USING SKILL CHANNELING EXPERIMENTS TO IDENTIFY THE LONG TERM EFFECTS OF MILITARY ACQUIRED SKILLS: PRELIMINARY EVIDENCE FROM THE ENLISTMENT BONUS EXPERIMENT

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ABSTRACT

This paper presents some preliminary results for a broad based study designed to measure the medium- and long-term economic and health outcomes for veterans as a result of an enlistment bonus experiment conducted by Rand in the early 1980s. The original design of the experiment was relatively straightforward – some recruiting areas provided higher enlistment bonuses for combat occupations while other control areas retained the same enlistment bonuses as before the experiment. We use a standard difference-in-differences (DID) identification strategy with a detailed panel dataset provided by the Defense Manpower Data Center (DMDC) to analyze the treatment effects across a large database of military recruits. We find the higher enlistment bonus incentives available to the treatment groups from the experiment did very little to attract a different pool of enlistees or to materially change the characteristics of the enlistees. Furthermore, we find the higher enlistment bonuses did not generate substantial market expansion effects. We do find, however, that the experiments had a "skill channeling" effect on enlistees in the treatment zones (i.e. high quality recruits were more likely to enlist in combat occupations as a result of the higher enlistment bonuses). In general, we find high quality enlistees in the treatment areas were more likely to marry and have at least one dependent in comparison to those in the control areas. High quality enlistees were also more likely to have faster promotion rates in comparison to those in the control areas.

JEL Classifications: D13, H43, I18, J38

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1. INTRODUCTION

People who serve in the military may develop skills that affect their labor market and health outcomes later in life. Indeed, the military highlights skill development in recruitment campaigns. There is a substantial literature in the social sciences devoted to understanding the causal effects of serving in the military relative to not serving in the military. In contrast, there is considerably less research concerned with how different types of military careers affect the later life outcomes of veterans. This is unfortunate because it seems intuitively true that the training and experience embedded in a military career varies considerably across individual veterans. For example, some recruits may follow occupational paths that develop both hard and soft skills that have value in many situations. Other recruits may follow military occupational paths that yield more specific skills that have less value in other settings. These differences may affect social and economic outcomes a person experiences throughout their life course.

Although a causal relationship is plausible, it is hard to convincingly estimate the causal effects of military acquired skills because individual military occupational choices are likely correlated with other factors that also shape later life outcomes. Recruits are selected into different occupational paths based on their interests, aptitudes, and abilities. And the military may also actively encourage recruits to pursue particular occupational paths. To address problems with selection bias and to examine the longer term effects of military recruitment policies, this study exploits a set of randomized 'skill channeling' experiments that were conducted by different branches of the military in the 1980s. The basic form of the experiments is not complicated. The military divides the United States into a set of recruitment zones and a separate Military Entrance Processing Station (MEPS) is established to manage the enlistment of people who live in each zone. In the experiments of interest here, experimental enlistment incentives were randomly assigned across the MEPS zones. While an experiment was operating, recruits who lived in treated zones were offered the experimental contract and recruits who lived in control zones were not. The goal of the original experiments was to determine the effectiveness of different recruitment instruments in terms of meeting the military's manpower objectives in the context of the all-volunteer military. The main concern was not simply how to increase the total number of enlistees, but with how to ensure that there were enough high quality recruits who agreed to serve in high-demand military occupations.

The basic idea of this paper is to use these recruitment experiments as a source of exogenous variation that can help us study the causal effects of military acquired skills on subsequent social and economic outcomes. Our approach is to combine data from administrative databases maintained by the Department of Defense (DOD) and the Veteran Administration (VA) in order to follow subjects during and after the experiment. This paper presents some very preliminary work that is based on data on army veterans who enlisted during an experiment conducted from 1982 to 1984. The original project was called the Enlistment Bonus Experiment and it was conducted by the Army in conjunction with researchers from Rand. The design of the experiment and an analysis of the effects of the incentives were presented in Polich et al (1986).

In this paper, we review the design of the experiment and the analytical strategy used in the original study of the Enlistment Bonus Experiment. We highlight the strengths and weaknesses of the experimental design as a platform for studying the longer term effects of military career attributes on veterans. And we also discuss some of the methodological differences between our approach and the approach pursued in the original project by Polich et al (1986). Next we present some simple analysis of the administrative data that is designed to assess some of the key threats to the validity of our work. As a first step towards the follow up analysis, we use the data to study some of the same questions that motivated the original project but using a different analytical strategy that is less wedded to a particular theoretical model of the enlistment process. In the second half of the paper, we study the medium term consequences of military skill channeling using several measures of the progression of each recruits military career. In particular, we ask whether the skill channeling effect of the experiment changed the speed with which recruits were promoted, reenlistment rates, and the extent to which the different career paths affected marriage and family size.

2. INSTITUTIONAL DETAILS

2.1. Enlistment Bonus Experiment

In conjunction with researchers from Rand, the Army conducted the Enlistment Bonus Experiment from July 1982 to July 1984. The goal of the experiment was to evaluate the extent to which skill targeted enlistment bonuses could be used to help the military satisfy its demand for high quality young recruits to fill shortages in particular military occupations. The design and evaluation of the study is reported in Polich, Dertouzos, and Press (1986), but the basic features of the study are not complicated. At the time of the study, the United States was divided into 71 enlistment zones. Polich et al explain that a statistical model called MISER was used to match treatment zones with respect to a set of covariates associated with each geographical area. These matching covariates included past enlistment rates, the local unemployment rate, the local wage rate, the local prevalence of high quality men aged 17-21 (HS graduates with above median AFQT scores), the local prevalence of nonwhite men age 17-21, the number of army recruiters per male 17-21 in the area, the recruitment goals of the local MEPS office, and the regional location (eastern, southern, western, northern) of the MEPS zone. After statistically matching

MEPS zones on these covariates, the sites were randomly assigned to one of three treatment groups.

The first group (Treatment A) represented the status quo control condition. In this group of zones, high quality recruits were offered a $5000 bonus in return for enlisting in a targeted occupation for a four year term, which was the existing policy at the time. In the second treatment group (Treatment B), high quality recruits were offered an $8000 bonus in return for enlisting in a targeted occupation for a four year term of service. In the third group (Treatment C), high quality recruits were offered $8000 for a four year term and $4000 for a three year term in a targeted occupation. The assignment process was not designed to generate a balanced sample size across the three treatment groups. Instead random assignment probabilities were chosen so that about 70% of the enlistment eligible young men in the country would reside in one of the areas assigned to the control condition, about 15% would reside in an area assigned to the $8000 bonus, and about 15% would reside in an area assigned to the $8000/$4000 bonus.

2.2 Conceptual Issues and Research Questions

2.2.1. Goals of the Original Study

The original purpose of the Enlistment Bonus Experiment was to answer some basic questions about the effects of the skill targeted enlistment bonus offerings. The study fits squarely into the human resource issues faced by the military after the first 10 years or so of the all-volunteer military. The Enlistment Bonus Experiment was part of a series of studies conducted by the various branches of the military that were intended to develop an evidence based set of policy instruments that could be used to help the military obtain the number and type of recruits it needed to meet its objectives, and to help it effectively train and manage the military workforce once it had obtained these numbers (Rostker, 2006).

The first question in the Enlistment Bonus Experiment was whether the bonuses would lead to “market expansion effects” that would increase the total number of people who enlist in the army. The rationale for market expansion effects is simply that the bonuses can be viewed as an increase in the total compensation package available to new recruits. Other things equal, an increase in the value of the compensation package should make people more likely to enlist in the army. The magnitude of the behavioral response to the change in bonus generosity depends on how many bonus-eligible recruits are close to the margin of entry and how responsive those recruits are to the relatively small change in total compensation that would be induced by the bonus policy change.

The second question was whether the military could use relatively small targeted bonus offerings to shape the distribution of occupational choices made by new enlistees. The military calls this concept “skill channeling” and the logic of the underlying mechanism is quite similar to the logic used in conditional cash transfer programs: recruits can only obtain the bonus if they enlist in the targeted occupations and so the bonuses raise the relative level of compensation associated with targeted and non-targeted occupations. The basic question studied in the original project was whether the incentives would lead to more enlistments in the targeted occupations.

The third question in the original project was whether the bonuses could be used to change the length of enlistment contracts selected by recruits. The two treatment cells provided different incentives with respect to term lengths. In treatment B, recruits were only eligible for the bonus if they agreed to serve in a targeted occupation for a four year term. In contrast, in treatment C, recruits could receive an $8000 bonus for a four year term in a targeted occupation or $4000 for a three year term in a targeted occupation. The different term conditions could generate a different distribution of contracts.

These questions were difficult to answer in a non-experimental setting because uncontrolled variation in military compensation levels often coincided with changes in economic conditions (wage and unemployment levels, for example) that also affect recruitment, and the bonus offerings themselves may have varied in non-random way because of differences in the behavior of individual recruiters and recruitment offices. The experimental design provided a way of separating the effect of the incentives from the effects of these other factors.

The original analysis used a structural model of the enlistment process to answer each of these questions. Polich et al report small market expansion effects in which the bonuses increased the number of high quality enlistments by 4-5%. They found much larger skill channeling effects, reporting that the incentives increased the number of high quality enlistments in targeted occupations by 30-40%. Finally, Polich et al report that the $8000 (Treatment B) incentive increased the number of four year enlistments by about 15%, while the $8000/$4000 (Treatment C) incentive increased the rate of three year enlistments by 87%, had no effect on four year enlistments, and reduced two year enlistments.

2.2.2. Extending the Original Study

The primary goal of this project is to learn how differences in military training and experience affect the later life outcomes of veterans. The appeal of the experimental design is that it seems to provide an exogenous source of variation in the type of military occupation that enlistees pursue during their military career. Occupational choices affect the type of training a person receives, the kind of work they are involved in doing, and the kind of co-workers they are likely to encounter during their term of service. In essence, we hope to interpret the randomized incentives as an instrumental variable that is correlated with military acquired skills but is not correlated with other factors that may affect later life outcomes.

Viewed from this perspective, the key potential problem with the design is that the randomly assigned incentives may not only affect a recruit's occupational choices. The incentives may also affect his decision to join the military in the first place. Put differently, if the incentives generate market expansion effects that induce a different group of people to join the army then the exclusion restriction at the core of our analysis is threated. The original study by Polich et al (1986) found very small market expansion effects of about 4-5% in the context of a structural enlistment model. These effects may be small enough to ignore in our study, but it is somewhat difficult to relate their structural framework to the experimental instrumental variables analysis that we have in mind. Thus, a key preliminary step in our work is to examine the extent to which market expansions were an important consequence of the bonus experiment and to develop strategies for assessing the sensitivity of our analysis to the assumption of no market expansion effects. A second initial goal of our work is to develop a better understanding of how the randomly assigned incentives induced new recruits to change some aspects of their military careers such as occupational choice and term of service. Changes in these channels represent the first stage of the instrumental variables analysis we hope to later use to study the longer term effects of military acquired skills.

3. DATA

3.1. Personnel Data

We obtained personnel data on people who enlisted in the military in the early 1980s from the Defense Manpower Data Center (DMDC). The DMDC is one of the main institutions in charge of holding personnel data for the U.S. military. To study the effects of the Enlistment Bonus Experiment, we retained data on people who enlisted either during the experiment (July 1982 – July 1984), or during the two years preceding the experiment (July 1980 – June 1982). We further limited the analysis to men who enlisted in the Army at a set of 66 Military Entrance Processing Stations (MEPS)[2]. This led to a data file of 483,727 recruits. We removed 642 observations that had missing data on one or more baseline variables, such as age, race, height, weight, AFQT score, education level, military occupation, and date of accession. These 642 observations represent about .13% of the data. Polich et al (1986) provided a map identifying the areas of the country assigned to each of the three treatment cells and we used this map to classify each of the MEPS zones in the data. Readers can view this map in the appendix in Figure 1. Table 1 shows the sample sizes in each of the three treatment cells based on our coding of the personnel data. The relative sizes of the cells line up almost exactly with the distribution intended by the designers of the experiment: about 70% of the recruits are from the control zones, and the remaining 30% of recruits are split evenly between the two treatment zones.

3.2. Balancing Tests

A natural first step in the analysis of experimental data is to examine the distribution of covariates in the treatment and control groups to help assess whether randomization was successful in creating groups of subjects that were comparable at baseline. In expectation,

[2] In fact, there are recruits from 71 MEPS zones in our data. We exclude enlistments that occurred at the MEPS representing the Pacific Area, Guam, Honolulu, Anchorage, and San Juan because we have not been able to determine whether these zones were part of the experiment and (if they were) which treatment cells they belonged too. This is unlikely to matter much because these are relatively small recruitment zones.

randomization should balance the distribution of all observed and unobserved pre-treatment variables in the different treatment groups. Obviously, we cannot observe pre-treatment variables that are not measured. But we can compare observable factors. In this preliminary work, we examine variables that are measured in the personnel files.

Table 2 reports the means of a vector of covariates measured in the personnel files in the three treatment zones for two different groups of enlistees. The first group consists of people who enlisted in the Army from the three treatment zones during the two years before the beginning of the experiment. These recruits were never offered the experimental bonuses and so if random assignment was successful then the three treatment groups should have the same distribution of covariates. Table 2 shows that the two groups are indeed nearly identical with respect to the variables measured in the personnel data, which is a good sign.

The second group consists of recruits who enlisted from the three treatment zones during the randomized experiments. What does random assignment imply about this group of recruits? On the one hand, the variables reported here are all mainly demographic, health, and ability variables that are not likely to be affected by the offer of an enlistment bonus. By this logic, we would expect a balanced distribution of covariates for this group as well. On the other hand, it is possible that the more generous bonuses offered in the treatment arms of the study changed the composition of the pool of enlistees. For example, if the incentives were successful at attracting more high quality recruits, then we might expect the treatment groups to score higher in the AFQT distribution than the control group. Thus, the covariate distribution for the second group of recruits reported in table 2 is informative about the question of whether the experimental incentives were successful at attracting more and different recruits. The results in the table suggest that the incentives did very little to attract a different pool of enlistees or to materially change the characteristics of the enlistees. The mean values of the covariates reported in table 2 are very similar in the three treatment groups.

Although table 2 supports the assumption that random assignment successfully created three groups that were statistically identical at baseline, it also makes it clear that the composition of the population of enlistees changed between the pretest and experimental time periods. For example, in all three zones, the fraction of white recruits was higher in the experimental period than in the pretest period. The fraction of black recruits fell between the pretest and experimental periods. The fraction of high school dropouts fell and the fraction of high school graduates increased. Likewise, the average recruit had an AFQT percentile score of 46 in the pretest period and 54 in the experimental period. These changes reflect an emphasis on the recruitment of high quality recruits – defined as high school graduates with above median AFQT percentile scores. The fraction of the recruits that meet this quality standard grow from about 29% in the pretest period to about 42% in the experimental period. Although these changes were substantial, it is important to note that the changes occurred uniformly across the three treatment groups and so they are not evidence of bias in the random assignment process.

4. ANALYSIS AND RESULTS

4.1. Market Expansion Effects

The covariate means presented in table 2 provide some initial evidence that market expansion effects did not materially affect the composition of the three treatment groups in the experiment. In this section we present some more direct evidence that the skill targeted enlistment bonuses did not generate meaningful changes in the number of high quality recruits who enlisted in the military. We started our analysis of market expansion effects by collapsing the data into month x

MEPS treatment group cells and recording the number of high and low quality enlistments in each cell. Then, for each of the 48 months in our study's time frame, we calculated the average number of high and low quality enlistments for the MEPS assigned to each of the three treatment groups. These monthly averages are plotted in figure 1.

The first 24 months of the time series (June 1980 to June 1982) shows that average enlistment levels were very similar in the treatment and control groups before the experiment was in operation. The first part of the time series also makes it clear that military enlistment patterns are subject a considerable amount of seasonal cyclicality. The second half of the time series (July 1982 to July 1984) shows enlistment levels in the three groups during the experiment. The enlistment levels in the treatment groups continue to track the control group very closely during this period. If there were market expansion effects, we would expect to see a gap between the number of enlistments in the control group and the treatment groups during the experimental period, but there is little evidence of such a gap for either high or low quality enlistments in figure 1.

Although figure 1 provides some simple evidence that the bonuses did not generate substantial market expansion effects, the structural analysis in Polich et al (1986) did find small effects of about 4-5%. Effects of this size might be difficult to detect in the crude time trends reported in figure 1, which are subject to obvious seasonal and month-to-month enlistment patters. To estimate the market expansion effects more precisely, we conducted a more formal statistical analysis of the effects of the treatment bonuses on the enlistment of high and low quality recruits. To understand our approach more clearly, use $t = 1 \ldots 48$ to index the calendar months from July 1980 to June 1986. Likewise, let $m = 1 \ldots 66$ index the individual MEPS zones in the United States. $Post_t = 1[t \geq 25]$ is a dummy variable set to one for the experimental months and set to zero for the pretest months. TB_m is a dummy variable set to one for the MEPS zones that are assigned to treatment B in which high quality recruits were offered an $8000 bonus in return for enlisting in a targeted occupation for a four year term. And TC_m is a dummy variable set to one for the MEPS zones assigned to treatment C in which high quality recruits were offered $8000 for a four year enlistment of $4000 for a three year enlistment in a targeted occupation. Finally, let H_{tm} and L_{tm} be the number of High Quality and Low Quality recruits who enlisted in month t in MEPS m. For the purposes of recruitment, the military defines a high quality recruit as a high school graduate who scores above the median on the AFQT. Recruits who do not meet these criteria are low quality. We collapsed out micro level personnel data into month x MEPS cells, recorded the number of high quality and low quality enlistments in each cell, and assigned each cell the appropriate values of the treatment and time period variables defined above. With the data in hand, we estimated the following basic models:

$$(1)\ L_{tm} = \alpha_0 + \alpha_1 TB_m + \alpha_2 TC_m + \alpha_3 Post_t + \alpha_4 (TB_m \times Post_t) + \alpha_5 (TC_m \times Post_t) + \varepsilon_{tm}$$

$$(2)\ H_{tm} = \beta_0 + \beta_1 TB_m + \beta_2 TC_m + \beta_3 Post_t + \beta_4 (TB_m \times Post_t) + \beta_5 (TC_m \times Post_t) + \mu_{tm}$$

In these DID regressions, the coefficients on the interaction terms represent the experimental treatment effects. That is, α_4 and α_5 measure the average treatment effect of Treatment B and Treatment C on the number of low quality enlistments at a site. Similarly, β_4 and β_5 represent the effects of the treatments on the number of high quality enlistments. In this framework, positive values of these coefficients would imply that the treatments generated a market expansion effect. Negative values would imply a market contraction. In theory, both of these effects are possible because the availability of the special bonuses at the treatment cells could have induced a change in recruiter effort levels that would offset or nullify the effects of the incentives on recruits.

The DID approach we pursue identifies the overall effect of the randomly assigned incentives on high and low quality enlistments. The structural model presented in the original analysis of the experiment sought to separate the effects of the incentives on the marginal enlistment decisions of potential recruits from the effects of recruiter effort levels. Identifying both of these effects is a difficult task and requires assumptions that are stronger than those implied by the experimental design. However, for the purposes of our study, the main concern is with the overall effect of the incentives on the number and type of people who enlisted in the army. This is why we pursue the DID approach.

We estimated the model parameters using OLS regressions. To account for dependencies between the observations within sites over time, we calculated standard errors using a robust variance matrix that allowed for clustering at the MEPS level. We estimated versions of the basic model that included month fixed effects to account for cyclical patterns in the enlistment process, and MEPS fixed effects to account for time invariant site specific factors. And we estimated the model using both the level number of enlistments and log of the number of enlistments as the dependent variable.

Table 3 reports estimates from the models of the number of high and low quality enlistments at each MEPS site. Table 4 presents estimates from models of the log of monthly enlistments. Note that the log enlistment models do not include observations from a small number of sites-month cells in which there were no enlistments. The results from the models confirm the intuitive findings presented in figure 1. The estimated coefficients are not statistically significantly different from zero, and they are also quantitatively small. Ignoring statistical precision, point estimates imply that Treatment B ($8000 bonus) increased the average number of high quality enlistments by about 1.7 recruits per month and the number of low quality enlistments by about 4.4 recruits per month. In contrast, the point estimates suggest that Treatment C actually reduced the number of high quality enlistments by about 1.4 recruits per month and reduced the number of low quality enlistments by about 5.1 recruits per month. These somewhat contradictory results further suggest that the true overall market expansion effect of the randomly assigned incentives is close to zero. Adding month and MEPS fixed effects into the model has virtually no effect on the estimated treatment effects. And the estimates from the log-linear specification are also similar to the level models. It is, however, worth noting that given a base of about 40 recruits per month, the coefficient on Treatment B of implies about a 4% increase in high quality enlistments. This is in line with the estimates reported by Polich et al (1986) that were based on different methods. One take-away from this analysis is that a 4% increase in the number of high quality enlistments is a small effect that it is not easy to estimate precisely in these data. Either way, we think the data support the conclusion that the randomly assigned bonuses did not alter the level of enlistment or the composition of enlistment in a quantitatively important way.

4.2. Skill Channeling Effect

The basic goal of this project is to study the effects training and work experience a person receives during military service on later life outcomes. The logic of our identification strategy hinges on the claim that the randomly assigned enlistment bonuses can serve as a source of exogenous variation in the military career paths pursued by new enlistees. A key threat to the validity of the design is that the instruments could also affect who actually enlists in the military in the first place. The previous section showed that the incentives did not generate much in the way of these market expansion effects. And the table 2 suggests that the treatments also do not seem to have altered the composition of the army recruits with respect to age, race, education, cognitive ability, or physical size. These results are encouraging, but they do not establish that the randomly assigned instruments actually did influence the nature of the military careers of the pursued by the enlistees. In this section, we study the effects of the bonuses on the take up of targeted military occupations. In the next section, we study how the bonuses affected the speed with which recruits were promoted, reenlistment rates, and the extent to which the different career paths affected marriage and family size.

The analysis in this section mostly takes place at the level of individual recruits. As before, we use t to index calendar months and m to index MEPS sites. Likewise, we use $i = 1 \dots N$ to index the individual enlistees in our sample and we let Q_{itm} be a dummy variable set to one if the recruit is high quality in the sense that he is a high school graduate with an AFQT percentile score of at least 50. The bonuses offered in the treatment groups in the Enlistment Bonus Experiment were only available to high quality recruits who agreed to enlist in one of 7 combat arms military occupations. These occupations are defined by three digit MOS codes: Signal/Intelligence (05H), Infantry (11X), Field Artillery (13B, 13E, 13F), Pershing Missile Crew (15E), and Armor (19A). We used the personnel data to identify the initial military occupation chosen by each recruit. To keep the individual occupational choices clear, use $j \in \{05H, 11X, 13B, 13E, 13F, 15E, 19A\}$ to denote the set of targeted occupations. And define a set of indicator variables $s(j)_{itm}$ that are set to one if the person enlisted in the jth occupation and a set to zero otherwise. Finally, let $B_{itm} = Max_j\{s(j)_{itm}\}$ be a dummy variable set to one if the person enlisted in one of the seven targeted military occupations. B_{itm} is the main dependent variable in the skill channeling analysis. The essential question is whether recruits from the treatment zones were more likely to choose an incentivized occupation than recruits from the control zones.

Figure 2 depicts the fraction of high and low quality recruits who opted for one of the seven incentivized occupations in the control group and the two treatment groups in each month in covered in our study. As before, the first 24 months of the time series represent a pretest period in which the randomly assigned bonuses were not available in any of the sites. The second 24 months starting in July 1982 are the experimental months when the bonuses were offered in the treatment cells but not in the control cells. The first panel of figure 1 shows take up rates among high quality recruits. The three groups have very similar take up rates in the pretest time period, but they begin to diverge during the experimental time periods. The take up of incentivized occupations increased in the two treatment groups relative to the control group almost immediately after the bonuses became available. And the difference in take up persisted until the experiment ended in July 1984. The second panel in figure 2 shows that there is no comparable effect for the low quality recruits, which makes sense because the bonuses were only offered to high quality recruits.

Figure 2 provides convincing evidence that the targeted incentives generated a substantial skill channeling effect. But it is not easy to estimate the magnitude of the effect because of the cyclical volatility in the time series plots. To measure the size of the effect and also to examine the strength of the instruments in a regression framework, we estimated DID type regressions with the following basic form:

$$(3)\ B_{itm} = \beta_0 + \beta_1 TB_{itm} + \beta_2 TC_{itm} + \beta_3 Post_{itm} + \beta_4 (TB_{itm} \times Post_{itm}) + \beta_5 (TC_{itm} \times Post_{itm}) + \mu_{itm}$$

In these models, the coefficients on the interaction terms represent the average treatment effects of the randomly assigned enlistment bonuses on the probability that a recruit chooses one of the incentivized occupations. Positive coefficients would provide evidence of a skill channeling effect and the size of the coefficients measure the magnitude of the effect. We estimated the models separately for high and low quality recruits using linear probability models. The standard errors were computed using a robust variance matrix that allowed for clustering at the MEPS level. We also estimated models that included month and MEPS fixed effects, and as a further robustness check we estimated a triple differenced model by pooling the high and low quality recruits and fully interacting the model to allow for a complete vector of site x month specific fixed effects.

Table 5 reports estimated coefficients and standard errors for the DID regressions. Table 5 presents the results from the full triple differenced regressions. Both analyses provide evidence of a substantial skill channeling effect for both treatment conditions among the high quality recruits, and for no effect among the low quality recruits. Both the $8000 incentive offered to recruits in Treatment A and the $8000/$4000 incentives offered to the recruits in Treatment B increase the take up of incentivized occupations by about 7 percentage points. The baseline take up rate is about 20 percent in these data and so this is s substantial relative increase of about 35%. The t-statistic on these coefficients is about 7 and so the coefficients are statistically significant. The F-Statistic on the joint significance of the two DID interaction terms that would serve as instrumental variables in follow up analysis is 129 when calculated using the standard

OLS covariance matrix and it is 19 when calculated using the cluster robust variance matrix. Thus, the randomly assigned bonuses seem to pass standard tests for weak instruments of F > 10.

The main results here survive in the fully interacted triple difference models presented in table 6, although the magnitude of the effect of Treatment B falls from 7 percentage points to only 4 percentage points. The effect of Treatment C is unchanged and both point estimates are sufficiently large and reject the null hypothesis of no skill channeling effects at conventional levels of significance.

4.3. Family and Professional Outcomes

In this section we expand upon our initial analysis with some preliminary estimates of medium term outcomes as a result of the experiment. Specifically, we estimate the effects of the experiment on enlistees' future family and professional outcomes with a standard difference-in-differences (D-D) linear probability model:

$$(4) \quad Y_{itm} = \beta_0 + \beta_1 TB_{itm} + \beta_2 TC_{itm} + \beta_3 Post_{itm} + \beta_4 (TB_{itm} \times Post_{itm}) + \beta_5 (TC_{itm} \times Post_{itm}) + \mathbf{X}'_{\mathbf{itm}}\boldsymbol{\gamma} + \mu_{itm}$$

where Y represents an outcome variable for enlistee *i* in time period *t* from MEPS zone *m* . The primary outcome variables in this analysis include a dummy variable equal to one if the enlistee is married anytime throughout their initial contract (three or four years depending on the contract) and zero otherwise, a dummy variable equal to one if the if the enlistee had any number of dependents greater than zero throughout their initial contract and zero otherwise, a dummy variable equal to one if the enlistee ever reenlisted in the military after their initial contract was completed and zero otherwise, and lastly, a dummy variable equal to one if the enlistee achieved the rank of E-5 or higher (note: a one for this variable also includes regular officers and warrant officers) anytime throughout the course of their first contract and zero otherwise.

Similar to equations (1), (2), and (3), TB_m is a dummy variable set to one for the military entrance processing station (MEPS) zones that are assigned to treatment B in which high quality recruits were offered an \$8000 bonus in return for enlisting in a targeted occupation for a four year term. And TC_m is a dummy variable set to one for the MEPS zones assigned to treatment C in which high quality recruits were offered \$8000 for a four year enlistment of \$4000 for a three year enlistment in a targeted occupation. $Post_{itm}$ is an indicator equal to one for observations in July 1982 or later and zero otherwise. The vector $\mathbf{X}'_{\mathbf{itm}}$ includes several baseline control variables which were measured for enlistee *i* (just before the start of their initial contract) such as age, race, height, weight, BMI, AFQT percentile, and education. Also included in the vector $\mathbf{X}'_{\mathbf{itm}}$ are monthly dummy variables to control for time effects, as well as dummy variables for each of the MEPS locations. Finally, μ_{itm} is a white noise error term.

In these DID regressions, the coefficients on the interaction terms represent the experimental treatment effects. Thus, β_4 and β_5 are the parameters of interest from equation (4). They measure the average treatment effect of Treatment B and Treatment C on outcome Y. In this analysis, we exclude recruits in the DMDC data if they enlisted prior to July 1980 (two years before the experiment started) and those recruits whom enlisted after July 1984 (after the experiment was over). We also restrict the sample to only high quality recruits since those are the observations of interest for this study.

Table 7 shows the findings for family outcomes such as marriage and whether or not the high quality enlistee had a dependent throughout the first term of his or her initial contract from equation (4). Columns 3 and 6 are of particular interest in this table since they include the full set of controls. Columns 1 and 4 have no controls and columns 2 and 5 include month and MEPS fixed effects.

We find some evidence that the treatment effects from the experiment induced high quality recruits in treatment areas to marry at higher rates than those in the control group. This is particularly true for the group of high quality recruits in treatment area B. The coefficients for the interaction term Treatment B x Post are statistically significant at the 5% level in columns 1 and 2 and at the 1% level in column 3. They range in value from 0.019 to 0.022. The evidence for higher marriage rates for recruits in treatment area C is not as strong as those for treatment area B, as can be viewed by the statistical insignificant coefficients for the interaction term Treatment C x Post across all three regressions in columns 1 through 3. It should be noted, however, that all three of the insignificant coefficients are positive.

We also find strong evidence that high quality recruits were more likely to have at least one dependent in the treatment areas in comparison to the control group as a result of the experiment. The coefficients in columns 4 through 6 for the interaction terms Treatment B x Post and Treatment C x Post are all positive and range in value from 0.014 to 0.023. In addition, they are all at least statistically significant at the 5% level.

Table 8 presents results from equation (4) in regards to professional outcomes. As stated previously, the professional outcomes of interest here are whether or not high quality enlistees in the treatment groups reenlisted at higher rates and whether or not they were promoted at faster rates than those in the control group.

There does not appear to be much evidence that high quality enlistees in the treatment areas reenlisted at higher rates than those in the control group. All six of the coefficients of interest for the interaction terms Treatment B x Post and Treatment C x Post are statistically insignificant from zero. In addition, the economic insignificance is also apparent when looking at the relatively small size of these coefficients. They range in value from 0.000 to 0.002.

In contrast to reenlistment rates, we find some evidence that the treatment area high quality enlistees were promoted at faster rates in comparison to those in control areas. All three of the coefficients for the interaction term Treatment C x Post in columns 4 through 6 are positive, ranging in value from 0.015 to 0.023 and are statistically significant at the 5% level. The promotion effect for those recruits in treatment area B is not as robust as that in treatment area C. While all of the coefficients for the interaction term Treatment B x Post are positive, they are smaller in magnitude in comparison to those from Treatment C x Post and none of them are statistically significant at any level.

5. CONCLUSION

This paper presents some preliminary results for a broad based study designed to measure the medium- and long-term economic and health outcomes for veterans as a result of an enlistment bonus experiment conducted by Rand in the early 1980s. The experiment provides us a unique source of exogenous variation that can help us study the causal effects of military acquired skills on subsequent health and economic outcomes. We use detailed panel data from DMDC to analyze these effects across a large database of military recruits.

We use a standard difference-in-differences identification strategy as a means to isolate the effects of the experiments on high quality enlistees. In our initial analysis, we find the higher enlistment bonus incentives available to the treatment groups did very little to attract a different pool of enlistees or to materially change the characteristics of the enlistees. Furthermore, we find the higher bonuses did not generate substantial market expansion effects. We do find, however, that the experiments had a “skill channeling” effect on enlistees in the treatment zones (i.e. high quality recruits were more likely to enlist in combat occupations as a result of the higher enlistment bonuses for these types of jobs).

The empirical evidence in this study suggests that the experiments had a significant effect on some of the family and professional outcome variables available in the DMDC data. In general, we find that high quality recruits in the treatment areas were more likely to marry and have at least one dependent in comparison to the control areas. High quality recruits were also more likely to have faster promotion rates in comparison to the control areas. We find little evidence that there was any difference between reenlistment rates between treatment and control groups as a result of the experiment.

The preliminary work here provides a nice addition to the growing literature about the effects of military acquired skills on economic and health outcomes for military veterans. In this study we only use data from DMDC in our analysis, but we plan to merge these data with those from the VA and expand our analysis to more specific health outcomes in the future. Likewise, we plan to merge these data with the Social Security Administration (SSA) data to obtain information about the effects of the experiments on long-term earnings.

Table 1: Sample sizes by treatment status in the DMDC sampled used in our analysis.

Treatment Cell	N	Percent
Treatment A (Control)	329,269	70
Treatment B ($8000)	68,347	15
Treatment C ($8000/$4000)	73,329	16
Total	470,945	100

Table 2: Covariate balance in the treatment and control groups before and during the experiment.

	Two Year Pretest Period			Two Year Experimental Period		
	Control	**Treatment B**	**Treatment C**	**Control**	**Treatment B**	**Treatment C**
Demographics						
Age	20	20	20	20	20	20
Race						
White	0.70	0.69	0.69	0.75	0.72	0.74
Black	0.26	0.28	0.27	0.21	0.25	0.22
Other	0.04	0.03	0.03	0.04	0.03	0.04
Physical Characteristics						
Height (Inches)	69	69	69	69	69	69
Weight (lbs)	157	158	157	158	159	158
BMI	28.4	32.2	30.6	23.1	30.3	23.1
BMI>25	0.25	0.25	0.25	0.25	0.26	0.25
BMI>30	0.04	0.04	0.04	0.03	0.04	0.03
Education and Ability						
AFQT Percentile	46	48	46	54	54	54
HS Dropout	0.29	0.30	0.29	0.18	0.19	0.16
HS Graduate	0.68	0.67	0.69	0.78	0.78	0.80
Some College	0.01	0.01	0.01	0.01	0.01	0.01
College Graduate	0.02	0.02	0.02	0.02	0.02	0.02
High Quality Recruit	0.29	0.30	0.29	0.42	0.42	0.42
N	**155,756**	**31,812**	**35,562**	**173,513**	**36,535**	**37,767**

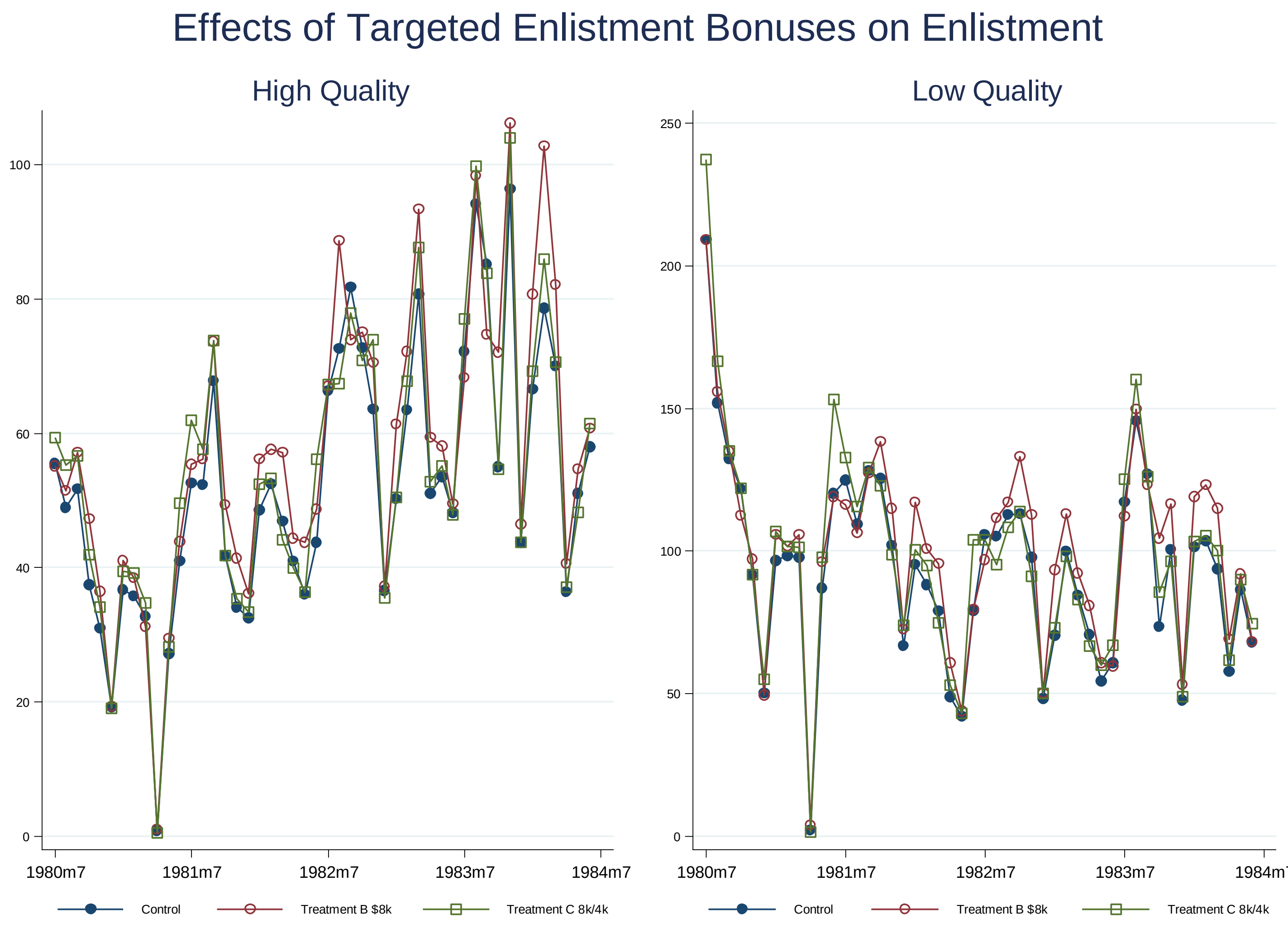

Figure 1: Effects of randomly assigned enlistment bonuses on the number of people who enlist in the army.

Table 3: Effects of incentives on the number of monthly high and low quality enlistment at each MEPS location.

	High Quality Enlistments			Low Quality Enlistments		
Variable	**1**	**2**	**3**	**4**	**5**	**6**
Constant	40.30	38.94	22.67	97.78	96.66	48.32
	3.63	3.68	1.57	9.62	9.56	1.84
Treatment B ($8000)	4.34	4.34	78.77	4.86	4.86	172.08
	11.54	11.56	3.77	29.96	30.01	1.78
Treatment C ($8000/$4000)	3.21	3.21	68.72	6.89	6.89	121.47
	8.59	8.61	2.63	16.53	16.56	1.90
Post	24.19	24.19	24.19	-8.45	-8.45	-8.45
	2.75	2.76	2.78	3.18	3.19	3.22
Treatment B x Post	1.74	1.74	1.74	4.38	4.38	4.38
	7.46	7.47	7.55	3.52	3.53	3.56
Treatment C x Post	-1.44	-1.44	-1.44	-5.12	-5.12	-5.12
	5.20	5.21	5.27	3.76	3.77	3.81
Month Fixed Effects	N	Y	Y	N	Y	Y
MEPS Fixed Effects	N	N	Y	N	N	Y
N	3168	3168	3168	3168	3168	3168
r2	0.081	0.158	0.769	0.005	0.132	0.762

Table 4: Effects of incentives on the monthly log number of high and low quality enlistments at each MEPS location. (Level)

	log(High Quality Enlistments)			log(Low Quality Enlistments)		
Variable	**1**	**2**	**3**	**4**	**5**	**6**
Constant	3.48	3.48	3.22	4.26	4.29	3.76
	0.09	0.09	0.02	0.10	0.10	0.02
Treatment B ($8000)	-0.04	-0.04	-0.55	-0.19	-0.19	-0.68
	0.27	0.27	0.02	0.35	0.35	0.02
Treatment C ($8000/$4000)	0.06	0.06	1.22	0.18	0.17	0.19
	0.20	0.20	0.02	0.16	0.16	0.02
Post	0.44	0.46	0.46	-0.05	-0.03	-0.03
	0.03	0.03	0.03	0.03	0.03	0.03
Treatment B x Post	-0.01	-0.01	-0.01	0.07	0.07	0.07
	0.05	0.04	0.05	0.04	0.04	0.04
Treatment C x Post	-0.01	-0.01	-0.01	-0.05	-0.04	-0.04
	0.04	0.04	0.04	0.05	0.05	0.05
Month Fixed Effects	N	Y	Y	N	Y	Y
MEPS Fixed Effects	N	N	Y	N	N	Y
N	3096	3096	3096	3112	3112	3112
r2	0.070	0.164	0.729	0.009	0.164	0.729

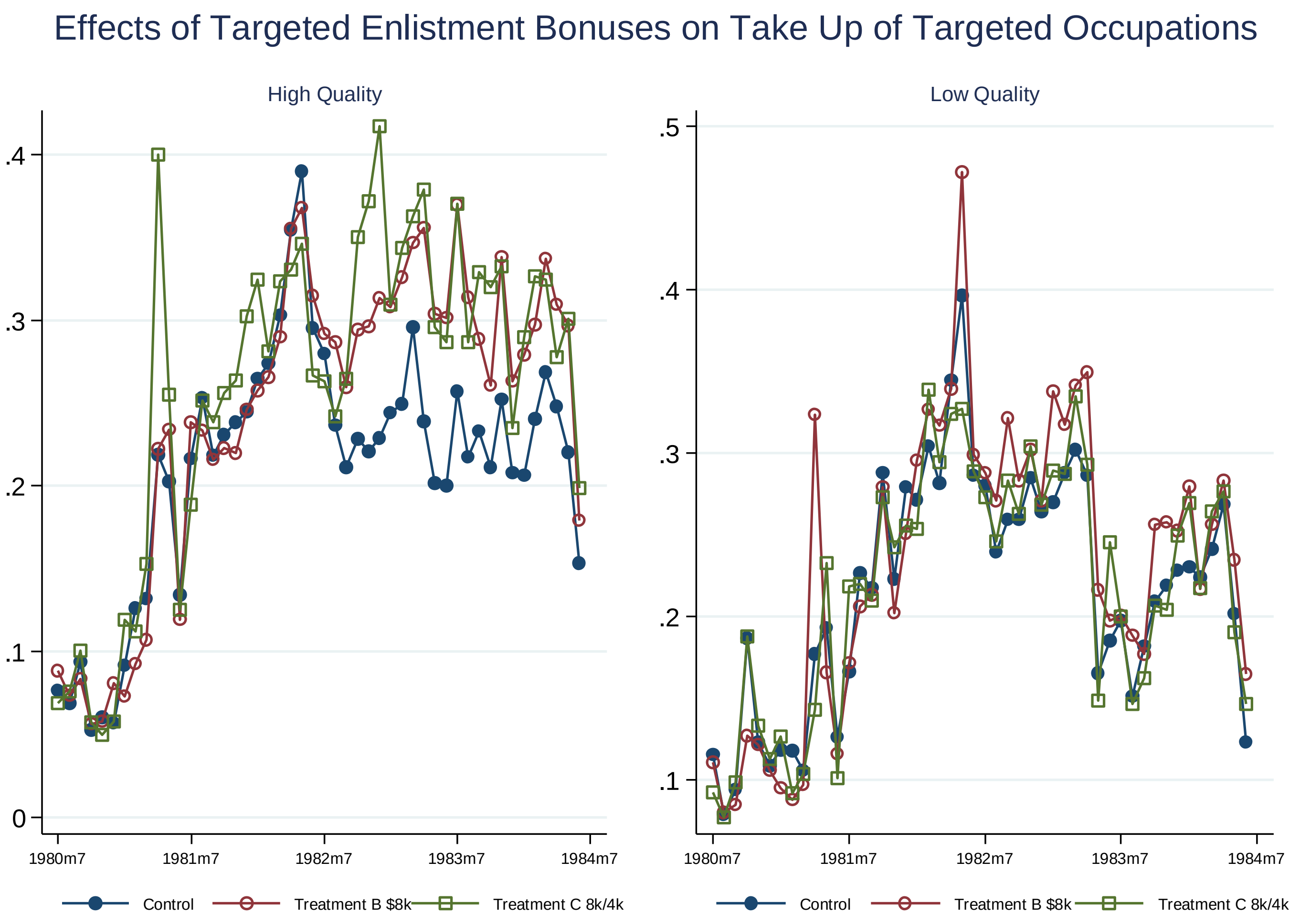

Figure 2: Effects of randomly assigned incentives on the proportion of enlistees who choose targeted occupations.

Table 5: Effects of incentives on the probability that enlistees choose a targeted occupation.(OLS)

	High Quality Enlistments			Low Quality Enlistments		
Variable	**1**	**2**	**3**	**4**	**5**	**6**
Constant	0.19	0.19	0.15	0.19	0.21	0.20
	0.00	0.01	0.01	0.00	0.01	0.00
Treatment B ($8000)	0.00	0.00	0.06	0.00	-0.01	0.01
	0.01	0.01	0.01	0.01	0.01	0.01
Treatment C ($8000/$4000)	0.00	0.01	0.05	0.00	0.00	0.01
	0.01	0.01	0.01	0.01	0.01	0.01
Post	0.04	0.04	0.04	0.05	0.04	0.04
	0.01	0.01	0.01	0.01	0.01	0.01
Treatment B x Post	0.07	0.07	0.07	0.03	0.03	0.03
	0.01	0.01	0.01	0.02	0.02	0.02
Treatment C x Post	0.08	0.07	0.07	0.01	0.01	0.00
	0.02	0.02	0.02	0.01	0.01	0.01
Month Fixed Effects	N	Y	Y	N	Y	Y
MEPS Fixed Effects	N	N	Y	N	N	Y
N	171,434	171,434	171,434	312,289	312,289	312,289
r2	0.01	0.01	0.02	0.00	0.01	0.01

Table 6: Effects of incentives on the probability that high skill recruits choose a targeted occupations. Triple Differences.

Variable	1	2	3
Constant	0.19	0.21	0.21
	0.00	0.01	0.00
High Quality Recruit	0.01	-0.02	-0.01
	0.00	0.01	0.01
Treatment B	0.00	-0.01	-0.04
	0.01	0.01	0.01
Treatment C	0.00	0.00	0.07
	0.01	0.01	0.00
Post	0.05	0.04	0.04
	0.01	0.01	0.01
Post x High Quality	-0.01	-0.01	-0.01
	0.00	0.00	0.00
Treatment B x High Quality	0.00	0.00	0.00
	0.01	0.01	0.01
Treatment C x High Quality	0.01	0.01	0.00
	0.01	0.01	0.01
Treatment B x Post	0.03	0.03	0.03
	0.02	0.02	0.02
Treatment C x Post	0.01	0.01	0.01
	0.01	0.01	0.01
Treatment B x Post x High Quality	0.04	0.04	0.04
	0.02	0.02	0.02
Treatment C x Post x High Quality	0.07	0.07	0.07
	0.01	0.01	0.01
Month Fixed Effects	N	Y	Y
Month x High Quality Fixed Effects	N	Y	Y
MEPS Fixed Effects	N	N	Y
MEPS x High Quality Fixed Effects	N	N	Y
MEPS x Month Fixed Effects	N	N	Y
N	483,723	483,723	483,723
r2	0.007	0.011	0.018

Table 7: Family Outcomes for High Quality Enlistees

	Marriage			Dependents		
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.001	-0.009	0.004	-0.000	-0.009	0.006
	(0.004)	(0.004)**	(0.0035)	(0.004)	(0.004)**	(0.003)*
Treatment B ($8000)	-0.002	0.026	0.056	0.003	0.027	0.043
	(0.024)	(0.006)***	(0.004)***	(0.025)	(0.005)***	(0.003)***
Treatment B x Post	0.019	0.020	0.022	0.018	0.019	0.022
	(0.008)**	(0.009)**	(0.007)***	(0.007)**	(0.008)**	(0.006)***
Treatment C ($8000/$4000)	-0.006	0.151	0.107	-0.007	0.152	0.094
	(0.017)	(0.007)***	(0.007)***	(0.018)	(0.005)***	(0.005)***
Treatment C x Post	0.020	0.172	0.013	0.023	0.019	0.014
	(0.012)	(0.123)	(0.012)	(0.009)**	(0.009)**	(0.008)*
Other Enlistee Characteristics	N	N	Y	N	N	Y
Month and MEPS FEs	N	Y	Y	N	Y	Y
N	107626	107626	107611	107626	107626	107611
R^2	0.0002	0.0289	0.1137	0.0003	0.032	0.1438

All estimated equations include a constant term. Robust standard errors are computed with clustering at the MEPS level. Other enlistee characteristics include measurements for age, race, height, weight, BMI, AFQT percentile, and education.

The symbols *** indicate statistically significant at the one percent level, ** at the five percent level, and * at the ten percent level. Sample is restricted to only high quality enlistees.

Table 8: Professional Outcomes for High Quality Recruits

	Reenlistment			E-5 or higher		
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.005	0.005	0.006	-0.010	-0.015	-0.010
	(0.001)***	(0.001)***	(0.001)***	(0.004)**	(0.004)***	(0.003)***
Treatment B ($8000)	-0.002	0.006	-0.002	0.004	0.026	0.078
	(0.001)	(0.001)***	(0.001)**	(0.015)	(0.006)***	(0.005)***
Treatment B x Post	0.002	0.002	0.002	0.008	0.008	0.009
	(0.001)	(0.001)	(0.001)*	(0.008)	(0.009)	(0.009)
Treatment C ($8000/$4000)	-0.002	0.005	0.013	-0.011	-0.011	0.058
	(0.001)*	(0.001)***	(0.001)***	(0.011)	(0.006)*	(0.006)***
Treatment C x Post	0.001	0.000	0.000	0.023	0.019	0.015
	(0.001)	(0.001)	(0.001)	(0.010)**	(0.009)**	(0.008)**
Other Enlistee Characteristics	N	N	Y	N	N	Y
Month and MEPS FEs	N	Y	Y	N	Y	Y
N	107626	107626	107611	107626	107626	107611
R^2	0.0009	0.0063	0.028	0.025	0.0253	0.114

All estimated equations include a constant term. Robust standard errors are computed with clustering at the MEPS level. Other enlistee characteristics include measurements for age, race, height, weight, BMI, AFQT percentile, and education.

The symbols *** indicate statistically significant at the one percent level, ** at the five percent level, and * at the ten percent level. Sample is restricted to only high quality enlistees.

APPENDIX:

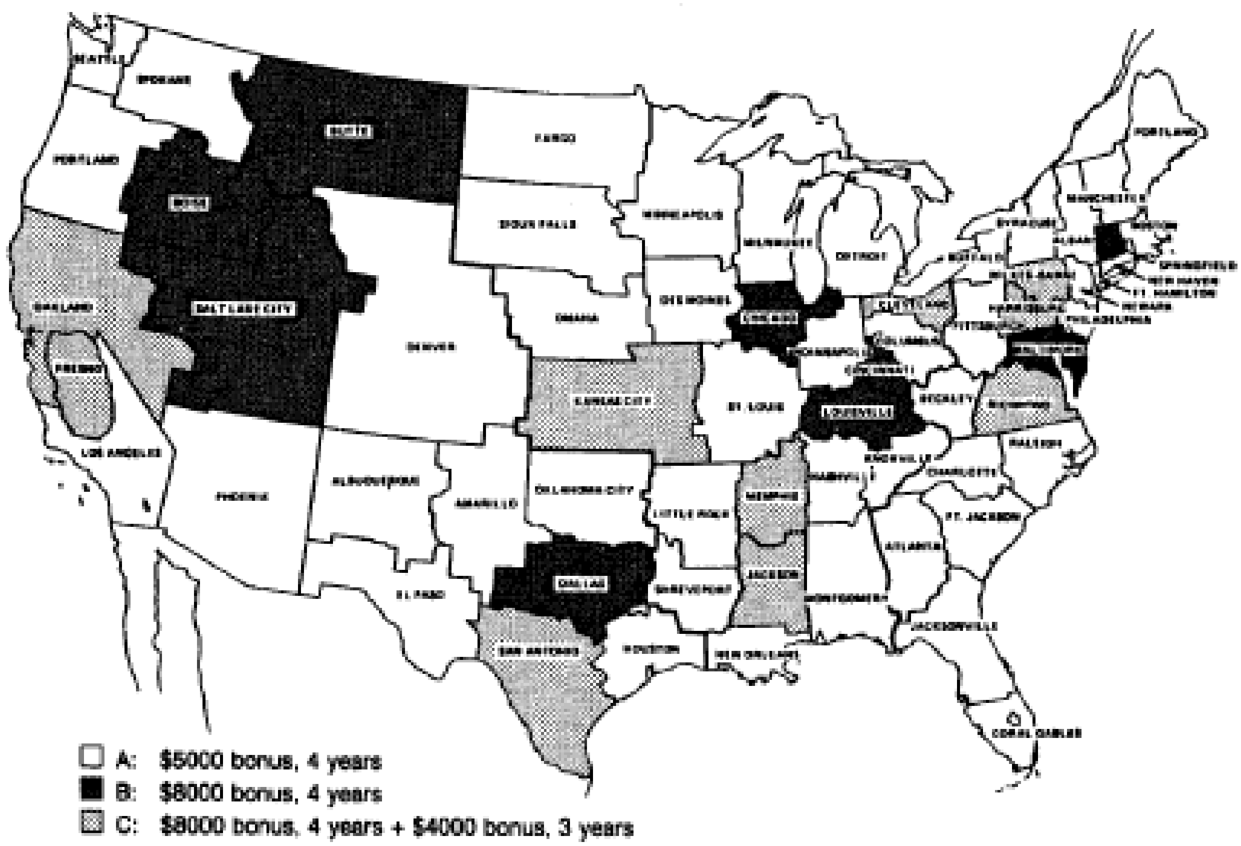

Fig. 1—Areas in the Enlistment Bonus Test

Figure 1A: Areas in the Enlistment Bonus Test