

COMMENTARY

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We are faced today with the need for a dramatically different model for reserve service. Employment of the reserves today differs greatly from that of the cold war, and from the first 20 years of the total force construct. Instead of a strategic force in reserve designed to be activated if the cold war turned hot, today's reserve force is an integral component of the U.S. military's operational forces. While this change has undoubtedly affected reservists and their families since the September 11, 2001 attacks, some would argue that the change began even earlier, with the end of the cold war, and was accelerated by the Persian Gulf war in 1990 and 1991.

The chapters in this section address three key questions concerning the new reserve model. First, in the face of significant changes in circumstances, how, if at all, have recruiting and retention changed for the reserve components? Second, how does the new reserve model, and its effects on recruiting and retention, affect force management and implementation of the volunteer force? Third, can the new reserve model be sustained indefinitely in the face of any significant changes it may have caused?

All three chapters address each of these questions to varying degrees, and present important findings that will help to redefine U.S. reserve forces. Each chapter uses different data and methods, which when considered together, offer a sometimes confusing collection of numbers, facts, and conclusions. This commentary attempts to simplify the interpretation—and our understanding—of their analyses and messages. I hope to connect the chapters and explain or resolve any seeming inconsistencies or contradictions. I also present an assessment of what these chapters suggest the new reserve model might mean for future recruiting and retention policies.

The Chapters

The chapter by Dolfini-Reed, Parcell, Gregory, and Horne is quite ambitious, as it considers retention in *all* the reserve components. This is a valuable contribution, as one can never be sure that results for one component will translate into the others because of important differences among them in terms of policy and deployment and

management practices. The chapter's other major strength is that since the authors calculate loss rates for each component individually, they try to incorporate into those estimates service-specific realities such as stop-loss and deployment patterns. The chapter also provides useful data about the number and percent of reservists activated and whether they were activated more than once.

Unfortunately, in calculating loss rates and assessing how they have changed over time, the authors inexplicably use fiscal year (FY) 2000 loss rates as their base—that is, as the benchmark against which subsequent rates are compared. I question why they did not use FY 2001 (which ended just 20 days after September 11, 2001) as the base. That one-year difference is important because rates in October 2001 are about 50 percent higher than those in FY 2000, but they have not changed significantly since. What happened during that period? What changed in FY 2001 to make reserve retention in October 2001 look so different from retention in FY 2000? Dolfini-Reed et al. do not address these questions.

Further, although the authors present loss rates and analysis of how loss rates have changed against their baseline, they offer no measures of statistical significance. That begs the question of how important any changes and differences might be, and also raises the issue of how much *practical* significance any changes might have, regardless of their statistical significance. The subject of practical significance is addressed at greater length below.

Despite these concerns, what does the analysis by Dolfini-Reed et al. show? At the minimum, it shows that across all components, since the start of the war on terror, loss rates may have increased. I say “may” because loss rates have changed little since October 2001. We can also conclude that activation length and multiple activations do not seem to have much effect on loss rates. To be complete in the analysis, though, requires noting that there are differences across services and there are also some counterintuitive results for some components, such as loss rates going *down* as activation lengths rise. Lastly, the analysis shows that deployment lowers loss rates among activated reservists. This is a key finding and may reveal a lot about reservists' attitudes regarding activation and how they are assigned and deployed after activation.

Dolfini-Reed et al. conclude that the retention consequences of the new reserve model are “not as bad” as many have thought or asserted. Strictly speaking, that is correct. But I think that conclusion seriously understates the significance of their findings. To me, their findings represent very good news and should be cause for optimism about the military's ability to manage the consequences of the new reserve model.

The chapter by Orvis, Lim, and Pint is different in several ways from the chapter by Dolfini-Reed et al. It does not examine all reserve components, but, rather, limits its analysis to the Army National Guard and the Army Reserve. To many, these two components have experienced the most important and significant changes as a result

of adopting the new reserve model. It is probably also true that the success or failure of the new model will hinge on its success or failure for the two Army components.

Orvis et al. use a different estimation method to calculate losses than Dolfini-Reed et al., but the results are roughly the same. That is, there is no clear trend in losses from the two Army components from 2003 to 2005. However, when the authors “standardize” loss rates across the years to account for activation status, they find FY 2004 loss rates increased over loss rates from both FY 2002 and 2003. But one year, of course, does not make a trend. Given that they use significantly different methodologies, these similarities between the two chapters are reassuring.

In terms of personnel gains (i.e., recruiting), Orvis et al. report declines since 2001, just as has been repeatedly reported in the news media. However, the authors did not address the question “Why?” Recruit quality is mostly unchanged, but we do not know from the authors’ analysis what other factors may have contributed to the decline, such as changes to recruiters’ goals, changes in the quantity and quality of other resources applied to the recruiting equation, changes in methods, or changes in any of the many other variables that affect recruiting results each year.

Finally, Orvis et al. provide a very good description of recent changes in the benefits offered to potential recruits and service members as incentives to enlist and reenlist. This aspect of their analysis is important, because their description implicitly emphasizes that most of the incentive improvements did not occur until 2005. In other words, the Army did not begin to react to the recent changes associated with the new reserve model, nor to the deployments to Iraq and Afghanistan, until several years after the new utilization model was put to use. Nonetheless, the effect of the new reserve model on recruiting and retention has not been dramatically adverse.

Taken as a whole, what does the analysis by Orvis et al. tell us? Like the picture Dolfini-Reed et al. draw, Orvis, Lim, and Pint’s results demonstrate that the recruiting and retention situation is far better than many stories in the popular media might suggest. The Army National Guard and Army Reserve are struggling with new challenges, but by and large they are successfully responding to the new force requirements.

Doyle’s interesting chapter is motivated by the same general questions as those raised by Dolfini-Reed et al. and Orvis et al., but uses a very different approach. Doyle’s analysis is based on an obvious, but not usually acknowledged, premise—that recruiting and retention are the result of more than activation (and the resulting compensation). Doyle argues that retention is also affected, perhaps significantly, by how activation policy is implemented and, perhaps most importantly, by how that implementation compares to the tastes and expectations of reserve members.

Doyle’s conclusions are based on a model of reserve decision making that estimates accession and retention behavior for reserve members as a function of the members’

expectations about likely activation and changes in monetary compensation. This same analytical perspective has been applied to other manpower issues, most notably to the retirement behavior of military officers, with good results. Using the approach here has promise, though Doyle only applies it to the behavior of a narrow subset of reservists: nonprior-service, 18-year-old male enlistees in the Army Reserve and the Army National Guard between 1997 and 2004.

Using this different approach, Doyle obtains results that are not inconsistent with those of Dolfini-Reed et al. and Orvis et al. That is, Doyle's analysis shows that activation will reduce accessions and retention among 18-year-olds, but practical amounts of additional monetary compensation can offset those effects. In other words, his findings suggest that the adverse effects of the new reserve model are manageable, even during wartime.

The Chapters Together

Considered together, these chapters are broadly consistent and tell a similar story. I would summarize and consolidate their conclusions this way: reservists' activation and the terms of their activation do not seem to have a significant effect on losses. Put another way, increases in the probability of activation do not appear to have a commensurate effect on the probability of retention. While some of the results reported in these chapters undeniably show adverse effects, I would argue that these effects are small and not of great consequence as a practical matter. Nor are these adverse consequences convincingly consistent across the three chapters or all of the reserve components.

This means that, for the most part, reservists understand and accept the new reserve model that entails more frequent and less predictable calls to active service. However, there is an important exception to this conclusion: deployment matters. Reservists who are activated for garrison duty in the United States—that is, to replace active duty units deployed to overseas theaters—exhibit greater losses than their counterparts who are activated and then deployed overseas. I do not find this surprising. Individuals who desire and welcome assignments to U.S. duty stations (driving a truck in and around Ft. Bragg, for instance), would more likely be serving in an active component than in the reserves.

To me, the analyses in these chapters further confirm the practical argument made by the Gates Commission, the panel whose recommendations formed the basis for the U.S. military's transition to an all-volunteer force.¹ Volunteerism inevitably leads to a force comprised of individuals who "like" service, for whatever reasons, and who will serve and not be deterred by what others may see as onerous terms of service. Indeed, they may even want assignments—such as those that include long shipboard duty or unusually high risk of injury or death—that those without their preferences for service would never accept. Thus, these conclusions serve as another piece of evidence

that the chronic skeptics of volunteerism are wrong. Volunteerism can work—and is working—even in the face of apparently adverse circumstances such as those the services face today.

The findings of the chapters discussed here should be cause for optimism. They suggest that the military can work through the current challenges. Of course, this is good news, especially in light of the negative news stories we have seen over the past few years. Regardless of whether one thinks that the military is managing well right now, these chapters present convincing evidence that the military *can* manage well, and that the new model of reserve service is not impractical. But it is important to note that these chapters, and especially the data Dolfini-Reed et al. present, suggest there may be important differences across reserve components. If the Department of Defense is to better manage reserve activation and employment of the reserve components, it needs to better understand the causes and policy implications of these differences.

Conclusion

It is reassuring that current research concludes the military can manage today's recruiting and retention challenges. It is especially reassuring since it is consistent with my observation that every past recruiting and retention problem I have examined has been a self-inflicted wound. For a variety of reasons, the services seem to forget from time to time how to recruit, or how to manage the constant challenge of retention. After a crisis presents itself, often with attendant news stories, the inevitable response occurs: form a "blue-ribbon panel," chaired by a retired general or admiral, to examine the problem and recommend solutions. The guilty service follows the recommendations and the problem (along with the news stories) disappears—until the next crisis.

This leads me to conclude with an appeal for perspective. We are in the midst of that next crisis, searching for solutions. Skeptics may argue that this crisis is significantly more challenging than others because it has been caused by a policy change to a new reserve model. But those skeptics ignore the experience of the much more significant policy change of transitioning from a force of conscripts to one of volunteers. So I am convinced, at least partly by these chapters, that solutions will be found, as they have been on every other occasion over the last 35 years since the country returned to volunteerism to staff its armed services.

Notes

1. Thomas S. Gates. 1970. *The Report of the President's Commission on an All-Volunteer Armed Force*, Washington, D.C.: U.S. Government Printing Office.