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MILITARY ENLISTMENTS: WHAT CAN WE LEARN FROM GEOGRAPHIC VARIATION?

Charles Brown

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ABSTRACT

This paper analyzes the determinants of the supply of enlistees to the U.S. Army, using quarterly data from 1975:4 through 1982:3 for the 50 states and the District of Columbia.

For high-quality enlistees, defined as those with test scores in the top half of the population or top scoring individuals who are also high school graduates, supply elasticities with respect to military compensation are estimated to be about 1.0. Elasticities with respect to the unemployment rate center on 0.5, larger than most previous estimates. Recruiting resources have the expected effects (Army recruiters increase and other services' recruiters reduce Army enlistments). Advertising (both national and local) does not have consistently positive effects. Results are similar for high school graduates, except that the effect of military compensation depends crucially on how it is measured. Estimates of the supply of enlistees of all qualities are weaker still: estimates of compensation effects vary widely, and estimated effects of recruiters and advertising are less plausible. Unemployment elasticities of about 0.3 are smaller than for high-quality recruits, but hardly negligible.

A tentative explanation for the weaker results of the latter two groups is that the number of such enlistees is not supply determined, but reflect demand constraints as well. Further work is needed to determine how standards for enlistees vary in each recruiting district in response to both national and local fluctuations in recruit supply.

Charles Brown
Department of Economics
University of Maryland
College Park MD 20742
(301) 454-4182

The decision to end the draft in 1973 and return to reliance on voluntary enlistments can be seen as a commitment, an experiment, or (perhaps most accurately) a commitment to an experiment. Given the size of the force considered necessary and a limit to the political willingness to pay for the All-Volunteer Force (AVF), the number of volunteers who could be attracted at politically realistic wages has been a key question.

This supply issue has arisen often in the brief history of the most recent AVF -- first when the end of conscription was being planned, and later when renorming of the entrance test (Armed Forces Vocational Aptitude Battery) showed a decline in the mental abilities of recruits. The more satisfactory recent recruiting experience has been attributed by some to uncommonly high unemployment but by others to the pay increases which occurred at about the same time. If the high-unemployment explanation is correct, the viability of the AVF could once again be in doubt if unemployment rates decline substantially.

It is difficult to summarize the results of previous econometric work on the supply of enlistees because the supply measure has been defined in so many ways. Some studies analyze total Defense Department enlistments, while others look at enlistments in particular services. Most focus on high-quality recruits, though "high-quality" is defined in different ways. Estimates of the elasticity of enlistments with respect to military compensation center a bit below 1.0 (Dale and Gilroy, 1983; Morey and McCann, 1983). Elasticities with respect to unemployment are much smaller; Morey and McCann's (1983) survey's range of 0.2 to 0.5 is representative. The strongest statement of the unimportance of unemployment is Ash, Udis, and McNown's (1983, p. 147) conclusion that "The evidence on the lack of an unemployment effect on accessions is overwhelming."

The purpose of this paper is to examine the supply of enlistees with a different empirical strategy-- pooling cross-section and time-series data from the all-volunteer period. In Section I, the analytic issues raised by previous studies are considered. One conclusion which motivates the remainder of the paper is the difficulty of learning about the supply of volunteers to a peacetime army from information about enlistments over a period which includes the Vietnam War, the draft, and the post-war experience. In Section II, the data for studying determinants of Army enlistments in fiscal years 1976-82 are described. The major results appear in Section III. While the response of enlistments to compensation is quite complicated, the effect of unemployment on enlistments is unambiguous and larger than typically estimated in previous studies. Conclusions are presented in Section IV.

I. Theory and Estimation Issues

The economic theory of voluntary military enlistments was outlined by Oi (1967) and Fisher (1969), and succeeding papers have been (at least implicitly) based on their approach. In this model, the proportion of those eligible who enlist is equal to the fraction for whom the military wage W_m exceeds the reservation wage for military service W_r (i.e., for whom W_m/W_r exceds one). An important determinant of W_r is the alternative civilian wage W_c. If W_r is a constant multiple of W_c, supply depends on the ratio W_m/W_c. This proportionality restriction is often convenient in theoretical (Friedman, 1976, pp. 239-40) as well as empirical work on occupational choice. However, while the theory implies that the supply function is homogeneous of degree one in W_m, W_c, and prices, homogeneity in W_m and W_c alone is an assumption rather than an implication of the theory.

In empirical work, aggregated data are often used, and W_c is represented by the mean of the civilian wage distribution. In principle, the distribution of W_c around its mean is also relevant, although this is usually assumed constant across the sample of observations.

Fisher noted that observed civilian wages do not include the best opportunity (perhaps zero) open to those unemployed. Thus, the unemployment rate (or some similar measure) is typically included in enlistment supply functions.

Finally, changes in tastes can affect the relationship between W_c and W_r. These can be included as determinants of supply when suitable proxy variables are available.

Since Fisher's paper, there have been relatively few theoretical developments in modelling enlistment behavior. Rather, most of the attention has been directed toward correctly estimating the determinants of the supply of volunteers.

One important point which was recognized in Fisher's original paper is that the Armed Forces are neither pure price takers nor pure quantity takers. Rather, they attempt to fill a predetermined number of positions at a predetermined wage, with recruit quality varying to equate supply and demand (DeVany and Saving, 1982). Thus, the number of high-quality enlistees is supply determined, but the total number of enlistees reflects both supply and demand forces (or demand alone if targets are always exactly fulfilled). High-quality enlistees are taken to be those with superior mental-test scores (Altman and Fechter, 1967; Fisher, 1969; Fechter, 1979), high school graduates (Dale and Gilroy, 1983) or high-scoring high school graduates (Cooper, 1977; Grissmer, 1979; Jehn and Shugart, 1979). One recent study of total enlistments (divided by race) cautions that demand constraints may be important (Ash, Udis, and McNown, 1983, pp. 147, 154).

Focusing on high-scoring graduates is probably the best strategy if one's goal is to isolate supply behavior. Studying either high scorers or graduates may reduce but not eliminate the problem of demand-side constraints. Because both test scores and high school graduation are desired attributes, a change in standards for either attribute may change the number of enlistees possessing the other attribute, even if supply conditions and the target number of enlistees is constant. For example, in 1981 the Army restricted enlistment of those with low test scores, leading to a drop in the proportion of enlistees who were high school graduates (Huck, Kusmin, and Shepard, 1982, p. 5) as higher-scoring nongraduates replaced low-scoring graduates. Periodic Congressional pressure to reduce the number of nongraduates may have analogous negative effects on the "supply" of high-scoring enlistees. An unintended experiment in the late 1970s may also have distorted apparent supply. An error was made in norming the entrance exam, leading to a substantial increase in the proportion of low-scoring enlistees (Office of the Assistant Secretary of Defense, 1982, pp. 17-19) and more enlistments than would have occurred otherwise at the announced wage and intended quality standards. Presumably, the error led to a real increase in the number of high-school graduate enlistees (as some with low "true" scores were mistakenly accepted) and an apparent increase in the number of high-scoring enlistees (unless one uses the corrected score data).

A second issue is the usefulness of pooling data from years in which there was both a war and a draft with data from the peacetime all-volunteer period. The existence of a draft stimulates draft-encouraged volunteers, who enlist "voluntarily" because the prospect of being drafted interferes with civilian employment (Oi, 1967, p. 60), or to secure preferred assignments, or just to avoid being drafted later. Nearly 40 percent of those who volunteered in the period before the 1965 Vietnam buildup were draft motivated (Altman and Fechter, 1967, p. 23). On the other hand, some of those who would have eventually volunteered are drafted, reducing the supply of volunteers. There are two strategies for dealing with the effects of the draft. Some studies (e.g., Fisher, 1969; Grissmer, 1979; Ash, Udis, and McNown, 1983) include a variable which measures the probability of being drafted;[1] other studies (e.g., Cooper, 1977) try to determine the number of "true" volunteers during the draft period. Cooper used the enlistment behavior of those with high draft-lottery numbers, who presumably faced little threat of being drafted, to gauge the supply of true volunteers during the lottery phase of the draft.

Each of these strategies has its problems. One difficulty with using a draft pressure variable is that the probability of induction for "high-quality" individuals (the population being studied) would vary with omitted factors like selective service regulations as well as with the level of overall inductions. The alternative strategy of inferring true volunteers from enlistments by those with high lottery numbers risks some error because it requires making assumptions about who perceived themselves to be draft-safe (Cooper, 1977, p. 194). This is particularly chancy during the early history of the lottery when the eventual probabilities were not well known. Because the draft period was by design a period of low military pay and by accident a period of low unemployment, errors in holding constant the effect of the draft are likely to bias the remaining coefficients.

Controlling for the effect of the draft, however, is not enough-- supply behavior might be expected to differ in periods of war or threat of war and peace, apart from the draft per se, because of "taste" factors such as wishing to be patriotic, or liking or disliking combat. Even the direction of these taste shifts may change as the period of danger wears on. For example, there is evidence of increased voluntary enlistments during the Berlin crisis (Altman and Fechter, 1967, p. 20), but it is not clear that such positive responses continued throughout the prolonged conflict in Vietnam. Fisher (1969), Cooper (1977), Grissmer (1979), and Ash, Udis, and McNown (1983) do not attempt to distinguish observations according to degree of military involvement, leaving the draft-pressure variable and perhaps the time trend to perform this chore as well.

A third estimation issue is lags in the response of enlistments to changes in military pay and civilian alternatives. In general, previous studies have short lags in these responses (one quarter for both relative pay and unemployment in Fisher, 1969; no lags in Cooper, 1977; six quarters for unemployment but none for relative pay in Fechter, 1979; six months for relative pay and two months for unemployment in Grissmer, 1979; one quarter for relative pay and none for unemployment in Ash, Udis, and McNown, 1983). These lags are either assumed or found to fit better than no lags; longer lags seem not to have been considered. The short lags are consistent with the lag between contract signing and beginning service which is sometimes offered as a justification (e.g., Grissmer, 1979, pp. 107-108). It is difficult to say much about the lag between contract signing and beginning of active duty because "enlistment" seems to mean contract signing in some studies (e.g., DeVany and Saving, 1982) and beginning of active duty in others (Grissmer, 1979). Lagged values are also used in order to avoid reverse causation running from enlistments to the rate of unemployment or the level of civilian pay. However, both unemployment and wages are influenced by the <u>stock</u> of military-age individuals in the armed forces; a quarter's fluctuation in the flow of enlistments cannot have a very large effect on this stock (and hence on civilian wages or unemployment). Nor is it clear that a one-quarter lag would be sufficient to avoid any simultaneity that exists if errors in the enlistment equation are serially correlated (Ash, Udis, and McNown's equation for Army enlistments suggests a correlation of about .35 between errors for <u>semiannual</u> observations) or if the previously mentioned lag between a decision to join the armed forces and beginning active duty leads to labor force withdrawal during this interval. In any case, the possibility that enlistments are dependent on alternatives expected over a three-year term of enlistment, and that these expectations are based on lagged values of the unemployment rate and level of wages, suggests that the possibility of somewhat longer lags ought to be considered. Of course, the age of the enlistees (typically in their late teens) argues against a very long lag that might be appropriate in other markets.

Dale and Gilroy (1983) follow earlier papers in lagging the unemployment rate, but use the military/civilian compensation ratio with a four-month lead. The lead is justified largely on goodness- of-fit grounds. Given that military pay is usually changed only at the start of the new fiscal year, that the change is usually known in advance, and that their dependent variable is the number of contracts signed (rather than the number beginning active duty), a short lead is not implausible.[2]

A final issue which has become important since the expiration of the GI Bill for new recruits in January 1977 is the appropriate way of incorporating changes in educational benefits into one's measure of military compensation. The GI Bill was replaced by VEAP (Veterans' Educational Assistance Program) which both offered lower benefits and required that the enlistee contribute one dollar for every two dollars the government provided. This was modified in fiscal year 1979 by the introduction of Ultra-VEAP (or the "Kicker"), under which the enlistee received an additional educational benefit for entering hard-to-fill occupations, typically in the combat arms (Military Manpower Task Force, 1982, Chapter V). Unlike the GI Bill and VEAP, the Kicker is available only to Army recruits. While maximum VEAP benefits have remained fixed at $5400 for a three year enlistment, the maximum Kicker grew from $4000 to $12000. Although the matching requirement of VEAP and the limited availability of the Kicker suggest that these dollar values overstate their importance, variations in these benefits are very large relative to the limited variation in other components of military compensation in the post-draft period.

Previous work has had limited success in estimating the effect of these benefits on recruitment. Ash, Udis, and McNown's (1983) military compensation variable ignores the switch from the GI Bill to VEAP and the introduction of modest Kicker payments (their sample ends before the sharp fiscal-year 1982 increase in the Kicker). Dale and Gilroy (1983) treat the GI Bill, VEAP, and the Kicker as separate variables with some success (all are significant in their Army-recruiting equation) but several puzzling results emerge (VEAP is more effective per dollar of benefits than the less restrictive GI Bill for Army, Navy, and Marine Corps recruiting, and neither helps Air Force recruiting.)

Thus far, the analysis has concentrated on time-series estimates, which are more common in the literature than are cross-section studies. Viewed in comparison with the time-series approach, the cross-section strategy (e.g., Fisher, 1969; Jehn and Shugart, 1979; Daula, Fagan, and Smith, 1982) offers both advantages and disadvantages. The main advantage is that one can obtain reasonably-sized samples without including observations which are burdened by draft-era complications. A second feature, which may be both an advantage and a disadvantage, is that there is no[3] variation in military pay across observations. If one is hesitant to accept the relative- pay restriction in the supply function, this is a serious limitation. If one is willing to accept this assumption, at least as an approximation, the lack of pay variation can become an advantage. Without such variation, there is no need to worry about the more complicated forms which such variation can take over time, i.e., the various educational benefits mentioned above.

The major disadvantage of the cross-section approach is the danger that unmeasured taste and ability factors will be correlated with civilian compensation. To cite the simplest example, the historically greater propensity to enlist in the South may be due to lower civilian wage opportunities or to differences in "cultural" attitudes toward the military. To the extent that such attitudes differ only between the South and the North (somehow defined) a dummy variable would solve the problem. But to the extent that both earnings and attitudes differ within regions (New Hampshire and Massachusetts come to mind), the problem is not so easily solved.

Failure to control for differences in entrance test scores across areas can also bias cross-section estimates. Ideally, one might want to study the proportion of high mental ability high school graduates who choose to enlist. In practice, one knows the ratio of high mental ability graduates who enlist to total graduates (or, even worse, total population) in the enlistment-prone age group. Thus, the dependent variable will be affected by differences in the proportion of high school graduates who score well on standardized tests, as well as differences in properly defined supply behavior. One might conjecture that test scores among high school graduates would be positively related to civilian pay; on this conjecture, the estimated effect of civilian pay would be less negative than its true effect. The estimated value could even be positive, though this does not appear to happen in practice (Fisher, 1969; Jehn and Shugart, 1979; Daula, Fagan, and Smith, 1982[4]).

A related omitted-variable issue is the dispersion of earnings. As Fisher noted, this is usually taken as constant in time-series work. But the variance of earnings does differ geographically, so that its omission might be important in cross-section work.

When one considers the strengths and weaknesses of the time-series and cross-section approaches, pooling cross-section data over the all-volunteer period emerges as an alternative worth considering. The cross-sectional dimension allows us to avoid worrying about holding constant the effects of the draft and the Vietnam War, and gives a good deal of variation in unemployment rates and some variation in civilian earnings. Pooling several cross-sections allows one to introduce state-specific dummy variables to deal with state-specific differences in tastes and ability.[5] It also provides some variation in military compensation, though obviously less than would be available in longer time series.

Pooling several cross-sections and adding state-specific intercepts changes the interpretation of the coefficients from that usually offered in purely cross-sectional work. It is often argued that, if there are lags in responses to the independent variables, cross-section estimates provide estimates of long-run impacts. This is because differences across states will be primarily "permanent" rather than "transitory" differences, and responses to these permanent differences will be more or less complete. With individual-specific intercepts, variations around state means for each of the variables over the sample period is what is identifying the coefficients, and so a shorter-run response is being captured. Of course, if one believes that such lags are really important, estimating the pattern of response over time would be desirable.

II. Data

The dependent variables used in this study are ratios of the number of contracts signed by male nonprior-service Army enlistees to the enlistment-age population[6]. Contracts cross-tabulated by high school graduation and mental-test category by state for fiscal years 1976-82 were made available by the Defense Manpower Data Center. The mental-test categories have been corrected when necessary by DMDC for the norming error mentioned above. Four types of recruits were analyzed: total, high school graduates, those in the top half of the test-score distribution (mental categories (CAT) I-IIIA) and high school graduate CAT I-IIIA's. For total and high test score contracts, the dependent variable is expressed as the ratio of contracts to population 18-20 years of age; the population data are from the U.S. Census Bureau (1980, 1982, 1983). For high school graduates and high scoring graduates, the dependent variable is the ratio of contracts to the number of high school graduates in the past three years; the latter series is published by state each year by the National Center for Educational Statistics. Because the numerator of the dependent variable is the number of contracts signed in the quarter, rather than the number of enlistees beginning active duty, the lag between decision to enlist and beginning military service mentioned above is not relevant.

Military pay is most often measured by basic military compensation, BMC, which includes the value of allowances and tax advantages as well as base pay. The level of BMC for first-year enlistees was obtained from the Army Research Institute, which also provided data on benefits under the various educational benefit programs .

An attempt was made to combine basic military compensation and educational benefits into an overall compensation measure. For various reasons-- the fact that such benefits are received in the future, the possibility that the benefits will not be used at all, and the reluctance of many VEAP participants to contribute the maximum amount-- these benefits are thought to be "worth" less to the recruit than their stated value. Huck, Kusmin, and Shepard (1982) calculated a range of estimates of the value of each type of educational benefit to the recruit. Using the average of the high and low estimate, and taking account of the fact that the Kicker is available to only about half the high-scoring graduates recruited by the Army, the various educational benefits were combined into a single variable. If total compensation equals BMC plus ED(the value of the educational benefits) then

$$\ln(\text{total compensation}) = \ln(BMC) + \ln(1+ED/BMC)$$

$$= \ln(BMC) + ED/BMC$$

Since the discounted ED averaged only about 4 percent of BMC, the approximation $\ln(1+x)=x$ is quite accurate.

Military compensation consists of a large number of benefits and bonuses not included in BMC. Indeed, a recent study (Office of the Secretary of Defense, 1982) listed 60 categories of compensation apart from those included in BMC. Adding them into an explicit measure of military pay would be difficult because they are usually available to only some members of the military, and the number of recipients and amount of benefits is often not detailed by service and length of service. Many are quantitatively unimportant (Experimental and Stress Duty Pay, Leprosy Duty Pay) but others (e.g., enlistment bonsues) may be quite important.[8] These problems can be avoided if one is willing to accept the hypothesis that it the ratio of military pay to civilian pay which determines enlistments. In this case, fiscal-year dummy variables can control for variations in W_m, and the effect of both military and civilian pay determined from the coefficient of W_c.

Civilian earnings W_c are measured by the quarterly average (by state) of monthly earnings of private workers, based on Unemployment Insurance (UI) records. Almost all private employment is covered, apart from railroad workers, domestics, agricultural workers, and some employees of small nonprofit organizations. The reported earnings are total earnings, not just the portion of earnings subject to UI taxes.

Current-dollar figures such as those mentioned above require deflation for economy-wide changes in prices; the consumer price index was used for this purpose. A further deflation may be necessary to compare data from different states at one point in time[7]. The value of one dollar of military compensation is, arguably, worth the same to a prospective recruit no matter what state he is currently living in, since his ultimate assignment will bear little relationship to his current residence. However, this is not true for civilian alternatives-- a dollar earned in Massachusetts will (apart from occasional tax-evading runs to New Hampshire package stores) be spent in Massachusetts, so its real value depends on the cost of living in Massachusetts. The regional Consumer Price Indices published along with the National CPI are not the appropriate deflator, since the base year takes a value of 100 in each location. Instead, data from the Bureau of Labor Statistics (BLS) Urban Family Budget series for a low-income family of 4 (a budget of about $14000 in 1981) were used. The index created for each state reflected the BLS budget indices for urban and nonurban areas of the region to which the state belongs, weighted by the share of the state's population which resides in urban and nonurban areas.

The unemployment rates used in this study are quarterly averages of monthly unemployment rates tabulated from the Current Population Survey (CPS). These were provided by the BLS, though they emphasize that the monthly CPS tabulations are "official" unemployment rate estimates only for the 10 largest states. Even these unofficial tabulations were not available for about 20 of the smallest states prior to January 1976; the missing values were imputed from regressions (using 1976-82 data) which related the state unemployment rate to the national rate, the state and national employment-population ratio, and a time trend.

Because military compensation is not the only tool available to the Army to influence the supply of volunteers, controlling for other dimensions of the recruitment effort is desirable. The number of Army and Defense Department recruiters and local and national media advertising expenditures were available for the period 1976:4 through 1982:2. Each of these series except national media advertising vary by state as well as over time. The four variables used in the estimation were REC, the logarithm of the number of Army recruiters divided by the military-age population; DoD, defined similarly for Defense Department (i.e., all four services) recruiters; NADV and LADV, the logarithms of real national and local media advertising expenditures divided by military-age population.

One might argue that these recruitment resources are not appropriate "independent" variables, since their allocation may depend on (as well as help determine) the number of recruits. While a full similtaneous-equation model of enlistment decisions and recruitment- resource allocation is beyond the scope of this paper, a less formal inspection of the data does not suggest that this a major problem. While number of Army and DOD recruiters increased during the period studied, their allocation across regions was very nearly constant. Indeed, much of the variation in the recruiter/population ratio seems to have come from population changes rather than recruiter shifts. Local advertising expenditures showed a bit less stability across regions, but the most striking fluctuations were the quarter- to-quarter fluctuations within fiscal years. In any case, the fact that the recruitement resources are available for a shorter sample period, coupled with the possibility that they are endogenous, argues for estimating supply equations using both the 1975:4-1982:3 period omitting recruiter resources and the 1976:4-1982:3 period including them.

The desire to build a quarterly file of data by state led to the use of statewide series on civilian earnings and unemployment, rather than the youth-specific variables which appear in several other studies. Particularly for forecasting purposes, this is not a very serious loss since forecasted values of young workers' wages and unemployment rate would, apart from trend, be very closely related to all workers' wages and unemployment rate.[9]

III. Results

Before turning to the regression results, it is useful to get a feel for the extent of regional variation over time in the most important variables. Regional patterns of the unemployment rate, civilian earnings, and two measures of enlistment supply are shown in Table 1. Each variable is expressed as an index number, with the national average in each fiscal year equal to 100. Thus, in the first panel of Table 1, the unemployment rate in the Northeast was 121.7 pecent of the national unemployment rate in fiscal year 1976, and declined to 93.0 pecent of the national unemployment rate in fiscal year 1982.

The well-known increase in unemployment in the Midwest appears quite clearly in the table, with the unemployment rate there rising from 89 percent of the national average in 1976 to 114 percent in 1982. Relative unemployment is roughly constant in the South (with the index increasing by 4.5 percentage points), and decreases significantly in the Northeast and West.

Civilian earnings show less regional variation. There is a modest rise in the South, a modest decline in the West, and very gentle U and inverted-U patterns in the Northeast and Midwest.

The regional pattern of the two recruiting measures mirrors the regional unemployment variations. Both measures show a sharp drop in the Northeast, where (relative) unemployment rates were falling, and a striking increase in the Midwest, where unemployment was soaring. Indeed, by either recruiting measure <u>the Midwest replaced the South as the dominant per capita supplier of recruits to the Volunteer Army.</u> Relative recruiting rates declined in the West, along with the relative unemployment rate. The one exception to the otherwise- regular pattern is the South, where modest declines in the recruiting index accompanies modest increases in relative unemployment.

Regional variations in civilian earnings are, as noted above, more modest and it is hard to find a clear relationship between them and changes in recruiting success. Thus, in contrast to unemployment effects (where we have a natural "experiment" which provides enormous geographic differences in patterns over time), the pooled cross- sections seem (at least at this level of aggregation) to give a less promising basis for estimating relative-earnings effects.

Regression results are presented in Table 2. These regressions are based on 28 quarterly observations (1975:4 through 1982:3) for 50 states and the District of Columbia. The four dependent variables are those discussed in section II. For each dependent variable, military compensation is measured by BMC or a set of dummy variables. Both OLS and GLS estimates are presented.[10] The GLS estimates (which correct for first-order serial correlation) probably deserve greater weight, but the fact that the serial correlation coefficient must be estimated makes the issue less clear cut.[11]

The dependent variables, BMC, and W_c are all entered logarithmically, so that the coefficients of BMC and W_c are elasticities. Given the variation in unemployment rates noted above, there seemed reason to hope that the unemployment response could be estimated with considerable precision; hence a quadratic specification was adopted. The unemployment rate is in percent, and coefficients of the quadratic unemployment term have been multiplied by 100. The remaining columns of the table are the coefficient of the time trend (which runs from 1 to 28), the standard error of the equation, and the elasticity of the recruiting measure with respect to the unemployment rate at the sample-mean unemployment rate of 7 percent. Not shown in the table are dummy variables for individual states[12] and quarter of the year.

Each state's observations were weighted by the average number of persons 18-20 years old in the state.

The dependent variable in the first four equations of Table 2 is the logarithm of the number of CAT I-IIIA high school graduate contracts divided by the number of high school graduates. The estimated elasticity of supply with respect to military compensation is about 0.5 when BMC is used, but the implied elasticity is 1.0 to 1.5 when the dummy-variable approach is used. Civilian wages are negatively related to enlistments. In the equations with BMC, the restriction that supply is determined by the ratio of military to civilian pay (i.e., equal, opposite-signed coefficients of BMC and W_c) cannot be rejected.

The unemployment rate has very large effects on enlistment. At the mean unemployment rate of 7 percent, a one percent reduction in the unemployment rate reduces enlistments by about 10 percent. Given the difficulty of estimating unemployment effects with any precision with time-series data (Ash, Udis, and McNown, 1983), the significance of these estimates is worth underlining.

The most anomolous result is the very large coefficient of Ed/BMC. In theory, its coefficient should equal that of BMC; in fact it is much larger. Part of the explanation may be that the procedure used in deriving this variable over-discounts the value of the benefits, but it is hard to argue that this is a full explanation. In any case, since Ed/BMC was highest when unemployment was highest in the sample period, overestimating the effect of Ed/BMC is likely to lead to underestimating the unemployment effects.

The second set of equations focuses on the supply of CAT I-IIIA enlistments (both graduates and nongraduates). The conclusions which emerge are very similar to those reached for CAT I-IIIA graduates. Compensation elaticities tend to be a bit larger,and the unemployment elasticities are a bit smaller. The unemployment elaticities remain important in practical terms.

When supply is measured by the proportion of high school graduates who enlist (third set of equations), the results are less satisfactory. Taken at face value, the first two specifications suggest that educational benefits significantly increase recruitment, but increased BMC reduces it. The dummy variable specifications show quite clearly, however, that recruitment success is inversely related to civilian alternatives, and the estimated elasticity (-1.1 or -1.2) is fairly sizeable. The effect of military compensation is just not very reliably estimated in this set of equations.

Estimated unemployment elasticities, however, remain significant in both a statistical and practical sense. Moreover, they are not very sensitive to the choice of military-pay specification.

The difficulties of estimating the effect of compensation on the ratio of total contracts to population is very similar to that encountered in analyzing the high-school graduate measure. Neither of the estimates based on explicit measures of military compensation (first two lines of this set) are plausible. The compensation elasticity of 1.85 implied by the OLS dummy variable specification is right-signed, but GLS estimation reverses this.

Although the estimated effects of unemployment on total contracts are smaller than for the other dependent variables, they are once again statistically significant and consistent across the three specifications. Given the relatively large percentage changes in unemployment rates that have occurred in the past decade, an elasticity of 1/3 implies non-trivial percentage changes in the number of recruits.

An unavoidable question is why the compensation elasticities are more satisfactory for the first two supply measures than for the last two. While the difficulties of measuring such compensation are certainly important, the rough similarity among the alternative estimates for each of the first two measures suggests that this is an unlikely explanation for why the last two measures do not exhibit such stability. A more plausible explanation is that the number of CAT I-IIIA enlistees is supply determined, but the number of total enlistees is largely demand determined. The simplest story along these lines-- that changes in standards offset any changes in the supply of enlistees-- would predict that estimated effects would be smaller than real ones, perhaps even zero, but would not lead one to expect wrong-signed "effects". This would seem to require that the total number of recruits demanded be negatively related to the offered wage. While military manpower demands are not usually thought of as having a significant demand elasticity in the short run, such a negative correlation could arise either by chance (recall there are only seven fiscal years' of data) or because higher military compensation reduces the demand for new recruits by improving retention. If in a period of excess supply recruitment standards are raised uniformly, one might expect that those variables with significant geographic variation would determine how the demand-limited total of enlistments would be distributed across the country. Thus, demand constraints might be consistent not only with wrong-signed estimates for military pay, but right-signed estimates of the impacts of civilian pay and unemployment. While DeVany and Saving have modelled the way in which discrepancies between the supply of enlistees of the desired quality and the desired number of such enlistees is reconciled at the national level, there seems to be little available research on how demand constraints make themselves felt locally.

Table 3 presents estimates for a slightly shorter sample period (1976:4

through 1982:2), for which the additional measures of recruitment effort were available. The effects of military compensation, civilian earnings, and unemployment are broadly similar to those in Table 2. The most consistent difference is the slightly smaller unemployment effect.

For the first three measures of high-quality recruit supply the recruiter variables tell a generally sensible story. Additional Army recruiters increase enlistments, but increases in other Services' recruiters (an increase in DoD holding REC constant) reduces them. Increasing both Army and other recruiters by, for example, 10 percent would increase both REC and DoD by .10, and increase enlistments by .10 times the sum of the coefficients of REC and DoD. Since this sum is positive, proportional increases in all services' recruiters increase Army enlistments. National media advertising sometimes has a positive effect, but this is unstable across specifications. Local media advertising seems to have no detectable positive effect on recruitment. One interpretation is that such advertising is concentrated on areas where enlistments are below expectations, but the limited available evidence seems inconsistent with this explanation (Morey and McCann, 1983).

The estimates for all enlistees (last three lines of Table 3) do not show a negative effect of DOD recruiters on Army enlistments, but do show consistent positive national advertising coefficients.

IV. Conclusions

In order to achieve an adequate sample size without including draft-period observations, and to take advantage of large regional differences in the path of unemployment in recent years, the determinants of Army enlistments were estimated from quarterly data by state for fiscal years 1976-82. In sharp contrast to several previous papers, unemployment rates had quite strong effects on recruitment success. For various categories of "high quality" recruits, the elasticity of contracts signed with respect to the unemployment rate ran from .4 to .8. For high quality recruits defined as those with scores in the top half of the distribution on the military's entrance test (or those with these scores and a high school degree), estimates of the elasticity of contracts with respect to military compensation centered roughly on 1.0. For total contracts and contracts signed by high school graduates (regardless of test score) the compensation elasticity could not be estimated with any confidence. This may be because the number of such enlistments is demand constrained rather than supply determined. There was, however, consistent evidence that the number of contracts was inversely related to alternative (civilian) earnings.

These results suggest that, if unemployment rates fall, the Army will have difficulties attracting current numbers of high-quality recruits at currently offered compensation levels. There are several options for dealing with this difficulty within the all-volunteer framework. The most obvious is to increase the compensation of new enlistees. A subtler strategy is to focus on retention of those already in the Army, reducing the number of high-quality enlistees who must be recruited.[13] A third option is to expand the role of women. A final issue is whether the current number of high-quality recruits ought to be maintained, since that level was at least partly caused by the recession. This involves comparing the marginal value of relatively able individuals in military and civilian employment.

Two quite different directions for future research seem desirable, given the results of this paper. First, research on how recruiting standards respond to local or national shortages or surpluses of recruits meeting a given standard would be very useful. Second, the lag (if any) in the response of enlistments to military and civilian compensation, unemployment, etc. deserves greater attention.

FOOTNOTES

1. More precisely, Fisher used the ratio of total accessions (volunteers plus draftees) to population rather than the ratio of draftees to population. He argued that with target levels of total accessions given, draft calls depend inversely on the number of volunteers, so that the draft/population ratio used in other studies was endogenous.

2. There is a two month lag between the announcement of the survey results on private-sector pay on which federal pay increases are based and the beginning of the fiscal year. However, pay increases which military personnel actually receive are not automatically determined by the survey results. Therefore, the lag between the decision on military pay and the start of the fiscal year is less than two months.

3. Daula, Fagan, and Smith (1982), using cross-section data on individuals, allow the allowances and tax advantages of the military pay system to vary with marital status and number of dependents, but this variation must be less than that contributed by variations in civilian earnings.

4. Daula, Fagan, and Smith (1982) attempt to account for tastes and ability in a two-equation model in which civilian earnings are determined by the usual factors and the decision to enlist depends on relative pay and various taste factors. However, they assume that several of the earnings determinants do not directly affect enlistments, but operate only through their effect on civilian earnings. Thus, the decline in enlistment propensity as people age is attributed only to the effect of aging raising experience and hence earnings; health problems are constrained to increase the probability of enlisting, since they lower earnings and are not allowed to directly affect enlistments.

5. Goldberg and Greenstein (1983) pool time series and cross section data but do not adopt the dummy-variable strategy. Instead they include the proportion black and proportion urban as controls for tastes and abilities.

6. The data and data sources are described in more detail in the Appendix.

7. The three cross-section studies cited earlier apparently did not deflate their civilian-earnings measure to account for regional variations in the cost of living.

8. A more basic objection to the BMC variable is that it fails to reflect the opportunities for those who contemplate a career in the military. This objection is more forceful the greater the changes in the service-earnings profile over time. If one assumes the most important "career" indicator is the compensation received by those beginning their second term of enlistment, then the omission of such compensation is not an important problem in the period studied. The sum of regular pay and housing and subsistence allowances of those beginning a second term (grade E-4 with three years of service) was consistently 121 to 123 percent of first year pay plus allowances.

9. Apart from data availability, there are other reasons for preferring "all-worker" variables to "youth-specific" ones. First, the civilian prospects of high school graduates with above-average abilities may be captured as well by all-worker means as by means for "youth" not stratified by high school graduation or mental ability. Second, even where available, youth-specific variables are subject to greater sampling error. Finally, the all-worker variables would be even less subject to reverse causation running from enlistment to civilian wages and earnings than would the youth-specific variables.

10. The first-order serial correlation was estimated by the method

described by Nickell (1982) and Solon (1983). The equation was then quasi-first-differenced using this estimate.

11. OLS underestimates the standard errors of coefficients in the presence of serial correlation, but in these data the standard errors changed little when GLS was used. GLS also provides smaller true standard errors when the serial correlation coefficient is known, but this gain does not necessarily occur when the correlation is estimated. In unpublished Monte Carlo experiments, Gary Solon finds virtually no gain when using 50 cross-sectional units and 10 observations per unit when the true correlation is 0.4. Whether the gains are appreciable when there are more observations per unit is uncertain, particularly when the true correlation is less than 0.4.

12. To reduce computational costs the equations were actually estimated by deviating all variables from their state-specific means, which is equivalent to including state-specific dummy variables.

13. Educational benefits may be effective in encouraging new high-quality enlistments, since they are a way of paying college-eligible recruits more than others. However, because they are valuable only when one returns to civilian life, their effect on retention may be perverse. Allowing those who have earned educational benefits but remain in the military to transfer their benefits to spouses or children has been proposed as a way of blunting the disincentive to re-enlist.

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Table 1. Key Variables by Region by Fiscal Year

(Index numbers: National average in each fiscal year = 100)

Region	1976	1977	1978	1979	1980	1981	1982
Unemployment Rate							
Northeast	121.7	121.7	116.4	114.5	103.8	98.4	93.0
Midwest	88.6	85.8	88.5	92.4	111.8	113.8	114.4
South	88.5	89.8	93.4	93.6	89.8	92.3	93.0
West	112.3	114.0	109.9	106.1	97.5	96.6	100.6
Civilian Earnings							
Northeast	101.1	100.0	99.6	100.0	100.9	101.4	101.3
Midwest	105.5	106.7	107.0	106.5	104.9	104.7	104.1
South	95.8	96.0	96.4	96.8	97.7	97.8	98.0
West	98.2	97.5	96.7	96.5	96.4	96.1	96.6
Contracts/Population							
Northeast	94.2	93.6	85.3	84.5	87.8	84.8	86.9
Midwest	98.4	96.0	87.4	91.6	104.2	111.7	115.3
South	107.7	113.1	127.5	123.7	108.8	108.6	104.3
West	95.1	89.7	85.6	87.1	92.0	85.4	86.0
High Scoring Graduate Contracts/High School Graduates							
Northeast	101.2	99.5	90.7	90.4	85.2	83.0	81.1
Midwest	90.6	95.5	90.5	98.4	109.9	116.0	114.4
South	105.0	103.3	114.4	106.6	96.0	95.6	99.4
West	106.3	102.8	104.5	104.7	110.2	103.8	102.4

Table 2. Enlistment Equations

	BMC	Ed/BMC	W_c	UR	UR^2	Trend	s.e.	ρ	n_U
A	.61	10.8	-1.04	.17	-.34	.019	.2358		.86
	(2.5)	(38.2)	(3.8)	(9.7)	(3.3)	(13.3)			
	.44	9.9	-.17	.10	-.05	.028	.2057	.42	.68
	(1.5)	(30.0)	(.6)	(5.5)	(.5)	(15.2)			
	d	d	-1.00	.10	-.06	d	.2468		.65
			(3.4)	(5.1)	(.5)				
	d	d	-1.50	.09	.01	d	.2382	.11	.64
			(6.1)	(4.5)	(.1)				
B	.96	9.1	-1.52	.11	-.15	.002	.2074		.60
	(4.5)	(36.5)	(6.3)	(6.9)	(1.7)	(1.6)			
	.95	8.3	-.99	.06	.06	.009	.1827	.34	.49
	(3.8)	(30.0)	(3.9)	(3.7)	(.6)	(6.3)			
	d	d	-1.59	.07	-.02	d	.2320		.49
			(5.9)	(3.9)	(.2)				
	d	d	-.09	.06	.03	d	.2268	.07	.44
			(.4)	(3.1)	(.3)				
C	-1.28	6.9	-.55	.10	-.09	.007	.2103		.65
	(5.9)	(27.5)	(2.2)	(6.6)	(.9)	(5.3)			
	-2.06	7.3	.22	.07	.03	.011	.1874	.50	.53
	(7.1)	(23.1)	(.8)	(4.1)	(.3)	(5.9)			
	d	d	-1.14	.08	-.01	d	.2130		.55
			(4.6)	(4.7)	(.1)				
	d	d	-1.21	.08	.01	d	.2046	.26	.55
			(5.3)	(4.1)	(.1)				
D	-3.06	4.8	-.75	.02	.21	-.025	.2019		.32
	(14.6)	(19.7)	(3.2)	(1.1)	(2.4)	(20.8)			
	-4.07	5.5	-.59	.02	.22	-.025	.1808	.51	.33
	(14.4)	(17.9)	(2.2)	(.9)	(2.2)	(14.0)			
	d	d	-1.85	.05	.02	d	.2055		.36
			(7.7)	(3.0)	(.2)				
	d	d	1.00	.04	.01	d	.2196	.26	.32
			(4.1)	(2.2)	(.1)				

Table 2. (continued)

Sample: 51 states, quarterly, from 1975:4 to 1982:3 = 1428 observations

Dependent Variables:

A = High Scoring Graduate Contracts/High School Graduates
B = High Scoring Contracts/Population
C = Graduate Contracts/High School Graduates
D = Contracts/Population

Notes:

d = BMC, ED/BMC, and TREND replaced by fiscal-year dummies
t statistics are in parenthese below coefficients
Each coefficient of the quadratic unemployment term is multiplied by 100

Table 3. Enlistment Equations with Additional Recruitment Variables

	BMC	ED/BMC	W_c	UR	UR^2	REC	DoD	NADV	LADV	Trend	s.e.	ρ	n_U
A	.87	13.6	-1.41	.12	-.16	.84	-.30	-.07	-.05	.010	.1987		.67
	(3.7)	(32.0)	(4.6)	(6.9)	(1.6)	(8.7)	(3.7)	(4.0)	(3.1)	(6.1)			
	1.16	4.9	-.87	.08	-.02	.57	-.14	-.07	-.01	.032	.1825	.34	.55
	(4.1)	(5.0)	(2.5)	(4.3)	(.2)	(5.2)	(1.7)	(4.2)	(.5)	(9.9)			
	d	d	.21	.10	-.05	.45	-.29	.19	-.03	d	.2269		.65
			(.6)	(4.9)	(.4)	(3.7)	(3.2)	(9.8)	(1.5)				
	d	d	-.69	.07	.03	.32	-.12	-.04	-.01	d	.1745	.15	.49
			(2.3)	(3.8)	(.3)	(3.1)	(1.5)	(2.4)	(.9)				
B	.89	9.4	-1.07	.07	-.03	.78	-.19	.08	-.07	-.007	.1575		.43
	(4.7)	(27.9)	(4.4)	(4.8)	(.4)	(10.2)	(3.0)	(5.4)	(5.1)	(5.4)			
	.79	6.9	-1.16	.05	.03	.64	-.09	.08	-.04	-.003	.1531	.29	.39
	(3.4)	(7.7)	(4.1)	(3.3)	(.3)	(7.1)	(1.2)	(5.9)	(3.3)	(1.0)			
	d	d	-.04	.07	-.02	.53	-.15	.26	-.06	d	.1942		.48
			(.1)	(4.1)	(.2)	(5.0)	(1.9)	(15.7)	(3.2)				
	d	d	-.99	.04	.06	.42	-.01	.05	-.05	d	.1535	.10	.34
			(3.9)	(2.7)	(.8)	(4.7)	(.2)	(3.6)	(3.5)				
C	-1.43	10.5	-1.34	.08	.02	.42	-.15	-.03	-.02	-.006	.1938		.57
	(6.2)	(25.3)	(4.5)	(4.7)	(.2)	(4.5)	(2.0)	(1.4)	(1.0)	(3.7)			
	-1.52	4.2	-.24	.05	.08	.27	-.05	-.01	.03	.016	.1681	.48	.43
	(5.3)	(5.9)	(.7)	(2.8)	(.7)	(2.6)	(.6)	(.9)	(2.1)	(4.9)			
	d	d	-.34	.08	.00	.31	-.21	.18	-.02	d	.2066		.58
			(1.1)	(4.4)	(.0)	(2.8)	(2.5)	(10.0)	(1.1)				
	d	d	-.28	.05	.05	.28	-.09	.01	.01	d	.1622	.34	.43
			(1.0)	(3.1)	(.5)	(2.7)	(1.1)	(.6)	(.7)				
D	-4.04	6.3	-1.17	.02	.18	.18	.02	.16	-.04	-.044	.1642		.30
	(20.5)	(17.8)	(4.7)	(1.2)	(2.2)	(2.2)	(.3)	(10.5)	(2.6)	(32.7)			
	-4.58	5.6	-.95	.02	.15	.09	.04	.14	-.01	-.042	.1497	.49	.30
	(18.0)	(8.9)	(3.3)	(1.4)	(1.6)	(.9)	(.5)	(11.3)	(1.1)	(14.6)			
	d	d	-1.01	.06	-.01	.31	-.01	.27	-.07	d	.18		
			(3.8)	(3.5)	(.1)	(3.2)	(.1)	(17.5)	(4.2)				
	d	d	-1.34	.03	.05	.25	.05	.11	-.05	d	.15		
			(5.0)	(2.2)	(.6)	(2.6)	(.7)	(7.3)	(3.5)				

Sample: 51 states, quarterly, from 1976:4 to 1982:2 = 1173 observations

Dependent Variables and Notes: Same as those in Table 2.

DATA APPENDIX

(1) Contracts - The number of contracts signed by Army recruits each month from October 1975 to September 1982 were provided by Philip Knorr of the Defense Manpower Data Center. The number of contracts was crosstabulated by state, race, sex, prior service, non-prior service, whether high school graduate, and mental category. To maintain comparability with previous research, non-prior service males were studied in this paper.

(2) Population - Data on resident population by state by age is available from the U.S. Census Bureau for 1970 and 1980 based on Census counts, and for intercensal years the Census counts are updated to take into account the estimated effect of births, deaths, and net migration. Data for April 1980 and July 1981-82 were taken from U.S. Census Bureau (1983, Tables 1-3), and for April 1970 and July 1971-78 from U.S. Census Bureau (1980, Tables 2-3). Unpublished intercensal estimates for 1979 and 1980 were provided by Ed Byerly of the Census Bureau.

1980 Census population totals exceeded those predicted by the intercensal updating of 1970 Census population by about 2 percent (U.S. Census Bureau, 1982, Table A-1.) Thus, the official national estimates of population by age for 1971-79 have updated the earlier estimates by assuming that this error in the intercensal estimates occurred gradually between the Censuses. The analogous procedure was applied to the intercensal estimates by state for 1971-79. As a check, these updated intercensal estimates were summed across states and compared with the official national totals (U.S. Census Bureau, 1982, Table 1). Monthly population estimates were then interpolated from the resulting annual estimates. Values for August-September 1982 are extrapolations based on the 1981 and 1982 values.

The estimates of population by state for non-Census years are made in fairly broad age groups, with boundaries at ages 5, 14, 18, 21, 45 and 65.

(3) High School Graduates - Number of graduates (of both private and public high schools) by state by year are published by the National Center for Educational Statistics. Tom Schneider kindly provided 1980-81 data prior to publication, and 1981-82 values were extrapolated from 1979-80 and 1980-81 values. Since contracts can be signed by those still in school for active duty to begin after graduation, the new graduates were assumed to be available for signing contracts in the second quarter (i.e., April-June) of their graduation year. No allowance was made for mid-year graduation.

(4) Unemployment Rates - Unemployment rates from the Current Population Survey were provided on tape by the Local Area Unemployment Statistics Program of the Bureau of Labor Statistics. For the 10 largest states, these are official monthly unemployment rates. For the remaining states they are not, and BLS is concerned that they be taken as CPS tabulations rather than official BLS estimates. The official monthly estimates combine CPS tabulations with other data, primarily establishment survey employment and unemployment of those covered by unemployment insurance (Norwood, 1977). However, reliable official estimates are not available prior to 1976, except for the 10

largest states. For the period 1976-82 when both series are available for all states, the unweighted correlation of quarterly averages was .85. (A weighted correlation would be higher, since the two series are identical for the 10 largest states.) The official rate had a larger standard deviation (3.3 vs 2.9), which is surprising since the introduction of non-CPS information in the official-rate calculation is intended to reduce sampling variation.

These tabulations were available for all states for 1976 and later years, but for only 29 in 1974-75, 28 in 1973, and 16 in 1972. The missing-data states were the smallest ones, weighted least heavily in the weighted regressions. To provide estimates for these missing data, equations were estimated separately for each state from 1976-82. The logarithm of the unemployment rate was a function of the logarithm of the national unemployment rate, the difference between the logarithm of the state and national employment/population ratio, and a time trend and month (seasonal) dummies. (The employment data were from the BLS series on nonagricultural employment (establishment survey) and the population data were the series on population age 18 and older described in section (2).) The estimated coefficients were then used to "backcast" the missing unemployment rates.

To check the accuracy of these imputations, the same method was applied to states which did have CPS unemployment rates for 1972-75, and the fit between these values and the predicted values was evaluated. For the 1972-75 period, the actual mean unemployment rate in the states in question was 6.27, and the mean of the predicted values was 6.11, and the correlation between the two was .77. Results for 1975 alone were similar.

(5) Civilian Earnings - Quarterly earnings of private workers covered by state unemployment insurance laws by state by two-digit industry were provided on tape by Bernard Bell of the Bureau of Labor Statistics. In 1981, 90 percent of all civilian employment was covered by the UI and UCFE programs, the principal exclusions being railroad workers, most domestic workers, agricultural workers, and some employees of small nonprofit organizations (U.S. Bureau of Labor Statistics, 1982a, p. 33). The state by two-digit industry tape deleted "cells" with less than three reporting establishments, leading to very minor discrepancies from published totals, judging from a comparison with published 1980-81 annual averages (U.S. Bureau of Labor Statistics, 1982b, Table 4.)

(6) Nonagricultural Employment - Monthly total nonagricultural employment by state were taken from the B.L.S. Establishment Survey file.

(7) Cost of Living - Indices of the cost of living relative to the national average were taken from the B.L.S. Urban Family Budget series, which provide estimates of the cost of obtaining an appropriate market basket of goods in different areas over time. The market basket varies geographically, reflecting differences such as climate-related allowances for utilities and clothing, but does not change over time. The lowest of the three budgets for a family of four, representing expenditures of about $14000 in 1981, was used.

Indices were published for 25 identified metropolitan areas throughout the period studied and for the nonmetropolitan parts of the four Census regions. To convert these to state indices, an unweighted average of the identified metropolitan indices was calculated for each region, and the state index was equal to the weighted average of metropolitan and nonmetropolitan indices for the state's region, with the weights based on share of its 1980 population in metropolitan areas. The latter was taken from U.S. Census Bureau (1981, Table 3). Honolulu and Anchorage were assigned to Hawaii and Alaska directly, rather than to the West region.

The Urban Family Budget program was discontinued after 1981. Indices for 1982 were calculated by using changes in the Consumer Price Index by region and size of place.

The indices were originally based on data for "autumn" of each year. We associated autumn with the beginning of the fourth quarter of the calender year, and interpolated other quarters.

(8) Military Pay and Benefits - Curtis Gilroy provided the four quarterly compensation series used in Dale and Gilroy (1983): first-year regular military compensation, maximum monthly benefit for a GI Bill beneficiary, maximum benefit for a Veterans Educational Assistance Program (VEAP) beneficiary, and maximum value of "kicker" payments offered to Army enlistees entering critical specialties.

These educational benefits are likely to be worth less than their stated dollar value to potential recruits, particularly the post-GI-Bill program. A VEAP participant must contribute to his VEAP account (the government matches that contribution on a 2-for-1 basis), and most participants do not make the maximum contribution. The benefits might never be used, if the individual decides against going to college after the Army or decides to stay in the Army. Finally, if they are received, the benefits must be discounted. The average of the high and low estimates of discounted valuations from Huck, Kusmin and Shephard (1982, Table 4) were used to convert educational benefits to current dollars, recognizing that these benefits are earned by three years of service. To value the GI Bill, VEAP, and the Kicker, the values for "Non Contributory Plan," "Contributory VEAP," and "Contributory VEAP with supplemental payment" minus "Contributory VEAP" were used.

(9) Recruiters and Advertising Expenditures-- A file containing the number of Army recruiters on production, the ratio of Army to Defense Department recruiters, and local and national media advertising expenditures was provided by Marvin Trautwein of the U.S. Army Recruiting Command. Quarterly data were available for fiscal years 1977-82. Number of Army recruiters was available quarterly for fiscal 1976, and annually for 1974-75.

Except for national media advertising, the data were compiled by recruiting districts which are aggregates of counties and often cross state lines. Data by district were converted to data by state using number of persons 18-20 years old in each county from the 1980 Census.

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