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14. ABSTRACT Three separate areas of military manpower are analyzed: the effects of retirement pay on quits, and of military service on lifetim earnings; and the quantity-quality trade-offs in the market for recruits.

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We analyze the effect of changes in quantity demanded on the supply of quality. Using data on military recruiting over an eight-year period in th 1990s provides sufficient variation in economic conditions and demand to estimate this relationship. Dividing the recruit population into its 70 initia processing stations provides interarea variation in unemployment, quantity demanded, and monetary enticements to test for the existence of a quantity-qualit trade-off. We find a significant negative relationship, with estimates of the elasticity of quality with respect to quantity ranging from –0.01 to –0.73 Furthermore, the effect of changes in unemployment at the local level dwarfs the effects of changes in demand. A one percentage-point reduction i unemployment requires either an additional 2.4 million dollars in enlistment bonuses or a reduction of 14% in the number of recruits demanded to maintai quality at its previous level.

15. SUBJECT TERMS

Finally, we look at lifetime earnings of former members of the United States All Volunteer Force and compare these to their civilian counterparts. Unlik previous studies, we use individual cross-sectional and longitudinal data and find a premium of 10.2% for veterans. Estimates using German data run in th opposite direction, although the penalty for military service is not so severe as for community service. The results imply that sorting into military servic appears to be very different in the US and Germany.

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Three Essays on the Economics of Military Manpower

by

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Three Essays on the Economics of Military Manpower

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Chapter 1: Introduction

The economics of military manpower is a broad area. All of the active duty armed forces personnel requirements as well as the National Guard, Reserves, and selective service fall under this heading. The multitude of categories covered includes recruiting, promotion, retention, retirement, job matching, compensation, and training.

There are many reasons why it is important to study the economics of military manpower. The military does not sell its services, so the market cannot efficient allocate resources. Furthermore, the Department of Defense is the largest employer in the United States. The expenditures on personnel are so large that they constitute a substantial part of gross national product. Finally, as with any public good or service, there is a question of equity of costs and benefits.

The military is not a competitive firm governed by market forces into efficient allocation of resources. The military does not sell its services or any production and therefore does not compete in any product market. Furthermore, Congress mandates the size of the military and its wage structure. While the military can recommend changes, it must look somewhere other than mere numbers to increase productivity.

The separation of budget authority from the military makes understanding the economics of defense a critical part of policy. Since Congress takes the military's recommendations and then decides what is best, there are no traditional market forces to help allocate resources. Policy decisions replace traditional market forces for much of the military's resource allocation. Certainly the military still uses markets in its demand for inputs. The acquisition of labor is one of its largest activities and must in the absence of a draft take place in a competitive labor market.

As the largest employer in the nation, the military expends considerable resources on manpower related issues. This includes not only wages and benefits for service members, but also recruiting and retirement benefits. While there are other firms that are large, part of what makes the military different is the lack of lateral entry. The only way to get a job in the military is to come in at the bottom and work your way up. This places a large demand on recruiting initial entrants into the military and on training.

The lack of lateral integration makes the military a perfect place to examine turnover in the labor market. With traditional firm data the number of employees at a plant or firm is known at various points in time, providing the researcher with net changes in employment. Rarely do economists have the opportunity to observe the actual churning. Since there is no lateral entry, the military allows one to track the same dwindling labor force over time and to look separately at new hires or recruits.

Since everyone who leaves the military (whether through retirement, completed tour, court martial, or death) must be replaced with recruits, the military demands a lot from the youth labor market. In the 1970's, at the beginning of the All-Volunteer Force (AVF), the military recruited roughly one-half million 17 to 24 year-olds annually. While the military has drawn down since the end of the Cold War, annual demand is still two hundred thousand recruits. This large demand of such a narrow age group suggests the military may have some power in the labor market for recruits; indeed, it may be a monopsony.

The restriction against lateral hiring means the military must provide all of the training it expects from service members. Whether the training is provided directly by the military, such as basic training, or indirectly, such as at college through scholarships from the Reserve Officer Training Corps (ROTC), the military still foots the bill. The extensive training needed for military specialties places a premium on finding the right initial job match for each recruit and on retaining highly skilled trained personnel.

The costs of retraining mean that the military must be deeply concerned with retaining the best of its trained personnel. But how does the military retain personnel? There are many methods the military has available to get individuals to choose to remain in the military. They can offer bonuses, key assignment, and other opportunities. What is the most efficient and useful way to retain personnel is a research question.

The change from conscription to a volunteer military in 1973 gives rise to a question of social equity. Who benefits and who pays the costs? At least some of the costs are borne by all taxpayers relative to their income. The danger involved in service, however, is not a shared burden among taxpayers. Are those who are at risk compensated accordingly?

Unfortunately most research to date indicates that volunteering for the military leads to reduced lifetime earnings. This suggests that the costs of service are multi-faceted. There is the cost associated with the possible loss of life in a conflict. There is also a supposed reduction in lifetime earnings.

The multi-faceted costs that seem to exceed any benefits suggest a conflict with classical economic theory. How do we explain individuals freely choosing an alternative that makes them worse off? This, too, is a question that awaits answers.

The need for objective policy analysis regarding the military has never been greater. The current situation in the military is best described by Colonel John D. Rosenberger in recent testimony before the Military Readiness Subcommittee of the House Committee on National Security:

> What I see is an Army reeling from the effects of decisions imposed upon it externally and internally: a sustained shortage of leaders and soldiers; high personnel turbulence created by an

> imbalance of force structure and national requirements; less-experienced leaders produced by a decreasing amount of time to serve in critical leader development positions; insufficient money at every level of command to train as necessary to sustain combat proficiency at home station; expanding peacekeeping operations which quickly erode warfighting knowledge, skill, and ability, creating a growing generation of young leaders who do not know how to fight as members of a combined arms team; increasing numbers of soldiers diverted from combat training to perform installation support services, backfilling cuts in the civilian work force and severe cuts in contractual support; and an absence of time and opportunity to focus, in a predictable fashion, on battle-focused training.[14]

Throughout the 45 years of the Cold War America rarely deployed its troops (Korea, Berlin Blockade, Vietnam, Dominican Republic, and Grenada). Since the end of the Cold War the number of deployments, even if we count multiple trips to the same destination as one, are too numerous to provide a comprehensive list (Panama, Haiti, Somalia, Persian Gulf, Bosnia, Macedonia, Kosovo, etc). The greater demands for deployment come despite a force less than one-quarter the size of the post Vietnam force.

The increased demands placed on fewer troops require efficient allocation of manpower by the US as well as other countries. Not only has the US increasingly become involved in a large number of peacekeeping, peacemaking, and warfighting operations, but so have England, France, Germany, and others. The need to understand the specifics of military manpower, to include differences across countries, is ripe for research.

The focus of this thesis is threefold. We want to examine the effects of changes in military retirement pay on the retention of soldiers. The possible monopsony power of the military in the market for recruits demands

[14] Quote is extracted from an email containing the text of COL Rosenberger's statement to the House Subcommittee on February 26, 1999.

investigation. And, lastly we examine the returns to military service and try to bring the empirical results into line with theory.

The military's ability to maintain a quality force requires it to retain the services of some of those it has trained. One of the components of compensation that may affect retention is retirement pay. Since retirement benefits begin immediately upon retirement after twenty years of service, these benefits may seem quite extensive and may induce enlistees to remain in the military until retirement.

Congress has twice thought the benefits accorded to military retirees were too high and reduced them, affording us the opportunity to examine the impact. While the initial change essentially affected everyone differentially, the change in 1986 is much cleaner. The latest change effectively reduces retirement pay by 20 percent. This coupled with the military's recruiting policies, creates a natural experiment that we exploit to understand the effects of retirement pay on retention.

The need for an accurate assessment of the effects of retirement pay on retention is pressing. Congress is currently debating changing the retirement system once again. Without understanding the effects of any changes, policy analysis cannot proceed. The effects are necessary to understand the full range of costs and benefits involved with each of the programs being debated.

The vast number of high school graduates that the military recruits annually combined with little flexibility in compensation lets us examine whether or not there is a trade-off between the quantity of recruits demanded and the quality received. This question has ramifications for all of labor economics. Is there a point at which the level of quality supplied deteriorates, or can a firm expect an indefinite supply of a set standard of quality regardless how much labor it demands?

The implications for the military of a quantity-quality trade-off are numerous as well. If it exists this means that maintaining quality in the face of increased demand requires additional resources. Furthermore, comparisons of pay within the military to supposedly comparable jobs in the civilian market are not sufficient if quality has not been accounted for. Finally, finding the magnitude of this trade-off if it exists is essential for policy analysis determining military wages and recruiting budgets. The existence of a quantity-quality trade-off reinforces the possibility of the military possessing monopsony power in the market for recruits.

Previous estimates of a negative return to voluntary military service require further empirical investigation and theoretical analysis. Since neoclassical theory is based upon rational individuals, the negative returns to a freely chosen act must have an explanation. Part of the issue may be an estimation problem arising from the need to account for selection in the returns. This is necessary because individuals choose whether or not to serve, and differences between those who serve and those who do not are probably not random.

Other than explaining a possible contradiction to theory, the returns to military service are important for a number of reasons. Low returns may be one of the underlying causes of the current scarcity of recruits. If the returns are negative and individuals are just now realizing it, they will become increasingly unlikely to choose to serve.

The variety of systems for acquiring military manpower across countries leads to more questions about the returns to military service. With multinational data is it possible to decompose the returns to military service? Is military service rewarded or punished similarly across countries? We use US and German data to examine these questions and the returns to military service.

The intent of this dissertation is to provide a greater understanding of some of the processes that can affect the military's readiness. The remainder is divided into four chapters. Chapter 2 addresses the question of the effects of changes in retirement pay on retention of Army enlistees. Chapter 3 examines the market for military recruits to test for a quantity-quality trade-off and determine its magnitude. Chapter 4 uses multi-country data to estimate the returns to military service and attempts to decompose these returns into separate skill enhancing and human capital depreciating components. Finally, Chapter 5 summarizes the findings and puts them into context of the problems being addressed. In the process areas for future research are identified.

Chapter 2: Retirement Pay: Does it Matter?

I. Introduction

In 1986 the United States Congress overhauled the military retirement system, reducing benefits for future retirees by as much as 20 percent. This exogenous change by itself amounts to a natural experiment. However, the experiment is cleaner than one expects due to the military's delayed entry program. The delayed entry program allows an individual to sign his enlistment contract and delay military service up to a year. The effect is that in the months immediately following the change in the retirement system recruits entering active duty together were governed by different retirement systems. Except for the retirement system they fell under, these recruits faced identical environmental changes, allowing us to focus solely on the effect of the change in the retirement system on retention.

The exogenous nature of the change in the retirement system is evident when the events leading up to it and surrounding it are reviewed. Prior to the 1986 change, Congress had reviewed the military retirement system and proposed changes six times in the previous twenty years. Five of these six times resulted in no change, while the other resulted in only a change from a high year to high three years for retirement pay calculation; certainly, a minor change in the view of potential recruits. Furthermore, the initial proposals for the 1986 change did not include a grandfather clause; the change if approved would have affected current and future retirees. Finally, the actual bill was not signed into law until weeks before the actual change and received little publicity. The presidential approval was not carried by UPI or AP services and the passage in Congress was buried on page A29 of the New York Times.[15] The result is potential recruits had no reason to believe the military retirement system would change, and when it did are unlikely to have known about it. We, therefore, assume throughout that the change in the retirement system had no effect on the date and therefore the governing retirement system that the recruit entered military service under.

While the framework exists for analyzing this natural experiment, why should we be interested? In a hearing before the Senate Armed Services Committee on September 29, 1998, General Henry H. Shelton, Chairman of the Joint Chiefs of Staff, warned US military "readiness is fraying and [that] the long-term health of the total force is in jeopardy" (Garamone, page 1). General Shelton clarified this by adding:

> Another key factor seriously affecting our force today is the different retirement system for the most junior two-thirds of the force. In 1986, Congress changed the Armed Forces retirement system to one that is increasingly perceived by our military members as simply not good enough to justify making a career of the military service.
>
> If we fail to address these critical personnel issues, we will put at risk one of our greatest achievements of the last quarter century: the All-Volunteer Force (Shelton, page 3).

These comments as well as similar comments by the Chief of Staff for each of the military services to the same Senate Committee are the newest and gloomiest addition in a series of concerns over compensation for the military. While one would be remiss to doubt the sincerity of these comments, are they factual? Indeed, while service members are no doubt complaining about the change in the retirement system, is this really a problem, or are there other factors at play?

A quick glance at Figure 1 shows that substantial sums of money are paid to military retirees. The total for 1997 exceeded ten billion dollars. Since the

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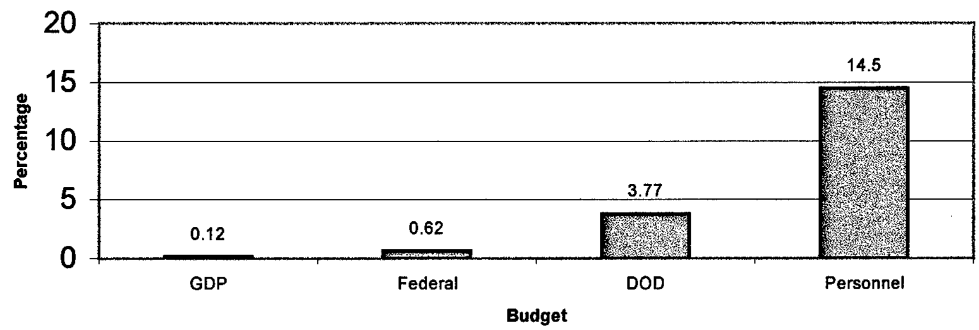
[15] These results are based upon an extensive search using Lexis-Nexis.

change in the retirement system occurred less than twenty years ago, no savings have yet been realized from it.[16] The present value of the projected savings, discounted at the current inflation rate, is in the neighborhood of 58 billion dollars. While there can always be debate over the appropriate discount rate, how much the military has downsized, and the life expectancy of retirees, there can be no doubt that we are talking about substantial sums of money. With this much at stake, an empirical analysis of the effects of the change in the retirement system on retention is critical. Yet, to date, no published research has attempted to estimate retention elasticities of retirement compensation based upon the data from the change to the REDUX retirement system. (The different retirement systems will be explained shortly.)

Figure 1. Expenditures on Military Retirement 1997



Since before the beginning of the All Volunteer Force (AVF) in 1973, the United States Armed Forces have been interested in determining the components of military compensation and advertising that are most cost effective at recruiting and retaining high quality individuals. This interest has spawned numerous studies over the years that have tried to estimate various elasticities. Some studies have tried to focus on a narrow range of elasticities - only looking at pay and

[16] Individuals are not vested until after completing twenty years of service.

bonuses or recruiting separately - while others have tried to be more comprehensive, taking into account these plus unemployment rates and advertising.

To a large extent studies of military manpower have been hampered by a lack of relevant data that are only becoming available as time progresses. While there are data available before 1973, estimates based upon AVF and pre-AVF data are likely to be biased, as they reflect dissimilar populations. Even worse is that there is no clear direction for the bias. Attempts to evaluate any elasticities related to retirement have been further hampered by a lack of significant changes in the retirement system until 1986. Only now, twelve years later, are we in a position to start examining the long-term effects of this change.

Retirement benefits are one part of an overall compensation package that can be changed to entice more and better recruits to join the military and to entice the best to stay. Therefore, in addition to addressing the Chiefs' of Staff concerns, knowledge of these elasticities is necessary to be able to provide a cost-minimizing scheme.

This chapter is divided into five sections. Section I has given a brief introduction to the question at hand and why it is important. Section II describes the series of retirement systems that have been in force since the emergence of the AVF. Section III specifies and examines the theoretical model that is estimated in Section IV. Finally, Section V summarizes the analysis and provides conclusions.

II. The US Armed Forces Retirement System 1973 - Present[17]

We must preface the exposition of the various retirement systems to note that the military is exempt from federal law requiring vesting in a retirement program at five years. Indeed, the military provides no retirement benefits for

[17]This exposition on the Armed Forces retirement system and its changes does not include changes in disability or retired disability compensation.

individuals who do not complete twenty years of service.[18] A summary of the various retirement systems is provided in Table 1.

Table 1. Summary of Military Retirement Systems (1973-Present).

Type	Entered Service	Percentage	Of	Min	Max
ORIGINAL	1/1/1973-9/7/1980	.025*YOS	Highest	50%	75%
FULL	9/8/1980-7/31/1986	.025*YOS	High 3	50%	75%
REDUX	8/1/1986-Present	.025*YOS-.01*(30-YOS)	High 3	40%	75%

Note: Vesting occurs at 20 Years of Service under all Programs. No benefits are received prior to the 20 Year mark.

A. Original AVF Retirement System

The US military retirement system remained unchanged with the emergence of the AVF in 1973. Under this system individuals are eligible to retire after 20 years of service, potentially allowing someone as young as 37 to retire. Unlike most pensions, there is no minimum age for receiving benefits; the day after retirement, the retiree begins to receive her retirement pay. Retirement pay is based upon years of service and the rank at which the individual retired. The amount is calculated by taking a percentage of the base pay for the highest rank attained and multiplying it by 2.5 percent for each year of service. Thus, an individual who retires with twenty years of service receives half of their base pay in retirement pay.[19] This system continues to cover all individuals who entered any of the armed forces prior to September 8, 1980.

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[18] Disability retirement is the exception and is ignored throughout.

[19] Note that a serviceperson's base pay accounts for only about 80 percent of their activity duty compensation, so that the individual retiring at twenty years does not receive half of their normal compensation, but only about forty percent.

B. Revised Full Retirement System

Individuals entering service from September 8, 1980 through July 31, 1986 fall under a slightly different system. Instead of basing retirement pay upon a percentage of the retired grade base pay, retirement pay is based upon the same percentage of the average of the high three years. This means that an individual can retire at one grade, having just been promoted, and be paid for retirement at a lower grade. Undoubtedly this system has reduced the compensation; the question of how much is ambiguous. Individuals who retire at a rank held for the previous three years that were not eligible for time in service raises will see no decrease. On the other hand, individuals who not only get promoted immediately prior to retiring, but also receive two longevity raises in the past three years will see a significant difference. For example, consider an army First Sergeant promoted the day she retires at twenty years. Instead of receiving half of $2,713.50, as she would have under the old system based upon the 1998 pay scale, she would receive half of the average of $2,298.90, $2,232.00, and $2,121.00. The difference is just over $248 per month, or a reduction of roughly nineteen- percent in compensation. Unfortunately, with the wide variety of pay differentials possible, even if we focus on retirement at twenty years, recovering the retention elasticities empirically is daunting and requires strong and unrealistic assumptions. For these reasons we have found no work that uses the difference between this system and the original retirement scheme to discern elasticities associated with retirement.

C. REDUX Retirement System

The REDUX retirement system covers those individuals entering service on or after August 1, 1986, and is not quite what most enlistees believe. The information given to recruiters and commanders, as well as service members and their families is simply a change in the percentage of the high three years of base pay. As explained by every official document or site we could find, the percentage is similar to the revised full system except that it is reduced by one percent for each year short of thirty served. This is what is printed in the Army's Retiring Soldiers and Families Handbook and what is listed on Department of Defense web sites, and what is commonly understood throughout the military, but it is not the whole story[20].

The entire REDUX retirement system is more of a reduction than generally understood, with a surprise benefit as well. The formula for calculating retired pay at retirement is as commonly understood, but the pay is not directly indexed to the Consumer's Price Index (CPI) as with other the other retirement systems. Instead, the retirement pay is indexed one point below the CPI. While it is not clear if this can result in a nominal reduction in pay, the result is diminished buying power. To complicate the system more, REDUX changes the percentage of the high three years of base pay when an individual reaches age 62. At 62 the percentage is calculated according to the previous retirement system and the base pay is fully adjusted this one time according to the CPI. Each year after age 62, however, the retirement pay erodes as the index returns to one point below the CPI.

Since the major change in the system was the new formula for calculating retirement pay, we focus solely on this component. We will assume that this accurately reflects REDUX as it is understood by enlistees. This assumption will carry throughout, although we will return to examine its significance, when we draw conclusions from the data.

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[20] The only place the author found the specifics on the REDUX retirement system was in a footnote of Asch, Johnson, and Warner (1998) and buried deep in an Air Force Finance Regulation. The Army's *Handbook for Retiring Soldiers and Their Families* explains only the reduction as do Army and Joint Finance regulations.

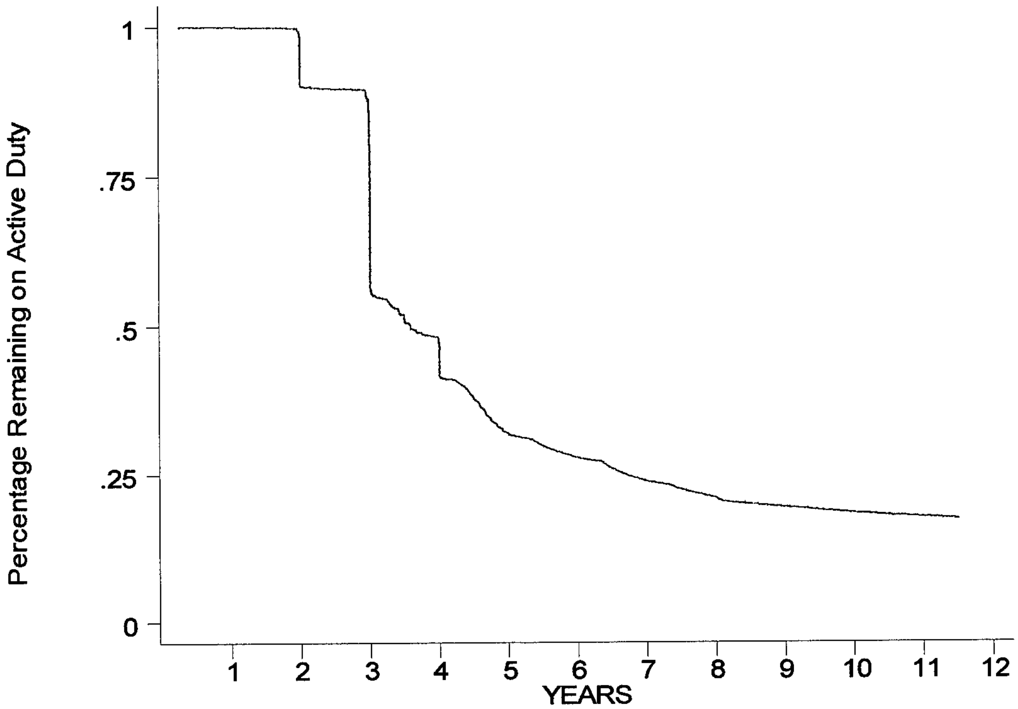
III. THEORETICAL MODEL

A great deal of work has been done on military compensation and the reenlistment decision. An overview of the development of these models is available in *The Handbook of Defense Economics* (1995), and in Daula and Moffitt (1995). Instead of reviewing the emergence of the different models, we will briefly describe the two major models and suggest a third as an alternative to estimate the reenlistment elasticity of retirement pay. However, before we get to the models, we must have a clear picture of some of the peculiarities of the military and military service.

The choice of staying or leaving the military for enlisted personnel comes in two forms. The first is when it is essentially the military's choice, and the other is when it is the individual's choice. The great majority of terminations by the military occur within the service member's first enlistment period. These individuals, for one reason or another, have proven themselves incapable or unwilling to meet the necessary minimum standards, whether physical, mental, or moral, to serve. The individual who successfully completes a term of enlistment has the option to stay or go. This is a simple binary choice put to the individual. If the individual decides to stay, he will be faced with a similar choice at the end of the new term of enlistment. Figure 2 depicts the probability of having left the service at any particular point in time. This includes both those whom the military has decided to remove from service and the decision of individuals.

Figure 2 mixes two very different decisions or processes. The decision by the military to remove someone from the service or even merely not to allow them to continue is decidedly different from an individual who has been offered the opportunity to reenlist and decides to enter the civilian sector instead. While the decision to stay or go might seem to occur over a continuous period, this is not the case.[21] The service member signs an enlistment contract for two to six years upon initial enlistment and also reenlists for the similar periods.[22]

Figure 2. Survivor Function for Military Enlistees



Using standard classical economic theory, a choice whether or not to change occupations is simply a comparison of the expected utility from staying versus the expected utility from the new profession. While the expected utility of leaving involves lots of uncertainty, the structure of the military makes the utility of staying almost certain. This framework is the underlying assumption used for two of the three methodologies that we will examine. We will focus first upon the Annualized Cost Of Leaving (ACOL) model, then the dynamic programming model, and finally a duration model.

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21 At the twenty year mark a service member can apply for retirement at any time, giving a minimum of 60 days notice. Therefore the decision to stay or leave becomes continuous over the interval 20 - 30 years of service.

22 Extensions are the exception to this rule and occur with enough irregularity that they are ignored.

A. ACOL

Warner and Goldberg (1984) first introduced the Annualized Cost Of Leaving (ACOL) methodology. This model of the reenlistment decision is based upon utility maximization, where the agent compares the utility from reenlisting to that from leaving for the civilian sector:

$$\sum_{i=1}^{n}\left(M_i^m + u_m\right)\beta^i + \sum_{i=n+1}^{T}\left(M_i^{mr} + M_i^c + u_c\right)\beta^i > \sum_{i=1}^{T}\left(M_i^c + u_c\right)\beta^i \qquad (1)$$

where M represents monetary compensation due to military pay, m, military retirement, mr, or civilian pay, c; β represents the discount rate; and u is the utility derived from non-pecuniary aspects of civilian, c, or military, m, life. In simple terms the right hand side is the present value of all future utility from staying in the military and the left-hand side is the present value of all future utility if the individual leaves. Rearranging terms, we can isolate the observables and unobservables:

$$\sum_{i=1}^{n}\left(M_i^m\right)\beta^i + \sum_{i=n+1}^{T}\left(M_i^{mr} + M_i^c\right)\beta^i - \sum_{i=1}^{T}\left(M_i^c\right)\beta^i > \sum_{i=1}^{n}\left(u_c - u_m\right)\beta^i \qquad (2)$$

This provides us with a the present value in dollars of the difference in taste for military versus civilian life. Dividing through by $\sum_{i=1}^{n}\beta^i$ and assuming that preferences are constant over time for any individual, we obtain the annualized cost of leaving (ACOL):

$$A_n = \frac{\sum_{i=1}^{n}\left(M_i^m\right)\beta^i + \sum_{i=n+1}^{T}\left(M_i^{mr} + M_i^c\right)\beta^i - \sum_{i=1}^{T}\left(M_i^c\right)\beta^i}{\sum_{i=1}^{n}\beta^i} > \left(u_c - u_m\right) = u \qquad (3)$$

The decision to stay in the military or leave is then a simple comparison of the ACOL and the taste difference, u. Yet, this is not an elementary calculation, as the ACOL for all possible term lengths of remaining in the service must be computed, and if any one exceeds the taste difference, the individual will choose to reenlist. Thus the relevant ACOL is the maximum of A_n over the remaining military time horizon, which is denoted A^*. If we assume that u is normally distributed, then the probability of reenlistment can be modeled with a simple probit. Obviously, other distributional assumptions can lead to different binary choice models.

Using a probit analysis on Navy enlistees, Warner and Goldberg (1984) estimate pay elasticities but cannot estimate retirement elasticities due to a lack of variation in their data set (all individuals fell under the same retirement system). With a more recent data set, this method could be used to recover retirement elasticities; however, as previously stated, the restrictive assumptions of this model are not necessary for our research. Specifically, the assumption of tastes being fixed over time and also the assumption of perfect foresight of income streams and non-pecuniary benefits over an individual's lifetime are burdensome and not necessary. Additionally, the model does not account for distributional changes as progressive reenlistment decisions are made. This means each successive group remaining in the military is self-selected; since the ACOL model fails to account for this selection, any results are necessarily biased.

B. Dynamic Programming

Daula and Moffitt (1995) extend the ACOL framework by putting the problem in context of a dynamic programming model where the individual is faced with similar choices each time she is up for reenlistment. This framework allows them to account for sample selection bias, as the choice is modeled dynamically. Letting V represent the expected utility of the alternatives, U represent the compensation, and assuming all future compensation from civilian opportunities is known, the decision becomes[23]:

$$V_t^{stay} = U_t^M + \beta E_t V_{t+1} + \varepsilon_t^M \quad (4)$$

$$V_t^{leave} = U_t^C + \sum_{i=t+1}^{T} \beta^{i-1} U_i^C \tag{5}$$

$$V_t = Max\left(V_t^{stay}, V_t^{leave}\right) \tag{6}$$

where β is the time preference/discount rate, T is the overall time horizon, C denotes civilian, M is for military, and the ε represents uncertainty. Military compensation is a function of rank, years of service, marital status, bonuses, and special pay, so the expected value of future military compensation is the sum of the probability weighted flows from achieving different ranks at different years of service. V_t, obviously, is not observed; instead the decision to stay is observed. This requires the use of a latent variable which we will denote R^*. Thus the decision to re-enlist can be represented as:

$$\begin{aligned} R_t &= 1 \quad if \quad R_t^* \geq 0 \quad stay \\ R_t &= 0 \quad if \quad R_t^* < 0 \quad leave \end{aligned} \tag{7}$$

$$R_t^* = V_t^{stay} - V_t^{leave}$$

$$= \left[U_t^M + \beta E_t V_{t+1} + \varepsilon_t^M\right] - \left[U_t^C + \sum_{i=t+1}^{T} \beta^{i-1} U_i^C + \varepsilon_t^C\right]$$

$$= \left[U_t^M - U_t^C - \sum_{i=t+1}^{T} \beta^{i-1} U_i^C\right] + \left[\beta E_t V_{t+1} + \varepsilon_t^M - \varepsilon_t^C\right]$$

$$= \alpha_t + \varepsilon_t \tag{8}$$

This means the index function for R can be represented as a function of a non-stochastic element, α, and a stochastic component, ε. Thus, the assumption on the distribution of ε will determine the appropriate binary choice model.

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23 While the notation differs, this follows Daula and Moffitt (1995) directly.

Daula and Moffitt (1995) show that by assuming $\varepsilon_t \sim N(0,\sigma_\varepsilon^2)$ and letting ϕ and Φ represent the standard normal density and cumulative distribution functions, we obtain the following:

$$\varepsilon_t = \varepsilon_t^M - \varepsilon_t^C \tag{9}$$

$$\alpha_t = U_t^M - U_t^C + \sum_{\tau=t+1}^{T} \beta^{\tau-t} r_\tau \left(U_\tau^M - U_\tau^C\right) + \sigma_\varepsilon \sum_{\tau=t+1}^{T} \beta^{\tau-t} r_{\tau-1} \phi\left(\frac{\alpha_t}{\sigma_\varepsilon}\right) \tag{10}$$

$$\text{where} \quad r_\tau = \prod_{k=t+1}^{\tau} \Phi\left(\frac{\alpha_k}{\sigma_\varepsilon}\right) \quad \textit{for} \quad \tau \geq t+1$$

$$= 1 \quad \textit{for} \quad \tau = t \tag{11}$$

The reenlistment decision is based upon tastes in addition to mere compensation difference. This requires adding a vector of observable characteristics, X (race, marital status, education, etc.), to allow for varying tastes for the military. Denoting the effects of the observable characteristics as the vector γ, equation 10 becomes:

$$\alpha_t = U_t^M - U_t^C + X\gamma + \sum_{\tau=t+1}^{T} \beta^{\tau-t} r_\tau \left(U_\tau^M - U_\tau^C + X\gamma\right) + \sigma_\varepsilon \sum_{\tau=t+1}^{T} \beta^{\tau-t} r_{\tau-1} \phi\left(\frac{\alpha_t}{\sigma_\varepsilon}\right) \tag{12}$$

Estimation at this point, while not trivial, is straightforward using probit analysis.

Daula and Moffitt's (1995) model is an extraordinary step forward and goes a step beyond this point to include individual heterogeneity. However, the model ignores uncertainty in the civilian market while accounting for it in the military market. The assumptions in this model, while more realistic than ACOL, are not necessary to estimate the reenlistment elasticity of retirement pay and therefore are overly restrictive for this research.

C. Duration Model

Duration, or survival models have been used in a variety of economic analyses. However to our knowledge this framework has never been used to examine any components of the reenlistment decision. There are several reasons why this technique has not been used, probably the most important being the difficulty in recovering pertinent elasticities from estimation. As we will see in a moment, this is not a problem in these data as long as we focus our attention on the elasticity of retention with respect to the change in the retirement system.

With this in mind, the reenlistment decision process can be thought of as involving a number of discrete decisions over time, where the individuals depart the service with positive probability at each reenlistment opportunity. Therefore, our spells are the length of time that an individual remains in the service. Generally we have:

$$T \sim f(t) \qquad \text{density function} \qquad (13)$$

$$\Pr(T \le t) = \sum_{s=o}^{t} f(s) = F(t) \qquad \text{distribution function} \qquad (14)$$

$$\Pr(T > t) = 1 - F(t) = S(t) \qquad \text{survival function} \qquad (15)$$

The hazard rate is then the probability of exit at t conditional upon reaching t or:

$$\lambda(t) = \frac{f(t)}{S(t)} \qquad \text{hazard function} \qquad (16)$$

Estimation proceeds after specifying the functional form of the hazard. Prior to looking at the data, Cox's proportional hazard model seems likely to be the preferred specification because it allows us to estimate multiplicative changes to a baseline hazard due to observable covariates.

$$\lambda_i(t) = \lambda(t) e^{(x_i'\beta)} \qquad (17)$$

The proportional hazard model allows us to estimate the effects of the covariates consistently without specifying the baseline hazard. As shown by Cox (1975), we need simply maximize the partial likelihood:

$$L = \prod_{i=1}^{s} \frac{e_i^{X_i \beta}}{\sum_{h \in R(t_i)} e^{X_h \beta}} \tag{18}$$

where i represents the duration ordered observations, and $R(t_i)$ represents all spells that survived to t_i.

IV. DATA ANALYSIS.

The data are an extract from the Army's Enlisted Master File maintained by the Army's Office of Economic and Manpower Analysis and contain the population of all enlisted personnel who came on active duty in the Army during fiscal years 1986 and 1987 (October 1, 1985 - September 30, 1987). The data include observations on these individuals through August 1998 or until departure from the Army, whichever occurs first. The resulting maximum spell length is twelve years.

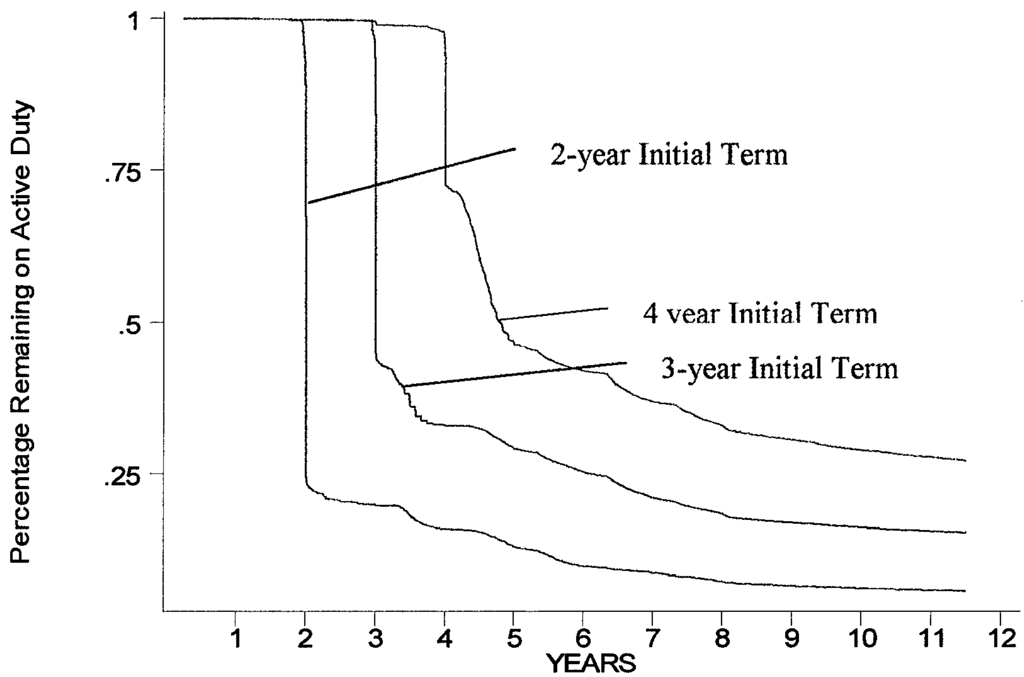
Table 2. Tabulation of Observations

Total Observations	246744	
Six Month Sample	67736	27.45%
NPS Retirement Sytem Known	62337	92.03%
Individuals with Choice	29636	43.75%
Still on Active Duty (8/31/98)	5115	17.26%
Chose to Depart for Civilain Sector	24521	82.74%
Individuals Who Completed First Term	25596	37.79%
Still on Active Duty (8/31/98)	5115	19.98%
Chose to Depart for Civilain Sector	20481	80.02%

Focusing on the reenlistment decision necessitates restricting the population in question to those individuals who have a choice to reenlist. This results in dropping a variety of individuals (see Table 2), from those who were not able to complete basic training to those who were court martialed and subsequently discharged, to those who were medically discharged. Additionally, it is reasonable to expect prior service individuals to behave different from non-prior service enlistees (NPS); therefore, the former also are dropped.

The resulting population is still quite heterogeneous. Individuals who enlist initially for a two year term instead of a three or four year term are observably different in their taste for the military. Someone who enlists for four or more years is making a much stronger commitment, and we therefore expect the probability that they choose the military as a career to be much higher. Figure 3 shows the observed survival data from our sample split by length of initial term.

Figure 3. Survivor Function for Military Enlistees By Initial Term



The presumption that longer terms result in higher probabilities of retention is supported by this casual analysis. Since we expect tastes for military to vary substantially by length of initial term, we must also expect the elasticity of retention with respect to retirement pay to vary accordingly. Indeed, we expect a higher elasticity for those who enlist for a shorter initial term.

In designing experiments, laboratory scientists maintain a control group and an experimental group, changing only the item of interest between the two and looking for the difference in outcomes between the two groups. Economists, for the most part, can only dream about being able to have such clean distinctions between otherwise identical groups. In estimating elasticities of reenlistment pay based upon changes in retirement, the distinction of interest between the two groups is clear. Those who signed up prior to 1 August 1986 receive full retirement and those who signed on or after receive a 20% reduction in retirement pay (if they retire at 20 years).

While the change is evident, how close are these two groups in terms of controlling for all other possible influences on the reenlistment decision? Certainly, the unobservable effects of societal, peer, and family pressure are going to change over time. This means that the farther apart in time the two groups are, the less likely that they will be faced with similar unobservable influences upon their decision to reenlist. This suggests the ideal samples would serve at the same time. Fortunately, the Army's delayed entry program offers the opportunity to exploit exactly this. Recruits who join under the delayed entry program commit when signing and fall under the retirement system in force at that time, but do not actually begin their service for up to twelve months. This means that soldiers entering basic training on the same day may fall under completely different retirement programs.

Figure 4 depicts monthly accessions by retirement program. The period from August 1, 1986 through January 31, 1987 is the longest available that has individuals from both retirement systems continuously coming onto active duty. While there are additional enlistees that fall under the FULL retirement system who arrive on active duty after this point, the months of February through May have essentially none. Figure 4 also suggests that a four-month period may be a better breaking point, since the accession of individuals under the FULL retirement system takes a substantial drop in November. For robustness, initial estimation will be conducted on 3, 4, 5, and 6-month samples. Results of the estimation on the 6-month sample follow in the text of this paper; the others are in the Appendix.

Figure 4. Monthly Accessions by Retirement Program.

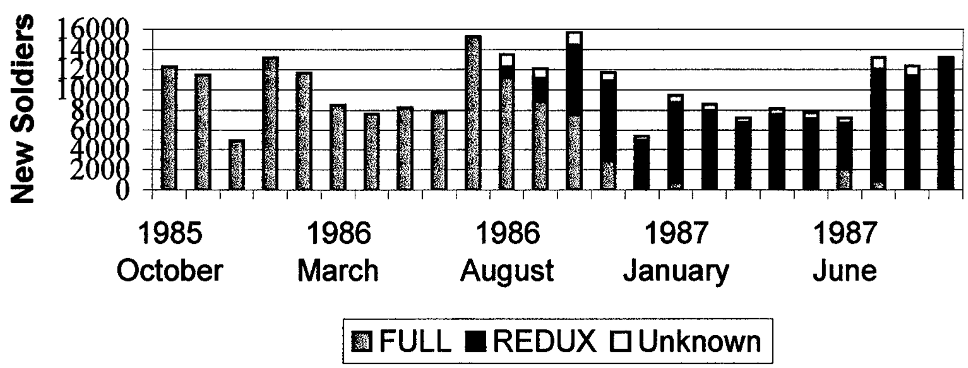


Table 3 provides descriptive statistics broken down by retirement system and length of initial term. A quick look at the mean length of service, years, and the percentage that have left the service, OUT, indicates higher retention rates under the REDUX system for everyone except two year enlistees. Given the standard deviation of the means of years, it is not clear that this difference is significant, but a simple means test easily rejects any equivalence at the 95% confidence level for all sub groups. The higher retention for three and four year enlistees under REDUX seems to be contrary to theory, which suggests that a reduction in retirement pay should reduce the probability of remaining in the Army. We have not, however, controlled for any differences between the REDUX

Table 3. Means for Variables for Those Who Could Choose

Initial Term	2		3		4+	
Percentage	12.8%		60.3%		26.9%	
Retirement	**FULL**	**REDUX**	**FULL**	**REDUX**	**FULL**	**REDUX**
Obs	1745	2047	8859	9025	4420	3540
years	2.7284	2.3281	3.7474	3.9136	4.9439	4.8240
	(1.6695)	(1.1608)	(1.5984)	(1.7102)	(1.6654)	(1.5234)
OUT	0.9186	0.9638	0.8666	0.8287	0.7571	0.6973
	(0.2735)	(0.1867)	(0.3401)	(0.3768)	(0.4289)	(0.4595)
FEMALE	0.1696	0.0847	0.1056	0.1042	0.1065	0.0945
	(0.3754)	(0.2785)	(0.3073)	(0.3055)	(0.3086)	(0.2926)
BLACK	0.1064	0.1054	0.2030	0.2692	0.1435	0.2086
	(0.3085)	(0.3071)	(0.4022)	(0.4436)	(0.3506)	(0.4063)
HISPANIC	0.0319	0.0362	0.0462	0.0527	0.0304	0.0514
	(0.1757)	(0.1868)	(0.2099)	(0.2235)	(0.1718)	(0.2209)
GED	0.0000	0.0003	0.0199	0.0242	0.0104	0.0135
	(0.0000)	(0.0173)	(0.1396)	(0.1538)	(0.1014)	(0.1154)
DROPOUT	0.0016	0.0003	0.0425	0.0459	0.0162	0.0240
	(0.0402)	(0.0173)	(0.2017)	(0.2093)	(0.1262)	(0.1531)
SOMECOL	0.1243	0.1331	0.0637	0.0700	0.0819	0.0817
	(0.3300)	(0.3397)	(0.2442)	(0.2552)	(0.2743)	(0.2739)
COLDEG	0.0173	0.0200	0.0151	0.0187	0.0259	0.0228
	(0.1304)	(0.1399)	(0.1220)	(0.1354)	(0.1587)	(0.1494)
afqsc	72.0962	71.5647	57.8157	54.5524	65.5697	58.6711
	(13.5386)	(14.0091)	(18.8119)	(19.2517)	(17.4804)	(19.4209)
CAT1	0.0821	0.0774	0.0417	0.0333	0.0635	0.0424
	(0.2746)	(0.2673)	(0.1998)	(0.1793)	(0.2440)	(0.2015)
CAT2	0.5759	0.5635	0.3168	0.2756	0.4545	0.3404
	(0.4943)	(0.4960)	(0.4653)	(0.4468)	(0.4980)	(0.4738)
CAT3B	0.0000	0.0012	0.3917	0.4193	0.1819	0.3147
	(0.0000)	(0.0345)	(0.4882)	(0.4935)	(0.3858)	(0.4644)
CAT4	0.0000	0.0006	0.0152	0.0591	0.0022	0.0431
	(0.0000)	(0.0244)	(0.1225)	(0.2358)	(0.0466)	(0.2031)
E2	0.1459	0.1328	0.1335	0.1462	0.1544	0.1324
	(0.3531)	(0.3394)	(0.3401)	(0.3533)	(0.3614)	(0.3390)
E3	0.1556	0.1646	0.1126	0.1240	0.1438	0.1395
	(0.3626)	(0.3708)	(0.3161)	(0.3296)	(0.3509)	(0.3465)
CBTARMS	0.1615	0.3506	0.2483	0.2596	0.1703	0.2725
	(0.3681)	(0.4772)	(0.4321)	(0.4384)	(0.3760)	(0.4453)

Note: Standard deviations are reported in parenthesis below means.

and FULL retirement groups and this paradox may be the result of other observable characteristics.

Sample composition differs between retirement system and length of initial term. Both the three and four year REDUX groups have a higher representation of all minority groups than their FULL counterparts. The FULL two-year sample has a higher proportion of females (FEMALE) than the REDUX sample, but equivalent proportions of blacks (BLACK) and hispanics (HISPANIC).

Education is divided into five subgroups: high-school dropouts (DROPOUT), GED holder (GED), high-school graduates, those with some college (SOMECOL), and those with at least a bachelor's degree (COLDEG). Those individuals who drop out prior to high-school are not represented in this sample of the Army. For the two-year enlistee sample, there are no high-school dropouts and few GED holders. Yet, those two year enlistees entering under REDUX seem to be slightly more likely to have some college, whether or not they actually get a degree. The three-year enlistee sample shows that those entering under REDUX are slightly more likely not to be just high-school graduates. This is evident in their slightly higher percentages of all other educational attainment categories. As for the four-year initial enlistees, those entering under FULL are slightly more likely to have a GED or have dropped out of high-school than those under REDUX and less likely to have any college. While, we can see differences in educational attainment between those entering under REDUX and FULL, these differences are all small.

The Armed Forces Qualification Test (AFQT) is administered to all prospective recruits and measures individuals' mental abilities against their age group. This has often been referred to as a racially unbiased skill test. Indeed, Neal and Johnson (1996) use the AFQT as a measure of pre-market ability and are able to explain all of the earnings difference between the races for women and much of that for men by differences in AFQT scores. This is an additional reason to believe that AFQT scores are indicative of alternative employment opportunities and higher wages. The test is normed and scores are reported in percentile rankings, such that a score of 50 means that the individual is of average mental ability. The military divides individuals into one of six categories based upon their AFQT score, as shown in Table 4. It is illegal for category 5 individuals to serve in any branch of the military, so no one in the sample falls into this category.[24] Across the board FULL enlistees are more likely to be in higher mental categories and less likely to be in the lower mental categories than their REDUX counterparts.

Table 4. AFQT Categories.

	AFQT Score
CAT1	93+
CAT2	64-92
CAT3A	50-64
CAT3B	31-49
CAT4	10-30
CAT5	0-9

Not all recruits are created equal, and in an attempt to be able to price discriminate, the Army offers selected individuals the choice to enter at slightly higher ranks than the lowest. The large majority are not offered this opportunity and join with a pay grade of E1. Depending upon the circumstances, individuals may be offered an entry grade of E2 or E3. The difference in pay between an E1 and an E2 is 11%, while the difference between E2 and E3 is approximately 7%. The two- and four-year enlistees are essentially identical in their likelihood of being accessed at E2, but those under REDUX are slightly more likely to join as

[24] Exceptions to the no CATV personnel have been granted by Congress prior to the AVF for experimental reasons and during wartime.

E3s. The three-year REDUX group has higher percentages of both E2s and E3s. This suggests a small premium paid to those under REDUX.

A possible indicator of a higher taste for the military is the occupational specialty that the individual chooses. While the individual only has choice among options that are needed by the Army, there is a difference between what are termed combat arms (CBTARMS) and the other specialties. Generally, there is little skill training that is transferable into civilian jobs, other than standard military discipline, for combat arms specialists. Across all terms, REDUX enlistees are more likely to be in the combat arms specialties. While the percentage is relatively close for three-year enlistees, four-year enlistees are 25% more likely and two-year enlistees are almost twice as likely to choose the combat arms.

Geographic location has historically been attributed to different preferences for the military, with much higher propensity to serve in the South. This Southern taste for the military dates back to pre-Civil War times and continued through the period in which our sample enlisted, although Asch (1994) finds this has changed in the 1990s. The regional breakdown, Northeast (NE), North Central (NC), South (S), and West (W) is based upon the Census Bureau's breakdown of the country and is established based upon the location the individual applied for their social security card. FULL enlistees are more likely to be from the Northeast or North Central and less likely to be from the South, regardless of term length.

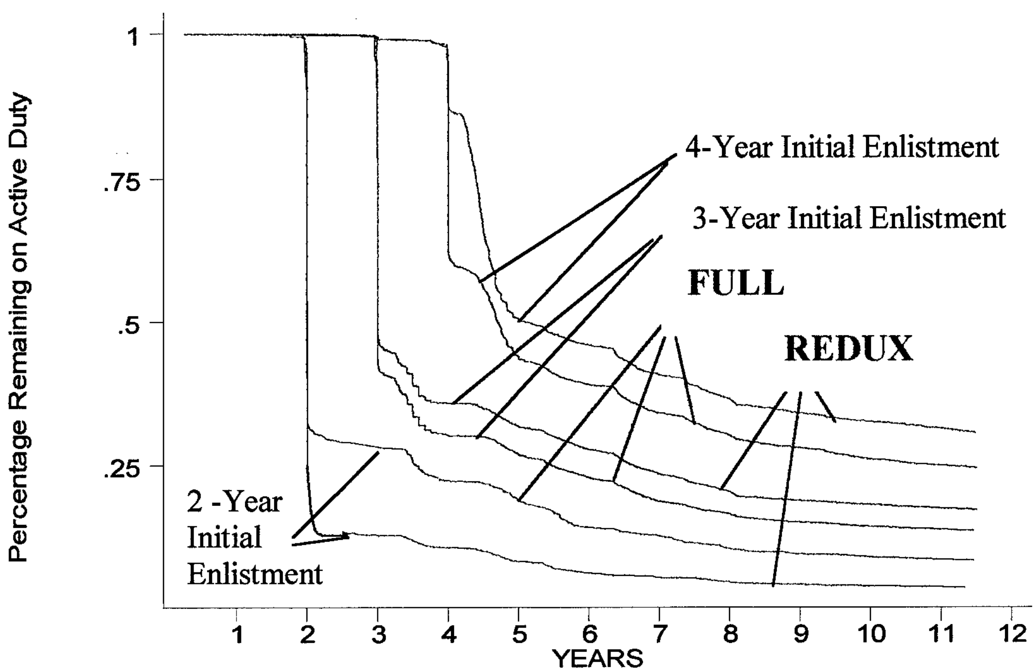
Finally, essentially all individuals have a delay between their enlistment and their accession. DELAY indicates a delay of at least three months. Since the FULL enlistees must enlist prior to the start of our sample and the REDUX enlistees cannot enlist until the start of this six month period we expect a much higher percentage of FULL enlistees to have delayed their entry at least 90 days.

This is indeed the case for all term lengths, although the 20+ percent of REDUX enlistees in the two- and four-year terms delaying their entry is surprising.

V. Estimation

Because of the differences in observable characteristics between the groups, it is not immediately clear what effect the retirement system had. Many of the effects seem likely to have counter-effects that must be controlled for. A quick look at the observed survival functions, separated by length of initial term and retirement system (see Figure 5) provides no clear answers.

Figure 5. Survivor Function for Military Enlisteees by Initial Term Length and Retirement System



Controlling for differences across groups requires using a proportional hazards model. Yet using a proportional hazards model necessitates that covariates only scale up or down the hazard function. Since there is a spike at the end of the initial term and term lengths are different, including all term lengths in one specification with a covariate for the length of the initial term is a misspecification. This suggests estimating three separate proportional hazards model, each grouped by length of initial term of service. The baseline hazard based upon omitted indicator variables is the hazard for a white male high-school graduate from the Northeast of mental category 3A, who enlisted under the FULL retirement system for a non-combat arms specialty.

The results from these three estimations are in Table 5 and are reported as odds ratios with asterisks indicating significance at the 90% level and double asterisks indicating significance at the 95% level. An odds ratio of one indicates the covariate has no effect on the hazard, ratios greater than one indicate higher probability of departure, and ratios less than one indicate a reduction in the hazard, or an increase in the probability of remaining in the military.

These results show a large effect on two-year enlistees; those entering under the REDUX system are roughly 35% to 68% more likely to depart the service depending upon whether or not they delayed their entry. As expected, the result for three year enlistees is far less, coming in positive, but insignificantly different from no effect. Perhaps somewhat more surprising is the negative effect that the REDUX system appears to have on four year enlistees - an increase in the probability of staying by 13% to 24% depending upon whether or not they delayed their entry!

Since the military has often been referred to as a means for minorities to gain substantially, we expect and find that minorities are more likely to stay. The results are significant for all minority groups for the three and four year terms, although only BLACK is significant in the two year group. Perhaps somewhat more surprising is the lack of any effect for the educational attainment variables with the exception of GED holders, who enlist for three years are 14.5% more likely to remain in the Army. This could easily be explained if there were a high correlation between high educational attainment and high mental categories; then

Table 5. Cox Proportional Hazard Estimation Results

	2 Year	3 Year	4+ Year
REDUX	1.6801 **	1.0250	0.7622 **
	(0.1349)	(0.0256)	(0.0335)
FEMALE	0.9583	0.9049 **	0.9044 **
	(0.0471)	(0.0245)	(0.0377)
BLACK	0.6631 **	0.6450 **	0.6869 **
	(0.0370)	(0.0139)	(0.0266)
HISPANIC	0.8880	0.8537 **	0.7786 **
	(0.0850)	(0.0323)	(0.0541)
GED	0.8228	0.8550 **	0.9659
	(0.5828)	(0.0439)	(0.1148)
DROPOUT		0.9787	0.8748
		(0.0364)	(0.0873)
SOMECOL	1.0243	1.0279	0.9667
	(0.0840)	(0.0470)	(0.0625)
COLDEG	1.0839	0.9594	1.1140
	(0.1459)	(0.0689)	(0.1016)
CAT1	1.1850 **	1.1613 **	1.0335
	(0.0802)	(0.0551)	(0.0620)
CAT2	1.1137 **	1.0359	0.9913
	(0.0412)	(0.0244)	(0.0316)
CAT3B	1.2422	0.9752	1.1144 **
	(0.7203)	(0.0231)	(0.0455)
CAT4		0.9219 *	1.2489 *
		(0.0416)	(0.1459)
E2	1.0189	0.8595 **	0.8883 **
	(0.0512)	(0.0211)	(0.0340)
E3	0.9187	0.7644 **	0.8741 **
	(0.0737)	(0.0298)	(0.0502)
CBTARMS	1.0300	0.9310 **	0.8875 **
	(0.0437)	(0.0179)	(0.0324)
NC	0.9687	1.0411	0.9933
	(0.0484)	(0.0265)	(0.0398)
S	0.9206	0.8936 **	0.8490 **
	(0.0473)	(0.0223)	(0.0339)
W	0.9254	1.0405	1.0321
	(0.0532)	(0.0288)	(0.0449)
FULL DEL	1.2293 **	1.1062 **	0.9262 **
	(0.0986)	(0.0291)	(0.0398)
REDUXDEL	1.6735**	1.1276 **	0.8076 **
	(0.1528)	(0.0455)	(0.0464)

Note: Standard errors are below estimated odds-ratios. One asterisk indicates significance at 90%, two at 95%.

we could reasonably expect the mental categories to provide all of the explanatory power and leave nothing for the educational variables. This, however, is not the situation. The correlations between the educational attainment variables and the AFQT categories range across the term lengths from +0.175 to -0.068, indicating some, but little correlation between these sets of variables. Furthermore, no more than two of the AFQT categories are significant in any of the estimates.

Surprisingly, in the four-year sample lower AFQT scores increase the probability of departure with increasing severity - 11% at CAT3B and almost -25% at CAT4. If this were true across all term lengths, it could be indicative of lower promotion possibilities for these individuals. Since it is not clear that this holds across groups and there is no difference in promotions by length of initial term, we are left at a loss to explain this. Tighter estimates could resolve part of this discrepancy, since the estimates of increased retention are insignificant.

The results also indicate higher propensities to remain in the service for higher initial ranks. While these are insignificant for the two-year group, they are significant and substantial for the three- and four-year groups. An E2 three-year initial term individual is 14% more likely to stay, while the same individual would be an additional 11% more likely to remain in the Army if he were an E3 at accession.[25]

Enlistment into a combat arms specialty is insignificant for two-year enlistees, but indicates a higher propensity to remain in the Army for three- and four-year enlistees. As suggested earlier, regional differences do have an effect. Individuals from the South are 11% more likely to remain than their other three-

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[25] The increase is calculated by taking the difference of ratio of the odds ratios and 1:

$$\frac{0.7644}{0.8595} - 1 = -0.1106.$$

year counterparts. This increases to 15% for four-year enlistees and is insignificant for two-year enlistees.

The variables FULL DELAY and REDUX DELAY indicate mutually exclusive groups of those individuals who chose to delay their entry by at least 90 days. This is included to allow for different effects of delaying entry between the two retirement systems, although there is no clear indication why there should be a difference. The variables are designed so that REDUX is exclusive of these and the combined effect can therefore be interpreted directly from the odds ratio. The effect of delaying entry by 90 or more days is significant across all groups, and maintains the same characteristic difference between the retirement systems for each term length. That is, two-year enlistees are still more likely under REDUX to depart, while three-year enlistees are insignificantly different, and four-year enlistees are more likely to remain.

A possible explanation is two- and three-year enlistees have a lower taste for the Army. They are likely to use their service as a means of obtaining skill training that they otherwise do not have access to, whether in the service or through the educational benefits provided after successful completion of their initial term. So, shorter term enlistees may be attempting to put off the inevitable, which they would prefer to avoid, but see enlistment as the only means of accomplishing a goal. Four year enlistees, on the other hand, have a higher taste for military service and therefore, a delay may not actually be a delay, but instead is prematurely enlisting. That is, they enlist as soon as they can, but for personal, professional, or educational reasons, are not able or ready to actually start their service. While these individuals can wait to enlist, they pre-commit instead.

Not grouping the data may result in a loss of efficiency, because the number of observations that contribute to the effects of the covariates is divided among three samples. However, we previously mentioned that the data are not in an appropriate form for estimation together. To group the data requires reformulating the entry time such that the spikes at the end of the initial term in the hazard functions occur at the same time and the difference in term length is then a proportional shift in the hazard function and not a time shift. This is accomplished by restricting the sample to those who complete their first term and assigning this date as epsilon away from the starting point.[26] This allows us to group all individuals together with indicator variables for length of service and the retirement system.

Once the data are grouped we need to add some variables. To aid in understanding the effects of these different categories, mutually exclusive indicator variables (REDUX2, FULL3, REDUX3, FULL4, and REDUX4) are assigned, with two-year FULL enlistees representing the baseline. Additionally, separate indicator variables are used for delayed entry by term length (DELAY2, DELAY3, and DELAY4). Since the results in the previous estimation allowing for differential effects of delaying entry by retirement system did not change the conclusions of the similarity of the retirement groups, these are not separately estimated on the grouped sample.

Table 6 presents the results of this estimation with separate columns for the full specification (I), estimation without educational attainment covariates (II), estimation without educational attainment covariates and insignificant regional covariates (III), estimation with only critical variables and significant variables (IV), and estimation with only critical variables (V). Regardless of the specification preferred, the results of interest vary little. The effect of the REDUX retirement system is an increase in the probability of departure of 41%

[26] The end of the first term cannot be assigned a time of zero, else these individuals would not be in the sample, so an infinitesimal time is added prior to the end of the first term. The time chosen is irrelevant in the estimation because the hazard is zero over this interval and proportional changes in a hazard of zero are still zero, so there is no effect on the partial likelihood based upon changing epsilon to any other number.

Table 6. Cox Proportional Hazard Estimation Resutls for Entire Sample.

	I	II	III	IV	V
REDUX2	1.4056 **	1.4083 **	1.4098 **	1.4091 **	1.4257 **
	(0.0585)	(0.0585)	(0.0586)	(0.0585)	(0.0591)
FULL3	0.8704 **	0.8638 **	0.8647 **	0.8709 **	0.7934 **
	(0.0381)	(0.0376)	(0.0375)	(0.0373)	(0.0338)
REDUX3	0.8482 **	0.8413 **	0.8421 **	0.8453 **	0.7760 **
	(0.0314)	(0.0309)	(0.0309)	(0.0307)	(0.0281)
FULL4	0.7620 **	0.7609 **	0.7611 **	0.7643 **	0.7354 **
	(0.0407)	(0.0406)	(0.0406)	(0.0407)	(0.0391)
REDUX4	0.5676 **	0.5666 **	0.5670 **	0.5681 **	0.5575 **
	(0.0217)	(0.0216)	(0.0216)	(0.0217)	(0.0213)
DELAY2	1.0417	1.0428	1.0434	1.0429	1.0973 **
	(0.0279)	(0.0280)	(0.0280)	(0.0279)	(0.0291)
DELAY3	1.1445 **	1.1468 **	1.1470 **	1.1436 **	1.2519 **
	(0.0326)	(0.0325)	(0.0325)	(0.0321)	(0.0346)
DELAY4	0.8981 **	0.8972 **	0.8979 **	0.8953 **	0.9760
	(0.0402)	(0.0402)	(0.0402)	(0.0400)	(0.0435)
FEMALE	0.9056 **	0.9095 **	0.9094 **	0.9099 **	
	(0.0203)	(0.0204)	(0.0203)	(0.0203)	
BLACK	0.6450 **	0.6455 **	0.6444 **	0.6457 **	
	(0.0125)	(0.0125)	(0.0124)	(0.0122)	
HISPANIC	0.8564 **	0.8576 **	0.8544 **	0.8556 **	
	(0.0291)	(0.0291)	(0.0287)	(0.0286)	
E2	0.8967 **	0.8984 **	0.8987 **	0.8982 **	
	(0.0186)	(0.0186)	(0.0186)	(0.0185)	
E3	0.8151 **	0.8410 **	0.8410 **	0.8407 **	
	(0.0262)	(0.0173)	(0.0173)	(0.0173)	
CBTARMS	0.9442 **	0.9439 **	0.9442 **	0.9435 **	
	(0.0161)	(0.0160)	(0.0160)	(0.0160)	
CAT1	1.1359 **	1.1467 **	1.1470 **	1.1391 **	
	(0.0405)	(0.0405)	(0.0405)	(0.0391)	
CAT2	1.0433 **	1.0477 **	1.0481 **	1.0406 **	
	(0.0192)	(0.0191)	(0.0191)	(0.0167)	
CAT3B	1.0104	1.0202	1.0201		
	(0.0213)	(0.0208)	(0.0208)		
CAT4	0.9668	0.9780	0.9781		
	(0.0433)	(0.0434)	(0.0434)		
NC	1.0195	1.0201			
	(0.0221)	(0.0221)			
S	0.8942 **	0.8937 **	0.8801 **	0.8805 **	
	(0.0191)	(0.0191)	(0.0135)	(0.0135)	
W	1.0225	1.0229			
	(0.0243)	(0.0243)			
GED	0.8991 **				
	(0.0456)				
DROPOUT	0.9717				
	(0.0372)				
SOMECOL	1.0358				
	(0.0377)				
COLDEG	1.0600				
	(0.0588)				

Note: Standard errors are in parenthesis below odds-ratios. One asterisk indicates significance at the 90% level, two at the 95% level.

for two year enlistees. This certainly seems a little more reasonable than the 68% obtained from separate estimation.

The effect on three-year enlistees requires comparing the coefficients on REDUX3 and FULL3. The result is a z-statistic of 0.95, resulting in a failure to reject the hypothesis that the two effects are identical. This means we observe no different behavior by three-year enlistees regardless of the retirement system they enter under. As seen in table 7, the value of the test statistic changes slightly with the different specifications, but the failure to reject the equivalence of these terms is robust across all specifications.

Table 7. Tests for Equivalence of Effects.

Specification		**REDUX3=FULL3**	**REDUX4=FULL4**	
I	**z-stat**	0.95	6.53	***
	p-value	0.3423	0.0000	
II	**z-stat**	0.97	6.54	***
	p-value	0.3298	0.0000	
III	**z-stat**	0.97	6.45	***
	p-value	0.3288	0.0000	
IV	**z-stat**	1.1	6.58	***
	p-value	0.2699	0.0000	
V	**z-stat**	0.82	6.15	***
	p-value	0.4102	0.0000	

***** indicates rejection of equivalence hypothesis at 99% level**

Four-year enlistees react contrary to theory, becoming more likely to stay with a reduction in their retirement benefits. The z-test for equivalence of FULL4 and REDUX4 results in a z-statistic of 6.53, firmly rejecting any equality. Those entering under the REDUX system are 13% to 26% more likely to remain in the

Army even after controlling for their observable characteristics. This certainly seems to be clear evidence of a problem with our theoretical model, even more so when we know that the z-test confirms this inequality for all specifications.

A possible contributing factor for the counter-intuitive result on the effect of the retirement system change for four-year enlistees is the differential timing of service of Desert Shield and Desert Storm in relation to the reenlistment decision of individuals. This is important for two reasons. The first is the impact of fighting in a war on the taste for service. The amenities and rewards of serving in a peacetime military are far different than those when you are actually in harm's way.

The second reason the Gulf War may be significant is a policy implemented during this period called Stop-Loss. Stop-Loss effectively ceased all exits from the military during the Desert Shield build-up and through Desert Storm. This means that individuals who entered active duty towards the end of our sample period for an initial term of four years and decided not to reenlist may have involuntarily been extended several months. This seems critical because 1163 of 3544 four-year REDUX enlistees terminated service between four and 4.8 years and only 563 of 4420 four-year FULL enlistees did the same. There is therefore the potential of attributing higher retention to the REDUX system because of the Stop-Loss policy. The problem we have is distinguishing involuntary extensions from voluntary extensions; the data provide no help.

Assuming all individuals who departed the Army between four years and 4.8 years were involuntarily extended, we reestimated the model for four-year enlistees, imputing a length of service of four years for all of these individuals. The results are qualitatively identical to the uncorrected sample, although the magnitude of the counter-intuitive effect on the retirement system change is lessened (see Table 38 in Appendix A). Because of the inability to distinguish between voluntary and involuntary extensions and the lack of any qualitative change in results, we maintain the unaltered sample for the remainder of the estimation.

Surprisingly, the educational attainment variables, with the exception of GED, are insignificant. Education playing no significant role suggests that there are no differential effects between the military and civilian sectors based upon education alone. This is quite surprising because although there is variation in promotions among soldiers and hence in pay, this is not directly linked to education. To see the profundity of this effect, we use rough bounds from the returns to education literature of between 4% and 10% per year and look at the differences between high-school graduates and college graduates. Using these bounds, the literature suggests that the difference should therefore be 16% to 40%.

For the Army to actually maintain the minimum differential (16%) requires soldiers with college degrees on average to be promoted to ranks almost two full pay grades higher than those with high-school diplomas and maintain this separation. As discussed earlier, this is possible at accession by bringing an individual onto active duty as an E3 instead of an E1, resulting in a pay differential of 18%. The correlation between E3 and COLDEG is 0.3517, and E2 and COLDEG is -0.0634. While this may be part of the reason, it is not all. At the other end, the upper bound, 40% requires, on average, a differentiation of slightly more than three pay grades is maintained; this simply is not feasible.

A more likely explanation is that a majority of individuals joining the military have not completed their education. The result is that any measure of educational attainment at enlistment systematically underreports the ability or human capital formation of those with less than a college degree. The degree of measurement error is likely to diminish the more education an enlistee receives prior to enlistment.

The significance of having a GED suggests that the military offers better opportunities for those entering with a GED than the civilian sector, resulting in an 10% higher retention than high-school graduates. This result supports the findings of Cameron and Heckman(1993), who find that GED holders are not equivalent to those with actual high-school diplomas. Indeed, they find that GED holders are insignificantly different from high-school dropouts. Our data do not allow us to make this type of comparison, only the civilian-military opportunity comparison. Since less than 2% of the sample has a GED, and none of the other educational variables are significant, all educational attainment measures are not included in specifications II-V.

Regional indicators, as in the separate estimation procedure, are insignificant except for individuals from the South. Those from the South are 11%-12% more likely to remain than individuals from the other geographic locations, depending upon the specification preferred. Since North Central and West are insignificant even after dropping the educational attainment variables they are also removed from estimations III - V.

Minorities are all more likely to remain, with blacks at 35% most likely to stay. Hispanics are 14% more likely to stay and women stay 9% more than men. This means that black women are 41% less likely to depart than white men are, a difference roughly equal to the effect of REDUX on two-year enlistees.

Initial rank has a significant effect upon retention. While indicating a premium paid for the individual and therefore suggesting a lower taste for the military, these individuals are 10% to 16% more likely to remain. Perhaps this is due to earlier promotion throughout the careers of these individuals. Since our data set did not include promotion data, this is merely a supposition.

As expected, individuals who chose a combat arms specialty are less likely to depart. This effect is probably two effects working together. The first is that choosing a combat arms specialty indicates a higher preference for the military.

Additionally, the effect of serving in the combat arms is to avoid gaining technical skills that are available in many of the other occupational specialties and therefore have fewer opportunities outside the Army.

Somewhat surprisingly, individuals with lower than average mental abilities, CAT3B and CAT4, are no different from the average to slightly above average group, CAT3A. As expected, however, individuals with high mental abilities, CAT1 and CAT2, are more likely to choose alternative careers. The effect is 13% to 14% for CAT1 and 4% to 5% for CAT2 depending upon the specification chosen.

A lingering problem is the counter-intuitive result for four-year enlistees under the REDUX system. This certainly is more disturbing than finding no effect for three-year enlistees. However, there may be a simple explanation. We have not, nor can we with the data, accounted for selection bias. Selection bias occurs because the individual at the margin deciding whether or not to enlist initially under the FULL retirement system is no longer a marginal case under the REDUX system; he will not consider enlisting. This means that for a given set of observable characteristics, an individual must have a higher taste for military service under REDUX prior to choosing to enlist.

The result is a more select group of individuals who on average have a greater preference for the Army and therefore are more likely to reenlist. The resulting bias, therefore, is downward. Since two-year enlistees have a lower taste for the Army to begin with, the effect of selection bias should be most severe for this group. The effect should be progressively smaller for three-year and four-year enlistees, because we assume longer initial terms indicate progressively higher tastes for military service. The negative effect of 13% to 24%, however, is too large to attribute to sample selection and still maintain the necessary assumption for a proportional hazards model. This suggests the model is not fully specified and the negative effect is largely due to an omitted variable bias instead of sample selection bias.

After reexamining the specification, age and fiscal year of entry seemed to be the only possibly relevant variables omitted. Age might be important because it can indicate whether or not an individual is coming directly from high-school or has been in the labor market for a while. Someone who has been in the labor market and decides to join the Army is different from the individual who joins straight out of high-school. Unfortunately, the data does not include age. Fiscal year of entry also could play an important role because incentives for departure are normally tied to an individual's year group. During the period covered by the data, the Army went through a severe reduction in size. To accomplish the reduction quickly as mandated by Congress required incentives to increase the number of individuals departing the service.

Table 8 presents the ungrouped results from adding a fiscal year indicator to the specification. The estimates for all observables except the indicators for REDUX and delay are essentially unchanged. The results for the effects of the retirement change are dramatically different. The 2-year REDUX enlistees are 11% to 22% more likely to depart the Army depending upon whether or not they delayed entry. The effects for three- and four-year enlistees are insignificant, although positive for those who delay entry. An 11% effect for two-year enlistees and minimal or no effect for three- and four-year enlistees seem far more likely than the previous results.

Grouping the data as done previously and including indicators for fiscal year and term results in similar changes to our results (see tables 9 and 10). The effect of observables remains essentially unchanged from our earlier estimation, however, the results of the retirement system change are dramatically different. The effect on two-year enlistees is 16%, while three- and four-year enlistees show no effect. We must emphasize that these results are simply lower bounds of the

Table 8. Cox Proportional Hazard Estimation Results (Fiscal Year Included)

	2 Year		3 Year		4 Year	
REDUX	1.2227	**	1.0121		0.9267	
	(0.1019)		(0.0293)		(0.0452)	
FEMALE	0.9494		0.9022	**	0.9376	
	(0.0467)		(0.0246)		(0.0393)	
BLACK	0.6880	**	0.6450	**	0.6825	**
	(0.0384)		(0.0139)		(0.0265)	
HISPANIC	0.9078		0.8535	**	0.7758	**
	(0.0870)		(0.0323)		(0.0539)	
GED	0.8265		0.8518	**	1.0883	
	(0.5854)		(0.0439)		(0.1301)	
DROPOUT			0.9754		0.9671	
			(0.0365)		(0.0971)	
SOMECOL	0.9985		1.0270		0.9738	
	(0.0816)		(0.0470)		(0.0630)	
COLDEG	1.0685		0.9587		1.1284	
	(0.1436)		(0.0689)		(0.1029)	
CAT1	1.1722	**	1.1602	**	1.0419	
	(0.0793)		(0.0551)		(0.0624)	
CAT2	1.1164	**	1.0356		0.9959	
	(0.0413)		(0.0244)		(0.0317)	
CAT3B	1.2466		0.9793		1.0285	
	(0.7229)		(0.0236)		(0.0429)	
CAT4			0.9244	*	1.2188	*
			(0.0418)		(0.1425)	
E2	1.0298		0.8589	**	0.8966	**
	(0.0517)		(0.0211)		(0.0344)	
E3	0.9272		0.7641	**	0.8900	**
	(0.0741)		(0.0298)		(0.0511)	
CBTARMS	1.0380		0.9311	**	0.8818	**
	(0.0441)		(0.0179)		(0.0322)	
NC	0.9904		1.0412		0.9947	
	(0.0495)		(0.0265)		(0.0398)	
S	0.9290		0.8933	**	0.8551	**
	(0.0478)		(0.0223)		(0.0341)	
W	0.9319		1.0401		1.0514	
	(0.0535)		(0.0288)		(0.0458)	
FULL DELAY	1.0873		1.1028	**	0.9588	
	(0.0879)		(0.0293)		(0.0413)	
REDUX DELAY	1.2093	**	1.1118	**	0.9982	
	(0.1139)		(0.0484)		(0.0620)	
FY	0.5166	**	0.9812		1.3756	**
	(0.0292)		(0.0214)		(0.0469)	

Note: Baseline is white male high school graduate from the Northeast of slightly above average mental ability (CAT3A) who does not delay entry and enlists as an E1 for a non-combat arms specialty. Standard errors are reported in parenthesis below estimated odds ratio. One asterisk indicates significance at 90% level, and two asterisks indicate significance at the 95% level.

Table 9. Grouped Results Including Fiscal Year

	I		II		III		IV		V	
REDUX2	1.1612	*	1.1617	*	1.1643	*	1.1643	*	1.1553	
	(0.1045)		(0.1045)		(0.1047)		(0.1047)		(0.1037)	
FULL3	0.6808	**	0.6746	**	0.6763	**	0.6743	**	0.6131	**
	(0.0620)		(0.0612)		(0.0613)		(0.0610)		(0.0554)	
REDUX3	0.6776	**	0.6725	**	0.6741	**	0.6706	**	0.5992	**
	(0.0591)		(0.0585)		(0.0586)		(0.0581)		(0.0517)	
FULL4	0.4753	**	0.4740	**	0.4749	**	0.4742	**	0.4485	**
	(0.0462)		(0.0461)		(0.0462)		(0.0461)		(0.0435)	
REDUX4	0.4408	**	0.4398	**	0.4407	**	0.4397	**	0.4201	**
	(0.0391)		(0.0390)		(0.0391)		(0.0390)		(0.0372)	
DELAY2	1.0934		1.0927		1.0950		1.0942		1.1252	**
	(0.0941)		(0.0941)		(0.0943)		(0.0942)		(0.0967)	
DELAY3	0.7707	**	0.7654	**	0.7674	**	0.7669	**	0.7560	**
	(0.0674)		(0.0668)		(0.0670)		(0.0669)		(0.0658)	
DELAY4	0.4536	**	0.4522	**	0.4534	**	0.4531	**	0.4662	**
	(0.0403)		(0.0401)		(0.0402)		(0.0402)		(0.0413)	
FY2	0.4846	**	0.4845	**	0.4846	**	0.4848	**	0.4760	**
	(0.0279)		(0.0279)		(0.0279)		(0.0279)		(0.0273)	
FY3	1.0376		1.0412	*	1.0411	*	1.0388	*	1.0039	
	(0.0232)		(0.0230)		(0.0230)		(0.0224)		(0.0211)	
FY4	1.4023	**	1.4040	**	1.4031	**	1.4017	**	1.3715	**
	(0.0443)		(0.0443)		(0.0443)		(0.0440)		(0.0428)	
FEMALE	0.9110	**	0.9145	**	0.9144	**	0.9153	**		
	(0.0206)		(0.0206)		(0.0206)		(0.0205)			
BLACK	0.6473	**	0.6476	**	0.6462	**	0.6445	**		
	(0.0126)		(0.0126)		(0.0125)		(0.0122)			
HISPANIC	0.8573	**	0.8583	**	0.8546	**	0.8533	**		
	(0.0291)		(0.0292)		(0.0287)		(0.0286)			
GED	0.9157	*								
	(0.0466)									
DROPOUT	0.9846									
	(0.0380)									
SOMECOL	1.0317									
	(0.0375)									
COLDEG	1.0641									
	(0.0591)									
CAT1	1.1354	**	1.1446	**	1.1451	**	1.1498	**		
	(0.0405)		(0.0404)		(0.0404)		(0.0396)			
CAT2	1.0439	**	1.0473	**	1.0479	**	1.0526	**		
	(0.0192)		(0.0191)		(0.0191)		(0.0171)			
CAT3B	0.9855		0.9916		0.9915					
	(0.0213)		(0.0208)		(0.0208)					
CAT4	0.9519		0.9594		0.9596					
	(0.0428)		(0.0428)		(0.0428)					
E2	0.8982	**	0.8995	**	0.8999	**	0.9005	**		
	(0.0186)		(0.0186)		(0.0186)		(0.0186)			
E3	0.8197	**	0.8436	**	0.8436	**	0.8439	**		
	(0.0264)		(0.0174)		(0.0174)		(0.0174)			
CBTARMS	0.9467	**	0.9464	**	0.9467	**	0.9453	**		
	(0.0161)		(0.0161)		(0.0161)		(0.0160)			
NC	1.0244		1.0248							
	(0.0222)		(0.0222)							
S	0.8972	**	0.8968	**	0.8798	**	0.8799	**		
	(0.0192)		(0.0191)		(0.0135)		(0.0135)			
W	1.0288		1.0291							
	(0.0244)		(0.0244)							

Note: Standard errors are reported in parenthesis below estimated odds ratio. One asterisk indicates significance at 90% level, and two asterisks indicate significance at the 95% level.

Table 10. Tests for Equivalence of Effects. (Full Sample, FY Included)

Specification		REDUX3=FULL3	REDUX4=FULL4
I	z-stat	0.14	1.49
	p-value	0.8784	0.1342
II	z-stat	0.10	1.49
	p-value	0.9187	0.1372
III	z-stat	0.17	1.50
	p-value	0.8598	0.1337
IV	z-stat	0.74	1.30
	p-value	0.4581	0.1927
V	z-stat	0.10	1.48
	p-value	0.9159	0.1381

effects of the change in the retirement system. The actual effect may be slightly higher due to uncorrected sample selection bias.

Translating these lower-bound estimates into elasticities is relatively straightforward. The elasticity of retention with respect to retirement pay is simply the percentage change in the retention rate divided by the percentage change in retirement pay.

$$\xi_{retention,retirement} = \frac{\%\Delta retention}{\%\Delta retirement\_pay} \geq \frac{\Delta odds\_ratio}{20\%} \qquad (19)$$

The inequality in equation 19 reflects the fact that the change in retirement pay is not straightforward; it changes depending upon when the individual retires. Indeed the change in the retirement pay approaches zero as the individual approaches 30 years of service. Additionally, we are ignoring the detail of the actual change. For all of these reasons, our estimate will be only a lower bound and probably much lower than the actual elasticities. Since we have different effects based upon length of initial term, we must also separately calculate elasticities. The elasticity for the two-year group is the highest, with a lower bound of 0.80 (using the 16% increase from the more efficient grouped results). The lower bound elasticity for three- and four-year enlistees is zero.

With a lower bound on the elasticity at 0.80 for two-year enlistees who make up 12.8% of our sample, and 0 for the remainder, an important question is: was this a net benefit in dollar terms to the nation? The estimated savings in 1986 when the Military Retirement Reform Act was being debated in Congress was18% of the annual retirement budget. With a maximum reduction of 20% for each individual, this estimate must assume full replacement of the retirement population by those governed by the REDUX system. Given that this is probably an upper bound on the savings, 18% of the retirement budget is approximately 1.8 billion dollars. Full replacement of the retirement population will take in excess of 20 years, probably more than 40 years. The estimated savings for each accession cohort is 90 million dollars. Assuming a cost of more than $20,000 dollars to train a new enlistee and similar effects for all of the services, the savings exceed the direct costs if we expect 64.66% or less of 2-year initial enlistees to depart after their first term.

$$\begin{aligned} savings = &(\cos t)(\%\Delta reten)(deparrate)(\#2year)+ \\ &(\cos t)(\%\Delta reten)(deparrate)(\#3year)+ \\ &(\cos t)(\%\Delta reten)(deparrate)(\#4year) \end{aligned} \tag{20}$$

$$90{,}000{,}000 = (20000)(.16)(x)(43500)+0 \tag{21}$$

$$x=0.6466$$

If the expectation is any higher the result is a loss. The extent of the loss increases with the percentage. Based upon our data, the first term departure rate for two-year enlistees is 77.6%; the result, therefore, seems to be a net loss. This is contingent upon all the assumptions made prior to the calculation about costs and savings and that the lower bound is the actual effect. If the actual savings are lower , the costs are higher, or if the lower bound does not hold the cost-benefit analysis changes drastically.

Missing in the cost-benefit analysis are the changes in the recruiting environment that must have taken place to adapt to the higher taste necessary to enlist under the new system. This means that recruiters must increase effort, whether through additional hours or additional recruiters, and spend more time and money finding the individuals who will enlist under the REDUX system. Currently this cost may be the highest and hardest to estimate. Projections for fiscal year 1999 accessions estimate a shortfall in meeting quality benchmarks. The cost of this projected reduction in the quality of the Armed Forces includes an inherent reduction in their capabilities in addition to direct costs of a less productive force. These ancillary costs could easily dwarf any direct costs or savings or leave them unchanged.

We return now to our assumption underlying all our results that the effect of the change in retirement system is a proportional shift in the hazard. Casual inspection of the realized survivor functions in Figure 5 suggests this is appropriate, with the possible exception of four-year enlistees during the four- to five-year period. As we have already noted, we expected a change in this group due to Stop-Loss, so there is no reason to reject the proportional shift hypothesis from visual inspection. Since the economics literature provides no formal test for the proportionality assumption, we must turn to biostatistics to test the assumption. The test developed by Grambsch and Therneau (1994) examines the Schoefield residuals. Under the assumption of proportionality, the covariates will retain the explanatory power and the Schoenfield residuals will have a slope coefficient of zero.

The results of the Grambsch-Therneau test are presented in Table 11 based upon the estimation reported in Table 8 and specification 1 of Table 9. The result is a failure to reject the proportionality hypothesis at any standard level of significance on the ungrouped data, The result for the grouped data, however, is not as clear. The grouped data rejects the proportionality between FULL and REDUX for all groups at the 90% level of confidence, but rejects none at the 99% level of confidence. Given the qualitative similarity of the results between the grouped and ungrouped samples and in an attempt to maintain the conservative nature of our conclusions, we suggest the grouped estimation results are sufficient for a lower bound.

Table 11. Proportionality Test Results

Ungrouped

	2 Year	3 Year	4 Year
t-stat	0.62	1.31	1.19
p-value	0.5381	0.1914	0.2339

Grouped

	2 Year	3 Year	4 Year
t-stat	1.75 *	2.03 **	2.31 **
p-value	0.0795	0.0423	0.0210

* indicates rejection of equivalence hypothesis at 90% level
** indicates rejection of equivalence hypothesis at 95% level
*** indicates rejection of equivalence hypothesis at 99% level

VI. SUMMARY AND CONCLUSIONS.

Given previously estimated discount rates in excess of 10%, *a priori* it was not clear that a significant change in the military retirement system in 1986 would have had any significant effect on retention. This analysis demonstrated that the Chairman of the Joint Chiefs of Staff was quite correct in stating that the effects were significant for at least the Army. Exactly how large an effect is not clear due to our inability to control for selection bias. Further research should attempt to correct for this selectivity bias, if the extensive data necessary becomes available. This is essential because part of the unmeasured effect is an increase in the efforts needed to recruit individuals.

We must emphasize that these results are only for the Army. Given the inherent differences among the services, there is no reason to expect the effects to be the same across the services. These different effects can only be estimated with data from all of the services. However, the results show that the change in the retirement system has a sizable net cost under some very plausible assumptions. Even if we were to assume that there were no effects on enlistees in the other services, while maintaining our other cost and savings assumptions, the lower-bound estimates for the Army alone have a cost in excess of 42 million dollars. (The Army accounts for 39% of annual recruits.) This is more than half the estimated savings for all services combined.

Furthermore, we cannot determine whether this change in the retirement system has an effect on career soldiers. The change is simply too recent. We can, however, hypothesize the effect of the new system for soldiers with 20 or more years of service. For each additional year past twenty up to thirty years of service, individuals under REDUX earn in excess of 75% more in retirement benefits than those under the FULL system. How much of an increase this will have on retention past twenty years is simply pure conjecture. Any increase, however, will have the effect of creating, on average, an older force. The more substantial this effect, the less is saved by the change in systems. Additionally, the older force can be anticipated, at least initially, to create further reductions in retention rates. The older force means that individuals who would have retired under the FULL system are now staying longer, holding positions in higher ranks, so that younger soldiers cannot be promoted until these individuals retire. The additional time necessary to wait until promotion reduces benefits and makes the military less attractive.

The estimation of retention elasticities with respect to retirement pay is an essential element for informed policy analysis regarding changes in the military retirement system. While important, this alone only allows us to examine one of many costs associated with changing the retirement structure. These results are necessarily incomplete, in that the full career path of individuals affected by the new system is yet to be seen. However, even with this and other limitations, the costs far exceed the projected savings.

Chapter 3: The Quantity Quality Trade-off in Labor Markets:

The Case of Military Recruiting

I. Introduction

We examine the relationship between quantity of labor demanded and the quality received. If we hold constant wages, it seems obvious that quality will fall as demand increases, but how much? And is it obvious?

Understanding the relationship between quality and quantity demanded is extremely important in a policy context. Most government organizations have limited wage flexibility. Some, like the military, have none. What is the impact of changing the size of these organizations on the quality of workers they obtain?

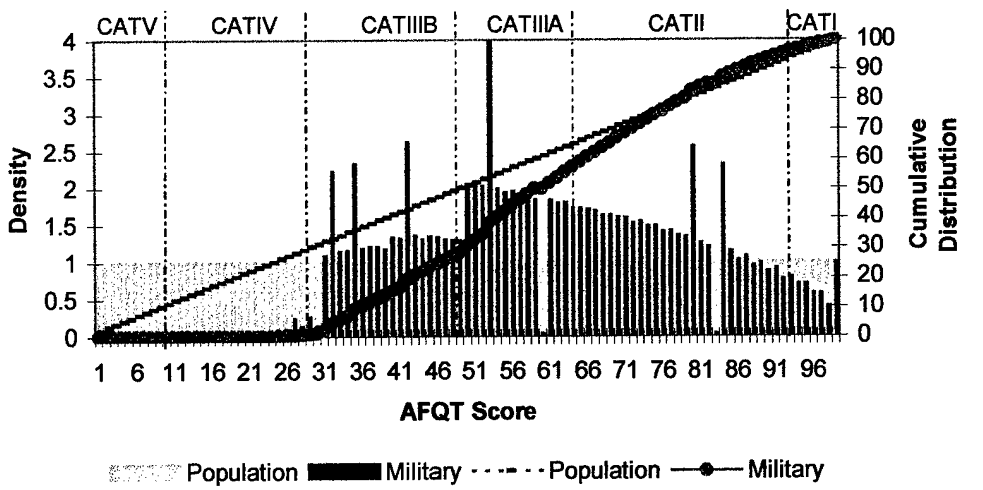
To analyze this question we need demand-based data. If the effect is large, then data from any medium to large firm should be able to generate estimates of the quantity-quality trade-off. However, if the effect is small, then data pertaining to a firm or group of firms that demand a significant portion of a particular labor market is necessary.

Even if one could get data from an extremely large firm like General Motors two problems would persist. First, GM, while employing 647 thousand people, draws its employees from the whole adult population. This broad labor market dwarfs any demand changes by GM. The other problem is a lack of quality measurements in the data. Certainly we expect a firm to keep educational attainment data, but the movement in this is likely to be due to changing job requirements within GM and increasing educational attainment by the population as a whole.

We tackle these two problems, size relative to labor market and measures of quality, with data on military recruits from the Defense Manpower Data Center

(DMDC). Annual demand for recruits in the 1990s was 149 to 205 thousand, and the projected need with military downsizing complete is 200 thousand personnel. These recruits are primarily in the 17 to 24 age group. Instead of being constrained to using educational attainment as a measure of quality, the military uses the Armed Forces Qualification Test (AFQT).

Figure 6. Mental Ability Comparison of Military and Population FY90-FY98



The AFQT has been used by many, most notably Neal and Johnson (1996), as a racially unbiased measure of skill or worker quality. The test is administered to all prospective recruits and scores are reported on a percentile basis, with scores normalized to the entire 17- to 24-year-old population. The consequence of normalizing the scores is that the military draws annually from the same distribution of relative quality. The military actually groups individuals into one of six mental categories based upon their AFQT score (see Table 4), and is Congressionally prohibited from accepting recruits in the lowest mental category. Figure 6 depicts the representation by AFQT score and mental category of the population aged 17 to 24 and the military's recruits for fiscal years 1990 through 1998. Having AFQT scores, therefore, provides a measure of quality that is both unbiased and broad enough to enable one to observe variation due to quantity changes if there is in fact any.

Whether or not there is a quantity-quality trade-off is in the end an empirical question. The military's changing demand for recruits offers the best hope to date of identifying this relationship, if it exists. If we are able to find this relationship, we need to determine its size. With the quantity-quality elasticity in hand, we have a remarkable opportunity to find the cost of quality and what the unemployment-price-quantity relationship is, holding quality constant. Finally, we should ask what the supply curve or curves for military recruits look like and how much monopsony power the military has.

The remainder of this chapter is divided into five sections. Section II examines the military and the market for recruits. Section III deals with the theory necessary to examine these questions. This is followed by a brief examination of the data in Section IV. Then in Section V focuses on estimation and analysis. Finally, we will summarize our findings and focus on how they answer the research questions in Section VI.

II. THE MILITARY AND THE MARKET FOR RECRUITS

The military is not a competitive firm. The military's market share is significant and raises the possibility of monopsony power. Furthermore, the military does not sell its services; instead it operates under a Congressionally approved budget and mandated wage structure.

How large is the military's share of the market it taps for recruits? The military of the 1990's is 84 percent male and over 95 percent high-school graduates, or roughly 160 thousand male high-school graduates from each age cohort. The corresponding population in 1998 was 1.544 million. The corresponding labor force, however, was only 1.158 million males (US Census

Bureau 1998). Thus the military accounts for at least 13.8 percent of the market for male high-school graduates.

The market, however, is even smaller, and the military's share is larger. Since we are focusing on enlisted recruits, whose educational attainment rarely includes a bachelor's degree, and college graduates compete in a different market for their services, we must further reduce the labor force of interest by the 27.3 percent expected college graduates from any age cohort. The result is the military accounts for 19 percent of the market. This figure is a lower limit because it ignores the prohibition of recruiting from the bottom ten percent of the quality distribution of the population and, furthermore, also ignores the focus on recruits from the upper three mental categories.[27]

While the military accounts for a significant part of its labor market, the quantity-quality trade-off requires variation in quantity to identify any relationship. The 1990's provide an excellent opportunity because the military's draw-down caused the number of recruits to fluctuate from a high of just over 204 thousand to a low of 149 thousand and then to return to almost 200 thousand. The result is a variation of 55 thousand, or at least 5.2 percent of the previously defined market.

III. THEORETICAL MODEL

The literature on quality and the labor market is practically void, with few even closely related articles. Rosen (1981) lays out a theoretical framework and discusses the quantity-quality trade-off in the market for services. But this exposition on superstars, while related, offers no empirical analysis and addresses a slightly different question: what is the economic impact of the exceedingly talented? The literature on monopsony while related is not generally applicable.

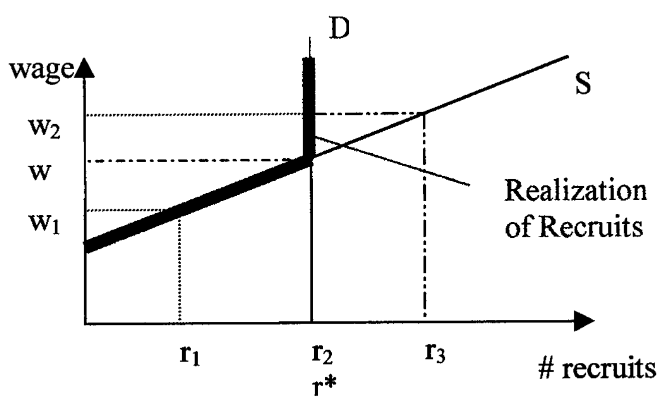
[27] The military further constrains the market through health and morality requirements. Not only are physical handicaps disqualifying, but also any history of Ritalin use or criminal record. This suggests that the market for recruits is 5 to 20 percent smaller than previously discussed.

The focus is generally on demonstrating market power by a firm or firms by estimating the differences between marginal product and salaries.

Our search for the quantity-quality trade-off in labor markets requires focusing on the military enlistment/recruiting process. Once we understand the enlistment process we can examine the interaction between quantity and quality.

Many enlistment models are based upon Fisher's (1969) observation that the actual number of recruits is not the intersection of the demand and supply curves, but instead the minimum of supply and demand. As seen in Figure 7 this is little more than a market with inelastic demand and wage stickiness. The inability of wages to adjust as they would in a standard neoclassical model forces the realization of recruits to change from the point (r*,w*) to the locus represented shown in Figure 7, where the number of recruits adjusts based upon the wage offered.

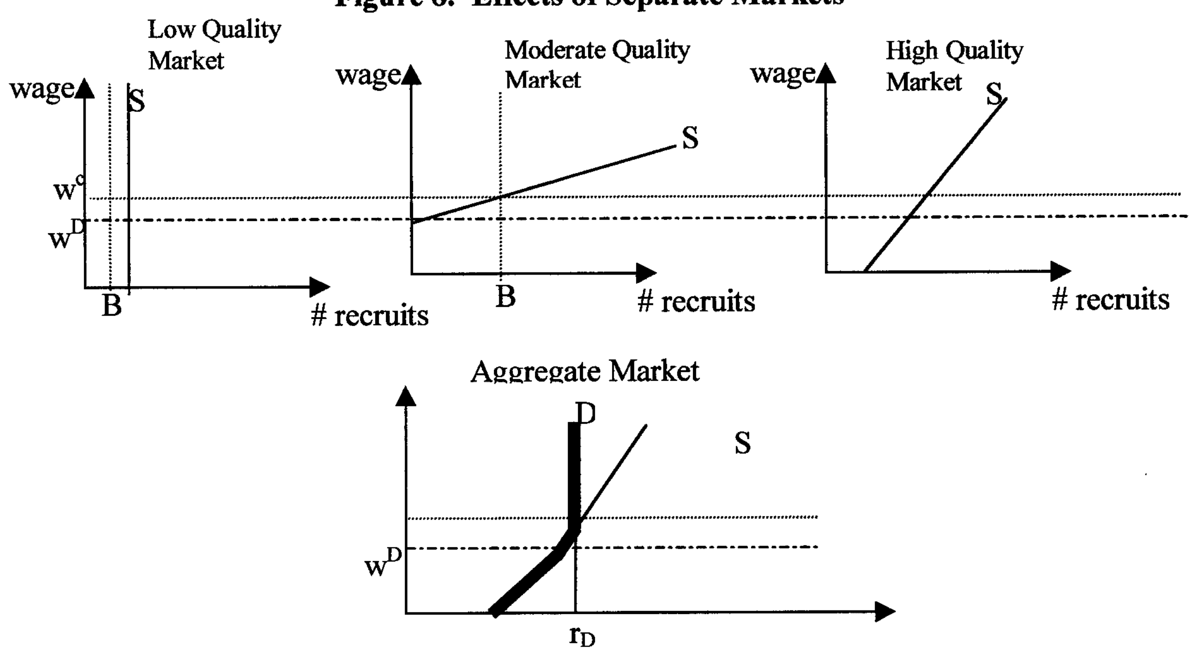
Figure 7. Inelastic Demand with Wage Stickiness



This representation has led many to suggest that at some wages, like w_1, the number of recruits is supply constrained, while at higher wages, like w_2, the number of recruits is demand constrained. Being demand constrained means that more individuals want to enlist than are permitted. This allows us to see some of the dynamics of the labor market for recruits, but it is too simple. There is actually a set of distinct labor markets that the military taps into to meet its demand for recruits. These separate markets are high quality, slightly above average quality, and low quality. This distinction is necessary because the US armed forces have benchmarks for the number of above average quality recruits desired and the alternative opportunities for these individuals are likely to be different. Benchmark gives the connotation that this is a goal, in reality it is a euphemism for the minimum required.

This separation of markets is complicated by Congressionally mandated standard compensation for the military and is the same for all individuals of a specific rank based upon time in service.[28] This hinders the ability to price discriminate across and within markets. Figure 8 depicts three distinct markets and includes a congressionally mandated wage, w^c, as well as benchmarks for two of the markets and the resulting simple aggregation graph.

Figure 8. Effects of Separate Markets



[28] Service members receive different cost of living adjustments based upon duty station location; the effect is to equate buying power.

The implication from examining the aggregate graph is that the military is being inefficient by paying a higher wage than is necessary to meet its manpower needs. Indeed it suggests that wages should be lowered to w^D. If wages are lowered to w^D, however, the actual requirements, as reflected by the benchmark in Market 2, will not be satisfied. Indeed, this slightly more complicated system shows that the demand in Market 3 is essentially a residual demand. The demand for labor in Market 3 becomes the difference between the total demand and the supply from Markets 1 and 2 based upon the Congressionally mandated wage, w^C. Clearly, any estimation based upon the aggregate model is inappropriate for policy analysis that is concerned about the different types of recruits.

With a full range of data on all markets, each curve can be estimated. If, however, the aggregate market is always demand constrained then things are more complicated. At least theoretically it is still possible to use the data from two markets plus the aggregate to back out the third. As a matter of practicality, this will lead to very imprecise estimates for the last market.

A significant shortfall of all previous research on the subject has been the use of a partial equilibrium framework for the analysis of recruiting. Most researchers do not account for the labor market as a dynamic interaction of competitors and supply. This has two potentially serious repercussions: civilian wages are taken as given and therefore do not respond to military wage changes, and it completely ignores potential market power. A common methodology uses the military civilian wage gap as the y-axis. This is incorrect, because it ignores the general equilibrium effects of a rise in the military wage, leaving no way of determining how much military wages must actually rise to change the gap by any specific amount. While the intent is to model the current situation, this methodology's inability to explain or even offer intuition to market dynamics leaves room for improvement.

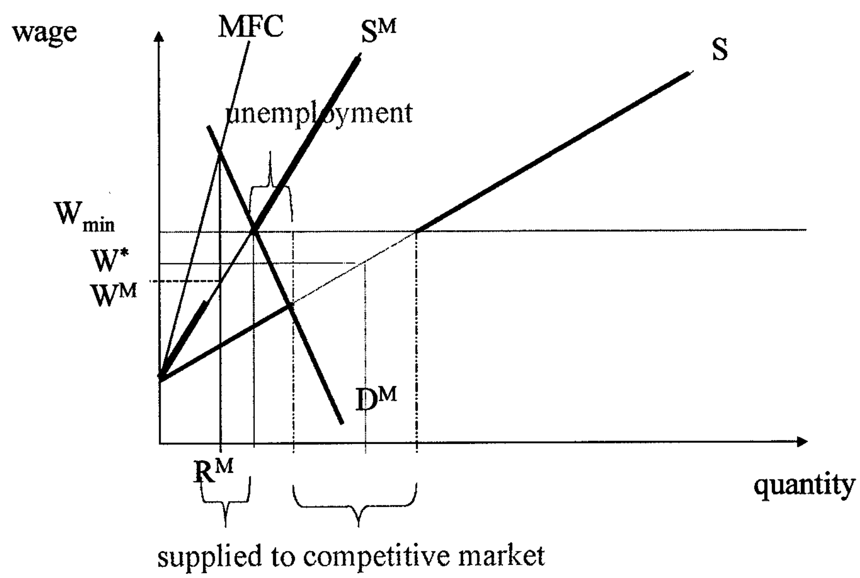
The military's market is different from firms'. Because an individual firm is faced with only a local labor market when concerned with hiring high-school graduates, it must compete locally for these individuals. The military, being a national employer is not constrained to one local market. The military has two options: first, to compete in the individual localized labor markets throughout the country; or second to tap into the residual supply of these markets on a national level. Certainly, entry by the military in one market does not preclude its entry into the other.

How does one explain a residual supply of high-school graduate laborers? The obvious story to tell uses the minimum wage as the linchpin holding the market out of a competitive equilibrium. Minimum wages, if they exceed the competitive equilibrium wage, will reduce demand and increase the number of agents who claim they are willing to work. Additionally, a minimum wage may actually cause those who would have been employed at the lower competitive wage to be displaced by more able agents who have higher reservation wages. Certainly there are other possible causes; our point is merely that there is an excess supply of workers to this market.

Following the classical model, it is natural to assume that the agents with the highest reservation wage are the ablest and therefore will be employed first. This means that the excess supply consists of the first set of individuals, not the last. This leads to a potential residual labor market as depicted in Figure 9. The supply curve remains as before, with the middle having been removed by the competitive market. Since the military is the only "firm" able to tap into this market, it has the ability to use this monopsony power to reduce costs by offering below market wages.

The military has the ability to enter the competitive market if it is unable to meet demand in the residual market. Simply focusing on the competitive

Figure 9. Monopsonistic Residual Labor Market



market as another firm is inefficient for the military. This means it must compete for these individuals, raising their costs if these individuals could be recruited from the residual market. It is simple to see that if the demand curve in the competitive market shifts out, the residual supply curve in Figure 9 loses more from the middle. While there is the ability to gain in this manner, it is also likely that this approach will recruit some agents at the minimum wage who would have been in the residual market. The answer to a cost-minimizing approach is to recruit the unemployed at the minimum wage, and if additional recruits are needed, use price discrimination to increase the supply to the market.

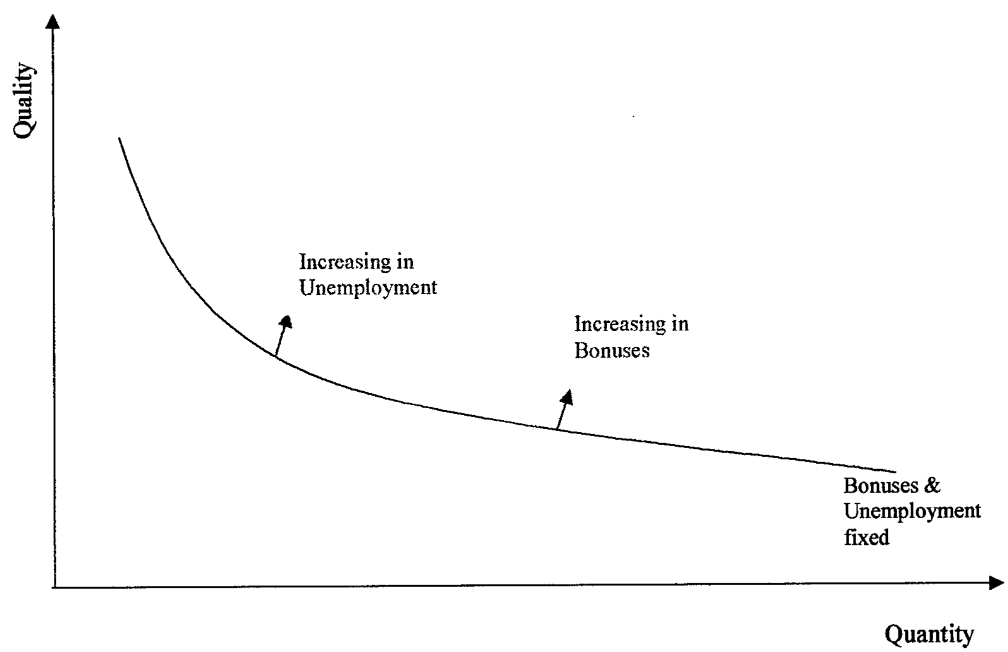
It is important to be able to price discriminate among agents. Otherwise, the marginal cost of hiring those who would not have been in the market is extremely high. The military enables its recruiters to price discriminate among recruits through a variety of bonuses and benefits that it can offer. Unfortunately, the number of bonuses available is extremely limited.

The limited bonus restricts the ability of recruiters to price discriminate, leaving them only with the ability to distinguish individuals based upon quality. If the military is recruiting efficiently, the most able of those willing should be recruited first. Only after the highest quality has been taken is it efficient to move down the quality scale.

With Congressionally mandated wages, the military would seem to have no choice but to reduce quality when it increases demand. The limited ability to price discriminate means this must be the case if the military is trying to maximize quality.

As with any model of labor supply, unemployment can affect the wage level, but the military's wage is inflexible. If quality is correlated with opportunities as we expect, then the distribution of quality should increase as the unemployment rate increases (see Figure 10). Bonuses should have a similar effect, however, the relative magnitude is the question.

Figure 10. Quantity-Quality Trade-off



Theory tells us unequivocally that there is no monopsony power if supply is perfectly elastic. Its power is only possible when we have an upward sloping supply schedule. The less elastic the supply, the greater the monopsony power. The first step in answering the question of whether or not the military possesses market power in the recruit market is to determine empirically whether or not changes in quantity effect quality.

Finally, we have suggested that the military recruits primarily in a set of residual markets. We believe, based upon general census and educational attainment data, the racial composition of these markets are slightly different. If these markets are composed of different mixes of the races, and unemployment affects the supply to the residual market, then we expect to see significant effects of quantity and unemployment on the overall racial composition of the recruits.

IV. DATA

The data are from two sources. Individual data are an extract from the Defense Manpower Data Center's (DMDC) enlisted accession file. This includes basic demographics on each active duty recruit (age, sex, race), date and place of entry, length and service of enlistment, whether or not a bonus was received, and Armed Forces Qualification Test score (**AFQT**). Unemployment data are from the Bureau of Labor Statistics and include local area and national rates.

The individual data cover all active duty accessions for fiscal years 1990 through 1998.[29] These recruits enter the military though one of 71 Military Entrance Processing Stations (MEPS). MEPS are located throughout the fifty states plus Guam and Puerto Rico. Figure 11 maps all of the MEPS except Guam. Areas with higher populations have more MEPS and areas with low populations have few stations.

[29] The data set used excludes National Guard and Reserve recruits.

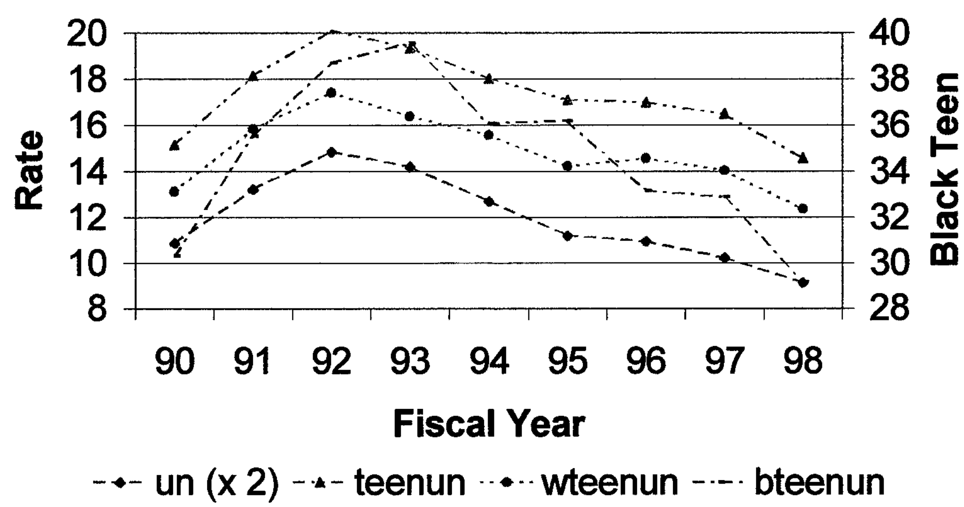
Figure 11. Military Entrance Processing Station (MEPS) Locations

MEPS are not recruiting districts; instead they are installations that provide the initial processing for all who pass through their doors. That said, there is nothing that prohibits a Hawaiian recruit from flying to San Juan and inprocessing at the MEPS there instead of Honolulu. This, however, is unlikely; all MEPS perform the same services and are equivalent except for their location. Given that there are no differences between MEPS, each can be thought of as having a catchment area, with recruits choosing the nearest MEPS.

The unemployment data are in two varieties: local and national. Local unemployment data are more limited than national data. Not all SMSAs, let alone local areas, meet the statistical requirements for release of teenage unemployment data. This limits our unemployment measure at the MEPS level to the unemployment rate for all adult males.

The unemployment rates available at the national level cover all adult males (**un**), teenagers (**teenun**), white teenagers (**wteenun**), and black teenagers (**bteenun**). Additionally, local area unemployment was weighted by the number of recruits in each area to provide an annual mean for the nation (**un(wt)**). The rates, with the exception of that for black teenagers, follow a similar pattern, rising until FY92 and then declining through FY98 (see Figure 12). The weighted unemployment rate is consistently less than the national statistic, although it remains within half a percentage-point. The teenage and white teenage rates are more than double the overall rates, with the teenage rate consistently two percentage-points higher than the white teenage rate. The black rate is more than double the white rate and five to six times the overall rate.

Figure 12. Unemployment Rate Comparison



The correlation between racial composition of the recruits and unemployment is striking (see Figure 13). The variety of unemployment and racial composition at the local level is indicated in Figure 14. While some MEPS have similar patterns, the effects of the 1990 recession and the recovery from it are different in many instances. Generally the correlation between percentage white and unemployment seen at the aggregate level holds for the individual MEPS as well.

Figure 13. Racial Composition and Unemployment FY90-98

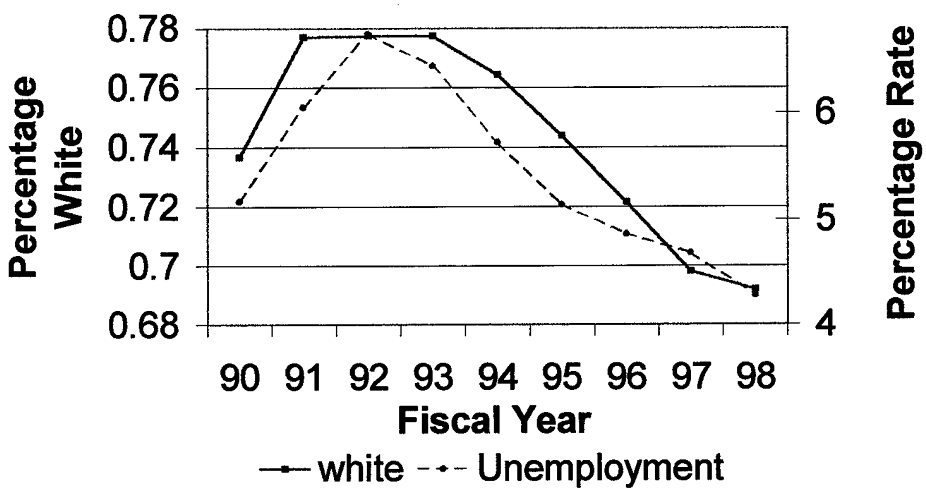
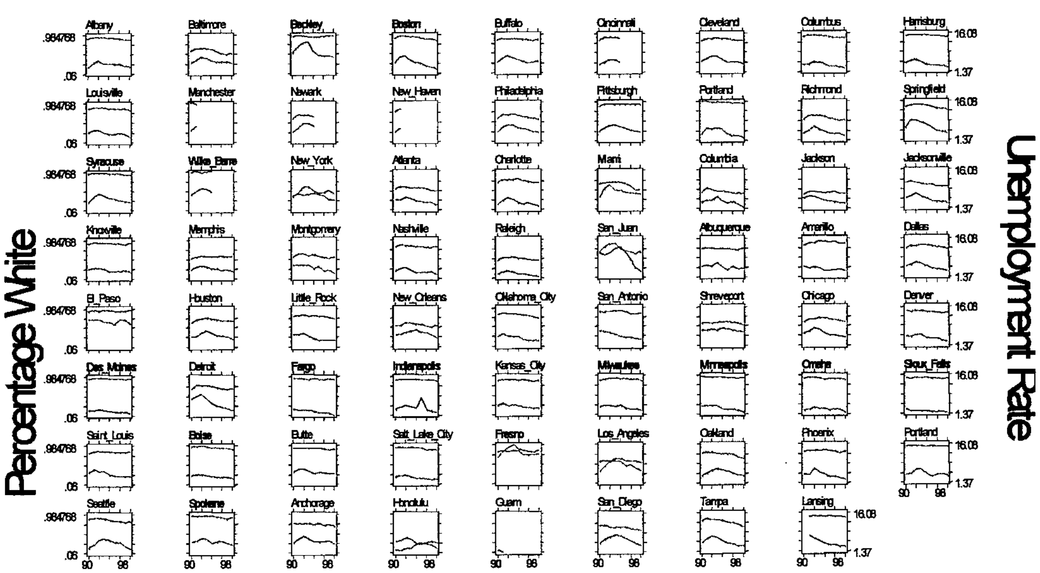


Figure 14. Unemployment Trends by MEPS



Unemployment data were not available for one MEPS: Guam. The consequence is a loss of almost one-tenth of one-percent of the individual observations and one-third of one-percent of the panel observations (see Table 12).

Table 12. Tabulation of Observations

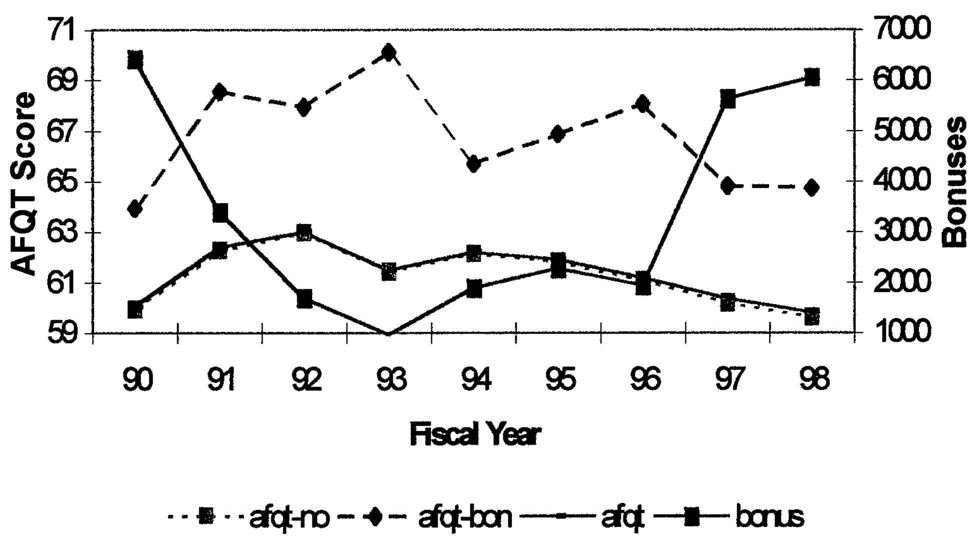
	Aggregate	MEPS Panel		Individual	
Original Observations	9	605		1560565	
Missing afqt scores	0	0		4997	0.32%
Missing individual data	0	0		8611	0.55%
Sub Total	9	605		1546957	
Lack of unemployment data	0	2	0.33%	208	0.01%
Final	9	603		1546749	

The data are combined for three levels of estimation. The local unemployment data are merged with the individual data to provide the individual level data set. The individual data set is used to calculate means of the individual characteristics for each MEPS by fiscal year. This creates a panel of 70 MEPS over 9 years. The final data set is a simple annual time series constructed by taking the means of individual data by fiscal year. This results in a sparse nine-observation data set, but allows the use of a variety of unemployment rates.

Four measures of quality, all based upon **AFQT** scores, are used throughout. The first is simply the raw **AFQT** score **(AFQT).** As noted earlier, **AFQT** scores categorize individuals into one of six mental categories (see Table 4). The next measure of quality will be individuals in the top mental category, **CAT1**. **CAT1** is a very exclusive category in that only eight-percent of the population falls into it. Since this is so exclusive, the next quality measure, **top2**, includes both **CAT1** and CAT2 individuals. Finally, since CAT3A is above average and the military defines a high quality recruit as being a high-school graduate and CAT3A or higher, **hi** includes all individuals from the 50th percentile and up on the **AFQT**.

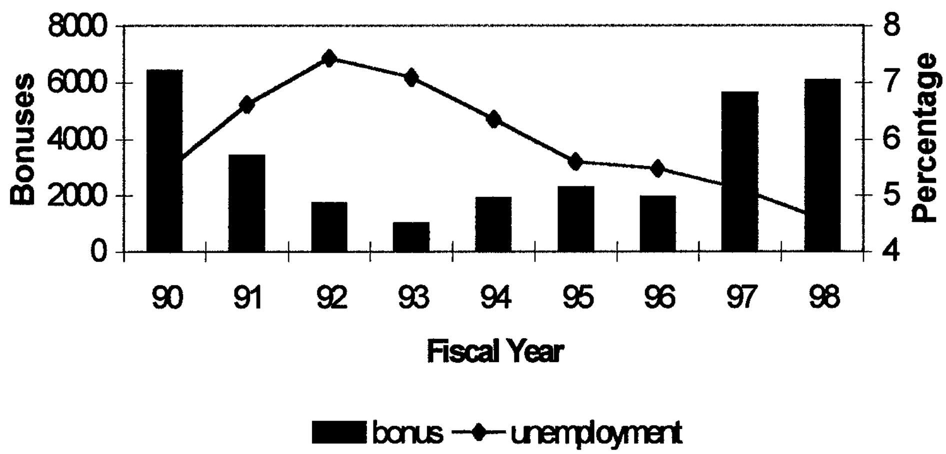
The military uses a dual criteria to establish what it calls a high quality recruit and we must limit ourselves to a single criterion due to a lack of information on educational attainment. While our data contain no information on education, we do know from Department of Defense (DOD) press releases that during fiscal years 1990 through 1998, the lowest percentage of high-school graduates in a given year was 94 percent in 1998. Since the minimum target AFQT changes based upon educational attainment (high-school graduates need lower scores while GED holders need higher scores, and dropouts need even higher scores to enlist) we need to be concerned that our quality measures are significantly different from the military's. However, in 1998, when we expect our quality measurements to be at their largest deviation from the military's, the military reported 68 percent of recruits were high quality versus our **hi** measure of 69.06%. Certainly there is a difference, but it is small and suggests our results should be robust to a high quality measurement including high-school diploma.

Figure 15. AFQT Scores and Bonuses FY90-98



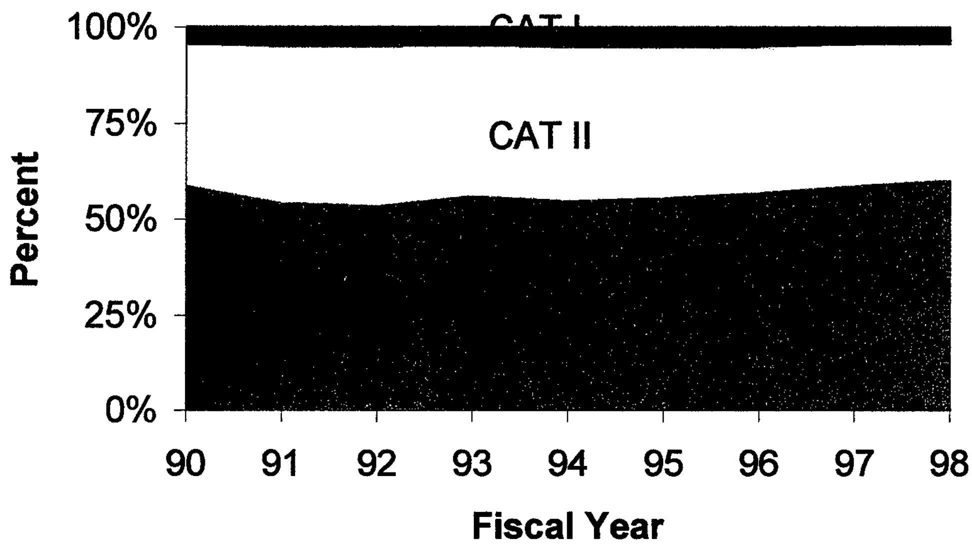
In an effort to increase quality, the military has used cash bonuses (**bonus**) to entice individuals into service. The number of bonuses given in our period started at just over 6000, or roughly three-percent of the recruits and fell through 1993 to a low of 973 and returned to more than 6000 in 1998 (see Figure 15). The mean AFQT score of bonus recipients has always exceeded non-recipients. The difference between the two is not constant; instead it seems to be negatively correlated with the number of bonuses awarded. A possible explanation is business cycle effects. Figure 16 shows that unemployment and bonuses move in opposite directions, at least at the national level. This coupled with the AFQT movements suggests that we must control for supply effects in our estimation. Also, it is likely that at any level of aggregation, we will assign erroneous effects to the use of bonuses, since they seem to be used only when quality is going down or high-quality recruits are hard to find.

Figure 16. Bonus Recipients and Unemployment FY90-98



During the 1990s the mean AFQT score fluctuated from 59 to 63 and the breakdown by mental category also varied slightly (see Figure 17). There are no large fluctuations in the percentages, although there seems to be a trade-off between CAT2 and CAT3B individuals. The number of recruits in 1990 was 203 thousand, and in 1995 when the number of recruits was a mere 149 thousand, the percentage of CAT1 recruits increased from 4.3 percent to 5.3 percent and the percentage of CAT4 was reduced from 2.4 percent to six-tenths of one-percent.

Figure 17. Mental Category Breakdown By Fiscal Year



The whole AFQT language can be puzzling at times. The military requires prospective recruits to take the AFQT. Normalizing the scores to the population imposes a uniform distribution of skills or quality. Since the military is unable to recruit from the lowest mental category, and has goals for at least 60 percent of recruits to score 50 or higher on the AFQT, we do not expect a uniform distribution of AFQT scores or quality among actual recruits. Figure 6 portrays the density and cumulative distribution of both the population and military recruits. The distribution of the military scores is not unexpected, except for the peculiarities created by the test itself. (There are a number of spikes away from any cut points and a lack of individuals with specific scores due to the inability to obtain particular scores.)

Quantity is measured in terms of recruits produced annually. At the aggregate level this is the total fiscal year's accessions of recruits nationwide and is denoted **fynobs** in the tables. At the MEPS **nobs** represents the total number of recruits processed at a particular MEPS during a fiscal year. The volume handled by the different MEPS varies greatly from a low of 50 to a high of 7327 (see Table 13).

Table 13. Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
age	19.7417	2.5015	17	35
afqt	61.3208	18.1803	10	99
female	0.1588	0.3655	0	1
black	0.1822	0.3861	0	1
othrace	0.0748	0.2631	0	1
nobs	3293.07	1356.64	50	7327
FY90	0.1314	0.3378	0	1
FY91	0.1113	0.3146	0	1
FY92	0.1160	0.3202	0	1
FY93	0.1154	0.3195	0	1
FY94	0.0972	0.2963	0	1
FY95	0.0965	0.2952	0	1
FY96	0.1059	0.3077	0	1
FY97	0.1152	0.3192	0	1
FY98	0.1111	0.3143	0	1
bonus	0.0196	0.1385	0	1
navy	0.2507	0.4334	0	1
airforce	0.1813	0.3852	0	1
marines	0.1846	0.3880	0	1
un	5.4604	2.0622	1.37	16.08
un-un[-1]	-0.1171	0.8478	-4.6	3.89

V. ESTIMATION AND ANALYSIS

The estimation is divided into three main categories: aggregate, panel, and individual. Within each category all measures of quality will be incorporated, as will two measures of minority representation: black and other minorities. Supply in a geographic area is a function of the available population and unemployment. Since we are only interested in a nine-year period, we assume that the local available labor force is fixed and the only fluctuations in supply come through variation in unemployment[30].

A. Aggregate Estimates

i. Quality

Nine observations allow for only a limited number of dependent variables, our five measures of unemployment allow for a comprehensive look at the components affecting aggregate quality and minority representation. Our specification is of one of the following forms:

A: $quality_t = \beta_0 + \beta_1 quantity_t + \beta_2 unemployment_t + \beta_3 bonus_t + \beta_4 X_t + \varepsilon_t$ (1)

where quality is any of our four measures, unemployment is any of our five measures, and X is a vector of aggregate demographic composition

B: $quality_t = \beta_0 + \beta_1 quantity_t + \beta_2 unemployment_t + \beta_3 bonus_t + \varepsilon_t$ (2)

C: $quality_t = \beta_0 + \beta_1 quantity_t + \beta_2 unemployment_t + \varepsilon_t$ (3)

Using our first measure of quality, **AFQT**, we find an extraordinary fit regardless of the specification (see Table 14). Our hypothesis that quantity demanded has a negative effect on quality is supported by all specifications including an adult measure of unemployment, but the test statistic becomes insignificant if either teenage or black teenage unemployment is used. Using white teenage unemployment still supports a negative effect of quantity on quality, but only at the 90 percent level. All bonus effects are insignificant. The

[30] Estimation including adult labor force size resulted in no qualitative change and little quantitative change.

Table 14. OLS Analysis on Aggregate Data

	fynobs	un(wted)	un	teenun	wteenun	bteenun	bonus	_cons	R-sq
afqt	-3.34E-05 **	1.1244 **						60.9712 **	0.8544 **
	(9.15E-06)	(0.1802)						(1.7933)	
	-2.89E-05 **		0.9535 **					60.7301 **	0.8442 **
	(9.28E-06)		(0.1595)					(1.8611)	
	-1.81E-05			0.4961 **				55.9613 **	0.7809 **
	(1.12E-05)			(0.1020)				(2.8649)	
	-2.12E-05 *				0.5702 **			56.6400 **	0.8333 **
	(9.69E-06)				(0.0993)			(2.3726)	
	-1.35E-05					0.2395 **		55.4840 **	0.6454 **
	(1.47E-05)					(0.0682)		(3.9358)	
	-3.82E-05 **	1.2796 **					15.2603	60.6599 **	0.8339 **
	(1.36E-05)	(0.3618)					(30.1100)	(2.0119)	
	-3.69E-05 **		1.2173 **				29.1104	59.9730 **	0.8399 **
	(1.28E-05)		(0.3300)				(31.7466)	(2.0591)	
	-2.15E-05			0.5966 **			20.4381	54.4169 **	0.7498 **
	(1.38E-05)			(0.2271)			(40.5309)	(4.3304)	
	-2.51E-05 *				0.6731 **		19.2142	55.4141 **	0.8129 **
	(1.22E-05)				(0.2037)		(32.5726)	(3.2610)	
	-1.27E-05					0.2206	-7.3197	56.1415 **	0.5761 *
	(1.70E-05)					(0.1550)	(52.6253)	(6.3926)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%. R-squared values are adjusted values with results of F-test for all coefficients equal to zero indicated using asterisks.

effects of quantity result in an estimated elasticity of quality with respect to quantity of -.0525 to -.1277.

Our next measure of quality is the percentage of recruits that fall into mental category one, **CAT1** (see Table 15). Quantity has a significantly negative effect in all specifications. When bonuses are excluded all measures of unemployment are significant except black teenage unemployment. The range of estimates for the quantity coefficients results in an estimated elasticity of -.3963 to -.7264. Again, the effect of quantity is significant and negative, supporting the theory that there is a quantity-quality trade-off.

Estimation with the **top2** mental categories combined results in almost a mirror image of the **AFQT** regressions (see Table 16). All specifications result in significant positive impacts of unemployment on quality, with the one exception of black teenage unemployment when bonuses are included. Quantity is only significant when measures of adult unemployment are used and drops from 95 percent level to 90 percent when bonuses are included. The coefficient on quantity, when significant, results in an estimated elasticity of quality with respect to quantity of -.1874 to -.3288.

Using **hi** quality results in weaker support of the quantity-quality trade-off theory. The specification with black teenage unemployment and bonuses result in an insignificant regression (see Table 17). Otherwise, all measures of unemployment are significant at the 95 percent level. Quantity is only significant at the 90 percent level when bonuses are included if a measure of adult unemployment is used and the only significant effect of quantity without bonuses is with our weighted measure of unemployment. The estimated quality quantity elasticity falls in the range from -.1173 to -.2445.

Regardless of the specification used for quality, the effect of quantity is always significant and negative. Bonuses are never significant, while all measures of unemployment are always positive, although not always significant. The

Table 15. OLS Analysis on Aggregate Data

	fynobs	un(wted)	un	teenun	wteenun	bteenun	bonus	_cons	R-sq
cat1	-1.78E-07 **	0.0019 **						0.0676 **	0.8076 **
	(3.30E-08)	(0.0007)						(0.0065)	
	-1.71E-07 **		0.0016 **					0.0676 **	0.8033 **
	(3.31E-08)		(0.0006)					(0.0066)	
	-1.55E-07 **			0.0008 *				0.0609 **	0.7523 **
	(3.78E-08)			(0.0003)				(0.0097)	
	-1.59E-07 **				0.0009 **			0.0617 **	0.7745 **
	(3.57E-08)				(0.0004)			(0.0088)	
	-1.48E-07 **					0.0004		0.0602 **	0.7197 **
	(4.13E-08)					(0.0002)		(0.0111)	
	-1.63E-07 **	0.0014					-0.0496	0.0686 **	0.7783 **
	(4.94E-08)	(0.0013)					(0.1092)	(0.0073)	
	-1.62E-07 **		0.0013				-0.0350	0.0686 **	0.7678 **
	(4.90E-08)		(0.0013)				(0.1213)	(0.0079)	
	-1.39E-07 **			0.0003			-0.0937	0.0680 **	0.7294 **
	(4.55E-08)			(0.0007)			(0.1337)	(0.0143)	
	-1.45E-07 **				0.0005		-0.0687	0.0660 **	0.7459 **
	(4.51E-08)				(0.0008)		(0.1204)	(0.0121)	
	-1.33E-07 **					0.0000	-0.1364	0.0725 **	0.7203 **
	(4.39E-08)					(0.0004)	(0.1356)	(0.0165)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%. R-squared values are adjusted values with results of F-test for all coefficients equal to zero indicated using asterisks.

Table 16. OLS Analysis on Aggregate Data

	fynobs	un(wted)	un	teenun	wteenun	bteenun	bonus	_cons	R-sq
top2	-6.35E-07 **	0.0240 **						0.4088 **	0.8515 **
	(1.92E-07)	(0.0038)						(0.0377)	
	-5.41E-07 **		0.0203 **					0.4041 **	0.8416 **
	(1.95E-07)		(0.0033)					(0.0391)	
	-3.12E-07			0.0105 **				0.3050 **	0.7545 **
	(2.47E-07)			(0.0022)				(0.0631)	
	-3.78E-07				0.0120 **			0.3187 **	0.8131 **
	(6.14E-07)				(0.0022)			(0.0523)	
	-2.13E-07					0.0051 **		0.2922 **	0.6333 **
	(3.10E-07)					(0.0014)		(0.0833)	
	-6.93E-07 *	0.0259 **					0.1867	0.4050 **	0.8247 **
	(2.92E-07)	(0.0077)					(0.6438)	(0.0430)	
	-6.68E-07 *		0.0245 **				0.4626	0.3920 **	0.8256 **
	(2.79E-07)		(0.0072)				(0.6898)	(0.0447)	
	-3.44E-07			0.0114 *			0.1884	0.2907 **	0.7078 **
	(3.10E-07)			(0.0051)			(0.9116)	(0.0974)	
	-4.21E-07				0.0132 **		0.2117	0.3052 **	0.7793 **
	(2.76E-07)				(0.0046)		(0.7364)	(0.0737)	
	-1.78E-07					0.0043	-0.3190	0.3209 *	0.5671 *
	(3.58E-07)					(0.0033)	(1.1069)	(0.1345)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%.

Table 17. OLS Analysis on Aggregate Data

	fynobs	un(wted)	un	teenun	wteenun	bteenun	bonus	_cons	R-sq
hi	-5.51E-07 **	0.0295 **						0.6528 **	0.7974 **
	(2.69E-07)	(0.0053)						(0.0528)	
	-4.32E-07		0.0248 **					0.6472 **	0.7708 **
	(2.80E-07)		(0.0048)					(0.0562)	
	-1.53E-07			0.0128 **				0.5260 **	0.6821 **
	(3.36E-07)			(0.0031)				(0.0858)	
	-2.34E-07				0.0147 **			0.5424 **	0.8096 **
	(2.97E-07)				(0.0030)			(0.0728)	
	-4.09E-08					0.0061 **		0.5169 **	0.5191 **
	(4.25E-07)					(0.0020)		(0.1140)	
	-8.39E-07 *	0.0387 **					0.9118	0.6342 **	0.8059 **
	(3.68E-07)	(0.0098)					(0.8115)	(0.0542)	
	-7.89E-07 *		0.0366 **				1.3018	0.6134 **	0.8119 **
	(3.46E-07)		(0.0089)				(0.8562)	(0.0555)	
	-3.22E-07			0.0177 **			1.0086	0.4498 **	0.6686 **
	(3.95E-07)			(0.0065)			(1.1604)	(0.1240)	
	-4.24E-07				0.0198 **		0.9420	0.4823 **	0.7460 **
	(3.54E-07)				(0.0059)		(0.9443)	(0.0945)	
	-6.27E-08					0.0066	0.2001	0.4989 **	0.4249
	(4.93E-07)					(0.0045)	(1.5250)	(0.1852)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%.

quantity-quality trade-off diminishes as we use broader definitions of quality, which supports the possibility of differentiated markets for recruits.

ii. Minority Representation

Minority representation was estimated using quantity, all of the unemployment measures, with and without bonuses, and with and without **AFQT** scores, requiring slightly different specifications:

A: $black_t = \beta_0 + \beta_1 quantity_t + \beta_2 unemployment_t + \beta_3 bonus_t + \varepsilon_t$ (4)

where black is either black or minority

B: $black_t = \beta_0 + \beta_1 quantity_t + \beta_2 unemployment_t + \beta_3 bonus_t + \beta_4 afqt_t + \varepsilon_t$ (5)

C: $black_t = \beta_0 + \beta_1 quantity_t + \beta_2 unemployment_t + \varepsilon_t$ (6)

D: $black_t = \beta_0 + \beta_1 quantity_t + \beta_2 unemployment_t + \beta_4 afqt_t + \varepsilon_t$ (7)

The first measure of minority representation used is the percentage of recruits who are black. The results are in Table 18. The coefficient on quantity is always positive, although only significant when measures of adult unemployment are used. Of significant note is the fact that all of the estimated coefficients on unemployment are negative and generally significant. AFQT scores are included to control for differences in quality and therefore allow unbiased estimation of the effect of quantity on racial composition.

The scores, while themselves not significant always increase the effect of quantity on black representation. This is exactly the opposite effect of what one expects to find in an organization in which blacks are over represented when compared to the population. If we look at the change from 1995, when recruits were at a low of 149 thousand, to 1997 when recruits were up to 178 thousand and teenage unemployment was roughly constant although adult unemployment was falling, we find the percentage of blacks rising from .1512 to .1678. Our

Table 18. Minority OLS Analysis on Aggregate Data

	fynobs	afqt	un(wt)	un	teenun	w teenun	b teenun	bonus	R-sq
black	4.98E-07 *	0.0014	-0.0193 **						0.9025 **
	(1.98E-07)	(0.0049)	(0.0059)						
	4.11E-07 *	0.0010		-0.0164 **					0.8971 **
	(1.84E-07)	(0.0050)		(0.0052)					
	1.27E-07	-0.0045			-0.0057				0.8129 **
	(1.88E-07)	(0.0057)			(0.0032)				
	1.97E-07	-0.0030				-0.0072			0.8077 **
	(2.11E-07)	(0.0066)				(0.0041)			
	2.58E-08	-0.0071					-0.0024		0.8117 **
	(1.73E-07)	(0.0045)					(0.0013)		
	4.50E-07 **		-0.0177 **						0.9174 **
	(1.03E-07)		(0.0020)						
	3.81E-07 **			-0.0154 **					0.9135 **
	(1.05E-07)			(0.0018)					
	2.07E-07				-0.0079 **				0.8251 **
	(1.52E-07)				(0.0014)				
	2.61E-07					-0.0089 **			0.8331 **
	(1.47E-07)					(0.0015)			
	1.21E-07						-0.0040 **		0.7660 **
	(1.80E-07)						(0.0008)		
	5.77E-07	0.0021	-0.0219 *					-0.1833	0.8846 **
	(2.72E-07)	(0.0055)	(0.0083)					(0.3864)	
	5.83E-07 *	0.0031		-0.0221 **				-0.4130	0.8967 **
	2.54E-07	(0.0054)		(0.0077)				(0.4168)	
	1.36E-07	-0.0044			-0.0060			-0.0430	0.7665 **
	(2.45E-07)	(0.0065)			(0.0051)			(0.6071)	
	1.86E-07	-0.0032				-0.0069		0.0391	0.7599 **
	(2.84E-07)	(0.0077)				(0.0062)		(0.5772)	
	2.71E-08	-0.0071					-0.0024	-0.0126	0.7647 **
	(2.02E-07)	(0.0050)					(0.0021)	(0.5942)	
	4.97E-07 **		-0.0192 **					-0.1482	0.9044 **
	(1.54E-07)		(0.0041)					(0.3411)	
	4.69E-07 **			-0.0183 **				-0.3233	0.9107 **
	(1.45E-07)			(0.0037)				(0.3585)	
	2.29E-07				-0.0086 **			-0.1320	0.7925 **
	(1.90E-07)				(0.0031)			(0.5583)	
	2.65E-07					-0.0090 **		-0.0215	0.7998 **
	(1.91E-07)					(0.0032)		(0.5097)	
	1.17E-07						-0.0039 *	0.0390	0.7194 **
	(2.10E-07)						(0.0019)	(0.6476)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%.

significant coefficients on quantity range from 3.81E-07 to 5.83E-07, resulting in an estimated change of .0110 to .01691 based upon the increased demand of 29 thousand recruits. Thus the change in quantity explains nearly all of the actual change in racial composition.

We have another measure of minority representation that we call **MINORITY** and includes two of the three racial categories used by the military in the data set: black and other, but not white. The results are much different from those for blacks (see Table 19). Quantity is never significant and changes sign depending upon the specification. Again AFQT scores and bonuses are never significant. All significant coefficients on unemployment are negative, although measures of teenage unemployment are only significant when AFQT and bonuses are not included in the specification. These results suggest that quantity has no effect on the overall demand for minorities.

The results from the **MINORITY** estimates imply different conclusions about the effect of changes in demand upon the racial composition of the recruits. Indeed, the results merely suggest that at the aggregate level an increase in demand results in an increased acceptance of blacks and that tighter labor markets increase the acceptance of all minorities into the military. We must remember that these results are at a highly aggregated level and may not be indicative of the underlying processes. For further insights into the underlying processes our estimation will move down a level of aggregation to our MEPS panel.

B. Panel Estimates

i. Quality

Estimation on the MEPS panel proceeded along similar lines as the aggregate data, except we now have both time series and cross-sectional data. Hausman specification tests rejected a random-effects specification at the 99

Table 19. Minority OLS Analysis on Aggregate Data

	fynobs	afqt	un(wt)	un	teenun	wteenun	bteenun	bonus	R-sq
minority	2.41E-07	-0.0004	-0.0367 **						0.8772 **
	(4.34E-07)	(0.0108)	(0.0128)						
	-4.99E-09	-0.0039		-0.0281 *					0.8349 **
	(4.55E-07)	(0.0124)		(0.0128)					
	-6.31E-07	-0.0225			-0.0041				0.6922 **
	(4.69E-07)	(0.0143)			(0.0079)				
	-5.87E-07	-0.0218				-0.0050			0.6901 **
	(5.22E-07)	(0.0164)				(0.0102)			
	-6.75E-07	-0.0196					-0.0034		0.7412 **
	(3.94E-07)	(0.0103)					(0.0030)		
	2.54E-07		-0.0371 **						0.8976 **
	(2.22E-07)		(0.0044)						
	1.09E-07			-0.0318 **					0.8597 **
	(2.60E-07)			(0.0045)					
	-2.25E-07				-0.0153 **				0.6162 **
	(4.38E-07)				(0.0040)				
	-1.25E-07					-0.0174 **			0.6509 **
	(4.13E-07)					(0.0042)			
	-4.10E-07						-0.0081 **		0.6272 **
	(4.43E-07)						(0.0021)		
	3.37E-07	0.0004	-0.0398 *					-0.2239	0.8491 **
	(6.07E-07)	(0.0124)	(0.0186)					(0.8610)	
	1.40E-07	-0.0022		-0.0328				-0.3470	0.7984 **
	(6.91E-07)	(0.0148)		(0.0211)				(1.1348)	
	-9.00E-07	-0.0255			0.0037			1.2799	0.6836 *
	(5.57E-07)	(0.0148)			(0.0116)			(1.3766)	
	-9.18E-07	-0.0258				0.0038		1.2068	0.6818 *
	(6.37E-07)	(0.0172)				(0.0140)		(1.2947)	
	-7.32E-07	-0.0193					-0.0020	0.5614	0.6903 *
	(4.52E-07)	(0.0113)					(0.0046)	(1.3279)	
	3.23E-07		-0.0393 **					-0.2174	0.8792 **
	(3.37E-07)		(0.0089)					(0.7472)	
	2.22E-07			-0.0355 **				-0.4116	0.8378 **
	(3.81E-07)			(0.0098)				(0.9417)	
	-3.52E-07				-0.0116			0.7589	0.5596 *
	(5.39E-07)				(0.0089)			(1.5844)	
	-2.69E-07					-0.0136		0.7102	0.6015 *
	5.25E-07					(0.0088)		(1.4008)	
	-4.87E-07						-0.0063	0.7028	0.5700 *
	(5.05E-07)						(0.0046)	(1.5618)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%.

percent level in favor of a fixed-effects regression for all specifications and are therefore not individually reported. The nine years of data along with the degrees of freedom gained from the panel allow us to estimate the effects of changes in unemployment in addition to the effect of the level of unemployment. This allows us to control for the level of unemployment and identify separately the effect of changes in unemployment(**Δun**). Unfortunately this is only possible by dropping the observations from 1990 due to the unavailability of local area unemployment data for all but the very largest metropolitan areas for 1989. The specifications for the quality estimations are as follows:

A: $quality_{jt} = \alpha_j + \lambda_t + \beta_1 quantity_{jt} + \beta_2 unemployment_{jt} + \beta_3 bonus_{jt} + \beta_4 X_{jt} + \varepsilon_{jt}$

where quality is any of our four measures, X is a vector of aggregate demographic composition, j indexes MEPS, and t indexes time. (8)

B: $quality_{jt} = \alpha_j + \lambda_t + \beta_1 quantity_{jt} + \beta_2 un_{jt} + \delta \Delta un_{jt} + \beta_3 bonus_{jt} + \beta_4 X_{jt} + \varepsilon_{jt}$ (9)

C: $quality_{jt} = \alpha_j + \lambda_t + \beta_1 quantity_{jt} + \beta_2 unemployment_{jt} + \beta_4 X_{jt} + \varepsilon_{jt}$ (10)

D: $quality_{jt} = \alpha_j + \lambda_t + \beta_1 quantity_{jt} + \beta_2 un_{jt} + \delta \Delta un_{jt} + \beta_4 X_{jt} + \varepsilon_{jt}$ (11)

All specifications incorporate MEPS fixed effects and fiscal year effects, and estimation is initially on our first quality measure, **AFQT** (see Table 20). Specifications without the change in unemployment are on nine years of data, while specifications with the change incorporate only eight years, reducing the number of observations from 603 to 533. The fit of all specifications is unsurprisingly not nearly as good as the results from the aggregate analysis.

The effect of quantity on quality is consistently negative and significant, ranging from -2.35E-04 to -2.62E-04. To operationalize these estimates we use San Diego to predict the change in AFQT from 1991 to 1992, when the number of recruits inprocessed at that MEPS increased by 3020, one of the largest changes in demand in our sample. This leads to a predicted decrease in AFQT score of .7097

Table 20. Fixed Effect Regression Results from MEPS Panel (FY Effects Included)

	nobs	un	un-un[-1]	bonus	female	black	othrace	age	_cons |	R-sq
afqt	-2.53E-04 **	0.0424		-0.2564	1.5207	-12.2757 **	-3.5521 **	0.6921 **	49.4130 **	0.3548
	(6.53E-05)	(0.0401)		(1.9962)	(2.2447)	(1.7578)	(1.0488)	(0.2793)	(5.4037)	
	-2.53E-04 **	0.0426			1.5201	-12.2580 **	-3.5537 **	0.6877 **	49.4848 **	0.3550
	(6.52E-05)	(0.0401)			(2.2426)	(1.7507)	(1.0477)	(0.2770)	(5.3695)	
	-2.52E-04 **	0.0446				-12.0133 **	-3.5858 **	0.6878 **	49.6355 **	0.3514
	(6.52E-05)	(0.0399)				(1.7122)	(1.0461)	(0.2768)	(5.3622)	
	-2.46E-04 **					-12.2875 **	-3.5575 **	0.7249 **	49.1267 **	0.3574
	(6.50E-05)					(1.6883)	(1.0381)	(0.2674)	(5.2235)	
	-2.62E-04 **	0.0635				-11.8683 **	-2.6324 **		62.9179 **	0.3760
	(6.54E-05)	(0.0394)				(1.7197)	(0.9780)		(0.4161)	
	-2.37E-04 **	0.0149	0.1085 **	-1.1215	-0.6445	-12.2270 **	-4.1429 **	1.0612 **	44.0095 **	0.3333
	(7.48E-05)	(0.0502)	(0.0462)	(2.1181)	(2.4260)	(2.0486)	(1.0712)	(0.3135)	(6.1189)	
	-2.37E-04 **	0.0161	0.1073 **		-0.6839	-12.1280 **	-4.1517 **	1.0419 **	44.3563 **	0.3339
	(7.47E-05)	(0.0501)	(0.0461)		(2.4229)	(2.0384)	(1.0702)	(0.3112)	(6.0788)	
	-2.38E-04 **	0.0149	0.1069 **			-12.2792 **	-4.1314 **	1.0384 **	44.3626 **	0.3361
	(7.45E-05)	(0.0499)	(0.0461)			(1.9647)	(1.0667)	(0.3106)	(6.0726)	
	-2.35E-04 **		0.1130 **			-12.4019 **	-4.1536 **	1.0550 **	44.1315 **	0.3439
	(7.38E-05)		(0.0413)			(1.9193)	(1.0630)	(0.3053)	(6.0169)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95

to .7912. The actual mean AFQT score decreased from 63.05 to 62.42, a reduction of .63. The actual change reflects the countering effects of changes in the demographic composition of the recruits at the MEPS between the two years. While this example is one of the largest changes during this period, it is less than the average increase expected if we increased the size of our military to that which we saw in the 1970s and early 1980s. The estimated elasticity of quality with respect to quantity is –0.0100 to –0.0112, much smaller than estimated with the aggregate data.

The level of unemployment has no significant effect on **AFQT** regardless of specification. The change in unemployment, however, is always significant and predicts an increase of roughly .11 in mean **AFQT** score for a one point increase in unemployment, or a change of .07 for the mean movement of .648 in unemployment. Conversely, this means that good economic times result in better opportunities elsewhere for higher quality youth and therefore lower average quality within the military. The coefficient on black is also always significant. It is also very large. The mean change in black within our sample is essentially zero, although this is caused by roughly equivalent numbers of increases and decreases. The mean absolute change in the fraction black is .013, which suggests an annual movement on average, given a coefficient estimate from -11.87 to -12.40, of .15 to .16 in **AFQT** scores based upon movements in the percentage black. This means that an average change in the black percentage of recruits is enough to more than wipe out the effects of a one percentage-point change in unemployment or even double them, depending upon the directions of the two.

Since we expect black and unemployment to move in opposite directions and they have opposite signs on their coefficients, the results are reinforcing. The average effect of the change in black composition is twice that of the average effect of the change in unemployment, thereby resulting in an overall effect that is three times that predicted by the change in unemployment alone. The results for other minorities are also always significant, but are much smaller and vary more depending upon the specification than blacks.

Estimation based upon our next measure of quality, **CAT1**, leads to much different results (see Table 21). The effect of quantity on quality remains significantly negative over all specifications, ranging from -1.54E06 to -1.96E-06. Using the same example of San Diego, an increase of 3020 from 1991 to 1992 results in a predicted decrease of .0047 to .0059, compared to the actual movement from .0518 to .0444; a decrease of .0084. Here again the change in quality can be explained by the change in quantity demanded. Interestingly, neither the level nor the change in unemployment has any effect on this measure of quality. Bonuses also have no effect. The estimated elasticity of quality with respect to quantity is –0.0801 to –0.1019, again much smaller than seen in the aggregate data, but not tiny.

The results from our next measure of quality, **top2**, are a mirror image of the results from using **AFQT** (see Table 22). The effect of quantity is always negative and significant, resulting in a predicted change of .0131 to .0164 for our San Diego example. The actual movement in San Diego during the period was from .4790 to .4544; a difference of .0246. Thus the change in demand, by itself, explains just over half of the actual movement.

The level of unemployment is never significant, while the change always is. The effect of an increase in unemployment of one percentage-point is to increase quality by .0034 to .0036, a movement of eight-tenths of one percent at the mean. Based upon our casual assessment, when observing the variation in composition of recruits by mental categories (see Figure 17) we should not be surprised that the results from **top2** and **AFQT** estimations are similar. After all, we noted earlier that most of the fluctuation in mental categories appeared to be a shift between CAT2 and CAT3B. The estimated elasticity of quality with respect to quantity is –0.0262 to –0.0328.

Table 21. Fixed Effect Regression Results from MEPS Panel (FY Effects Included)

	nobs	un	un-un[-1]	bonus	female	black	othrace	age	_cons |	R-sq
cat1	-1.60E-06 **	0.0004		-0.0120	-0.0670 **	-0.0231	-0.0244 **	0.0084 **	-0.1023 **	0.0766
	(6.09E-07)	(0.0004)		(0.0186)	(0.0209)	(0.0164)	(0.0098)	(0.0026)	(0.0503)	
	-1.59E-06 **	0.0004			-0.0671 **	-0.0223	-0.0245 **	0.0082 **	-0.0989 **	0.0752
	(6.08E-07)	(0.0004)			(0.0209)	(0.0163)	(0.0098)	(0.0026)	(0.0500)	
	-1.54E-06 **				-0.0651 **	-0.0258	-0.0255 **	0.0090 **	-0.1131 **	0.1245
	(6.06E-07)				(0.0208)	(0.0161)	(0.0097)	(0.0025)	(0.0487)	
	-1.69E-06 **	0.0005			-0.0729 **		-0.0235 **	0.0081 **	-0.0999 **	0.0000
	(6.04E-07)	(0.0004)			(0.0205)		(0.0097)	(0.0026)	(0.0501)	
	-1.75E-06 **	0.0006				-0.0313 *	-0.0118		0.0522 **	0.1993
	(6.17E-07)	(0.0004)				(0.0162)	(0.0092)		(0.0039)	
	-1.82E-06 **	0.0003	0.0005	-0.0103	-0.0741 **	-0.0238	-0.0228 **	0.0089 **	-0.1078 **	0.0740
	(7.17E-07)	(0.0005)	(0.0004)	(0.0203)	(0.0233)	(0.0196)	(0.0103)	(0.0030)	(0.0587)	
	-1.82E-06 **	0.0003	0.0004		-0.0744 **	-0.0229	-0.0229 **	0.0087 **	-0.1046 *	0.0720
	(7.17E-07)	(0.0005)	(0.0004)		(0.0232)	(0.0195)	(0.0103)	(0.0030)	(0.0583)	
	-1.96E-06 **	0.0001	0.0004			-0.0393 **	-0.0207 **	0.0083 **	-0.1039 *	0.1532
	(7.23E-07)	(0.0005)	(0.0004)			(0.0191)	(0.0103)	(0.0030)	(0.0589)	
	-1.93E-06 **		0.0005			-0.0404 **	-0.0209 **	0.0085 **	-0.1060 *	0.1699
	(7.16E-07)		(0.0004)			(0.0186)	(0.0103)	(0.0030)	(0.0584)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%.

Table 22. Fixed Effect Regression Results from MEPS Panel (FY Effects Included)

	nobs	un	un-un[-1]	bonus	female	black	othrace	age	_cons |	R-sq
top2	-5.16E-06 **	0.0007		0.0203	0.0552	-0.2767 **	-0.0998 **	0.0186 **	0.1069	0.3741
	(1.60E-06)	(0.0010)		(0.0489)	(0.0549)	(0.0430)	(0.0257)	(0.0068)	(0.1323)	
	-5.18E-06 **	0.0007			0.0552	-0.2781 **	-0.0997 **	0.0189 **	0.1012	0.3734
	(1.60E-06)	(0.0010)			(0.0549)	(0.0429)	(0.0256)	(0.0068)	(0.1314)	
	-5.15E-06 **	0.0007				-0.2692 **	-0.1008 **	0.0189 **	0.1067	0.3669
	(1.60E-06)	(0.0010)				(0.0419)	(0.0256)	(0.0068)	(0.1313)	
	-5.03E-06 **					-0.2693 **	-0.0922 **	0.0163 **	0.1587	0.3797
	(1.60E-06)					(0.0417)	(0.0256)	(0.0066)	(0.1290)	
	-5.42E-06 **	0.0012				-0.2652 **	-0.0746 **		0.4721 **	0.3994
	(1.60E-06)	(0.0010)				(0.0422)	(0.0240)		(0.0102)	
	-4.38E-06 **	-0.0006	0.0036 **	-0.0221	0.0289	-0.3069 **	-0.1064 **	0.0269 **	-0.0188	0.3747
	(1.84E-06)	(0.0012)	(0.0011)	(0.0522)	(0.0598)	(0.0505)	(0.0264)	(0.0077)	(0.1509)	
	-4.39E-06 **	-0.0005	0.0036 **		0.0281	-0.3049 **	-0.1065 **	0.0266 **	-0.0120	0.3753
	(1.84E-06)	(0.0012)	(0.0011)		(0.0598)	(0.0503)	(0.0264)	(0.0077)	(0.1499)	
	-4.34E-06 **	-0.0005	0.0036 **			-0.2987 **	-0.1074 **	0.0267 **	-0.0122	0.3716
	(1.84E-06)	(0.0012)	(0.0011)			(0.0485)	(0.0263)	(0.0077)	(0.1498)	
	-4.44E-06 **		0.0034 **			-0.2947 **	-0.1067 **	0.0262 **	-0.0047	0.3610
	(1.82E-06)		(0.0010)			(0.0473)	(0.0262)	(0.0075)	(0.1484)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%.

We now shift attention to the quality measure that most closely resembles the military's publicly used measure of quality, the percentage of recruits in the top three mental categories, **hi** (see Table 23). The effect of quantity on this measure of quality is again always significant and negative. Female and bonuses are insignificant and dropping them from the specification results in changing the effect of unemployment or the change in unemployment to become significant. The estimated elasticity of quality with respect to quantity is –0.0192 to –0.0201.

The MEPS panel quality estimates all show a significantly negative effect of quantity on quality. The magnitude of the effect changes with the measure of quality and the specification used, but regardless, that the change in demand affects quality supports the maintained hypothesis that there is a quantity-quality trade-off.

ii. Minority Representation

We now turn to analyzing the demand for blacks. In all cases the effect of the level of unemployment is negative and significant. This effect is mitigated with the incorporation of AFQT. The effect of AFQT, or controlling for quality, on unemployment is small if we only include the level of unemployment; but if we include the change in unemployment as well controlling for quality cuts the effect in half. This is easily seen in that the specification without AFQT results in an estimated coefficient on the level of unemployment of -.0054 (see Table 24) and an insignificant effect for the change in unemployment. Changing the specification to include merely one control for quality, **AFQT**, results in a jump in fit from an R-squared of .1344 to .4252, reduces the effect of the level of unemployment to -.0049, and makes the change in unemployment significant. The net result is that a one percentage-point increase in unemployment without controlling for quality results in a reduction in the percentage black by .0054, but

Table 23. Fixed Effect Regression Results from MEPS Panel (FY Effects Included)

	nobs	un	un-un[-1]	bonus	female	black	othrace	age	_cons |	R-sq
hi	-5.25E-06 **	0.0015		0.0237	0.0824	-0.2915 **	-0.0700 **	0.0040	0.6694 **	0.3621
	(1.62E-06)	(0.0010)		(0.0495)	(0.0557)	(0.0436)	(0.0260)	(0.0069)	(0.1341)	
	-5.28E-06 **	0.0015			0.0824	-0.2931 **	-0.0698 **	0.0044	0.6628 **	0.3607
	(1.62E-06)	(0.0010)			(0.0557)	(0.0435)	(0.0260)	(0.0069)	(0.1333)	
	-5.22E-06 **	0.0016				-0.2798 **	-0.0715 **	0.0044	0.6709 **	0.3550
	(1.62E-06)	(0.0010)				(0.0426)	(0.0260)	(0.0069)	(0.1333)	
	-5.29E-06 **	0.0018 *				-0.2789 **	-0.0655 **		0.7557 **	0.3644
	(1.62E-06)	(0.0010)				(0.0425)	(0.0242)		(0.0103)	
	-5.43E-06 **	0.0011	0.0018	0.0341	0.0241	-0.2932 **	-0.0901 **	0.0122	0.5549 **	0.3496
	(1.86E-06)	(0.0012)	(0.0011)	(0.0526)	(0.0602)	(0.0508)	(0.0266)	(0.0078)	(0.1519)	
	-5.41E-06 **	0.0011	0.0019		0.0253	-0.2962 **	-0.0898 **	0.0128 *	0.5444 **	0.3480
	(1.85E-06)	(0.0012)	(0.0011)		(0.0601)	(0.0506)	(0.0266)	(0.0077)	(0.1509)	
	-5.37E-06 **	0.0011	0.0019			-0.2906 **	-0.0905 **	0.0129 *	0.5441 **	0.3458
	(1.85E-06)	(0.0012)	(0.0011)			(0.0488)	(0.0265)	(0.0077)	(0.1507)	
	-5.16E-06 **		0.0023 **			-0.2997 **	-0.0922 **	0.0141 *	0.5270 **	0.3663
	(1.83E-06)		(0.0010)			(0.0477)	(0.0264)	(0.0076)	(0.1495)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%.

Table 24. Minority Fixed Effect Regression Results from MEPS Panel (FY Effects Included)

	afqt	nobs	un	un-un[-1]	bonus	female	age	_cons |	R-sq
black		1.66E-06	-0.0054 **	0.0017	-0.0964 **	0.3222 **	0.0116 *	-0.1039	0.1344
		(1.69E-06)	(0.0011)	(0.0011)	(0.0487)	(0.0538)	(0.0066)	(0.1299)	
	-0.0056 **	-9.83E-08	-0.0049 **	0.0022 **	-0.0972 **	0.3012 **	0.0141 **	0.2015	0.4252
	(0.0010)	(1.67E-06)	(0.0011)	(0.0010)	(0.0471)	(0.0522)	(0.0064)	(0.1372)	
	-0.0056 **	-1.31E-07	-0.0049 **	0.0021 **		0.3008 **	0.0124 *	0.2323 *	0.4182
	(0.0010)	(1.67E-06)	(0.0011)	(0.0010)		(0.0524)	(0.0064)	(0.1368)	
	-0.0044 **	8.08E-07	-0.0041 **	0.0024 **		0.2788 **	0.0152 **	0.0959	0.4068
	(0.0007)	(1.56E-06)	(0.0008)	(0.0008)		(0.0386)	(0.0052)	(0.1136)	
	-0.0045 **		-0.0041 **	0.0024 **		0.2774 **	0.0152 **	0.1046	0.3924
	(0.0007)		(0.0008)	(0.0008)		(0.0385)	(0.0052)	(0.1123)	
		4.15E-06 **	-3.93E-03 **		-0.0903 *	0.2683 **	0.0026	0.0956	0.1385
		(1.60E-06)	(9.89E-04)		(0.0498)	(0.0548)	(0.0065)	(0.1264)	
	-6.70E-03 **	1.90E-06	-3.32E-03 **		-0.0854 *	0.2595 **	0.0048	0.4623 **	0.4600
	(1.00E-03)	(1.58E-06)	(9.54E-04)		(0.0478)	(0.0527)	(0.0063)	(0.1332)	
	-6.73E-03 **	2.00E-06	-3.27E-03 **			0.2609 **	0.0033	0.4895 **	0.4577
	(1.00E-03)	(1.58E-06)	(9.55E-04)			(0.0528)	(0.0062)	(0.1326)	
	-6.70E-03 **	2.01E-06	-3.17E-03 **			0.2605 **		0.5511 **	0.4589
	(1.00E-03)	(1.58E-06)	(9.37E-04)			(0.0527)		(0.0618)	
	-6.97E-03 **		-3.08E-03 **			0.2629 **		0.5728 **	0.4203
	(9.79E-04)		(9.35E-04)			(0.0527)		(0.0594)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%.

when we include a control for quality the effect is a reduction of only .0027 (-.0049 + .0022).

The effect of female is significant and quite large, indicating a strong correlation between black and female recruitment. Unlike any of the previous estimations, bonuses are now significant, although their effect is to reduce the percentage of blacks. If we believe that bonuses are used in tight markets to recruit higher quality individuals whom the military would be unable to entice to serve otherwise, then we should expect the percentage of blacks to be higher in those markets, based upon the estimates of the impact of unemployment. This is not the case. Higher levels of bonus decrease black representation. The effects of quantity on black representation are insignificant unless we fail to control for quality. This means that blacks continue to be overrepresented among recruits when compared to the population, regardless of the quantity demanded.

Including all racial minorities as the dependent variable changes the effects of many key variables (see Table 25). Increased demand now always has a positive and significant effect. This means that when the military wants more recruits it takes a higher percentage of minorities, and when it needs fewer people, the percentage of minorities decreases. This effect persists even after controlling for quality. The effect of the level of unemployment is always negative and significant. Controlling for quality mitigates the effect of unemployment, but only slightly, resulting in a reduction of 14 to 25 percent from the effect without controlling for quality. Bonuses, changes in unemployment, and percent female have no significant effect.

While the lack of effect of demand on the percentage black can be explained by redefining the population of comparison, the positive effect on minorities as a whole is not as simple. When we look at 1997 high-school graduates, 14.23 percent are black, while 26.36 percent are black or Hispanic (US Census Bureau 1997). If we further restrict this population to those not enrolled

Table 25. Minority Fixed Effect Regression Results from MEPS Panel (FY Effects Included)

	afqt	nobs	un	un-un[-1]	bonus	female	age	_cons |	R-sq
minority		1.55E-05 **	-0.0075 **	0.0020	-0.0501	0.1169	0.1274 **	-2.3254 **	0.3441
		(3.52E-06)	(0.0024)	(0.0022)	(0.1014)	(0.1120)	(0.0138)	(0.2705)	
	-0.0120 **	1.17E-05 **	-0.0064 **	0.0030	-0.0518	0.0721	0.1327 **	-1.6715 **	0.5447
	(0.0021)	(3.46E-06)	(0.0023)	(0.0021)	(0.0980)	(0.1085)	(0.0133)	(0.2851)	
	-0.0120 **	1.17E-05 **	-0.0064 **	0.0030		0.0719	0.1318 **	-1.6551 **	0.5434
	(0.0021)	(3.46E-06)	(0.0023)	(0.0021)		(0.1084)	(0.0132)	(0.2832)	
	-0.0121 **	1.18E-05 **	-0.0064 **	0.0031			0.1321 **	-1.6465 **	0.5317
	(0.0021)	(3.45E-06)	(0.0023)	(0.0021)			(0.0132)	(0.2827)	
		1.32E-05 **	-0.0042 **		-0.0564	0.1394	0.0988 **	-1.7300 **	0.3409
		(3.03E-06)	(0.0019)		(0.0940)	(0.1036)	(0.0123)	(0.2388)	
	-0.0115 **	9.35E-06 **	-0.0032 *		-0.0479	0.1242	0.1024 **	-1.1002 **	0.5547
	(0.0019)	(3.00E-06)	(0.0018)		(0.0910)	(0.1003)	(0.0119)	(0.2535)	
	-0.0115 **	9.41E-06 **	-0.0031 *			0.1250	0.1016 **	-1.0849 **	0.5537
	(0.0019)	(3.00E-06)	(0.0018)			(0.1002)	(0.0118)	(0.2517)	
	-0.0116 **	9.54E-06 **	-0.0030 *				0.1014 **	-1.0610 **	0.5314
	(0.0019)	(3.00E-06)	(0.0018)				(0.0118)	(0.2511)	

Note: Standard Errors are in Parenthesis below estimates. One asterisk indicates significance at 90%, two indicates significance at 95%.

in college, the percentages increase to 17.42 for blacks and 30.12 for blacks and Hispanics. The overall percentage of black military recruits varies from 13.6 percent in 1991 to 16.9 percent in 1990. Since not all recruits have no college and we have no way to control for education, we should expect the percentage of blacks to be between 14 and 17 percent if recruiting were completely random over this population. The overall percentage of minorities varies from 20.3 percent in 1993 to 27.0 percent in 1998.

A random draw of minorities would be expected to fall between 26 and 30 percent. While blacks certainly seem to fall within what we expect, minorities do not, especially given that the military includes all non-whites in the definition of minority and the comparable statistic is restricted to blacks and Hispanics. Certainly, the 1998 number seems representative of a random draw, but the earlier lower percentages are in conflict with the 1997 CPS data. We do know that minority representation in the overall population has been increasing throughout this period, and our data is from the end of our sample, so it is possible that the minority representation is representative.

Now that we have at least a plausible explanation for the effects of demand on minority representation, what about unemployment? Perhaps the negative effect of unemployment really indicates a shift in the possible recruit pool from merely the non-college high-school graduates to include some portion of those who enrolled in college. Certainly, the military prefers some college experience to simply a high-school diploma, and the unemployment rate will affect the supply of these individuals. The only way of knowing for sure if this is the case is by controlling for education, which we cannot do. The prominent alternative explanation, would, of course, be racism, but without a control for education this is unsubstantiated. The implication, regardless of the explanation preferred, is that the military possesses the ability to discriminate across markets.

C. Individual Estimates

i. Quality

1. OLS Estimation

Individual estimation begins with simple OLS using our continuous measure of quality, **AFQT**. The specifications are very similar to the ones used previously, except we are now indexing over individual recruits and have only one observation per recruit:

A: $afqt_i = \beta_0 + \beta_1 quantity_i + \beta_2 unemployment_i + \beta_3 bonus_i + \beta_4 X_i + \lambda_t + \varepsilon_i$ (12)

where X is a vector of aggregate demographic composition

B: $afqt_i = \beta_0 + \beta_1 quantity_i + \beta_2 unemployment_i + \beta_3 bonus_i + \beta_4 X_i + \lambda_t + \xi_j + \varepsilon_i$ (13)

C: $afqt_i = \beta_0 + \beta_1 quantity_i + \beta_2 un_i + \delta \Delta un_i + \beta_3 bonus_i + \beta_4 X_i + \lambda_t + \varepsilon_i$ (14)

D: $afqt_i = \beta_0 + \beta_1 quantity_i + \beta_2 un_i + \delta \Delta un_i + \beta_3 bonus_i + \beta_4 X_i + \lambda_t + \xi_j + \varepsilon_i$ (15)

All specifications allow for yearly fixed effects. Specifications A and B use the all years of the full sample, resulting in 1.5 million observations, while specifications C and D use the change in unemployment and therefore reduce the data available for estimation by one year to 1.3 million observations. Specification B is equivalent to A, and D to C, but with the addition of MEPS fixed effects.

Table 26 provides the results from the regressions using all specifications. The first thing to note is the adjusted R-squared values range from .0789 to .0856. That does not seem very impressive especially given the number of observations, but perhaps it should be. In all specifications, we know nothing about the development of the individual, only essentially outwardly visible characteristics. Let us reiterate this; we know nothing about the individual's education, family background, or work experience, yet we still can explain a full eight percent of the variation in quality among these individuals.

Table 26. Regression Results from Individual Data

	afqt		afqt		afqt		afqt	
R-sq	0.0789		0.0856		0.0762		0.0827	
un	-0.4237	**	0.0514	**	-0.4455	**	0.0686	**
	(0.0076)		(0.0219)		(0.0082)		(0.0292)	
un-un[-1]					0.4676	**	0.0703	**
					(0.0238)		(0.0264)	
nobs	0.0002	**	-0.0003	**	0.0002	**	-0.0003	**
	(0.0000)		(0.0000)		(0.0000)		(0.0000)	
black	-12.2685	**	-12.2247	**	-12.0531	**	-12.0022	**
	(0.0372)		(0.0395)		(0.0402)		(0.0426)	
othrace	-6.6374	**	-6.4214	**	-6.4873	**	-6.2448	**
	(0.0547)		(0.0567)		(0.0577)		(0.0598)	
female	0.8152	**	0.7705	**	0.5361	**	0.4949	**
	(0.0387)		(0.0386)		(0.0411)		(0.0409)	
age	0.4131	**	0.4270	**	0.4269	**	0.4420	**
	(0.0056)		(0.0056)		(0.0060)		(0.0060)	
bonus	1.5066	**	1.4996	**	1.5476	**	1.5365	**
	(0.0281)		(0.0280)		(0.0312)		(0.0311)	
FY91	2.2669	**	1.5402	**				
	(0.0580)		(0.0639)					
FY92	3.2088	**	2.1783	**	1.0642	**	0.6430	**
	(0.0582)		(0.0695)		(0.0594)		(0.0636)	
FY93	1.6077	**	0.7241	**	-0.0730		-0.7340	**
	(0.0578)		(0.0664)		(0.0660)		(0.0703)	
FY94	2.1583	**	1.4058	**	0.6646	**	-0.0150	
	(0.0604)		(0.0687)		(0.0726)		(0.0754)	
FY95	1.7585	**	1.2978	**	0.1838	**	-0.1237	*
	(0.0602)		(0.0666)		(0.0707)		(0.0732)	
FY96	1.0058	**	0.8300	**	-0.7391	**	-0.6065	**
	(0.0583)		(0.0615)		(0.0660)		(0.0696)	
FY97	0.1402	**	0.2295	**	-1.6061	**	-1.2031	**
	(0.0569)		(0.0593)		(0.0654)		(0.0727)	
FY98	-0.4726	**	-0.2147	**	-2.1475	**	-1.6270	**
	(0.0578)		(0.0629)		(0.0677)		(0.0798)	
constant	56.0599	**	55.2275	**	57.7150	**	56.3649	**
	(0.1256)		(0.1944)		(0.1360)		(0.2307)	
F(69,1546664)			163.152	**				
F(69, 1343012)							138.41	**

Note: OLS results include MEPS fixed effects when F-statisitic is reported. Standard errors are reported in parenthesis directly below coefficients. One asterisk indicates significance at the 90% level, two at the 95% level.

The effect of unemployment differs based upon whether or not MEPS fixed effects are included. If we do not control for inherent unmeasured difference between areas, then increased unemployment has a negative effect on quality. Certainly a competitive labor market predicts no change in quality, but not a negative effect, so while this certainly does not support a monopsony argument, it cannot be seen as evidence against it. Instead, the wrong sign is indicative of an omitted variable bias that is caused by mis-specification resulting from the absence of MEPS fixed effects. This is further substantiated by conducting an F-test on the set of fixed effects that clearly rejects the null that they are jointly zero.

Focusing on the fixed-effects regressions shows that higher unemployment means higher quality, as do increases in unemployment. The effect of demand on quality is negative and significant, resulting in an estimated decrease in **AFQT** of .906 based upon our San Diego example. To see the equivalent effect caused by unemployment necessitates a decrease of more than 17 percentage-points using the full sample, or six and a half percentage-points using specification D with the change in unemployment.

The effect of bonuses is significant, positive, and looks large. The effect is to increase **AFQT** score by a full point and a half. This seems at first seems to dwarf the effect from the change in demand from our San Diego example, but this is not the case. The bonus change affects but one individual, the one who receives the bonus, the changes in demand affect all individuals who entered the service through the San Diego MEPS in 1992, more than five thousand individuals. Therefore, to offset the negative effect of increasing demand by 3020 individuals, the military would have to issue 3,525 bonuses. If we assume a cost of one thousand dollars per bonus (the smallest bonus available[31]), this results in a

[31]Enlistment bonuses range from one to eight thousand dollars and are capped at twelve thousand by Congress (Brophy 1996).

cost of 3.5 million dollars to offset the effect of the increased demand. If instead of looking at San Diego, we focus on 1995 to 1996 when the nationwide demand increased from 149 thousand to 178 thousand, then almost 15 thousand bonuses would be necessary to offset the reduction in quality. At one thousand dollars a piece, that is 15 million dollars. If on the other hand bonuses tend to be from the high end of the structure, the cost could easily triple.

A more interesting question is what is the cost of increasing the quality by one percentage-point *ceteris paribus*. Given that the military has completed its drawdown and demand looks like it will stabilize at 200 thousand recruits per year, a on- point increase will require bonuses for two-thirds of entrants, or 133 thousand, at a cost of at least 123 million dollars. This represents an increase of roughly 50 percent in the DOD's recruiting budget, based upon figures from 1997.

Minority effects are all negative, as seen in most of the previous estimates. The strength of the effect is surprising, consistent with the full panel estimation for blacks, suggesting blacks score a full twelve points lower on the **AFQT** than whites. The effect of other races is even stronger than seen in the panel estimation, showing more than a six-point lower **AFQT** score for racial minorities other than blacks when compared to whites. Additionally females score one-half to three-fourths of a point higher on the **AFQT**.

While **AFQT** scores are one way to measure quality, what is the difference between an individual who scores 50 versus 51? The common argument is that given the possibility of measurement error, there is no difference. So are we interested in increasing the mean **AFQT** score, or the percentage of the recruits from one of our other measures of quality? The other measures of quality are all based upon the mental category breakdown used by the military (see Table 4).

2. Probit Estimation

Unlike our panel and aggregate estimations, the mental categorization by **AFQT** scores gives rise to an ordered latent variable model, because individuals either do or do not fall into one of these categories. The latent variable, **AFQT,** is actually observed. This coupled with the desire to allow for non-linear effects and an assumption of normality results in an ordered probit[32], with the following log-likelihood:

$$LF=\prod_{i=1}^{n} P_{i,cat}^{y_{i,cat}}=\prod_{i=1}^{n} P_i(cat1)^{y_{i,cat1}}\, P_i(cat2)^{y_{i,cat2}}\, P_i(cat3a)^{y_{i,cat3a}}\, P_i(cat3b)^{y_{i,cat3b}}\, P_i(cat4)^{y_{i,cat4}}$$

$$\begin{aligned}LF=\prod_{i=1}^{n}&\left[1-\Phi\left(cat1-\sum_{i=1}^{n}X_i\beta\right)\right]^{y_{i,cat1}}\left[\Phi\left(cat1-\sum_{i=1}^{n}X_i\beta\right)-\Phi\left(cat2-\sum_{i=1}^{n}X_i\beta\right)\right]^{y_{i,cat2}}\\&\left[\Phi\left(cat2-\sum_{i=1}^{n}X_i\beta\right)-\Phi\left(cat3a-\sum_{i=1}^{n}X_i\beta\right)\right]^{y_{i,cat3a}}\\&\left[\Phi\left(cat3a-\sum_{i=1}^{n}X_i\beta\right)-\Phi\left(cat3b-\sum_{i=1}^{n}X_i\beta\right)\right]^{y_{i,cat3b}}\left[\Phi\left(cat3b-\sum_{i=1}^{n}X_i\beta\right)\right]^{y_{i,cat4}}\end{aligned} \tag{16}$$

Φ and ϕ represent the normal cumulative and normal density functions respectively. This formulation includes parameters Category 1, Category2, Category 3A, and Category 3B, which are the separation points in probability space between categories. Reaching these break points is equivalent to climbing up one category. The actual contribution to the likelihood by any given observation is much simpler because $y_{i,cat?}=1$ if the observation falls into that mental category and zero otherwise. Even so, the resulting estimation is somewhat time consuming when we use our 1.5 million observations.[33] While this provides us with one set of coefficient estimates, by itself it does us little

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[32] We ignore and boundary problems and assume normality for computational ease.

[33] Actual estimation time on a SPARC20 machine with 256M of RAM was around four hours and varied slightly depending upon the specification.

good if we are after the effects of the covariates. Indeed, the sign of the coefficient does not tell us even the direction of the effect by itself. Additionally, the marginal effect is different depending upon the category of interest, and must change directions over the categories. This is clear when we examine Figure 18. The marginal effect is the change in probability of being in a given category. When we shift the distribution, it must have a zero net effect. The formula for recovering the marginal effects is straightforward:

Marginal effect on category1 $\varphi(\text{cat1} - X_0\beta)\beta$ (17)

Marginal effect on category2 $[\varphi(\text{cat2} - X_0\beta) - \varphi(\text{cat1} - X_0\beta)]\beta$ (18)

Marginal effect on category3a $[\varphi(\text{cat3a} - X_0\beta) - \varphi(\text{cat2} - X_0\beta)]\beta$ (19)

Marginal effect on category3b $[\varphi(\text{cat3b} - X_0\beta) - \varphi(\text{cat3a} - X_0\beta)]\beta$ (20)

Marginal effect on category4 $-\phi(X_0\beta)\beta$ (21)

Because the model is nonlinear the marginal effects are also nonlinear and are only valid around the point where they are taken. This necessitates choosing an X vector around which to compute the marginal effect. While any vector could be chosen, it is reasonable to use the mean for continuous variables and to turn off all indicator variables. The calculations of marginal effects are then straightforward using equations 17-21. A z-statistic (because of the normality assumption we have z instead of t) is reported instead of standard errors to save space. All categories have the same statistic because the marginal effects though different are all calculated from the same coefficient estimate. Therefore the z-statistic incorporates all of the information necessary to obtain standard errors or confidence intervals, without giving the illusion that the individual category effects are based upon separate estimates.

Five category ordered probits were estimated using specifications B and D (fixed effects only) from the simple OLS regressions (see Table 27). All covariates are significant at the 95 percent level. Unemployment increases the

Figure 18. Marginal Effects in Five Category Ordered Probit

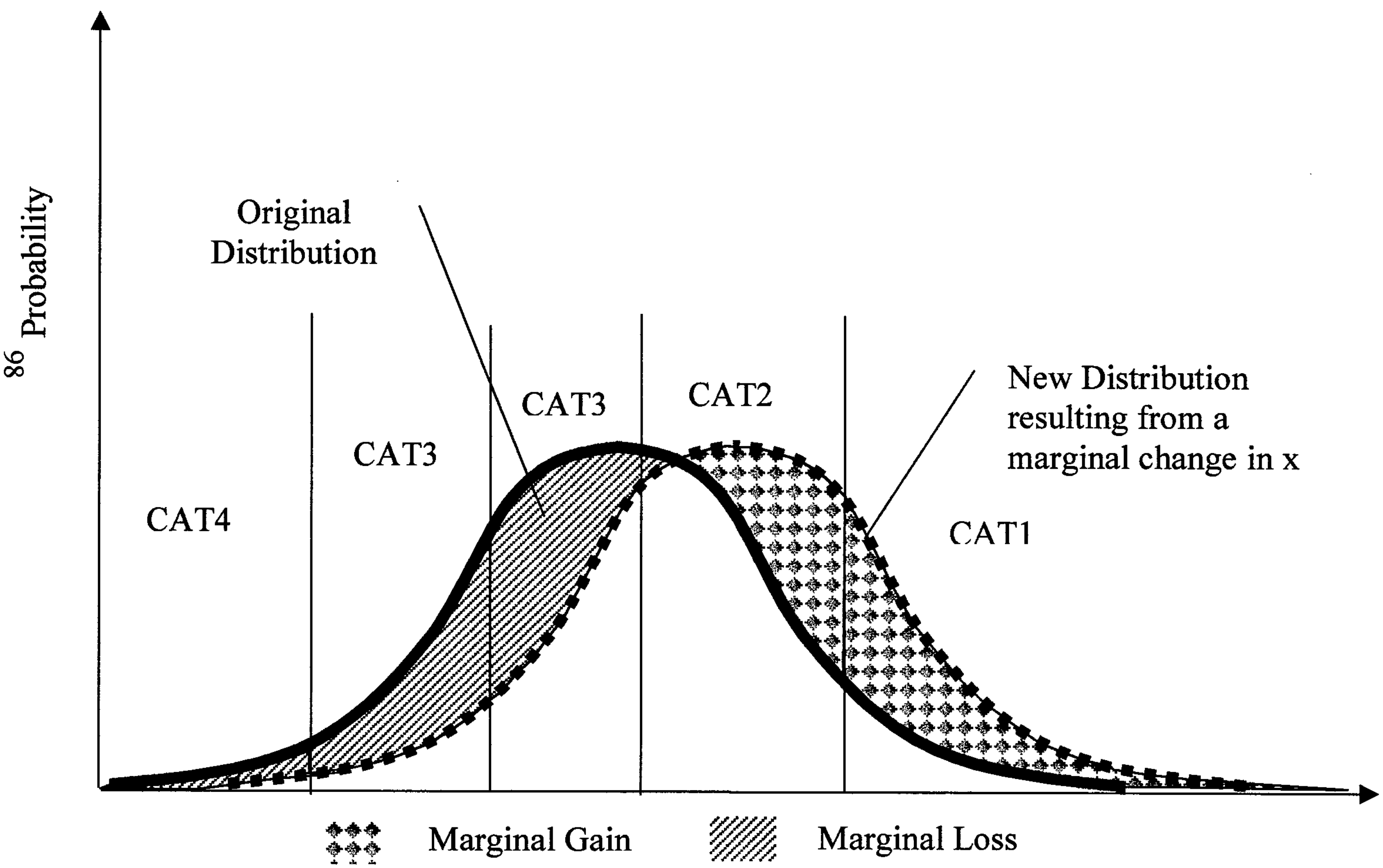


Table 27. Marginal Effects from Five Category Ordered Probit on Individual Data.

	cat1	cat2	cat3a	cat3b	cat4		z
un	0.0004	0.0016	-0.0003	-0.0016	-0.0001	**	3.77
nobs	-1.40E-06	-5.15E-06	8.86E-07	5.36E-06	3.04E-07	**	-8.607
black	-0.0598	-0.2192	0.0377	0.2283	0.0129	**	-285.65
othrace	-0.0310	-0.1137	0.0196	0.1184	0.0067	**	-105.02
female	0.0055	0.0202	-0.0035	-0.0211	-0.0012	**	27.633
age	0.0020	0.0072	-0.0012	-0.0075	-0.0004	**	66.719
bonus	0.0258	0.0947	-0.0163	-0.0986	-0.0056	**	49.403

	cat1	cat2	cat3a	cat3b	cat4		z
un	4.74E-04	1.43E-03	-3.83E-04	-1.47E-03	-5.17E-05	**	2.63
un-un[-1]	4.39E-04	1.32E-03	-3.54E-04	-1.36E-03	-4.79E-05	**	2.70
nobs	-1.89E-06	-5.68E-06	1.52E-06	5.84E-06	2.06E-07	**	-8.51
black	-0.0697	-0.2095	0.0562	0.2154	0.0076	**	-259.69
othrace	-0.0359	-0.1079	0.0290	0.1109	0.0039	**	-97.03
female	0.0045	0.0136	-0.0036	-0.0139	-0.0005	**	17.90
age	0.0024	0.0073	-0.0020	-0.0075	-0.0003	**	65.41
bonus	0.0316	0.0950	-0.0255	-0.0976	-0.0034	**	45.36

Note: Ordered Probit estimation includes both MEPS fixed effects and fiscal year effects. Reported results are marginal effects, with z-statistic reported in far right column for each covariate. One asterisk indicates significance at the 90% level, two at the 95% level.

probability of being in the top two categories and decreases the probability of being in all three others. Quantity has a negative effect on quality, reducing the percentage of Category 1 and Category 2, while increasing all other categories. Bonuses are significant at any level and have a strong effect on quality.

If we use bonuses to determine a measure of the cost of increasing quality we can arrive at four different, but all useful figures. The first is the cost of increasing the number of Category 1 individuals by a full percentage-point. Since the receipt of a bonus increases the probability of being in category one by .0258, our results suggest raising **CAT1** by one percentage-point requires giving 100 of every 258 recruits a bonus. If we assume a recruit population of 200 thousand, as is expected for the next several years, this results in a need for 77,519 bonuses. If we are able to induce these individuals cheaply, this results in a cost of 77.5 million dollars.

Using the same logic, increasing the top two mental categories by a total of one percentage-point necessitates 16,598 bonuses or a cost of 16.6 million dollars. Just increasing **hi** quality (top three categories) by one point actually costs more. This is due to the predicted negative effect bonuses have on Category 3a. The result is a need for 19,194 bonuses, or a cost of at least 19.2 million dollars.

If we want a number comparable to the continuous estimates, we must calculate the cost of increasing AFQT scores by one point. The cheapest way to do this is to increase the number of Category 1 and Category 2 sufficiently to increase the average AFQT by one point. Most of the movement is actually between Category 3B and Category 2. These individuals are more likely to be at the top (Category 3B) or bottom (Category 2) of their AFQT range, so we will use a difference of 16 points for this change (65-49) and 44 for the change to Category 1 (93-49). This means that a one point increase in Category 1 increases AFQT scores .0044. Instead of just requiring a one percentage-point increase in

CAT1 or **top2** we now require more. In fact we need an increase of 4.547 points in **top2** to accomplish an overall increase of one point in AFQT scores. This results in a cost of 75.5 million dollars and should be considered towards the high end of possible estimates. Even so, this figure is little more than half what we found using OLS.

In addition to the ordered probit models, specifications B and D were used with our three discrete quality measures in estimating standard probit models (see Table 28). Using **CAT1**, unemployment is insignificant and the effect of bonuses is very small. The effect of quantity is nearly the same as in the ordered probit. The small effect of bonuses, however, results in a predicted inability to raise the percentage of category one individuals by a full one point using bonuses.

The results from **top2** indicate that changes in unemployment matter, but the level does not. The effect of quantity is negative and results in an estimated elasticity of –0.0426 to –0.0441. Bonuses, while still significant, have a smaller effect. The result is a cost of 32 million dollars to increase the percentage of **top2** by one point, or roughly double the estimate from the ordered probit model.

Probit estimation on **hi** results in insignificant effects on the change in unemployment, while the level is significant. The effect of bonuses is quite strong, reducing the estimated cost of increasing **hi** by one point to 10.3 million dollars. Quantity has a slightly smaller effect than in **top2**, resulting in an elasticity of –0.0222 to –0.0261.

ii. Minority Representation

Table 29 depicts the marginal effects of probit estimation on minority variables. Using black as the dependent variable, the level and change in unemployment are significant, although they have opposite signs. The effect is that higher levels of unemployment result in higher representation of blacks, but increasing unemployment reduces representation. This is indicative of higher

Table 28. Marginal Effects from Probit Analysis on Individual Data

	un	un-un[-1]	nobs	black	othrace	female	age	bonus
cat1	2.38E-04		-1.44E-06 **	-0.0441 **	-0.0217 **	-0.0142 **	0.0058 **	0.0060 **
	(2.33E-04)		(3.34E-07)	(0.0003)	(0.0004)	(0.0004)	(0.0001)	(0.0011)
cat1	2.47E-04	8.99E-05	-1.80E-06 **	-0.0442 **	-0.0216 **	-0.0146 **	0.0060 **	0.0080 **
	(3.13E-04)	(2.80E-04)	(3.89E-07)	(0.0003)	(0.0004)	(0.0004)	(0.0001)	(0.0013)

	un	un-un[-1]	nobs	black	othrace	female	age	bonus
top2	9.74E-04		-7.05E-06 **	-0.2705 **	-0.1401 **	-0.0047 **	0.0104 **	0.0624 **
	(6.35E-04)		(9.07E-07)	(0.0009)	(0.0015)	(0.0011)	(0.0002)	(0.0030)
top2	7.35E-04	2.56E-03 **	-7.29E-06 **	-0.2679 **	-0.1375 **	-0.0103 **	0.0107 **	0.0641 **
	(8.46E-04)	(7.65E-04)	(1.04E-06)	(0.0010)	(0.0016)	(0.0012)	(0.0002)	(0.0033)

	un	un-un[-1]	nobs	black	othrace	female	age	bonus
hi	2.15E-03 **		-5.98E-06 **	-0.2619 **	-0.1372 **	0.0667 **	0.0006 **	0.1866 **
	(5.67E-04)		(8.09E-07)	(0.0011)	(0.0016)	(0.0009)	(0.0001)	(0.0017)
hi	2.88E-03 **	1.25E-03	-7.02E-06 **	-0.2562 **	-0.1333 **	0.0582 **	0.0008 **	0.1945 **
	(7.54E-04)	(6.86E-04)	(9.26E-07)	(0.0012)	(0.0017)	(0.0010)	(0.0002)	(0.0018)

Note: Probit estimation includes both MEPS fixed effects and fiscal year effects. Quality variables are indicator variables for the individual. Reported results are marginal effects, with standard errors directly below in parenthesis. One asterisk indicates significance at the 90% level, two at the 95% level.

Table 29. Marginal Effects from Probit Analysis of Minority Variables on Individual Data

	un	un-un[-1]	nobs	afqt	female	age	bonus
black	-3.68E-03 **		3.28E-06 **		0.0896 **	0.0018 **	-0.0222 **
	(4.63E-04)		(6.36E-07)		(0.0009)	(0.0001)	(0.0020)
black	-4.44E-03 **	2.42E-03 **	6.67E-07		0.0893 **	0.0017 **	-0.0278 **
	(6.19E-04)	(5.60E-04)	(7.28E-07)		(0.0010)	(0.0001)	(0.0021)
black	-3.28E-03 **		1.31E-06 **	-4.56E-03 **	0.0902 **	0.0030 **	0.0020
	(4.40E-04)		(6.02E-07)	(1.61E-05)	(0.0009)	(0.0001)	(0.0021)
black	-3.73E-03 **	2.40E-03 **	-1.27E-06 *	-4.42E-03 **	0.0880 **	0.0029 **	-0.0034
	(5.88E-04)	(5.33E-04)	(6.89E-07)	(1.71E-05)	(0.0009)	(0.0001)	(0.0023)

	un	un-un[-1]	nobs	afqt	female	age	bonus
minority	-1.85E-03 **		8.05E-06 **		0.0949 **	0.0042 **	-0.0393 **
	(5.52E-04)		(7.48E-07)		(0.0010)	(0.0001)	(0.0024)
minority	-5.08E-03 **	3.16E-03 **	5.34E-06 **		0.0957 **	0.0041 **	-0.0458 **
	(7.40E-04)	(6.80E-04)	(8.57E-07)		(0.0011)	(0.0001)	(0.0026)
minority	-1.34E-03 **		5.84E-06 **	-5.86E-03 **	0.0976 **	0.0060 **	-0.0111 **
	(5.47E-04)		(7.41E-07)	(1.99E-05)	(0.0010)	(0.0001)	(0.0025)
minority	-4.27E-03 **	3.46E-03 **	3.09E-06 **	-5.71E-03 **	0.0964 **	0.0060 **	-0.0176 **
	(7.34E-04)	(6.75E-04)	(8.48E-07)	(2.14E-05)	(0.0011)	(0.0001)	(0.0028)

Note: Probit estimation includes both MEPS fixed effects and fiscal year effects. Minority variables are indicator variables for the individual. Reported results are marginal effects, with standard errors directly below in parenthesis. One asterisk indicates significance at the 90% level, two at the 95% level.

representation of blacks in markets where unemployment is higher. Controlling for quality by including **AFQT** results in diminishing the effect of unemployment, but only slightly. Controlling also for quality reduces the negative effect of bonuses to zero.

Using all minorities, unemployment and the effect of controlling for quality are the same as with blacks. Now, however, the effect of bonuses is still negative when quality is controlled for, although it is less than one-third of the uncontrolled estimate. As with the panel estimates, the effect of quantity is to increase minority representation.

VI. CONCLUSIONS

While the results presented have been quite numerous and at times contradictory, one thing is clear: no matter the level of aggregation or the measure of quality one prefers, there is a quantity- quality trade-off in the labor market. Table 30 summarizes the results from the different levels of aggregation and measures of quality. We prefer the estimates from the individual level because they are indicative of the underlying processes and also allow us to quantify costs through the use of bonuses.

Using either **AFQT** or **hi** as our measure of quality allows us to determine a quantity-unemployment-bonus trade-off, or iso-quality surface. Figure 19 depicts one iso-quality surface for **hi** individuals. The trade-off for **hi** is:

$$(4.27)\%\Delta quantity - (3.23)\%\Delta un = \%\Delta bonus \qquad (22)$$

The trade-off for **AFQT** is:

$$(25.4)\%\Delta quantity - (9.2)\%\Delta un = \%\Delta bonus \qquad (23)$$

These iso-quality equations, (22) and (23), belie the magnitude of the unemployment effect. A one percentage-point change in unemployment is almost a 20 percent change, making unemployment by far the largest effect.

Table 30. Summary of Estimated Elasticities by Level of Aggregation

Elasticity of quality, quantity

	aggregate	MEPS	Individual
afqt	-0.0525	-0.0100	-0.0127
	-0.1277	-0.0112	-0.0128
CAT1	-0.3963	-0.0801	-0.0749
	-0.7264	-0.1019	-0.0936
top2	-0.1874	-0.0262	-0.0426
	-0.3288	-0.0328	-0.0441
hi	-0.1173	-0.0192	-0.0222
	-0.2445	-0.0201	-0.0261

Elasticity of quality, unemployment

	aggregate	MEPS	Individual
afqt	0.0854	0.0000	0.0046
	0.1145	0.0000	0.0061
CAT1	0.1747	0.0000	0.0000
	0.2075	0.0000	0.0000
top2	0.2578	0.0000	0.0000
	0.3111	0.0000	0.0000
hi	0.1935	0.0000	0.0168
	0.2855	0.0000	0.0225

Effect of a one point increase in unemployment on quality

	aggregate	MEPS	Individual
afqt	1.5631	0.0000	0.0843
	2.0977	0.0000	0.1125
CAT1	3.2000	0.0000	0.0000
	3.8000	0.0000	0.0000
top2	4.7209	0.0000	0.0000
	5.6977	0.0000	0.0000
hi	3.5429	0.0000	0.3071
	5.2286	0.0000	0.4114

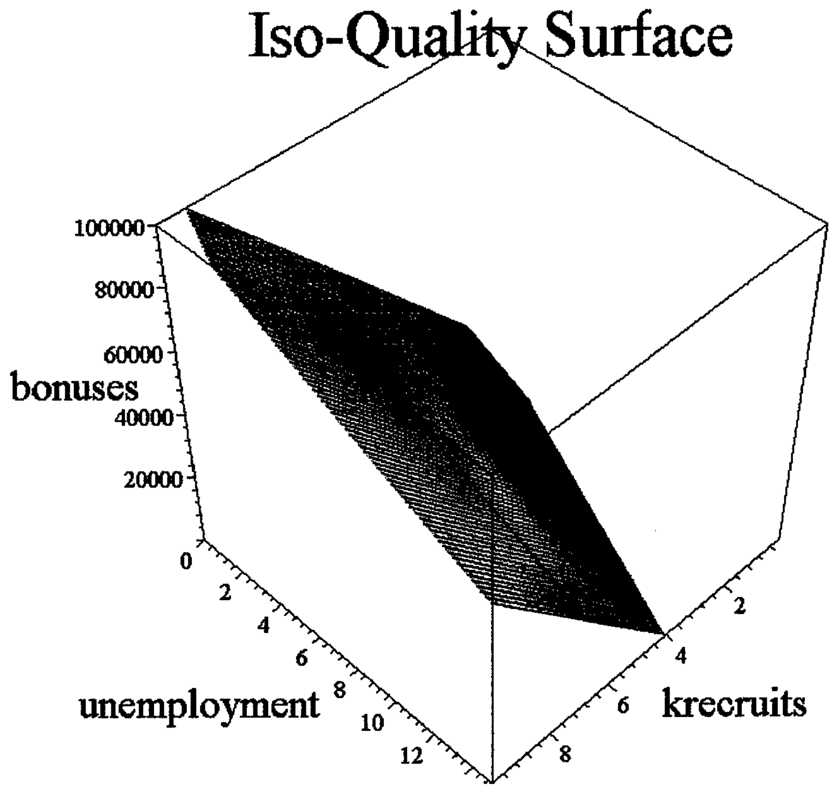
Elasticity of quality, bonuses

	aggregate	MEPS	Individual
afqt	0.0000	0.0000	0.0005
	0.0000	0.0000	0.0007
CAT1	0.0000	0.0000	0.0024
	0.0000	0.0000	0.0043
top2	0.0000	0.0000	0.0028
	0.0000	0.0000	0.0040
hi	0.0000	0.0000	0.0052
	0.0000	0.0000	0.0075

Note: Upper and lower estimates of elasticities are shown by level of aggregation and quality measure. A zero indicates that all relevant specification resulted in insignificant elasticities at the 90 percent level.

The estimated effects of quantity and unemployment are much greater using **AFQT** as our measure of quality. The cost to maintain quality of a one percentage-point reduction in unemployment is therefore at least 2.4 million dollars, while the cost of an increase in quantity of one percent is at least 170 thousand dollars.

Figure 19. Quantity, Unemployment, Bonus Trade-off (Iso-Quality Surface)



The estimation of minority representation provides new insights into the dynamics of labor markets. The AVF has always been overrepresented by minorities, but this overrepresentation does not hold when we focus on the appropriate market. Instead, it appears that minorities are proportionally represented from the available pool of male high-school graduates.

There is no indication of racial discrimination by the military. Indeed, the opposite seems to be the case. Minority representation in the recruit market increases as unemployment decreases, suggesting the civilian market prefers others. The military, on the other hand, prefers quality regardless of color.

The result of a quality-quantity trade-off, is likely to be a quality sacrifice under conditions of low unemployment. The economy in 1999 is at the lowest level of unemployment since the draft ended. The military's budget has been consistently reduced throughout the 1990's. To make matters worse, the military has completed its downsizing and must now maintain a full 200 thousand recruits annually. The future is clear: Decreased unemployment coupled with increased demand and no more monetary enticements will lead to lower quality recruits.

Despite the presence of a fixed wage rate mandated by Congress, we have still found a precondition for monopsony. To exploit monopsony power, a firm must be faced with an upward sloping supply schedule. The lack of flexibility in the military wage scale makes this seem impossible. However, the supply schedule is indeed upward sloping when we consider quality. This suggests at a minimum the potential for monopsony.

Added to the manpower problems of the military is the ability of the Congress to wield monopsony power unknowingly. Since the military faces an upward sloping supply schedule it is possible to exploit recruits by taking some of the consumer's surplus (see figure 9). This exploitation can be seen as a tax for national defense. Even if we view this as a tax, it is not fair because of the overrepresentation of minorities and the lack of representation by college graduates. The military by definition cannot exploit monopsony power because it does not have the ability to set wages. The Congress does that and any analysis of military pay is incomplete without accounting for the uneven splitting of the surplus.

While this research has demonstrated that the quantity-quality trade-off exists, future research can do much more. The cost figures can be estimated much more precisely with information on the amount of bonus received. Additionally, specifics on any other enticements like the buddy program[34], would allow estimates of the value of these programs and could lead to a more efficient use of recruiting resources.

Whether the military services compete with each other for recruits or have synergistic effects is important in efficient allocation of recruiting resources. This requires data similar to what we used for this study, but must also include a breakdown by service. A similar question arises in the competition between the National Guard and the Active components for recruits. This question would need additional data on the National Guard recruits to be answered.

The idea of a quantity quality trade-off certainly is not new. The empirical support of its existence is, however, new. The magnitude of the trade-off and the effects of unemployment and bonuses should allow policy analysts to make informed recommendations when considering changes in military manpower.

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[34] One of the many "buddy programs" offers the same initial assignment location to friends who sign up together.

Chapter 4: Is Military Service Good For Those who Serve:

The US and German Cases

I. Introduction

As the volunteer military in the United States approaches its thirtieth anniversary, the question that looms is whether or not those who choose to serve benefit from their sacrifice. We explore the effects of military service on lifetime earnings to answer this question.

Our focus is on veterans of the All-Volunteer Force (AVF) of the United States, but we will also briefly look at Korean and Vietnam era veterans. This look backwards is necessary to show consistency with previous research. Additionally, this retrospective provides insight into the problems inherent in using aggregate or cross-section data to identify the effect of military service on lifetime earnings.

We also use data on German youth to understand a variety of questions. We look at returns in Germany, where conscription forces male youth to choose military or community service, not whether to serve. Using German data provides comparisons of returns to military service and allows us to see whether sorting patterns are similar across countries. Finally, the differences in the methods used to acquire new recruits for military service across countries should allow us to distinguish the returns to skill acquisition in the military from the loss of civilian work experience.

Why should one be interested in the effects of military service on lifetime earnings? Currently, the US Congress is debating a major restructuring of the entire military compensation package. This includes both increases in salary and retirement benefits. An integral component in deciding which of the proposed plans is best requires understanding the total costs and benefits to the individuals who choose to serve. The basis of this cost/benefit analysis must include the lifetime effects on individuals. Additionally, there is a question of social equity. While we are not currently at war, the possibility is always real. Equity requires that the increased risk of injury either be spread equally among the classes of society or compensated monetarily.

The remainder of this chapter is divided into five sections. Section II will briefly review the literature on the returns to military service, with an emphasis on empirical findings. Following the literature review, Section III focuses on the theoretical framework necessary for our empirical work. With a theoretical model in hand, Section IV delves into the data, and Section V presents the estimation and analysis. Finally, Section VI summarizes our results and provides conclusions.

II. Literature Review

The numerous studies on the effects of military service, as with most empirical work, are constrained by data. The availability of data limited early research to cross-section studies of the effects in the US only. The availability of more extensive data and greater computing power has led to pooled, or aggregate panel, estimation and also matched panels. Only within the last ten years have studies extended to other countries, and these are either driven by policy questions in terms of the cost of a draft, or are tertiary results of other work.

The use of cross-section or aggregate data is inherently problematic for the question of returns to military service. The significant hurdle to overcome is at least a two-sided selection problem. First, the military screens individuals medically and mentally before accepting them for service. This means that even if everyone wanted to serve, some would be found unfit for service. An individual found unfit for military service is likely to have lower potential earnings than someone who is fit. The result is not a random. Furthermore, individuals have the ability either to choose whether or not to serve or possibly influence their likelihood of being called for a draft. The resulting population in the military, therefore, cannot be a random draw from the population. Any research that does not account for this selectivity is necessarily biased.

Research on the US has either focused upon the veterans of a particular era, or has separately identified the effects for veterans of several eras. The eras of interest coincide with the major conflicts the US has fought (World War II, Korea, and Vietnam), and the current post-draft period. The research that examines returns to military service separately for each of these eras is looking for changes by era in addition to quantifying the total effect.

The results of previous research tabulated by country and era are summarized in Table 31. The table further identifies studies by econometric technique used, type of instrument if one was used, and data type. Studies involving multiple eras are listed separately for each era for which they identify returns to military service.

All research on the World War II era finds significant positive returns to veteran status, except Angrist and Krueger (1994) who control for selection into the military by using the individual's quarter of birth as an instrument. Surprisingly, the two studies that control for selection find opposite effects. Rosen and Taubman (1982) find an 11.6 percent premium for veterans while Angrist and Krueger (1994) find a 6 to 15 percent reduction in post service earnings. Based upon the data sets, Rosen and Taubman seem to have a stronger case, because they have a micro panel versus a cross-section. However, the micro panel is matched using earnings over a twenty-year period and demographics from the 1970 census. Unfortunately the resulting data are invariant with respect to education or marital status leaving computed experience as necessarily ripe with measurement error.

Table 31. Summary of Recent Research on Returns to Military Service

Country	Period	Author(s)	Returns to Military Service	Technique	Instrument	Data Type
US	WWII	Angrist & Krueger (1994)	-6% to -15%	IV	1/4 of birth	micro cross-section
		Rosen & Taubman (1982)	+11.6%	Tobit		micro matched panel
		Berger & Hirsch (1983)	+3.3%	OLS		micro cross-section
		Martindale & Poston (1979)	+.9% to +11.0%	OLS		micro cross-section
	WWII & Korea	De Tray (1982)	+7.3% to +31%	OLS		micro cross-section
	Korea	Borjas & Welch (1986)	-36.9% to -39.0% *	OLS w/bias correction		micro cross-section
		Rosen & Taubman (1982)	+10.3%	Tobit		micro matched panel
		Berger & Hirsch (1983)	+3.8%	OLS		micro cross-section
		Martindale & Poston (1979)	+9.1% to +9.2%	OLS		micro cross-section
	Vietnam	Angrist (1994)	-15%	IV	draft lottery	aggregate panel
		Rosen & Taubman (1982)	-19.3%	Tobit		micro matched panel
		Berger & Hirsch (1983)	-1.8%	OLS		micro cross-section
		Martindale & Poston (1979)	-6.7% to +8.6% **	OLS		micro cross-section
	AVF	Crane & Wise (1987)	-9.9% to -17.4%	OLS		micro cross-section
		Angrist (1998)	-5% to +10% **	OLS & IV	AFQT misnorm	aggregate panel
		Bryant, Samaranayake, &Wilhite (1993)	-6.4% to -2.6%	OLS w/bias correction		micro cross-section
Netherlands		Imbens & van der Klauuw (1998)	-5%	IV	Special Exemption	aggregate
Germany		Acemoglu and Pischke (1998)	-2.2% to +4.5% ***	OLS		micro cross-section

* Estimates are for returns to military service in second career following military retirement for a sample of air force retirees.
** Negative returns for whites and positive for non-whites.

*** Acemoglu and Pischke (1998) are not focusing on military per se, however they find a positive return for individuals who were employed prior to being drafted, though the overall effect of military service the find is negative.

Similar results hold for Korea, where the only negative effect is from Borjas and Welch (1986) who study career air force retirees by comparing their second career earnings to comparable civilians. The extremely select population of retired military in their second career suggests that these results are not generalizable to all Korean veterans. All other research finds positive and significant returns to veteran status for the Korean era, with the estimates of the premium ranging from 3.8 to 31 percent.

When we turn our attention to Vietnam, the results are all negative regardless of sample or technique, except for Martindale and Poston (1979) who find positive returns for non-whites. The two studies that control for selection, Rosen and Taubman (1982) and Angrist (1994), find remarkably similar results given the different techniques and data used. Both find a large negative effect with Angrist's 15 percent slightly smaller than Rosen and Taubman's 19.3 percent estimated reduction in lifetime earnings.

The research on the AVF is limited due to the short time that it has been in existence. Just twenty-six years ago, in 1973, the US ended the draft and turned to volunteers. All estimates of the effect of military service are negative except for Angrist's (1998) separate estimates of returns for whites and non-whites. Whites continue to have a cost associated with their service. Non-whites, however, appear to receive a premium of 10 percent.

The recent research on countries other than the US finds small negative effects of service and hence conscription. Acemoglu and Pischke (1998) are interested in the effects of training in Germany, not military service in particular, but they find the overall effect of military service is negative but insignificant. If an individual had to quit employment to fulfill his military obligation, however, the returns are positive and significant. This finding is perplexing; why should military service benefit those who had jobs differentially over those who did not? Acemoglu and Pischke suggest military service forces an individual to break their apprenticeship agreement after training is received. This permits the individual to market his skills after military service at post apprenticeship wages even though he would otherwise still be in his apprenticeship.

While there is a vast literature on the returns to military service, the results are inconclusive. There is a common perception that any obligatory military service has a negative effect on lifetime earnings even though this is not substantiated empirically. Certainly, the research summarized in Table 31 has several negative findings in the draft era in the US and in other countries. Yet, while there is some evidence of a negative effect, the preponderance of evidence from the draft eras is inconclusive. The results are as often positive as negative, with the magnitude and even direction of the effect changing by war era in the US. The result is that the effect of obligatory military service is not known. Furthermore, as we will see shortly, the negative effects found for voluntary service seem at odds with theory and are missing, the micro panel data that is essential for appropriate estimation. This research attempts to fill this void in the literature.

III. Theory

As with any work on labor market returns to occupational choice or schooling, we rely heavily on the seminal work of Mincer (1958) and Becker (1962). The notion of human capital formation and the components of its production are integral to understanding and explaining the returns to any labor-market decision.

We begin with simple neoclassical theory. Our agents maximize their expected utility subject to a budget constraint. Each agent holds preferences over leisure time and goods.

$$E(U) = E\left(\int_{t=0}^{\infty} \beta^t f(leisure_t, goods_t)dt\right) \qquad (1)$$

As seen in equation (1), each agent has the opportunity for both intertemporal and intratemporal substitution. Leisure can be sacrificed today for goods today, or future goods or leisure. The limiting factor in how much utility each agent receives is his or her budget constraint.

Each agent's budget constraint is a function of the choices that agent makes. Each agent can choose how much to work, giving up the utility of the equivalent amount of leisure in the process. Thus, for any level of ability an individual agent has an entire spectrum of possible incomes. This, however, results in but one combined budget constraint. Since leisure provides utility directly, working has a cost associated with it. Including the fact that there are a limited number of hours in a day reduces the budget choice to an integral part of the utility maximization process itself.

The addition of human capital to our theoretical framework increases the choices available to each agent to affect his or her budget constraint and hence utility. The notion of human capital and each agent's ability to influence his productive capacity stems directly from the work of Mincer (1958) and Becker (1962). If we assume that labor income is proportional to labor's marginal product, then increasing labor productivity increases wages. The concept of human capital provides the framework for explaining the varying productivity of agents. A production function governs the accumulation of human capital such that investment in the production process results in higher future wages.

Following Becker (1962), human capital accumulation and therefore productivity and wages are an increasing function of schooling and training. Schooling, or education, affects each agent's ability to understand complicated tasks and processes. Increased schooling therefore is expected to increase wages. In Mincer's formulation, the returns to schooling are constant, suggesting that if education were costless agents would choose to receive all schooling available. Education is not costless. Even when the school itself requires no tuition, the agent must sacrifice leisure for education, just as she would sacrifice it for work. When schools actually charge tuition, as is the case for higher education, there is a tangible cost as well.

The US's largest provider of general training is the military. According to Becker (1962):

> The military offers training in a wide variety of skills and many are very useful in the civilian sector. Training is provided during part or all of the first enlistment period and used during the remainder of the first period and hopefully during subsequent periods. This hope, however, is thwarted by the fact that reenlistment rates tend to be inversely related to the amount of civilian type skills provided by the military (Becker page 59).

If the military continues to provide the general training that Becker describes, then it is natural to include military service as one of the explanatory variables in a wage regression just as education and experience are included.

While we expect investment in human capital to have positive returns, how do we explain the estimated negative effects for Vietnam and AVF veterans that we find in the literature? The first explanation is that the endogenous choice to serve either is not accounted for or is not properly specified. Yet, there are still many plausible explanations.

Military service may not be a choice, or at least not a first choice, for some individuals. A full draft is an obvious example where choice is absent. There are, however, many possibilities in the current AVF where military service is not a first choice of enlistees. Youth who find themselves in trouble with the law are sometimes faced with the onerous choice of military service or prison. Others find themselves with a wife and child at the end of high school and may elect to forego future earnings for the extra benefits the military provides its married members. Certainly there are other instances where exigent circumstances compel individuals to choose military service.

There are several more reasons why rational individuals would choose military service even if it diminished their lifetime earnings. An individual, who feels trapped geographically or socially, whether on the farm in Kansas or the ghetto in Chicago, may see enlisting as the only door to a better life. Another person may choose military service incorporating the expected benefits of retirement after twenty years of service, without ever realizing their expectation. Finally, the information on which the individual bases his enlistment decision is likely not complete. An incomplete information set is likely to result in sub-optimal choices, but this is not enough, on average, to result in a negative return to service.

While we have just examined several possible explanations for finding a negative effect for military service on lifetime earnings, these are not the explanations chosen by the researchers who correct for self-selection and still obtain negative estimates. Instead, returns to education are said to differ between the group that served and the one that did not. If we believe that education is part of human capital production, then this argument requires incorporating differential depreciation of human capital in the two groups. If education is merely a screening device, then it is not clear why the military service would have a negative effect.

The other preferred explanation for negative returns to military service suggests that military service is not equivalent to an equal amount of civilian experience. This argument is normally pushed no further than to say that military and civilian labor market experiences are not perfect substitutes. The question of why is almost never addressed. On the rare occasion when it is addressed, the explanation involves tenure in addition to experience. The negative effect of military service on lifetime earnings is simply due to a change of employers.

With an abundance of explanations for possible negative returns, it is reasonable to decompose the military effect into two components. The first component includes all of the training the military gives its recruits. This should encompass all human capital improvements provided by the military. The other component includes the human capital depreciation and absence of civilian experience. The decomposition thereby breaks the effects of military service into its human capital appreciating and depreciating effects.

Decomposing military service in this manner places onerous demands on a data set. The data set must have information on individuals who served in the military. It must also contain information on others who serve in a presumably non-skill building but also non-detrimental service. Finally, information on others who perform neither service must be included. Presumably it is possible to construct such a data set using information on the US alone if one can include enough Peace Corps volunteers, but this is not practical.

If military service has similar effects on lifetime earnings across countries, the necessary data may be available by combining data sets from different countries. Data from Germany present the opportunity for information on all three types of individuals needed. Since 1984, Germany has maintained a large, but not 100 percent draft, where individuals choose either *Wehrpflicht* (military service) or *Zivildienst* (community service) when called up for service. The lack of a full draft and the inability of an individual to affect his odds of being called for duty provide the group of individuals who perform no service. Individuals who choose *Wehrpflicht* perform their obligatory service in the military, while those who choose *Zivildienst* spend their time in hospitals and communities performing community service.

If we find that military service has similar effects in separate estimations for both the US and Germany, we can then decompose the effect through a cross-country regression.

$$\ln(w_{ijt}) = \alpha_j + \beta_j X_{ijt} + \theta Service_i + \delta Military_i + \varepsilon_{ijt} \qquad (2)$$

where i indexes individuals, j indexes countries, t indexes times, Service is an indicator for community or military service, and Military is an indicator for military service. Thus, with some reasonable assumptions, the returns to military service in the US can be decomposed to determine what the military adds to human capital and what the cost of the time absent from education or the civilian labor market is. Even if it is unreasonable to pool the US and Germany, the effects for Germany can still be decomposed.

What should we expect the returns to military service to be? Given that there is no other literature that suggests differential returns to schooling without some form of explicit discrimination, we expect the experience loss and depreciation components to be only the costs of the civilian labor-market experience that are foregone in order to serve. Since experience is imputed and not observed, we expect the coefficient on service, θ, to equal the coefficient on experience multiplied by the standard years of service. The average term of service in the US in the 1980's was three years and in Germany it was primarily around a year. The indicator variable for service in the pooled regression will need to be adjusted accordingly (Germany=1 if served, US=3 if served, otherwise=0).

The skill building and experience-enhancing component is also expected to vary with time served. The longer the period of service, the more skills can be learned. Increased service length also means increased leadership responsibility. We expect the human capital enhancing effect to equal the equivalent amount of civilian labor market experience lost plus up to a year of education.

The military experience should not be worth less than civilian labor market experience. Why should we expect the skills learned from a year of flipping burgers at McDonalds to be more than from the military? Certainly any civilian job teaches valuable skills like timeliness, communication, and group interactions, but does the military do any less? The answer is unequivocally no.

Indeed the military provides much more than merely on the job training (OJT). Most service members are sent to military schools to learn technical skills associated with their assigned occupational specialty. While in Germany this school time is limited to less than a year, in the US it is not. While in the US more than a year of this education is possible, the average is likely to be less than one year. This education should be similar to an equivalent amount of time spent in pursuing a degree in a technical field.

The overall effect of military service is combination of these separate components. Using the separate effects, we can construct an expectation of the bounds on the components and the overall return. The service component should fall between no effect and a negative effect equivalent to experience. The skill-enhancing component by construction should always be non-negative, with the upper bound at the equivalent of experience plus one year of education. The net overall effect is anywhere from the complete loss of civilian labor market experience to the gain of one year of education.

IV. DATA

The data come primarily from three sources. The individual data on the US come from the National Longitudinal Study of Youth (NLSY), while the individual data on Germany are from the German Socio-Economic Panel (GSOEP). The third source of data is the Mare and Winship Uniform March CPS Extracts (MW-CPS) from the National Bureau of Economic Research (NBER), including the extension for 1988 through 1992 by Christine Collins. Additionally, the consumer price index was obtained from Web sites through the US Bureau of Labor Statistics (http://www.bls.gov) and the Federal Statistical Office of Germany (http://www.statistik-bund.de).

The MW-CPS data set contains observations on individuals that are transformed into a panel. The MW-CPS does not track individuals over any number of years, so males age 25 to 35 in a given year were divided into cells based upon cohort, education, and veteran status. Only individuals reporting positive earnings and a usual work week of thirty hours or more are included. A cohort is defined by birth year. Each year of data provides observations on 11 cohorts. Educational attainment is divided into four categories: high school dropouts, which includes all dropouts; high school diploma, which includes GED holders; some college, those with less than a four year degree; and four year college degree or higher (BS/BA+). Veteran status is simply yes or no.

Once the individual observations for 1964 through 1992 are divided into cells, the panel is formed. Each cell is assigned the mean of its individual observations. The result is eight categories for each cohort, with cohorts followed for 11 years. This provides 88 observations per cohort for those born 1939 through 1956 and 80 for those born in 1957 (see Table 32). Veteran status is further divided into Korea, Vietnam, and AVF eras. Cohorts born prior to 1945 are pre-Vietnam and are classified in the Korea era although they are too young to have served in the Korean War. The 1945 through 1954 cohorts are in the Vietnam era. Finally, the 1955, 1956, and 1957 cohorts were never eligible for the draft and are categorized in the AVF era.

Table 32. Sample Observations from the MW-CPS 1964-1992

	Korea	Vietnam	AVF	Total
Years	39-44	45-54	55-57	
Cohorts	6	10	3	19
Observations	528	880	256	1664

The numbers of observations within each cell vary considerably. Veteran cells contain fewer observations than non-veteran cells do. Additionally, the numbers of observations on veterans at either end of the education distribution are extremely limited.

Table 33. Sample Observations from the NLSY 1979-1996

Original Observations	12686	
Males	6403	
Born subject to choice	6403	
With earnings history	6284	98.14%
With family background	4723	73.76%

The data from the NLSY are already a panel. Males are tracked from 1979 through 1996. Table 33 documents the loss of individuals due to a lack of relevant data. Of the 6403 males in the NLSY, none of whom ever faced the draft, 4723 provide the necessary information for estimation. This results in 50351 observations, or an average of 10.66 observations per individual over the 17-year period.

Table 34. Sample Observations from the GSOEP 1984-1997

Original Observations	29328	
Males	14591	
Born subject to choice	2908	
With earnings history	1869	64.27%
With family background	1273	43.78%

The German data are from the GSOEP (see Table 34). The relevant cut-off date in Germany is 1984, because that is the beginning of the individual's right to choose *Zivildienst* over *Wehrpflicht*. This limits the possible sample to 2908 individuals. The number of males reporting either service, however, is far too low. Conscription rates have been in the range of 65 to 95 percent over this period[35], while only 21 percent of the 2908 eligible males acknowledge serving.

[35] Military service accounted for 135,000 to 225,000 men annually from 1989 through 1997(http://www.dfg-vk.de/bundeswehr/personal.htm) and alternative service ranged from 136,000 to 155,000 males over the same period according to the German information consulate (gicpress@germany-info.org). The corresponding available male population was around 400,000 (http://www.bundesregierung.de/english/02/0202/020204/4.jpg) resulting in the stated range of percent drafted.

Part of this problem stems from a stigma attached to service or the choice of service, but part is also inherent in the survey design.[36] The GSOEP appears to ask retrospective questions on service only in the first year of interview, which is 1984 or age 18 for most of the relevant sample. The problem is the years when individuals were not interviewed and the data are missing. The result is a large number of individuals whose service status and type are unknown. Furthermore, the number of individuals with complete records and no reported service is simply too high.

Even among the 602 who acknowledge serving, differentiating between *Wehrpflicht* and *Zivildienst* is not simple and results in losing almost half the remaining observations. The GSOEP asks for activity last year month by month, and among the options are *Wehrpflicht/Zivildienst*. Individuals are never asked to differentiate by type of service. This forces us to rely on the different lengths of service for *Zivildienst* and *Wehrpflicht* to differentiate between the two. Since not all individuals report a number of months of service reflective of *Wehrpflicht* or *Zivildienst*, there remain 327 individuals. This further declines to a paltry 177 individuals once individuals without an earnings history or family background are omitted. The result is 1285 observations, or 7.3 observations per individual over this 14-year period.

While the initial reaction is to use only observations where complete information is available, this is too severe and not necessary. The restricted sample is simply too small. A larger set of individuals can be used when we understand that the difficulty is really a reporting problem which biases our standard estimates of the components of the effects of military service toward zero. For individuals whom we identify as *Wehrpflicht* or *Zivildienst,* we know

[36] In an e-mail, Dr. John P. Haisken-Denew of the German Institute for Economic Research (DIW) explained, "The question about military OR community service is very loaded. For that

for certain what they did. Individuals whom we cannot identify as *Wehrpflicht* or *Zivildienst*, however, could have done either or neither. The consequence is that individuals who served in either capacity but are not classified as such, attenuate the effect.

Knowing that there is a problem with the service and military variables, however, allows us to overcome this bias. In latent variable context the problem is that we observe:

$$\begin{aligned} &\text{Military=1 if Military*=1} \\ &\text{Military=0 if Military*=1 or 0} \end{aligned} \tag{3}$$

where Military* represents actual, not reported military service. If we know the percentage of the population that served in the military we can adjust the estimates:

$$\hat{\delta} = \delta(1 - M^{**}) \tag{4}$$

$$\text{where } \Pr(Military^* = 1 \mid Military = 0) \equiv M^{**} \tag{5}$$

and δ is the returns to military service. The same technique can be applied to the coefficient on service.

Thus simply through understanding the problem in the data we are able to maximize the usable observations in the data set. While the sample size has been maintained, the question of bias looms. Bias is a distinct possibility when some individuals have chosen to report their service and others have chosen not to. This endogenous selection may be random, but there is unfortunately no way to test this assumption. Furthermore, the direction of a bias, if one exists, is not clear.

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reason, it was never differentially asked (people might drop out of the panel, if they were insulted by the question)."

V. Estimation

Recovering returns to military service in a standard wage regression, equation (6), is simple. The effect is merely the coefficient on the military service indicator variable.

$$\ln(w_i) = \alpha + \beta X_i + \gamma Z_i + \delta Military_i + \varepsilon_i \tag{6}$$

where i indexes individuals, w is the wage, X is time varying covariates, and Z are time invariant covariates, Military is an indicator equal to one if the individual served in the military, and ε is a stochastic element. This technique of recovering the returns to military service also holds for a random effects panel model.

$$\ln(w_{it}) = \alpha + \beta X_{it} + \gamma Z_i + \delta Military_i + \varepsilon_{it} \tag{7}$$

Estimation using the MW-CPS panel produces results that are within the range of those found in the literature (see Table 35). All estimates reflect the random effects model depicted in equation (7), where X is a vector of experience and experience squared and Z is a vector of indicators for level of educational attainment. The result is systematically higher returns for military service the earlier the era, except for college graduates. The same is also true for education, where the lower the education the higher the returns to military service, except for college graduates.

The differential returns by era appear to reflect a changing premium for military service. The premium for service in the Korean War era is 3 percent, while Vietnam shows no premium, and the AVF has a cost of 7 percent. While the returns may indeed be changing over time, it is more likely that the result is driven by changes in the pattern of selection into the military. If selection is the explanation, these results suggest that the ability, whether observed or unobserved, of individuals in the military has steadily diminished.

Table 35. Estimate Returns to Military Service From MW-CPS 1964-1992

	All		HS Dropouts		HS Diploma		Some College		BS/BA +	
					Education Sample					
R-square	0.6415	0.6732	0.1275	0.3292	0.1449	0.3642	0.4846	0.6236	0.5549	0.5667
Experience	0.1091 ***	0.1093 ***	0.0536 ***	0.0535 ***	0.0538 ***	0.0539 ***	0.1022 ***	0.1030 ***	0.1393 ***	0.1387 ***
	(0.0034)	(0.0034)	(0.0182)	(0.0189)	(0.0084)	(0.0082)	(0.0077)	(0.0078)	(0.0119)	(0.0119)
Experience^2	-0.0040 ***	-0.0040 ***	-0.0018 ***	-0.0018 ***	-0.0018 ***	-0.0018 ***	-0.0036 ***	-0.0037 ***	-0.0064 ***	-0.0063 ***
	(0.0002)	(0.0002)	(0.0007)	(0.0007)	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0011)	(0.0011)
Military	-0.0418		0.1397 ***		0.0283 ***		-0.1018 ***		-0.0810 ***	
	(.0069)		(.0124)		(.0070)		(.0083)		(.0186)	
Korea		0.0299 **		0.2206 ***		0.0672 ***		-0.0291 **		-0.0540
		(.0121)		(.0217)		(.0118)		(.0143)		(.0330)
Vietnam		-0.0053		0.1277 ***		0.0349 ***		-0.1135 ***		-0.0956 ***
		(.0094)		(.0171)		(.0092)		(.0112)		(.0256)
AVF		-0.0708 ***		0.0144		-0.0743 ***		-0.2124 ***		-0.0849 *
		(.0174)		(.0307)		(.0168)		(.0203)		(.0475)
HS Dropout	-0.3306 ***	-0.3307 ***								
	(0.0102)	(0.0101)								
Some College	0.2220 ***	0.2221 ***								
	(0.0101)	(0.0100)								
BA/BS+	0.4478 ***	0.4480 ***								
	(0.0117)	(0.0117)								

Note: Standard errors are in parenthesis below point estimates. Aserisks indicate level of significance, with one for 90%, two for 95%, and three for 99%. Hausman Specification test is a Chi-square with three degrees of freedom, where the null is that the random effects specification is correct.

The distinctly different results for college graduates should not be surprising. While college graduates have been drafted and do enlist, the majority go into the military as officers. As such they are subject to different training and often given responsibilities far beyond what their contemporaries in the civilian labor-market are given. These responsibilities include oversight of a number of individuals and fairly large budgets, which are comparable to the responsibilities of upper management at large companies. This suggests that these individuals receive general on the job training which is beyond what their civilian peers receive and should increase their human capital and provide positive returns to military service. The MW-CPS results, however, consistently show a negative return to service for college graduates, although it is insignificant for the Korean era. The counter-intuitive result can be attributed to selection, but may also be due to the few observations, generally around 20 to 35, in each of the veteran college graduate cells used in this estimation.

A random effects model is appropriate for the MW-CPS panel, but not necessarily for the GSOEP or NLSY panels. Since the MW-CPS panel does not follow specific individuals over time, instead following representative individuals, what a fixed effect would control for is unclear. Using the NLSY or GSOEP, the fixed effect is an individual's unobserved ability. Failure to control for this important component of earnings leads to biased estimates if they are non-random. A Hausman specification test allows us to verify our assumption that individual fixed effects are significant and appropriate.

If we reject random effects in favor of a fixed effects model, the estimation process involves a couple of additional steps. The fixed effects for each individual will include all effects of time invariant covariates, necessitating a decomposition of the errors to identify returns to military service. Estimation starts with a standard fixed effects model.

$$\ln(w_{it}) = \alpha + \beta X_{it} + \nu_i + \lambda_{it} \tag{8}$$

where ν is the individual fixed effect and λ is a stochastic element. The next step involves predicting the residual, $v_i + \lambda_{it}$. This is accomplished using:

$$\hat{u}_{it} = \ln(w_{it}) - \hat{\alpha} + \hat{\beta} X_{it} \tag{9}$$

The predicted residuals are then regressed on the time invariant covariates to recover the returns to each of these as in equation (10).

$$\hat{u}_{it} = \gamma Z_i + \delta Military_i + \zeta_{it} \tag{10}$$

All the previously mentioned techniques are, however, misspecified in the context of estimating returns to military service. The problem is that each individual chooses whether or not to serve in the military. This means the indicator variable for military service is endogenous, and if the variable is correlated with any of the covariates, the results are biased. The simplest way to obtain consistent estimates of returns to military service is to use an Instrumental Variables (IV) technique.

The question, as with any IV procedure, is what can we use as an instrument. Since both the GSOEP and NLSY offer no obvious candidates, we construct a binary predicted military service variable based on probit estimation. Probit estimation proceeds on the actual military service as a function of indicator variables for high and low education for the individual's mother and father, an indicator for whether or not the father is a native, and an indicator for whether or not the father was alive at the respondent's age 18.

$$Military_i = \phi_1 Ma\_lowed_i + \phi_2 Ma\_hied_i + \phi_3 Pa\_lowed_i + \\ \phi_4 Pa\_hied_i + \phi_5 Pa\_native_i + \phi_6 Pa\_alive_i + \varepsilon_i \tag{11}$$

None of the covariates in the probit equation enters into the wage regression. The resulting estimates (see Table 39 in Appendix B) are then used to construct a predicted military service variable, which replaces the actual military variable in equations (6), (7), and (10). Consistent estimation then proceeds in the same manner as previously discussed.

$$\ln(w_i) = \alpha + \beta X_i + \gamma Z_i + \delta \hat{M}ilitary_i + \varepsilon_i \quad (6')$$

$$\ln(w_{it}) = \alpha + \beta X_{it} + \gamma Z_i + \delta \hat{M}ilitary_i + \nu_i + \lambda_{it} \quad (7')$$

$$\hat{u}_{it} = \gamma Z_i + \delta \hat{M}ilitary_i + \zeta_{it} \quad (10')$$

The results of estimation on the NLSY panel are provided in Table 36. In all cases and as expected, the Hausman specification test rejects random effects in favor of a fixed effects specification. OLS and 2SLS estimates are included to provide a comparison to the literature on the subject and depict the variation in results that can be obtained from the same data set given different specifications.

Estimation on the NLSY panel without controlling for the endogenous choice of service results in an estimated cost of service ranging from 22 to 37 percent. The failure to account for individual fixed effects results in a 10 percent higher cost of military service. This suggests that individuals with lower unobserved abilities choose military service.

Controlling for selection results in drastically different results. Returns to the AVF are positive and significant when the panel is used. Furthermore, the fixed effects increase the premium to a reasonable 10.2 percent. The similarity in change between the random and fixed effects regardless of whether or not selection is corrected for, is strong evidence that military service is not chosen by the most able. Additionally, 2SLS estimation from a simple cross-section results in a negative effect that is clearly at odds with the premium found in the panel estimation. The panel fixed effects estimation is superior to the others because it includes longitudinal data and controls for individual unobservables.

What do we make of a premium for military service in the US? First, the fact that accounting for self-selection reversed the direction of the effect indicates that the military is not chosen by the most able. Furthermore, the reversal indicates that the military provides higher lifetime benefits for these less able

Table 36. Estimated Returns to Military Service Using NLSY 1979-1996

	Exogenous			Endogenous		
	Random Effects	Fixed Effects	OLS	Random Effects	Fixed Effects	2SLS
Observations	50351	50351	1758	50351	50351	1758
Individuals	4723	4723	1758	4723	4723	1758
R-Squared	0.3080	0.2666	0.1581	0.2993	0.2666	0.1516
R-Squared (2d)#		0.0269			0.0196	
Education	0.2219 ***	0.2673 ***	0.1346 ***	0.2259 ***	0.2673 ***	0.1326 ***
	(0.0033)	(0.0046)	(0.0085)	(0.0034)	(0.0046)	(0.0087)
Experience	0.2475 ***	0.2356 ***	0.0879 ***	0.2459 ***	0.2356 ***	0.0788 ***
	(0.0025)	(0.0027)	(0.0299)	(0.0025)	(0.0027)	(0.0299)
Experience^2	-0.0087 ***	-0.0083 ***	-0.0020 **	-0.0086 ***	-0.0083 ***	-0.0017 *
	(0.0001)	(0.0001)	(0.0010)	(0.0001)	(0.0001)	(0.0010)
Black	-0.3515 ***	-0.3114 ***	-0.4032 ***	-0.3828 ***	-0.3483 ***	-0.4233 ***
	(0.0267)	(0.0112)	(0.0496)	(0.0275)	(0.0113)	(0.0497)
Other race	-0.1524 ***	-0.0443 **	-0.3199 ***	-0.1355 ***	-0.0317	-0.3461 ***
	(0.0484)	(0.0207)	(0.0947)	(0.0497)	(0.0209)	(0.0952)
Military	-0.3688 ***	-0.2711 ***	-0.2236 ***	0.0539 **	0.1023 ***	-0.0751 **
	(.0257)	(.0121)	(.0414)	(.0226)	(.0091)	(.0310)
Hausman Test	286.58 ***			295.06 ***		
	reject			reject		

Note: Standard errors are in parenthesis below point estimates. Aserisks indicate level of significance, with one for 90%, two for 95%, and three for 99%. Hausman Specification test is a Chi-square with three degrees of freedom, where the null is that the random effects specification is correct. # indicates R-squared from regression of predicted fixed effects on time invariant covariates.

individuals than they would be able to garner in the civilian sector. Presumably these benefits are in the form of increased skills. However, De Tray (1982) implies that these premia merely reflect the use of military service as a screening device. Yet regardless of whether the causation is increased skills or simply the effect of screening, the net result is the same: AVF veterans earn a premium of 10.2 percent over their otherwise comparable civilian counterparts.

Estimation on the GSOEP requires a slightly different specification than do the data for the US, due to the ability to decompose returns to military service. Equations (6'), (7'), (10'), and (11) are transformed:

$$\ln(w_i) = \alpha + \beta X_i + \gamma Z_i + \theta Service_i + \delta \hat{Military}_i + \varepsilon_i \qquad (6'')$$

$$\ln(w_{it}) = \alpha + \beta X_{it} + \gamma Z_i + \theta Service_i + \delta \hat{Military}_i + \nu_i + \lambda_{it} \qquad (7'')$$

$$\hat{u}_{it} = \gamma Z_i + \theta Service_i + \delta \hat{Military}_i + \zeta_{it} \qquad (10'')$$

$$Military_i = \phi_1 Ma\_lowed_i + \phi_2 Ma\_hied_i + \phi_3 Pa\_lowed_i + \phi_4 Pa\_hied_i + \phi_5 Ma\_alive_i + \phi_6 Pa\_alive_i + \varepsilon_i \qquad (11'')$$

The results from the GSOEP are far different from those for the US (see Table 37). (Probit estimates are in Table 40 in Appendix B.) When analyzing the effects from the GSOEP estimation we must remember that the estimates are not of returns to military service, but instead biased estimates of its components.

Given that we think there is no actual depreciation of human capital, from simply missing a year of experience, we expect the coefficient on service when adjusted for the actual percentage who served to equal (opposite sign) the coefficient on experience. This suggests an adjustment of 46% to bring the estimated percentage who served to 72%, which is slightly low but reasonable.

The returns to military service could not be expected to be more than a year of education, since the period is less than a year. A full year suggests an adjustment of 80%, which is ridiculous. More reasonable is an adjustment to 40

Table 37. Estimated Returns to Military Service Using GSOEP 1984-1997

	Exogenous			Endogenous		
	Random Effects	**Fixed Effects**	**OLS**	**Random Effects**	**Fixed Effects**	**2SLS**
Observations	6484	6484	831	6484	6484	831
Individuals	1273	1273	831	1273	1273	831
R-Squared	0.4434	0.3896	0.3646	0.4433	0.3896	0.3555
R-Squared (2d)#		0.0139			0.0137	
Education	0.1725 ***	0.2374 ***	0.1007 ***	0.1724 ***	0.2374 ***	0.1012 ***
	(0.0048)	(0.0068)	(0.0073)	(0.0048)	(0.0068)	(0.0074)
Experience	0.3206 ***	0.3200 ***	0.2121 ***	0.3205 ***	0.3200 ***	0.2076 ***
	(0.0075)	(0.0081)	(0.0231)	(0.0075)	(0.0081)	(0.0233)
Experience^2	-0.0124 ***	-0.0128 ***	-0.0063 ***	-0.0124 ***	-0.0128 ***	-0.0062 ***
	(0.0005)	(0.0005)	(0.0012)	(0.0005)	(0.0005)	(0.0012)
Service	-0.2350 ***	-0.3163 ***	-0.3091 ***	-0.1331 ***	-0.1729 ***	-0.0763 *
	(0.0900)	(0.0460)	(0.0769)	(0.0354)	(0.0185)	(0.0418)
Military	0.1236	0.1747 ***	0.3082 ***	0.0413	0.0483 ***	0.0442
	(0.0950)	(0.0486)	(0.0840)	(0.0277)	(0.0155)	(0.0344)
Hausman Test	239.73 ***			239.58 ***		
	reject			reject		

Note: Standard errors are in parenthesis below point estimates. Aserisks indicate level of significance, with one for 90%, two for 95%, and three for 99%. Hausman Specification test is a Chi-square with three degrees of freedom, where the null is that the random effects specification is correct. # indicates R-squared from regression of predicted fixed effects on time invariant covariates.

to 50% of the population, bounding the actual military effect between .0755 and .0894, or equivalent to a third of a year of education or a quarter of a year of experience.

The net effect of service in Germany is negative. *Zivildienst* appears to waste a year in which individuals would otherwise be gaining civilian labor market experience. *Wehrpflicht* does little better, reducing the cost of service by a quarter to a third. The apparent premium for *Wehrpflicht* over *Zivildienst* may merely reflect the shortened length of service.

Including fixed effects ascribes greater costs to service and a higher premium to military service. This suggests sorting in an opposite manner than the US. Those with higher unobserved abilities appear to serve, as the fixed effect estimate of service is far more negative than either the random effects or 2SLS estimates. The exogenous estimates suggest the most able choose *Wehrpflicht* over *Zivildienst,* as the large premium disappears when unobserved ability is included in the estimation. This does not follow when we control for endogeneity; the estimates of returns to military service are virtually unchanged, suggesting a random sorting between *Wehrpflicht* and *Zivildienst.*

The consequence of the GSOEP estimation is an inability to combine the German and US panels. The net estimate of a negative effect for military service from Germany coupled with an overall positive effect from the US result in an incompatibility. The cause is not the differential sorting between countries, as the fixed-effects control for this. Instead, the cause must be a difference in the returns across countries. The reasons for this difference are pursued in the conclusions.

VI. CONCLUSIONS

The use of individual longitudinal and cross-section data on two countries allows for new insights in the field of military manpower economics. The returns to voluntary military service in the US are positive and significant. These returns differ across countries, as does sorting into military service. Failure to control for endogeneity and individual fixed effects results in erroneous estimates.

The returns to voluntary military service in the US are positive and significant, suggesting that prior negative estimates did not fully control for selection. With positive returns to voluntary service, the need for a decomposition of the returns is unnecessary. Indeed the explanation of military service and civilian labor market experience as incomplete substitutes seems erroneous and certainly unnecessary. The same is true for any human-capital depreciation caused by military service. Indeed the result, regardless of the components that compose it, is a 10 percent increase in lifetime earnings for veterans of the AVF.

Is a 10 percent premium large? Since the sample only looks at younger workers, 10 percent is not as large as one would expect. While this is equivalent to a full year of education in a sample of all workers, it is only half a year in this sample. Still, to gain experience at the same rate as their civilian peers and add a premium equal to one-half a year of education in addition to any actual education made available by service related benefits is large.

The returns to military service are not the same in the US and Germany for a variety of reasons. The returns to military service in Germany are negative. The different lengths of service between the countries (currently the average US enlistment is 4 years, while in Germany it is only 10 months) results in an increased ability to train in the US. Certainly 10 months is not long enough to provide much technical training after the military essentials of marching and marksmanship are taught. Society's value of service may also differ across countries. Throughout the study period, German service members were never in an armed conflict and constitutionally could not be deployed to a foreign country unless they volunteered. The US in contrast was continuously deploying troops and fought in Grenada, Panama, Iraq, and Somalia. The differential returns to military service may merely reflect rents paid in the US for increased probability of injury or death.

The different systems of obtaining personnel may also explain some of the difference between the US and Germany. In the US the military must compete in the labor market for its recruits. Germany in contrast simply drafts its recruits. This means that military service in the US must be at least seem equivalent, if not better, than the civilian alternatives for enough individuals to meet the military's need for recruits. Indeed the one of the biggest slogans for recruiting is that the military does not demand experience, it provides it. Another difference is the ability of firms in the US to use service as a screening device.

Finally, the failure to control for both endogeneity and fixed effects in estimating the returns to military service results in erroneous results. In both the NLSY and GSOEP the results reverse sign when the choice of service is included as an endogenous decision. While correcting for endogeneity using 2SLS, the cross-section estimates also fail because they cannot control for individual unobserved ability. Estimating the returns to military service requires cross-sectional and longitudinal data that include the ability to control for endogenous selection.

The use of data from multiple countries merits further research as data becomes available. While the estimates of returns differ between the US and Germany, the use of micro panel data from both countries provides insights heretofore unknown. Not only is the premium for veterans of the AVF a new result; the identification of sorting patterns in the US and Germany provides new insights into the possible benefits and costs of a draft. Data on other countries can help in understanding the effects of different systems of obtaining military personnel.

Chapter 5: Conclusions and Future Research

We have examined several aspects of the economics of military manpower. The first research topic was the effect of changes in retirement pay on the retention of enlisted personnel. Then we looked at the quantity-quality trade-off in the market for recruits. Finally we investigated the returns for military service in the US and Germany.

Since the military uses 14% as the discount rate for its personnel and previously estimated discount rates are in excess of 10%, *a priori* it was not clear that a major change in the military retirement system in 1986 would have had any significant effect on retention. This analysis demonstrated that the effects were significant for at least the Army. Exactly how large an effect is unclear due to selection bias. Further research should attempt to correct for this selectivity bias, if the extensive data necessary presents itself. This is essential because part of the unmeasured effect is an increase in the efforts needed to recruit individuals.

The retirement pay results are only for the Army. The differences amongst the services and their personnel mean there is no reason that the effects must be similar across the services. This is a question only of magnitude, however, since theory tells us that the effect if there is one will be to diminish retention. These different effects can only be estimated with data from all of the services.

A simple cost benefit analysis using the findings for the Army shows that the change in the retirement system has a sizable net cost under some very plausible assumptions. The lower bound estimates for the Army alone have a cost in excess of 42 million dollars. This is more than half the estimated savings for all services combined. Since the Army accounts for 39% of annual recruits and roughly the same percentage of retirees, this suggests the direct costs exceed the savings of changing the retirement system.

Indirect costs and the actual reduction in retirement benefits may make the costs far exceed the projected savings. Indirect costs for recruiting and advertising may be significantly increased. Lower benefits for enlistees means that the marginal recruit will no longer sign up. Furthermore, we cannot determine whether this change in the retirement system has an effect on career soldiers. This may reduce the actual savings. For each additional year past twenty up to thirty years of service, individuals under REDUX earn in excess of 75% more in retirement benefits than those under the FULL system. How much of an increase this will have on retention past twenty years is simply pure conjecture. Savings are calculated based upon everyone retiring at twenty years. This we know does not happen and is less likely to happen in the future since the incentives to stay have been increased. The more substantial this effect, the less is saved by the change in systems. Additionally, this older force may create further reductions in retention rates by increasing time to promotion. The additional time necessary to wait until promotion reduces benefits and makes the military less attractive.

The estimation of retention elasticities with respect to retirement pay is essential for informed policy analysis regarding changes in the military retirement system. While important, this alone only allows us to examine one of many costs associated with changing the retirement structure. These results are necessarily incomplete in that the full career path of individuals affected by the new system is yet to be seen, however, even with this and other limitations the costs far exceed the projected savings.

The results from the quantity-quality trade-off are clear: no matter the level of aggregation or the measure of quality one prefers, there is a quantity-quality trade-off in the labor market. The estimates from the individual level are preferred because they are indicative of the underlying processes and also allow us to quantify costs through the use of bonuses. The estimated elasticity of quality with respect to quantity is -.0267.

Individual level data also enables us to calculate the cost associated with changing demand or movements in unemployment. Using **AFQT** as our measure of quality, the cost to maintain quality of a one-point reduction in unemployment is therefore at least 2.4 million dollars, while the cost of an increase in quantity of one-percent is at least 170 thousand dollars.

The minority representation estimation provides new insights into the dynamics of the labor markets. The AVF has always been over represented by minorities. But this over representation does not hold when we focus on the appropriate market. Instead, it appears that minorities are proportionally represented from the available pool of male high school graduates.

There is no indication of racial discrimination by the military. Indeed, the opposite seems to be the case. Minority representation in the recruit market increases as unemployment decreases, suggesting the civilian market prefers others even after controlling for quality. The military, on the other hand, prefers quality regardless of color.

The question of a quality quantity trade-off is likely to be a quality sacrifice for the military under current conditions. The lasting good health of the strong economy coupled with a decade of declining military spending means the military will see reduced quality. How far the level of quality falls remains to be seen, however, we expect the problem to be so severe that quality benchmarks and recruiting quotas will not both be met for most of the services in FY99 for the first time in over a decade.

Added to the manpower problems of the military is the ability of the Congress to unknowingly wield monopsony power. The quantity-quality trade-off means that even though the wage rate is fixed the military still faces an upward sloped supply schedule. This combined with its role as the sole national employer of unskilled youth makes the military a monopsony in the market for recruits. Any analysis or debate about military compensation is therefore incomplete without accounting for the possibility of monopsony exploitation of our youth.

While this research has demonstrated that the quantity quality trade-off exists, future research can do much more. The cost figures can be estimated much more precisely with information on the amount of the bonus received. Additionally, actual costs of any other enticements would allow estimates of the value of these programs and could lead to a more efficient use of recruiting resources.

Efficient allocation of recruiting resources leads to the question of whether the military services compete with each other for recruits or have synergistic effects. This requires data similar to what we used for this study but must also include a breakdown by service. A similar question arises in the competition between the National Guard and the Active components for recruits.

The existence of a quantity quality trade-off certainly is not new. The empirical support of its existence is, however, new. The magnitude of the trade-off and the effects of unemployment and bonuses should allow policy analysts to make informed recommendations when considering changes in military manpower

The returns to military service have often been investigated and estimated. Our multi-country panel data allow us to provide more reliable and believable estimates. The multi-country nature of the data also allows a comparison of manpower acquisition and returns across nations.

The returns to voluntary military service in the US are positive and significant suggesting prior negative estimates did not fully control for selection. With positive returns to voluntary service the explanation of military service and civilian labor market experience as incomplete substitutes seems erroneous and certainly unnecessary. The same is true for any human capital depreciation caused by military service. Indeed the result, regardless of the components that compose it, is a 10.2 percent increase in lifetime earnings for veterans of the AVF.

Is a 10 percent premium large? Yes, but since the sample only looks at younger workers, 10 percent is equivalent to only half a year of education in this sample. This means that service members earned as much as there civilian counterparts and got a premium equal to half a year of college without having the expense or losing the time that requires.

The returns to military service differ between the US and Germany for a variety of reasons. The returns to military service in Germany are negative. This contrasts to the 10.2 percent premium in the US. The US has a greater ability to train because of the longer term of service. Society's value of service may also differ across countries. Also there is a distinct difference in the risk of injury or death between the countries. Throughout the study period, German service members were never in an armed conflict and constitutionally could not be deployed to a foreign country unless they volunteered. The US in contrast deployed troops 30 times and fought in four conflicts. The difference in returns to military service may merely reflect rents paid in the US for increased probability of injury or death.

The different systems of obtaining manpower may also explain some of the difference between the US and Germany. In the US the military must compete in the labor market for its recruits. Germany in contrast simply drafts its recruits. This means that military service in the US must be at least seem equivalent, if not better, than the civilian alternatives for enough individuals to meet the military's need for recruits. Indeed the military in the US still remains the largest provider of general training. Another difference is the ability of firms in the US to use service as a screening device.

Finally, the failure to control for both endogeneity and fixed effects in estimating the returns to military service results in erroneous results. In both the NLSY and GSOEP the results reverse sign when the choice of service is included as an endogenous decision. While correcting for endogeneity using 2SLS, the cross-section estimates also fail because they cannot control for individual unobserved ability. The conclusion is estimating the returns to military service requires cross-sectional and longitudinal data that includes the ability to control for the endogenous selection.

The use of data from multiple countries merits further research, as data becomes available. While the estimate of returns differ between the US and Germany, the use of micro panel data from both countries provides insights heretofore unknown. Not only is the premium for veterans of the AVF a new result, the identification of sorting patterns in the US and Germany provides new insights into the possible benefits of a draft. Data on other countries or more extensive data on those studied can help in understanding the effects of different systems of obtaining military manpower.

These three essays on the economics of military manpower provide new insights that are an integral part of policy analysis regarding military compensation and recruiting. Additionally, the quantity-quality trade-off has general implications for labor demand. Indeed, the strong effect of unemployment on quality is necessary for all employers to understand if they are to efficiently allocate resources.

In conclusion some of the results were less strong than others, but three things are clear. Changes in retirement benefits affect retention even though the benefits will not be received for twenty years. Quantity effects quality *ceteris paribus*. And, finally, the All-Volunteer Military is beneficial to those who serve.

Appendix A: Supporting Documentation for Chapter 2

Table 38. Stop-Loss Adjusted 4 Year Results

REDUX	0.8227	**
	(0.0361)	
FEMALE	0.9116	**
	(0.0380)	
BLACK	0.7289	**
	(0.0283)	
HISPANIC	0.8031	**
	(0.0556)	
GED	0.9804	
	(0.1164)	
DROPOUT	0.8838	
	(0.0882)	
SOMECOL	0.9572	
	(0.0619)	
COLDEG	1.0748	
	(0.0982)	
CAT1	1.0162	
	(0.0609)	
CAT2	0.9899	
	(0.0315)	
CAT3B	1.0822	*
	(0.0441)	
CAT4	1.1969	
	(0.1398)	
E2	0.9070	**
	(0.0348)	
E3	0.9059	*
	(0.0521)	
CBTARMS	0.8938	**
	(0.0327)	
NC	1.0028	
	(0.0401)	
S	0.8762	**
	(0.0349)	
W	1.0444	
	(0.0454)	
FULL DELAY	1.0476	
	(0.0514)	
REDUX DELAY	0.8086	**
	(0.0746)	

Note: Baseline is white male high school graduate from the Northeast of slightly above average mental ability (CAT3A) who does not delay entry and enlists as an E1 for a non-combat arms specialty. Standard errors are reported in parenthesis below estimated odds ratio. One asterisk indicates significance at 90% level, and two asterisks indicate significance at the 95% level.

Appendix B: Supporting Documentation for Chapter 4

Table 39. Marginal Effects from Probit Estimation of Military Service NLSY

		Marginal Effect	
Mother	**Low education**	0.0055	
		(0.0038)	
	High education	-0.0313	***
		(0.0046)	
Father	**Low education**	-0.0555	***
		(0.0038)	
	High education	-0.0469	***
		(0.0043)	
	Alive	0.0269	***
		(0.0060)	
	Native	0.0753	***
		(0.0036)	
Psuedo R-squared		0.0933	

Note: Standard errors are in parenthesis below point estimates. Aserisks indicate level of significance, with one for 90%, two for 95%, and three for 99%.

Table 40. Marginal Effects from Probit Estimation of Military Service GSOEP

		Marginal Effect	
Mother	**Low education**	-0.1783	***
		(0.0092)	
	High education	-0.0779	***
		(0.0097)	
	Alive	0.0664	***
		(0.0252)	
Father	**Low education**	-0.1018	***
		(0.0132)	
	High education	-0.0075	
		(0.0110)	
	Alive	-0.0148	
		(0.0149)	
Psuedo R-squared		0.0911	

Note: Standard errors are in parenthesis below point estimates. Aserisks indicate level of significance, with one for 90%, two for 95%, and three for 99%.

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