?
 - Yes
 - No
 - Do not remember

In this section, you will be asked about several things that someone from work might have done to you that were upsetting or offensive, and that happened AFTER 9/1/2013.

When the questions say "someone from work," please include any person you have contact with as part of your military duties. "Someone from work" could be a supervisor, someone above or below you in rank, or a civilian employee/contractor. They could be in your unit or in other units.

These things may have occurred on-duty or off-duty, on-base or off-base. Please include them as long as the person who did them to you was someone from work.

Remember, all the information you share will be kept confidential.

6. Since 9/1/2013, did someone from work repeatedly tell sexual "jokes" that made you uncomfortable, angry, or upset?
- Yes
 No
7. Since 9/1/2013, did someone from work embarrass, anger, or upset you by repeatedly suggesting that you do not act like a man is supposed to? For example, by calling you "a woman, a fag, or gay."
- Yes
 No
8. Since 9/1/2013, did someone from work repeatedly make sexual gestures or sexual body movements (for example, thrusting their pelvis or grabbing their crotch) that made you uncomfortable, angry, or upset?
- Yes
 No
9. Since 9/1/2013, did someone from work display, show, or send sexually explicit materials like pictures or videos that made you uncomfortable, angry, or upset?
- Yes
 No
10. Since 9/1/2013, did someone from work repeatedly tell you about their sexual activities in a way that made you uncomfortable, angry, or upset?
- Yes
 No
11. Since 9/1/2013, did someone from work repeatedly ask you questions about your sex life or sexual interests that made you uncomfortable, angry, or upset?
- Yes
 No
12. Since 9/1/2013, did someone from work make repeated sexual comments about your appearance or body that made you uncomfortable, angry, or upset?
- Yes
 No
13. Since 9/1/2013, did someone from work either take or share sexually suggestive pictures or videos of you when you did not want them to?
- Yes
 No → **Go to Question 14**
- 13a. Did this make you uncomfortable, angry, or upset?
- Yes
 No
14. Since 9/1/2013, did someone from work make repeated attempts to establish an unwanted romantic or sexual relationship with you? These could range from repeatedly asking you out for coffee to asking you for sex or a 'hook-up.'
- Yes
 No → **Go to Question 15, page 3**
- 14a. Did these attempts make you uncomfortable, angry, or upset?
- Yes
 No

"Someone from work" includes any person you have contact with as part of your military duties. "Someone from work" could be a supervisor, someone above or below you in rank, or a civilian employee/contractor. They could be in your unit or in other units.

These things may have occurred off-duty or off-base. Please include them as long as the person who did them to you was someone from work.

Remember, all the information you share will be kept confidential.

15. Since 9/1/2013, did someone from work intentionally touch you in a sexual way when you did not want them to? This could include touching your genitals, breasts, buttocks, or touching you with their genitals anywhere on your body.

- Yes → **Go to Question 17**
- No

16. Since 9/1/2013, did someone from work repeatedly touch you in any other way that made you uncomfortable, angry, or upset?

This could include almost any unnecessary physical contact including hugs, shoulder rubs, or touching your hair, but would not usually include handshakes or routine uniform adjustments.

- Yes
- No

17. Since 9/1/2013, has someone from work made you feel as if you would get some workplace benefit in exchange for doing something sexual?

For example, they might hint that they would give you a good evaluation/fitness report, a better assignment, or better treatment at work in exchange for doing something sexual. Something sexual could include talking about sex, undressing, sharing sexual pictures, or having some type of sexual contact.

- Yes
- No

18. Since 9/1/2013, has someone from work made you feel like you would get punished or treated unfairly in the workplace if you did not do something sexual?

For example, they hinted that they would give you a bad evaluation/fitness report, a bad assignment, or bad treatment at work if you were not willing to do something sexual. This could include being unwilling to talk about sex, undress, share sexual pictures, or have some type of sexual contact.

- Yes
- No

19. Since 9/1/2013, did you hear someone from work say that men are not as good as women at your particular job, or that men should be prevented from having your job?

- Yes
- No

20. Since 9/1/2013, do you think someone from work mistreated, ignored, excluded, or insulted you because you are a man?

- Yes
- No



Please read the following special instructions before continuing the survey.

Questions in this next section ask about unwanted experiences of an abusive, humiliating, or sexual nature. These types of unwanted experiences vary in severity. Some of them could be viewed as an assault. Others could be viewed as hazing or some other type of unwanted experience.

They can happen to both women and men.

Some of the language may seem graphic, but using the names of specific body parts is the best way to determine whether or not people have had these types of experiences.

When answering these questions, please include experiences no matter who did it to you or where it happened. It could be done to you by a male or female, Service member or civilian, someone you knew or a stranger.

Please include experiences even if you or others had been drinking alcohol, using drugs, or were intoxicated.

The following questions will ask you about events that happened AFTER 9/1/2013.

Remember, all the information you share will be kept confidential. RAND will not give your identifiable answers to the DoD.

21. Since 9/1/2013, did you have any unwanted experiences in which someone put his penis into your anus or mouth?

Yes
 No → **Go to Question 31, page 5**

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened.

	Yes	No
22. They continued even when you told them or showed them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
23. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.	<input type="checkbox"/>	<input type="checkbox"/>
24. They physically injured you.	<input type="checkbox"/>	<input type="checkbox"/>
25. They threatened to physically hurt you (or someone else).	<input type="checkbox"/>	<input type="checkbox"/>

	Yes	No
26. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.	<input type="checkbox"/>	<input type="checkbox"/>
27. They did it when you were passed out, asleep, or unconscious.	<input type="checkbox"/>	<input type="checkbox"/>
28. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
29. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).	<input type="checkbox"/>	<input type="checkbox"/>
30. Did you answer "Yes" to any question from 22 to 29?		
<input type="checkbox"/> Yes → Go to End of Survey, page 10		
<input type="checkbox"/> No → Go to Question 31, page 5		

31. Since 9/1/2013, did you have any unwanted experiences in which someone put any object or any body part other than a penis into your anus or mouth? The body part could include a finger, tongue, or testicles.

- Yes
- No → **Go to Question 41, page 6**

31a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

31b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

31c. Did you answer "Yes" to either Question 31a or 31b?

- Yes → **Continue to next column**
- No → **Go to Question 41, page 6**

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened.

	Yes	No
32. They continued even when you told them or showed them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
33. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.	<input type="checkbox"/>	<input type="checkbox"/>
34. They physically injured you.	<input type="checkbox"/>	<input type="checkbox"/>
35. They threatened to physically hurt you (or someone else).	<input type="checkbox"/>	<input type="checkbox"/>
36. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.	<input type="checkbox"/>	<input type="checkbox"/>
37. They did it when you were passed out, asleep, or unconscious.	<input type="checkbox"/>	<input type="checkbox"/>
38. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
39. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).	<input type="checkbox"/>	<input type="checkbox"/>
40. Did you answer "Yes" to any question from 32 to 39?		
	<ul style="list-style-type: none"> <input type="checkbox"/> Yes → Go to End of Survey, page 10 <input type="checkbox"/> No → Go to Question 41, page 6 	



41. Since 9/1/2013, did anyone make you put any part your body or any object into someone's mouth, vagina, or anus when you did not want to? A part of the body could include your penis, testicles, tongue, or fingers.

- Yes
- No → **Go to Question 51, page 7**

41a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

41b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

41c. Did you answer "Yes" to either Question 41a or 41b?

- Yes → **Continue to next column**
- No → **Go to Question 51, page 7**

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened.

	Yes	No
42. They continued even when you told them or showed them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
43. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.	<input type="checkbox"/>	<input type="checkbox"/>
44. They physically injured you.	<input type="checkbox"/>	<input type="checkbox"/>
45. They threatened to physically hurt you (or someone else).	<input type="checkbox"/>	<input type="checkbox"/>
46. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.	<input type="checkbox"/>	<input type="checkbox"/>
47. They did it when you were passed out, asleep, or unconscious.	<input type="checkbox"/>	<input type="checkbox"/>
48. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
49. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).	<input type="checkbox"/>	<input type="checkbox"/>

50. Did you answer "Yes" to any question from 42 to 49?

- Yes → **Go to End of Survey, page 10**
- No → **Go to Question 51, page 7**



51. Since 9/1/2013, did you have any unwanted experiences in which someone intentionally touched private areas of your body (either directly or through clothing)?

Private areas include buttocks, inner thigh, breasts, groin, anus, vagina, penis, or testicles.

- Yes
- No → **Go to Question 61, page 8**

51a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

51b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

51c. Did you answer "Yes" to either Question 51a or 51b?

- Yes → **Continue to next column**
- No → **Go to Question 61, page 8**

The following statements are about things that might have happened to you when you had this experience. In these statements, "they" means the person or people who did this to you.

Please indicate which of the following happened.

- | | Yes | No |
|--|--|--------------------------|
| 52. They continued even when you told them or showed them that you were unwilling. | <input type="checkbox"/> | <input type="checkbox"/> |
| 53. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down. | <input type="checkbox"/> | <input type="checkbox"/> |
| 54. They physically injured you. | <input type="checkbox"/> | <input type="checkbox"/> |
| 55. They threatened to physically hurt you (or someone else). | <input type="checkbox"/> | <input type="checkbox"/> |
| 56. They threatened you (or someone else) in some other way.

For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities. | <input type="checkbox"/> | <input type="checkbox"/> |
| 57. They did it when you were passed out, asleep, or unconscious. | <input type="checkbox"/> | <input type="checkbox"/> |
| 58. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling. | <input type="checkbox"/> | <input type="checkbox"/> |
| 59. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor). | <input type="checkbox"/> | <input type="checkbox"/> |
| 60. Did you answer "Yes" to any question from 52 to 59? | | |
| | <ul style="list-style-type: none"> <input type="checkbox"/> Yes → Go to End of Survey, page 10 <input type="checkbox"/> No → Go to Question 61, page 8 | |

61. Since 9/1/2013, did you have any unwanted experiences in which someone made you touch private areas of their body or someone else's body (either directly or through clothing)? This could involve the person putting their private areas on you.

Private areas include buttocks, inner thigh, breasts, groin, anus, vagina, penis, or testicles.

- Yes
- No → **Go to Question 71, page 9**

61a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

61b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

61c. Did you answer "Yes" to either Question 61a or 61b?

- Yes → **Continue to next column**
- No → **Go to Question 71, page 9**

The following statements are about things that might have happened to you when you had this experience. In these statements, "they" means the person or people who did this to you.

Please indicate which of the following happened.

	Yes	No
62. They continued even when you told them or showed them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
63. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.	<input type="checkbox"/>	<input type="checkbox"/>
64. They physically injured you.	<input type="checkbox"/>	<input type="checkbox"/>
65. They threatened to physically hurt you (or someone else).	<input type="checkbox"/>	<input type="checkbox"/>
66. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.	<input type="checkbox"/>	<input type="checkbox"/>
67. They did it when you were passed out, asleep, or unconscious.	<input type="checkbox"/>	<input type="checkbox"/>
68. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
69. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).	<input type="checkbox"/>	<input type="checkbox"/>

70. Did you answer "Yes" to any question from 62 to 69?

- Yes → **Go to End of Survey, page 10**
- No → **Go to Question 71, page 9**



71. Since 9/1/2013, did you have any unwanted experiences in which someone attempted to put a penis, an object, or any body part into your anus or mouth, but no penetration actually occurred?

- Yes
- No → **Go to End of Survey, page 10**

71a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

71b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

71c. Did you answer "Yes" to either Question 71a or 71b?

- Yes → **Continue to next column**
- No → **Go to End of Survey, page 10**

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened.

	Yes	No
72. They continued even when you told them or showed them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
73. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.	<input type="checkbox"/>	<input type="checkbox"/>
74. They physically injured you.	<input type="checkbox"/>	<input type="checkbox"/>
75. They threatened to physically hurt you (or someone else).	<input type="checkbox"/>	<input type="checkbox"/>
76. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.	<input type="checkbox"/>	<input type="checkbox"/>
77. They did it when you were passed out, asleep, or unconscious.	<input type="checkbox"/>	<input type="checkbox"/>
78. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
79. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).	<input type="checkbox"/>	<input type="checkbox"/>

End of Survey

This information will help improve the climate and safety of the U.S. military. You may have found that the questions did not completely cover your experiences. Nonetheless, the answers you provided are very important to this study.

Sometimes answering questions like the ones on this survey can be upsetting. If you feel you need support or would like to talk to someone, you can call:

- DoD Safe Helpline number (877-995-5247)
- Military Crisis Line (1-800-273-8255)
- RAINN (1-800-656-HOPE)

A SAFE helpline counselor can also explain how to report a sexual assault and how to find out the current status of a sexual assault report.

Thank you for completing the survey.

Please return your survey using the enclosed postage-paid envelope. No postage is needed.

If your return envelope has been misplaced, please mail your survey to:

2014 RAND Military Workplace Study
Westat 6236.02.14
1600 Research Blvd, RW 2634
Rockville, Maryland 20850-9973

Westat Survey Helpdesk toll free number: 1-855-365-5914
(OCONUS please call collect: 240-453-2620)



2014 RAND Military Workplace Study

PRIVACY ADVISORY

The Defense Manpower Data Center has provided certain information about you to allow RAND to conduct this survey. Your name and contact information have been used to send you notifications and information about this survey. The Defense Manpower Data Center has provided certain demographic information to reduce the number of questions in the survey and minimize the burden on your time. Your response and demographic data are linked by RAND to allow for a thorough analysis of the responses by the demographics. RAND has not been authorized by DoD to identify or link survey response and demographic information with your name and contact information. The resulting reports will not include analysis of groupings of less than 15.

RCS#: DD-P&R(QD)1947
Expires: 7/25/2015

Before you begin this survey, please read the informed consent statement that follows.

INFORMED CONSENT STATEMENT

Introduction: The RAND Corporation and Westat are conducting a survey that asks about whether or not you have experienced harassment, discrimination, or inappropriate sexual behavior. We need your responses whether or not you have had these experiences. RAND is a private, nonprofit organization that conducts research and analysis to help improve public policy and decision making. RAND's research partner is Westat, an internationally known research and statistical survey organization.

Purpose: The Department of Defense (DoD) and Congress are working to understand the full extent of harassment and assault in the military and whether current efforts to reduce them are helping. The DoD has funded RAND to conduct an independent assessment of the military work environment during the past year. You and other Service members, including all women and approximately 25% of men, are being urged to participate in order to ensure that DoD and Congress have a full understanding of Service member experiences. The survey results will have a direct impact on training, military justice, and services that affect you and other Service members.

Survey Length: This survey will take about 5 minutes to complete.

Incentive: To thank you in advance for participating in our mail survey, we enclosed \$4.00 in cash with the first survey packet we mailed you. This is a small token of our appreciation for your time and support.

Voluntary Participation: Your participation is completely voluntary, and you may stop at any time. You can skip any question you don't want to answer.

Privacy: RAND will not give the DoD information about who participated in the study, nor will RAND link your individual responses on this survey with your name or identity. DoD has agreed to this condition to protect your privacy. RAND also received a federal "Certificate of Confidentiality" that provides RAND with additional protection against any attempt to subpoena confidential survey records. This Certificate helps ensure the confidentiality of your information by protecting the researchers from being forced to release information that might identify you, even under a court order or subpoena. However, the protections of a Certificate of Confidentiality are not absolute. If you tell us that a child or elderly person is being abused, or that you intend to harm yourself or someone else, the researchers may report it to the authorities. The Certificate of Confidentiality is not an endorsement of the project by the U.S. Department of Health and Human Services.

Added Protection Procedures: Only members of the RAND-Westat study team will have access to your individual responses, and we will take great care to protect your privacy and data. For example, RAND will collapse some categories or ranges of potentially identifying variables to prevent identification by inference. Study staff members have been trained to deidentify data to protect your identity and are subject to civil penalties for violating your confidentiality. Our research team has a number of safeguarding procedures in place to ensure that survey data are protected from accidental disclosure.

Risks of Participation: For most respondents, the survey involves no risks of participation. However, if you have ever experienced sexual harassment or assault, some questions may cause discomfort or distress. Some questions may be explicit; therefore, you may prefer to take the survey in a private setting.

Reporting Harassment or Assault: It is important to note that this survey is not a means of making a formal complaint or report that you wish to have DoD act upon. The survey will not collect the identity of any perpetrators of assault or harassment. Instead, we provide information below and at the end of the survey about how you can make a formal report of harassment or assault.

Resources Available to You: If you need resources or assistance, the DoD Safe Helpline (<https://www.safehelpline.org/>) provides worldwide live, confidential support, 24/7. You can initiate a report or search for your nearest Sexual Assault Response Coordinator (SARC). You can find links to Service-specific reporting resources and access information about the prevention of and response to sexual assault on their website or by calling the hotline at 1-877-995-5247.

Some questions in the survey may ask about upsetting experiences. If you feel distressed, for confidential support and consultation you can contact the Military Crisis Line (<http://veteranscrisisline.net/ActiveDuty.aspx>) or call them at 1-800-273-8255 (then press 1).

Who do you contact if you have questions or concerns about the survey?

- **RAND for questions about the overall study:** Contact the RAND team by email at WGRS2014@rand.org or go to the RAND website www.WGRS2014.rand.org
- **Westat Survey Helpdesk for computer, technical, or survey questions:** By telephone toll free at 1-855-365-5914 (OCONUS call collect 240-453-2620) or by email at support@randmilitarystudy.org.
- **Questions about your rights as a participant in this study:** Contact the RAND Human Subjects Protection Committee at 310-393-0411, ext. 6369, in Santa Monica, California.
- **Questions about the licensing of the survey:** Information about DoD surveys can be found at http://www.dtic.mil/whs/directives/corres/intinfocollections/iic_search.html. This survey's RCS is DD-P&R(QD)1947, Expiration July 25, 2015.

If you would like to participate in this survey, fill out the survey booklet and mail it to us in the enclosed pre-addressed privacy envelope. No postage is needed if you use the return envelope provided.

2014 RAND Military Workplace Study

Survey Introduction: Some of the questions in this survey will be personal. Please answer each question thoughtfully and truthfully. This will allow us to provide an accurate picture of the different experiences of today's military members. If you prefer not to answer a specific question for any reason, just leave it blank. For your privacy, you may want to take this survey where other people won't see your answers. Thank you for agreeing to participate in this important study.

Marking Instructions:

- ▶ Please use a black or blue pen to complete this form.
- ▶ Mark to indicate your answer. If you want to change your answer, darken the box and mark the correct answer.

Start Here

Please try to think of any important events in your life that occurred near 9/1/2013 such as birthdays, weddings, or family activities. These events can help you remember which things happened before 9/1/2013 and which happened after as you answer the rest of the survey questions.

The following questions will help you think about your life one year ago.

1. Do you currently live in the same house or building that you did on 9/1/2013?
 - Yes
 - No
 - Do not remember
2. Are you the same rank today that you were on 9/1/2013?
 - Yes
 - No
 - Do not remember
3. Are you in the same military occupation today as you were on 9/1/2013?
 - Yes
 - No
 - Do not remember
4. Were you on vacation or leave on 9/1/2013?
 - Yes
 - No
 - Do not remember
5. Were you married or dating someone on 9/1/2013?
 - Yes
 - No
 - Do not remember

In this section, you will be asked about several things that someone from work might have done to you that were upsetting or offensive, and that happened AFTER 9/1/2013.

When the questions say "someone from work," please include any person you have contact with as part of your military duties. "Someone from work" could be a supervisor, someone above or below you in rank, or a civilian employee/contractor. They could be in your unit or in other units.

These things may have occurred on-duty or off-duty, on-base or off-base. Please include them as long as the person who did them to you was someone from work.

Remember, all the information you share will be kept confidential.

6. Since 9/1/2013, did someone from work repeatedly tell sexual "jokes" that made you uncomfortable, angry, or upset?
- Yes
 No
7. Since 9/1/2013, did someone from work embarrass, anger, or upset you by repeatedly suggesting that you do not act like a woman is supposed to? For example, by calling you "a dyke, or butch."
- Yes
 No
8. Since 9/1/2013, did someone from work repeatedly make sexual gestures or sexual body movements (for example, thrusting their pelvis or grabbing their crotch) that made you uncomfortable, angry, or upset?
- Yes
 No
9. Since 9/1/2013, did someone from work display, show, or send sexually explicit materials like pictures or videos that made you uncomfortable, angry, or upset?
- Yes
 No
10. Since 9/1/2013, did someone from work repeatedly tell you about their sexual activities in a way that made you uncomfortable, angry, or upset?
- Yes
 No
11. Since 9/1/2013, did someone from work repeatedly ask you questions about your sex life or sexual interests that made you uncomfortable, angry, or upset?
- Yes
 No
12. Since 9/1/2013, did someone from work make repeated sexual comments about your appearance or body that made you uncomfortable, angry, or upset?
- Yes
 No
13. Since 9/1/2013, did someone from work either take or share sexually suggestive pictures or videos of you when you did not want them to?
- Yes
 No → **Go to Question 14**
- 13a. Did this make you uncomfortable, angry, or upset?
- Yes
 No
14. Since 9/1/2013, did someone from work make repeated attempts to establish an unwanted romantic or sexual relationship with you? These could range from repeatedly asking you out for coffee to asking you for sex or a 'hook-up.'
- Yes
 No → **Go to Question 15, page 3**
- 14a. Did these attempts make you uncomfortable, angry, or upset?
- Yes
 No

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"Someone from work" includes any person you have contact with as part of your military duties. "Someone from work" could be a supervisor, someone above or below you in rank, or a civilian employee/contractor. They could be in your unit or in other units.

These things may have occurred off-duty or off-base. Please include them as long as the person who did them to you was someone from work.

Remember, all the information you share will be kept confidential.

15. Since 9/1/2013, did someone from work intentionally touch you in a sexual way when you did not want them to? This could include touching your genitals, breasts, buttocks, or touching you with their genitals anywhere on your body.

Yes → **Go to Question 17**
 No

16. Since 9/1/2013, did someone from work repeatedly touch you in any other way that made you uncomfortable, angry, or upset?

This could include almost any unnecessary physical contact including hugs, shoulder rubs, or touching your hair, but would not usually include handshakes or routine uniform adjustments.

Yes
 No

17. Since 9/1/2013, has someone from work made you feel as if you would get some workplace benefit in exchange for doing something sexual?

For example, they might hint that they would give you a good evaluation/fitness report, a better assignment, or better treatment at work in exchange for doing something sexual. Something sexual could include talking about sex, undressing, sharing sexual pictures, or having some type of sexual contact.

Yes
 No

18. Since 9/1/2013, has someone from work made you feel like you would get punished or treated unfairly in the workplace if you did not do something sexual?

For example, they hinted that they would give you a bad evaluation/fitness report, a bad assignment, or bad treatment at work if you were not willing to do something sexual. This could include being unwilling to talk about sex, undress, share sexual pictures, or have some type of sexual contact.

Yes
 No

19. Since 9/1/2013, did you hear someone from work say that women are not as good as men at your particular job, or that women should be prevented from having your job?

Yes
 No

20. Since 9/1/2013, do you think someone from work mistreated, ignored, excluded, or insulted you because you are a woman?

Yes
 No



Please read the following special instructions before continuing the survey.

Questions in this next section ask about unwanted experiences of an abusive, humiliating, or sexual nature. These types of unwanted experiences vary in severity. Some of them could be viewed as an assault. Others could be viewed as hazing or some other type of unwanted experience.

They can happen to both women and men.

Some of the language may seem graphic, but using the names of specific body parts is the best way to determine whether or not people have had these types of experiences.

When answering these questions, please include experiences no matter who did it to you or where it happened. It could be done to you by a male or female, Service member or civilian, someone you knew or a stranger.

Please include experiences even if you or others had been drinking alcohol, using drugs, or were intoxicated.

The following questions will ask you about events that happened AFTER 9/1/2013.

Remember, all the information you share will be kept confidential. RAND will not give your identifiable answers to the DoD.

21. Since 9/1/2013, did you have any unwanted experiences in which someone put his penis into your vagina, anus, or mouth?

- Yes
- No → **Go to Question 31, page 5**

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened.

- | | Yes | No |
|---|--------------------------|--------------------------|
| 22. They continued even when you told them or showed them that you were unwilling. | <input type="checkbox"/> | <input type="checkbox"/> |
| 23. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down. | <input type="checkbox"/> | <input type="checkbox"/> |
| 24. They physically injured you. | <input type="checkbox"/> | <input type="checkbox"/> |
| 25. They threatened to physically hurt you (or someone else). | <input type="checkbox"/> | <input type="checkbox"/> |

26. They threatened you (or someone else) in some other way.

For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.

Yes No

27. They did it when you were passed out, asleep, or unconscious.

Yes No

28. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.

Yes No

29. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).

Yes No

30. Did you answer "Yes" to any question from 22 to 29?

- Yes → **Go to End of Survey, page 10**
- No → **Go to Question 31, page 5**



31. Since 9/1/2013, did you have any unwanted experiences in which someone put any object or any body part other than a penis into your vagina, anus, or mouth? The body part could include a finger, tongue or testicles.

- Yes
- No → **Go to Question 41, page 6**

31a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

31b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

31c. Did you answer "Yes" to either Question 31a or 31b?

- Yes → **Continue to next column**
- No → **Go to Question 41, page 6**

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened.

- | | Yes | No |
|--|--|--------------------------|
| 32. They continued even when you told them or showed them that you were unwilling. | <input type="checkbox"/> | <input type="checkbox"/> |
| 33. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down. | <input type="checkbox"/> | <input type="checkbox"/> |
| 34. They physically injured you. | <input type="checkbox"/> | <input type="checkbox"/> |
| 35. They threatened to physically hurt you (or someone else). | <input type="checkbox"/> | <input type="checkbox"/> |
| 36. They threatened you (or someone else) in some other way.

For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities. | <input type="checkbox"/> | <input type="checkbox"/> |
| 37. They did it when you were passed out, asleep, or unconscious. | <input type="checkbox"/> | <input type="checkbox"/> |
| 38. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling. | <input type="checkbox"/> | <input type="checkbox"/> |
| 39. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor). | <input type="checkbox"/> | <input type="checkbox"/> |
| 40. Did you answer "Yes" to any question from 32 to 39? | | |
| | <ul style="list-style-type: none"> <input type="checkbox"/> Yes → Go to End of Survey, page 10 <input type="checkbox"/> No → Go to Question 41, page 6 | |

41. Since 9/1/2013, did anyone make you put any part of your body or any object into someone's mouth, vagina, or anus when you did not want to? A part of the body could include your tongue or fingers.

- Yes
- No → **Go to Question 51, page 7**

41a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

41b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

41c. Did you answer "Yes" to either Question 41a or 41b?

- Yes → **Continue to next column**
- No → **Go to Question 51, page 7**

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened.

	Yes	No
42. They continued even when you told them or showed them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
43. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.	<input type="checkbox"/>	<input type="checkbox"/>
44. They physically injured you.	<input type="checkbox"/>	<input type="checkbox"/>
45. They threatened to physically hurt you (or someone else).	<input type="checkbox"/>	<input type="checkbox"/>
46. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.	<input type="checkbox"/>	<input type="checkbox"/>
47. They did it when you were passed out, asleep, or unconscious.	<input type="checkbox"/>	<input type="checkbox"/>
48. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
49. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).	<input type="checkbox"/>	<input type="checkbox"/>

50. Did you answer "Yes" to any question from 42 to 49?

- Yes → **Go to End of Survey, page 10**
- No → **Go to Question 51, page 7**

51. Since 9/1/2013, did you have any unwanted experiences in which someone intentionally touched private areas of your body (either directly or through clothing)?

Private areas include buttocks, inner thigh, breasts, groin, anus, vagina, penis, or testicles.

- Yes
- No → **Go to Question 61, page 8**

51a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

51b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

51c. Did you answer "Yes" to either Question 51a or 51b?

- Yes → **Continue to next column**
- No → **Go to Question 61, page 8**

The following statements are about things that might have happened to you when you had this experience. In these statements, "they" means the person or people who did this to you.

Please indicate which of the following happened.

	Yes	No
52. They continued even when you told them or showed them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
53. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.	<input type="checkbox"/>	<input type="checkbox"/>
54. They physically injured you.	<input type="checkbox"/>	<input type="checkbox"/>
55. They threatened to physically hurt you (or someone else).	<input type="checkbox"/>	<input type="checkbox"/>
56. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.	<input type="checkbox"/>	<input type="checkbox"/>
57. They did it when you were passed out, asleep, or unconscious.	<input type="checkbox"/>	<input type="checkbox"/>
58. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
59. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).	<input type="checkbox"/>	<input type="checkbox"/>
60. Did you answer "Yes" to any question from 52 to 59?		
	<ul style="list-style-type: none"> <input type="checkbox"/> Yes → Go to End of Survey, page 10 <input type="checkbox"/> No → Go to Question 61, page 8 	

61. Since 9/1/2013, did you have any unwanted experiences in which someone made you touch private areas of their body or someone else's body (either directly or through clothing)? This could involve the person putting their private areas on you.

Private areas include buttocks, inner thigh, breasts, groin, anus, vagina, penis, or testicles.

- Yes
- No → **Go to Question 71, page 9**

61a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

61b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

61c. Did you answer "Yes" to either Question 61a or 61b?

- Yes → **Continue to next column**
- No → **Go to Question 71, page 9**

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened.

	Yes	No
62. They continued even when you told them or showed them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
63. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.	<input type="checkbox"/>	<input type="checkbox"/>
64. They physically injured you.	<input type="checkbox"/>	<input type="checkbox"/>
65. They threatened to physically hurt you (or someone else).	<input type="checkbox"/>	<input type="checkbox"/>
66. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.	<input type="checkbox"/>	<input type="checkbox"/>
67. They did it when you were passed out, asleep, or unconscious.	<input type="checkbox"/>	<input type="checkbox"/>
68. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
69. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).	<input type="checkbox"/>	<input type="checkbox"/>

70. Did you answer "Yes" to any question from 62 to 69?

- Yes → **Go to End of Survey, page 10**
- No → **Go to Question 71, page 9**

71. Since 9/1/2013, did you have any unwanted experiences in which someone attempted to put a penis, an object, or any body part into your vagina, anus, or mouth, but no penetration actually occurred?

- Yes
- No → **Go to End of Survey, page 10**

71a. Was this unwanted experience (or any experiences like this if you had more than one) abusive or humiliating, or intended to be abusive or humiliating? If you aren't sure, choose the best answer.

- Yes
- No

71b. Do you believe the person did it for a sexual reason? For example, they did it because they were sexually aroused or to get sexually aroused. If you aren't sure, choose the best answer.

- Yes
- No

71c. Did you answer "Yes" to either Question 71a or 71b?

- Yes → **Continue to next column**
- No → **Go to End of Survey, page 10**

The following statements are about things that might have happened to you when you had this experience. In these statements, 'they' means the person or people who did this to you.

Please indicate which of the following happened.

	Yes	No
72. They continued even when you told them or showed them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
73. They used physical force to make you comply. For example, they grabbed your arm or used their body weight to hold you down.	<input type="checkbox"/>	<input type="checkbox"/>
74. They physically injured you.	<input type="checkbox"/>	<input type="checkbox"/>
75. They threatened to physically hurt you (or someone else).	<input type="checkbox"/>	<input type="checkbox"/>
76. They threatened you (or someone else) in some other way. For example, by using their position of authority, by spreading lies about you, or by getting you in trouble with authorities.	<input type="checkbox"/>	<input type="checkbox"/>
77. They did it when you were passed out, asleep, or unconscious.	<input type="checkbox"/>	<input type="checkbox"/>
78. They did it when you were so drunk, high, or drugged that you could not understand what was happening or could not show them that you were unwilling.	<input type="checkbox"/>	<input type="checkbox"/>
79. They tricked you into thinking that they were someone else or that they were allowed to do it for a professional purpose (like a person pretending to be a doctor).	<input type="checkbox"/>	<input type="checkbox"/>

End of Survey

This information will help improve the climate and safety of the U.S. military. You may have found that the questions did not completely cover your experiences. Nonetheless, the answers you provided are very important to this study.

Sometimes answering questions like the ones on this survey can be upsetting. If you feel you need support or would like to talk to someone, you can call:

- DoD Safe Helpline number (877-995-5247)
- Military Crisis Line (1-800-273-8255)
- RAINN (1-800-656-HOPE)

A SAFE helpline counselor can also explain how to report a sexual assault and how to find out the current status of a sexual assault report.

Thank you for completing the survey.

Please return your survey using the enclosed postage-paid envelope. No postage is needed.

If your return envelope has been misplaced, please mail your survey to:

2014 RAND Military Workplace Study
Westat 6236.02.14
1600 Research Blvd, RW 2634
Rockville, Maryland 20850-9973

Westat Survey Helpdesk toll free number: 1-855-365-5914
(OCONUS please call collect: 240-453-2620)

Supplementary Tables for Chapter Three

This appendix contains supplementary tables for Chapter Three.

Table C.1
Adjusted and Unadjusted Associations of Respondent Characteristics with Response for Active-Duty DoD Women

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^b	P-Value	Joint Test
Demographics									
Age in years as of 08/01/14 ^c	8-year increase	28.6	30.8	1.32	<0.0001		1.16	<0.0001	
Race-ethnicity						<0.0001		<0.0001	<0.0001
Non-Hispanic white (ref)		49.0	53.5	–	–				
Non-Hispanic black		27.1	25.2	0.85	<0.0001		0.91	<0.0001	
Hispanic		12.8	10.9	0.78	<0.0001		0.95	<0.0001	
Asian		4.5	4.8	0.97	0.0554		1.01	0.3998	
Other		6.6	5.6	0.77	<0.0001		0.95	<0.0001	
Marital status						<0.0001		<0.0001	<0.0001
Married (ref)		45.9	51.8	–	–				
Never married		44.6	37.3	0.74	<0.0001		0.93	<0.0001	
Divorced/separated/ other		9.4	10.9	1.02	0.0191		0.92	<0.0001	
Number of dependents ^c	1 additional dependent	0.9	1.0	1.09	<0.0001		1.01	<0.0001	
Education level						<0.0001		<0.0001	<0.0001
High school or less (ref)		58.0	44.9	–	–				
Some college		16.3	19.2	1.52	<0.0001		1.11	<0.0001	
Bachelor's degree		15.6	20.2	1.67	<0.0001		1.25	<0.0001	
Graduate degree		10.1	15.6	1.99	<0.0001		1.27	<0.0001	

Table C.1—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^d	P-Value	Joint Test
Military Career									
Service branch						<0.0001		<0.0001	<0.0001
Air Force (ref)		30.0	40.1	–	–				
Army		35.2	34.2	0.73	<0.0001		0.76	<0.0001	
Navy		27.8	20.3	0.55	<0.0001		0.61	<0.0001	
Marine Corps		7.0	5.4	0.58	<0.0001		0.68	<0.0001	
Pay grade						<0.0001		<0.0001	<0.0001
E1–E3 (ref)		23.6	14.9	–	–				
E4		20.6	16.1	1.24	<0.0001		1.19	<0.0001	
E5–E6		28.3	29.7	1.67	<0.0001		1.55	<0.0001	
E7–E9		8.0	11.8	2.34	<0.0001		2.14	<0.0001	
W1–W5		0.9	1.3	2.41	<0.0001		2.44	<0.0001	
O1–O3		12.5	16.4	2.08	<0.0001		1.91	<0.0001	
O4–O6		6.1	9.7	2.53	<0.0001		2.29	<0.0001	
AFQT percentile (enlisted only) ^c	18-percent increase	60.1	62.0	1.12	<0.0001		1.12	<0.0001	
Years of active military service ^c	7 additional years	7.1	8.7	1.29	<0.0001		1.08	<0.0001	
Deployment status						<0.0001		<0.0001	<0.0001
Never deployed (ref)		49.5	44.3	–	–				

Table C.1—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^b	P-Value	Joint Test
Months deployed since 9/11/2001	11 additional months	11.6	12.5	1.11	<0.0001		1.05	<0.0001	
Months deployed since 7/1/2013	3 additional months	2.8	3.0	1.08	<0.0001		1.10	<0.0001	
Separated/retired		4.5	0.6	0.13	<0.0001		0.13	<0.0001	
DoD occupational area						<0.0001		<0.0001	<0.0001
Infantry, guncrews, and seamanship specialists		4.3	3.4	1.14	<0.0001		1.00	0.9038	
Electronic equipment repairers		5.7	5.0	1.28	<0.0001		1.20	<0.0001	
Communications and intelligence specialists		8.5	8.0	1.39	<0.0001		1.15	<0.0001	
Health care specialists		12.4	14.0	1.65	<0.0001		1.34	<0.0001	
Other technical and allied specialists		2.3	2.5	1.56	<0.0001		1.25	<0.0001	
Functional support and administration (ref)		20.6	21.9	1.56	<0.0001		1.26	<0.0001	
Electrical/mechanical equipment repairers		10.6	7.2	–	–				
Craftworkers		2.0	1.4	0.98	0.4576		0.96	0.2282	
Service and supply handlers		11.1	7.8	1.03	0.0906		0.92	<0.0001	
Nonoccupational		2.9	1.5	0.77	<0.0001		0.83	<0.0001	

Table C.1—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^b	P-Value	Joint Test
Tactical operations officers		2.4	3.2	1.94	<0.0001		2.32	<0.0001	
Intelligence officers		1.4	2.0	2.03	<0.0001		2.36	<0.0001	
Engineering and maintenance officers		1.7	2.6	2.15	<0.0001		2.55	<0.0001	
Scientists and professionals		1.2	2.0	2.42	<0.0001		2.77	<0.0001	
Health care officers		7.7	11.0	2.09	<0.0001		2.48	<0.0001	
Administrators		2.2	3.3	2.24	<0.0001		2.64	<0.0001	
Supply, procurement, and allied officers		1.9	2.7	2.05	<0.0001		2.45	<0.0001	
Other officers (20, 21, 29)		0.8	0.7	1.30	<0.0001		1.73	<0.0001	
Unit location									
Continental United States (ref)		82.7	81.3						
Outside the continental United States		17.3	18.7	1.10	<0.0001		1.07	<0.0001	
Military Environment									
Percentage male in occupation ^c	15 additional percentage points	74.8	72.8	0.88	<0.0001		0.96	<0.0001	

Table C.1—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^b	P-Value	Joint Test
Size ^d of occupation group ^c	32,000 additional people	31,083.4	28,031.0	0.90	<0.0001		0.98	<0.0001	
Percentage male in unit ^c	11.2 additional percentage points	77.3	76.5	0.93	<0.0001		1.00	0.1151	
Size ^d of unit ^c	500 additional people	404.9	281.8	0.83	<0.0001		0.91	<0.0001	
Percentage male in installation (zip code) ^c	6.4 additional percentage points	82.2	81.9	0.95	<0.0001		0.99	<0.0001	
Size ^d in installation ^c	10,000 additional people	9,864.3	9,051.6	0.92	<0.0001		0.95	<0.0001	
Fieldwork Indicators									
Change in assigned unit zip since 08/01/2013		28.1	24.6	0.83	<0.0001		1.02	0.0101	
Change in assigned unit zip since 04/01/2014		19.9	14.6	0.69	<0.0001		0.72	<0.0001	
Change of mailing address since 04/01/2014		32.6	27.1	0.77	<0.0001		0.84	<0.0001	
No valid mailing address		2.0	1.4	0.70	<0.0001		0.84	<0.0001	
No valid email address		4.1	0.8	0.19	<0.0001		0.23	<0.0001	
Mailing 1 is postal nondeliverable		15.0	8.9	0.55	<0.0001		0.71	<0.0001	

Table C.1—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^b	P-Value	Joint Test
Marine Corps sent email		0.5	0.2	0.39	<0.0001		0.57	<0.0001	
Percentage of emails bounced ^c	10.4 percentage points	8.6	1.0	0.78	<0.0001		0.81	<0.0001	

NOTE: P-values from individual tests of significance are shown in the “P-Value” columns, while the p-values for a joint test come from a chi-square score test; shown in the “Joint Test” columns. Variables marked “ref” are the reference variables in their categories.

^a The binary variable is coded so that 1 = responded and 0 = did not respond, so that a risk ratio greater than 1 indicates a higher likelihood of responding for that category compared to the reference.

^b The adjusted risk ratio comes from a model that includes race/ethnicity (indicated levels), service branch, and pay grade.

^c Indicates variables entered as continuous, for which the risk ratio corresponds to one standard deviation change in the variable (standard deviations are listed in the column labeled “unit change for continuous variables”).

^d Size measured by number of people.

Table C.2
Adjusted and Unadjusted Associations of Respondent Characteristics with Response for Active-Duty DoD Men

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^b	P-Value	Joint Test
Demographics									
Age in years as of 08/01/2014 ^c	8-year increase	29.1	32.9	1.56	<0.0001		1.26	<0.0001	
Race-ethnicity						<0.0001			<0.0001
Non-Hispanic white (ref)		65.5	68.9	–	–				
Non-Hispanic black		14.6	13.4	0.87	<0.0001		0.97	0.0031	
Hispanic		11.7	10.1	0.82	<0.0001		1.03	0.0119	
Asian		3.7	4.2	1.09	<0.0001		1.18	<0.0001	
Other		4.5	3.5	0.73	<0.0001		0.96	0.0131	
Marital status						<0.0001			<0.0001
Married (ref)		57.9	72.6	–	–				
Never married		38.6	23.3	0.48	<0.0001		0.79	<0.0001	
Divorced/separated/ other		3.6	4.1	0.92	<0.0001		0.83	<0.0001	
Number of dependents ^c	1 additional dependent	1.5	2.0	1.19	<0.0001		1.05	<0.0001	
Education level						<0.0001			<0.0001
High school or less (ref)		67.7	48.7	–	–				
Some college		12.1	17.3	1.98	<0.0001		1.20	<0.0001	
Bachelor's degree		12.3	18.3	2.08	<0.0001		1.29	<0.0001	
Graduate degree		7.9	15.7	2.77	<0.0001		1.34	<0.0001	

Table C.2—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^d	P-Value	Joint Test
Military Career									
Service Branch						<0.0001			<0.0001
Air Force (ref)		22.8	34.1	–	–				
Army		38.7	37.4	0.65	<0.0001		0.67	<0.0001	
Navy		23.1	18.1	0.53	<0.0001		0.55	<0.0001	
Marine Corps		15.4	10.4	0.45	<0.0001		0.57	<0.0001	
Pay grade						<0.0001			<0.0001
E1–E3 (ref)		23.0	9.5	–	–				
E4		19.4	11.6	1.45	<0.0001		1.39	<0.0001	
E5–E6		29.9	31.6	2.57	<0.0001		2.43	<0.0001	
E7–E9		10.2	17.6	4.19	<0.0001		3.96	<0.0001	
W1–W5		1.6	2.7	4.26	<0.0001		4.43	<0.0001	
O1–O3		9.2	13.6	3.61	<0.0001		3.37	<0.0001	
O4–O6		6.7	13.4	4.91	<0.0001		4.47	<0.0001	
AFQT percentile (enlisted only) ^c	18-percent increase	63.9	65.5	1.09	<0.0001		1.09	<0.0001	
Years of active military service ^c	7 additional years	7.8	11.0	1.50	<0.0001		1.16	<0.0001	
Deployment status						<0.0001			<0.0001
Never deployed (ref)		40.0	28.2	–	–				

Table C.2—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^b	P-Value	Joint Test
Deployed before 08/01/2013		48.3	61.5	1.81	<0.0001		1.04	<0.0001	
Deployed after 08/01/2013		11.6	10.3	1.26	<0.0001		0.87	<0.0001	
Months deployed since 9/11/2001 ^c	11 additional months	14.6	16.3	1.14	<0.0001		1.06	<0.0001	
Months deployed since 7/1/2013 ^c	3 additional months	3.0	3.1	1.04	0.0013		1.10	<0.0001	
Separated/retired		4.4	0.7	0.16	<0.0001		0.17	<0.0001	
DoD occupational area						<0.0001			<0.0001
Infantry, guncrews, and seamanship specialists		15.7	8.6	0.63	<0.0001		0.71	<0.0001	
Electronic equipment repairers		8.1	7.5	1.06	<0.0001		1.10	<0.0001	
Communications and intelligence specialists		8.7	7.6	1.00	0.8353		0.99	0.4665	
Health care specialists		5.2	5.5	1.22	<0.0001		1.33	<0.0001	
Other technical and allied specialists		2.6	3.0	1.32	<0.0001		1.16	<0.0001	
Functional support and administration (ref)		8.9	11.0	1.41	<0.0001		1.27	<0.0001	
Electrical/mechanical equipment repairers		17.9	15.6	–	–				
Craftworkers		3.1	2.7	1.00	0.9541		1.01	0.4596	

Table C.2—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^b	P-Value	Joint Test
Service and supply handlers		9.3	7.4	0.90	<0.0001		0.92	<0.0001	
Nonoccupational		3.1	1.3	0.47	<0.0001		0.89	0.0001	
Tactical operations officers		7.1	11.6	1.86	<0.0001		4.08	<0.0001	
Intelligence officers		1.1	1.9	1.91	<0.0001		4.16	<0.0001	
Engineering and maintenance officers		2.5	4.7	2.14	<0.0001		4.71	<0.0001	
Scientists and professionals		1.1	2.2	2.32	<0.0001		4.75	<0.0001	
Health care officers		2.0	3.6	2.03	<0.0001		4.30	<0.0001	
Administrators		1.0	2.0	2.27	<0.0001		4.93	<0.0001	
Supply, procurement, and allied officers		1.4	2.5	2.03	<0.0001		4.46	<0.0001	
Other officers (20, 21, 29)		1.1	1.3	1.31	<0.0001		3.19	<0.0001	
Unit location									
Continental United States (ref)		82.4	80.5						
Outside the continental United States		17.6	19.5	1.13	<0.0001		1.10	<0.0001	

Table C.2—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^d	P-Value	Joint Test
Military Environment									
Percentage of occupation group that is male ^c	15 additional percentage points	86.1	84.8	0.85	<0.0001		0.88	<0.0001	
Size ^d of occupation group ^c	32,000 additional people	39,198.9	31,088.0	0.82	<0.0001		0.93	<0.0001	
Percentage male in unit ^c	11.2 additional percentage points	86.4	84.6	0.82	<0.0001		0.90	<0.0001	
Size ^d of unit ^c	500 additional people	393.7	281.8	0.81	<0.0001		0.94	<0.0001	
Percentage male in installation (zip code) ^c	6.4 additional percentage points	85.2	84.1	0.84	<0.0001		0.94	<0.0001	
Size ^d of installation ^c	10,000 additional people	11,847.4	9,852.0	0.86	<0.0001		0.92	<0.0001	
Fieldwork Indicators									
Change in assigned unit zip since 08/01/2013		26.3	21.8	0.78	<0.0001		1.05	<0.0001	
Change in assigned unit zip since 04/01/2014		19.6	15.6	0.76	<0.0001		0.79	<0.0001	
Change of mailing address since 04/01/2014		29.9	24.6	0.76	<0.0001		0.84	<0.0001	
No valid mailing address		2.2	1.3	0.58	<0.0001		0.89	<0.0001	

Table C.2—Continued

Participant Characteristics	Unit Change for Continuous Variables	Population and Full Sample Mean	Respondent Mean	Unadjusted RR ^a	P-Value	Joint Test	Adjusted RR ^b	P-Value	Joint Test
No valid email address		6.2	1.0	0.14	<0.0001		0.24	<0.0001	
Mailing 1 is postal nondeliverable		16.5	7.6	0.41	<0.0001		0.62	<0.0001	
Marine Corps sent email		1.8	0.4	0.20	<0.001		0.44	<0.0001	
Percentage of emails that bounced ^c	10.4 percentage points	10.8	1.2	0.78	<0.0001		0.83	<0.0001	

NOTE: P-values from individual tests of significance are shown in the "P-Value" columns, while the p-values for a joint test come from a chi-square score test; shown in the "Joint Test" columns. Variables marked "ref" are the reference variables in their categories.

^a The binary variable is coded so that 1 = responded and 0 = did not respond, so that a risk ratio greater than 1 indicates a higher likelihood of responding for that category compared to the reference.

^b The adjusted risk ratio comes from a model that includes race/ethnicity (indicated levels), service branch, and pay grade.

^c Indicates variables entered as continuous, for which the risk ratio corresponds to one standard deviation change in the variable (standard deviations are listed in the column labeled "unit change for continuous variables").

^d Size measured by number of people.

Table C.3
Design Effect of RMWS Weights for Key Reporting Categories

RMWS Weights	Sample Size	Mean	Standard Deviation	Design Effect
Gender				
Male	62,161	1.63	2.10	2.66
Female	53,598	0.33	0.22	1.43
Service				
Army	41,678	1.09	1.76	3.57
Navy	22,159	1.28	2.14	3.79
Air Force	42,572	0.67	0.59	1.78
Marine Corps	9,350	1.81	2.71	3.25
Pay grade				
E1–E5	29,687	1.72	3.00	4.04
E6–E9	55,271	0.89	0.77	1.75
O1–O3	17,259	0.67	0.53	1.63
O4–O6	13,542	0.58	0.37	1.40
Males				
Service				
Army	23,286	1.69	2.17	2.64
Navy	11,290	2.07	2.75	2.76
Air Force	21,103	1.10	0.59	1.28
Marine Corps	6,482	2.42	3.06	2.61
Pay grade				
E1–E5	13,105	3.29	3.98	2.46
E6–E9	32,214	1.32	0.75	1.33
O1–O3	8,515	1.09	0.46	1.18
O4–O6	8,327	0.81	0.28	1.12
Females				
Service				
Army	18,392	0.34	0.18	1.28
Navy	10,869	0.46	0.36	1.60
Air Force	21,469	0.25	0.05	1.04
Marine Corps	2,868	0.44	0.25	1.32
Pay grade				
E1–E5	16,582	0.48	0.33	1.48
E6–E9	23,057	0.29	0.09	1.09
O1–O3	8,744	0.26	0.06	1.05
O4–O6	5,215	0.21	0.03	1.02

Table C.4
Design Effect of WGRA Weights for Key Reporting Categories

WGRA Weights	Sample Size	Mean	Standard Deviation	Design Effect
Gender				
Male	62,161	1.63	1.53	1.87
Female	53,598	0.33	0.17	1.27
Service				
Army	41,678	1.09	1.47	2.79
Navy	22,159	1.28	1.49	2.35
Air Force	42,572	0.67	0.46	1.47
Marine Corps	9,350	1.81	1.94	2.14
Pay grade				
E1–E5	29,687	1.72	2.24	2.69
E6–E9	55,271	0.89	0.63	1.50
O1–O3	17,259	0.67	0.45	1.46
O4–O6	13,542	0.58	0.30	1.27
Males				
Service				
Army	23,286	1.69	1.74	2.06
Navy	11,290	2.07	1.73	1.70
Air Force	21,103	1.10	0.25	1.05
Marine Corps	6,482	2.42	2.05	1.72
Pay grade				
E1–E5	13,105	3.29	2.62	1.63
E6–E9	32,214	1.32	0.48	1.13
O1–O3	8,515	1.09	0.24	1.05
O4–O6	8,327	0.81	0.10	1.01
Females				
Service				
Army	18,392	0.34	0.16	1.22
Navy	10,869	0.46	0.25	1.30
Air Force	21,469	0.25	0.03	1.02
Marine Corps	2,868	0.44	0.18	1.16
Pay grade				
E1–E5	16,582	0.48	0.24	1.26
E6–E9	23,057	0.29	0.07	1.06
O1–O3	8,744	0.26	0.03	1.01
O4–O6	5,215	0.21	0.02	1.01

Table C.5
Balance of Weighted Respondents Relative to the DoD Active-Duty Population Mean of Proxy Variables

Proxy Variable for	Variable Name	Full Sample Mean with Design Weights (Percentage)	Respondent Mean with RMWS Weights (Percentage)	Respondent Mean with WGRA Weights (Percentage)
Discrimination	p_any_disc	3.15	3.14	3.11
<i>Quid pro quo</i>	p_any_quid	0.39	0.39	0.37
Hostile work environment	p_any_host	8.21	8.30	7.83
Sexual assault penetration	p_any_sa_pen	0.46	0.46	0.43
Sexual assault non-penetrative	p_any_sa_con	0.77	0.77	0.69
Sexual assault attempted	p_any_sa_att	0.02	0.02	0.02

Supplementary Tables for Chapter Seven

Table D.1
Changes in Top-Line MEO Violation Estimates as a Result of Programming Error, for Men by Service

MEO Violation	With Coding Error	Error Corrected	Change
Sexually Hostile Work Environment			
Army	7.65% (6.81–8.56)	7.65% (6.81–8.56)	+0.0059%
Navy	8.34% (7.02–9.81)	8.34% (7.02–9.81)	0.0000%
Air Force	3.26% (2.80–3.77)	3.26% (2.80–3.77)	0.0000%
Marine Corps	6.11% (4.76–7.70)	6.11% (4.76–7.70)	0.0000%
Coast Guard	3.74% (2.94–4.68)	3.74% (2.93–4.68)	0.0000%
Sexual Harassment			
Army	7.67% (6.83–8.58)	7.67% (6.83–8.59)	+0.0059%
Navy	8.37% (7.05–9.84)	8.37% (7.05–9.84)	0.0000%
Air Force	3.29% (2.82–3.80)	3.29% (2.82–3.80)	0.0000%
Marine Corps	6.11% (4.76–7.70)	6.11% (4.76–7.70)	0.0000%
Coast Guard	3.75% (2.94–4.69)	3.7472% (2.94–4.69)	0.0000%
Any MEO Violation			
Army	8.53% (7.67–9.45)	8.5290% (7.67–9.45)	0.0000%
Navy	9.61% (8.25–11.11)	9.6063% (8.25–11.11)	0.0000%
Air Force	3.84% (3.36–4.37)	3.8398% (3.36–4.37)	0.0000%
Marine Corps	6.65% (5.28–8.25)	6.6538% (5.28–8.25)	0.0000%
Coast Guard	4.51% (3.60–5.57)	4.5122% (3.60–5.57)	0.0000%

Table D.2
Changes in Top-Line MEO Violation Estimates as a Result of Programming Error, for Women by Service

MEO Violation	With Coding Error	Error Corrected	Change
Sexually Hostile Work Environment			
Army	22.87% (21.92–23.84)	22.92% (21.97–23.89)	+0.0472%
Navy	27.72% (26.21–29.26)	27.73% (26.23–29.27)	+0.0163%
Air Force	12.32% (11.72–12.95)	12.35% (11.74–12.98)	+0.0269%
Marine Corps	27.19% (24.68–29.80)	27.19% (24.68–29.80)	0.0000%
Coast Guard	19.15% (17.05–21.39)	19.27% (17.16–21.52)	+0.1227%
Sexual Harassment			
Army	23.07% (22.12–24.05)	23.12% (22.16–24.09)	+0.0473%
Navy	27.82% (26.31–29.36)	27.84% (26.33–29.38)	+0.0163%
Air Force	12.43% (11.82–13.07)	12.46% (11.84–13.09)	+0.0271%
Marine Corps	27.30% (24.79–29.92)	27.30% (24.79–29.92)	0.0000%
Coast Guard	19.19% (17.09–21.43)	19.32% (17.21–21.56)	+0.1229%
Any MEO Violation			
Army	28.62% (27.61–29.64)	28.65% (27.64–29.68)	+0.0325%
Navy	32.16% (30.62–33.72)	32.17% (30.63–33.74)	+0.0163%
Air Force	15.66% (14.99–16.35)	15.69% (15.02–16.38)	+0.0269%
Marine Corps	31.43% (28.85–34.11)	31.43% (28.84–34.11)	0.0000%
Coast Guard	23.32% (21.10–25.66)	23.44% (21.22–25.79)	+0.1228%

Table D.3
Changes in Top-Line MEO Violation Estimates as a Result of Programming Error, for Men by Pay Grade

MEO Violation	With Coding Error	Error Corrected	Change
Sexually Hostile Work Environment			
E1–E4	9.66% (8.54–10.87)	9.66% (8.54–10.87)	0.0000%
E5–E9	4.65% (4.25–5.08)	4.65% (4.25–5.08)	+0.0055%
O1–O3	4.48% (3.82–5.22)	4.48% (3.82–5.22)	0.0000%
O4–O6	2.06% (1.65–2.52)	2.06% (1.65–2.52)	0.0000%
Sexual Harassment			
E1–E4	9.68% (8.56–10.90)	9.68% (8.56–10.90)	0.0000%
E5–E9	4.67% (4.27–5.10)	4.68% (4.28–5.11)	0.0055%
O1–O3	4.52% (3.85–5.27)	4.52% (3.85–5.27)	0.0000%
O4–O6	2.06% (1.66–2.53)	2.06% (1.66–2.53)	0.0000%
Any MEO Violation			
E1–E4	10.37% (9.24–11.59)	10.37% (9.24–11.59)	0.0000%
E5–E9	5.62% (5.18–6.08)	5.62% (5.18–6.08)	0.0000%
O1–O3	5.26% (4.56–6.04)	5.26% (4.56–6.04)	0.0000%
O4–O6	3.15% (2.65–3.71)	3.15% (2.65–3.71)	0.0000%

NOTE: Too few warrant officers were included in the sample to break them out as a separate pay grade. For the purposes of this table, warrant officers have been included with the E5–E9 category.

Table D.4
Changes in Top-Line MEO Violation Estimates as a Result of Programming Error, for Women
by Pay Grade

MEO Violation	With Coding Error	Error Corrected	Change
Sexually Hostile Work Environment			
E1–E4	26.35% (25.20–27.53)	26.39% (25.24–27.57)	+0.0361%
E5–E9	18.08% (17.35–18.82)	18.10% (17.38–18.85)	+0.0295%
O1–O3	19.70% (18.52–20.93)	19.72% (18.54–20.95)	+0.0185%
O4–O6	9.43% (8.38–10.56)	9.43% (8.38–10.56)	0.0000%
Sexual Harassment			
E1–E4	26.53% (25.38–27.71)	26.57% (25.41–27.75)	+0.0363%
E5–E9	18.21% (17.48–18.96)	18.24% (17.51–18.99)	+0.0296%
O1–O3	19.85% (18.66–21.08)	19.87% (18.67–21.10)	+0.0185%
O4–O6	9.48% (8.43–10.62)	9.48% (8.43–10.62)	0.0000%
Any MEO Violation			
E1–E4	29.62% (28.43–30.82)	29.65% (28.47–30.85)	+0.0361%
E5–E9	23.20% (22.42–24.01)	23.22% (22.43–24.02)	+0.0155%
O1–O3	25.29% (24.01–26.60)	25.31% (24.02–26.62)	+0.0185%
O4–O6	17.78% (16.41–19.23)	17.78% (16.41–19.23)	0.0000%

NOTE: Too few warrant officers were included in the sample to break them out as a separate pay grade. For the purposes of this table, warrant officers have been included with the E5–E9 category.

Table D.5
Changes in Top-Line MEO Violation Estimates as a Result of Programming Error, for Reserve-Component Service Members by Gender

MEO Violation	With Coding Error	Error Corrected	Change
Sexually Hostile Work Environment			
Men	5.97% (4.62–7.58)	5.97% (4.62–7.58)	0.0000%
Women	13.53% (11.95–15.23)	13.55% (11.98–15.26)	+0.0267%
Sexual Harassment			
Men	5.98% (4.62–7.58)	5.98% (4.62–7.58)	0.0000%
Women	13.62% (12.04–15.32)	13.64% (12.06–15.35)	+0.0268%
Any MEO Violation			
Men	6.68% (5.30–8.29)	6.68% (5.30–8.29)	0.0000%
Women	18.12% (16.35–19.99)	18.14% (16.38–20.02)	+0.0268%

Abbreviations

AFQT	Armed Forces Qualifying Test
CI	confidence interval
DMDC	Defense Manpower Data Center
DoD	Department of Defense
GBM	Generalized Boosted Model
MEO	military equal opportunity
MSE	mean squared error
NA	not applicable
NR	not reportable
OSD	Office of the Secretary of Defense
PTSD	posttraumatic stress disorder
RMWS	RAND Military Workplace Study
RR	risk ratio
SAPR	Sexual Assault Prevention and Response
SAPRO	Sexual Assault Prevention and Response Office
UCMJ	Uniform Code of Military Justice
WGRA	Workplace and Gender Relations Survey of Active Duty Members
WGRR	Workplace and Gender Relations Survey of Reserve Component Members

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In early 2014, the Department of Defense Sexual Assault Prevention and Response Office asked the RAND National Defense Research Institute to conduct an independent assessment of the rates of sexual assault, sexual harassment, and gender discrimination in the military—an assessment last conducted in 2012 by the Department of Defense using the Workplace and Gender Relations Survey of Active Duty Members. The resulting RAND Military Workplace Study invited close to 560,000 U.S. service members to participate in a survey fielded in August and September of 2014. This volume presents the results of methodological investigations into sources of potential bias in the survey estimates for active- and reserve-component service members. It includes evaluations of follow-up studies of survey nonrespondents and the efficacy of sampling weights to correct nonresponse bias, an assessment of total survey error using an administrative records benchmark, estimates of potential under- and overcounting of service members exposed to sexual assault, comparisons of events identified by prior survey forms and the RAND forms, analysis of survey nonconsent and breakoff, and evaluation of service member tolerance of the RAND forms. In the final chapter, the report draws conclusions and recommendations for future administrations of sexual assault and harassment surveys in the military.



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