# The Effect of Transfer Income on Labor Force Participation and Enrollment in Federal Benefits Programs: Evidence from the Veterans Disability Compensation Program*

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## Abstract

We analyze the behavioral responses of near-elderly males to unconditional grants of substantial cash disability benefits from the Veterans Administration's Disability Compensation (DC) program. The largely unstudied DC program provides income and health insurance to approximately three million veterans of military service who have service-connected disabilities. In contrast to other federal disability programs, receipt of DC benefits depends only on a veteran's diagnoses and is not contingent upon labor force status; thus, analysis of the behavioral responses to the DC program may inform economic understanding of the incentive and non-incentive effects of receipt of transfer income. We analyze these behavioral effects on the labor force participation of veterans, as well as their spillovers to claims on other federal benefits programs. We study a unique policy change, the 2001 Agent Orange decision, which expanded eligibility for DC benefits to a broader set of covered conditions—in particular, type II diabetes—to Vietnam veterans who had served in-theater (with 'Boots on the Ground' or BOG). Notably, the Agent Orange policy excluded Vietnam era veterans who did not serve in-theatre ('Not on Ground' or NOG), thus allowing us to assess the causal affects of DC edibility by contrasting the outcomes of BOG and NOG veterans.

We find clear evidence that the increase in DC enrollment caused by the 2001 policy change had a significant negative effect on the labor supply of Vietnam veterans who had Boots on the Ground. Almost thirty percent of individuals who became eligible for the DC program dropped out of the labor force. The policy change also had an effect on enrollment in benefits programs administered by the Social Security Administration. Most notably, it raised SSDI enrollment among veterans who became newly eligible for DC benefits by a full percentage point. We also find suggestive evidence of spillovers from DC to both SSI and OASI benefits. Our ongoing work analyzes in detail the causal channels through which these labor supply and programmatic responses operate.

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## I. Introduction

A large body of research investigates the effect of the Old Age, Survivors, and Disability Insurance (OASDI) program on the labor supply of near elderly individuals. There are two significant challenges for this work. First, OASDI is a federal program with the same benefit formula for all U.S. residents covered by the program. Thus any differences in benefits across individuals are typically driven either by prior earnings or family structure, which may themselves exert an indirect effect on labor supply. Second, OASDI can affect individuals' labor supply decisions both through its effect on its incentives and through its effect on unearned income—more formally, though an incentive-response substitution effect or a non-incentive income effect. Determining whether incentive effects or income effects are responsible for any labor supply impacts of OASDI is crucial for assessing the efficiency consequences of the OASDI program. If OASDI primarily reduces labor supply by penalizing labor force participation beyond the Substantial Gainful Activity level, this reflects an efficiency-reducing distortion of incentives. If, by contrast, OASDI primarily reduces labor force participation by providing beneficiaries with sufficient financial and healthcare security that they prefer not to work, this reflects a non-distortionary behavioral response to provision of transfer benefits.

The distinction between these two sources of behavioral responses to OASDI benefits, substitution versus income effects, is critical for program design. Prior efforts by Congress and the Social Security Administration to increase labor force participation among SSDI recipients—most notably, the Ticket to Work program—have targeted the substitution effect by reducing the work disincentives built into the structure of SSDI benefits. The efficacy of such policies turns on the assumption that, were it not for the implicit tax that SSDI levies on labor supply, many beneficiaries would prefer to work (i.e., while keeping their benefits). If, however, the primary

means by which SSDI reduces labor force participation is through an income effect, such efforts may be close to ineffectual. The reason is that income effects induce behavioral change by increasing household resources rather than by altering incentives for labor force nonparticipation—thus, they are non-incentive effects. To date, there is no research of which we are aware that empirically distinguishes income from substitution effects in the causal relationship between receipt of disability benefits and labor supply. A likely reason is that since its inception, the DI program has provided benefits exclusively on a work-contingent basis. Thus, income and substitution effects cannot readily be separately identified.

The current paper complements and extends the existing body of research on incentive effects in disability benefits program by studying a federal disability program that has previously been ignored by economists, the Veterans Administration's Disability Compensation (DC) program, and considering how behavioral responses to this program may inform our understanding of incentive and non-incentive responses to receipt of transfer income. The VA's Disability Compensation program currently provides benefits and health insurance to approximately 3 million veterans of military service. In July of 2001, the program was expanded to cover a broader set of conditions for Vietnam veterans who had served in the Vietnam theater (with 'boots on the ground') than for all other veterans. This policy change allows us to use other veterans as a comparison group when estimating the effect of this program. Additionally, in contrast to Social Security Disability Insurance benefits, the DC program should not by design affect labor supply incentives for most recipients—though we discuss important caveats to this generalization below.

These features of the DC program allow us to estimate the effect of unearned income caused by the policy-induced increase in DC enrollment on the labor supply of the near-elderly veterans

who were affected by the policy. This research can shed light on the channels through which social security affects the labor supply decisions of individuals who are approaching their retirement. In our second project, we examine the extent to which the plausibly exogenous increase in DC enrollment affected enrollment in the SSDI, SSI, and OASI programs. There are a number of possible channels through which such an effect could occur. For example, if DC enrollment causes some individuals to leave the labor force, then this lowers their cost of applying for SSDI benefits. Similarly, as a veteran's DC benefits increase, he may become ineligible for the means-tested SSI program.

In the pages that follow, we provide a brief overview of the DC program and the recent policy change that expanded the medical eligibility criteria for certain Vietnam era veterans. We then describe the administrative data that we use to estimate the effect of this policy. For this project, we obtained data from several different sources, including the U.S. Army's Office of Economic and Manpower Analysis, the Department of Veterans Affairs, and the Social Security Administration. We then use these data to provide a richer characterization of the dynamics of benefits receipt for DC claimants. We next outline our strategy for estimating the effect of the DC program on labor supply and the receipt of SSDI, SSI, and OASI benefits, and then present results for all of these outcome variables of interest. We conclude with the implications of this research as well as outlining the next steps for our research as we acquire additional data in the months ahead.

## II. Background on Federal Disability Programs

The number of individuals receiving benefits from the federal government's two largest disability programs has grown significantly in recent years. Most notably, the fraction of adults between the ages of 25 and 64 receiving benefits from the Social Security Disability Insurance

(SSDI) program approximately doubled (from 2.2 percent to 4.3 percent) from 1983 to 2006. Similarly, the fraction of adults in that age range receiving benefits from the means-tested Supplemental Security Income (SSI) program increased by more than 70 percent, from 1.4 to 2.4 percent, during the same period. Recent enrollment trends for both programs suggest that the pace of these increases will continue in the near future, though in the discussion that follows we focus on the SSDI program given its larger size and greater labor market impacts.

Enrollment in the SSDI program increased rapidly during this period for several reasons. First, a liberalization of the medical eligibility criteria for both programs in the mid-1980s made it easier for individuals to qualify for benefits for conditions that are self-reported or difficult to verify, such as mental disorders and back pain. These individuals tend also to have lower mortality rates than did SSDI recipients with other conditions, which serves to increase the average duration on the program. Second, the well-documented decline in earnings for low-skilled workers led to an increase in the effective replacement rate, which is the ratio of potential benefits to potential earnings. Third, the increase in female labor force attachment made it possible for more women to qualify for the SSDI program, which for most applicants requires work in at least five of the ten most recent years before application and at least ten years of earnings during one's lifetime.[1] Fourth, changes in the demographics of the population driven by the aging of the baby boom population has served to increase enrollment rates, as the fraction of individuals receiving both SSDI and SSI benefits rises with age. Finally, the liberalization of the programs has made them significantly more sensitive to economic conditions, so that a given negative shock to the economy induces more applications and eventually more enrollments.[2]

[1] This has actually served to reduce SSI enrollment, as individuals who would previously have qualified for this means-tested program now have SSDI benefits that are sufficiently high to make them ineligible for SSI. This partially explains why SSDI has grown more rapidly than SSI.
[2] These factors are discussed in detail in Autor and Duggan 2003 and 2006, and Duggan and Imberman 2006.

An important consequence of the substantial rise in SSDI enrollment has been a decrease in labor force participation, with this effect concentrated among low-skilled workers. While there is some disagreement about the exact magnitude of the effect, a consensus has emerged that a sizeable fraction of SSDI recipients would be working in the absence of the program. This has important implications for the OASDI trust fund, both because it reduces the amount of tax revenue coming in to the system and increases the amount of benefits paid out. It also suggests that a large fraction of individuals retire sooner than they otherwise would.

One challenge for reliably estimating the effect of the SSDI program on labor supply, as noted above, is that it is a federal program with essentially the same benefit formula and medical eligibility criteria nationwide. It is therefore difficult to construct a comparison group that would allow a researcher to disentangle the effect of the program's benefit levels and screening stringency from other factors that would also influence disability enrollment. Because of this, recent research as well as the most influential paper in this literature used rejected applicants as a comparison group (Bound 1989; von Wachter et al, 2008). The key assumption is that labor supply would be no greater among accepted applicants given that they are presumably in worse health than rejected applicants, though this assumption has been criticized by Parsons (1991).

Even if one knew the exact labor supply effects of the SSDI program, however, it would be unclear whether these effects are driven by the income effect of cash benefits or the incentive effects that the program creates. Most reforms of the SSDI program have assumed it is the latter mechanism that leads to such large labor supply effects and low return-to-work rates. For example, the Ticket-to-Work program, which provided an array of incentives for SSDI recipients to return to work, had virtually no detectable impact on labor supply despite almost 11 million

"tickets" issued.[3] It therefore seems plausible that the income itself, rather than the program's incentives, may be responsible for much of the program's labor supply effect. Determining the magnitude of the income and incentive effect would be helpful to policymakers in reforming the program.

One program that has been virtually ignored in previous research but provides an opportunity to shed light on these issues is the U.S. Department of Veterans Affairs' Disability Compensation program. This program is the federal government's third largest disability program, and pays benefits to military veterans with disabilities or injuries that were caused or aggravated during their military service. In contrast to both SSDI and SSI, the DC program does not explicitly reduce recipients' incentives to work. It therefore presents an opportunity to estimate the income effect alone of disability benefits—though, as we detail below, subtle incentives in the DC program and the program's interaction with other federal benefits programs suggest caution is warranted in concluding that the DC program generates exclusively income and not incentive effects.

In making comparisons between the DC and SSDI programs, one must first acknowledge some fundamental differences between the programs. Perhaps most notably, only military veterans are eligible for DC benefits. It would therefore be important to account for these differences when considering the generalizability of any behavioral estimates for the DC program to the SSDI program. This would be especially relevant for women, virtually none of whom are eligible for DC benefits. However, given that more than 40 percent of men born between 1935 and 1950 are veterans, the DC program does offer a unique venue for studying the effect of unconditional cash transfers on the labor supply of men in their fifties and sixties.

[3] As of October 6, 2008, 10.8 million tickets had been issued, 16.6 thousand had been assigned to Employment Networks, and a mere 1,174 had resulted in an Employment Network receiving an award for achieving an employment milestone (http://www.ssa.gov/work/tickettracker.html, accessed 11/2/2008).

Another important difference between the two programs, discussed in greater detail below, is that while SSDI pays all-or-nothing benefits, DC benefits are an increasing function of the severity of a beneficiary's "combined disability rating" (CDR).

A key impetus for our study of the DC program is a major legislative change that took effect in 2001, which generated a plausibly exogenous policy increase in the generosity of disability benefits for one group of potential beneficiaries but not another.[4] More specifically, in July of 2001, the VA expanded the medical eligibility criteria for Vietnam veterans who served in the Vietnam theater (Vietnam, Cambodia, or Laos) during the 1964 to 1975 period to include diabetes as a covered condition. This change was motivated by an Institute of Medicine study that linked exposure to Agent Orange and other herbicides used by the U.S. military during the Vietnam War, to the onset of Type II diabetes, detailed below. Adopting the terminology used by the military, we distinguish among 'boots on the ground' (BOG) Vietnam era veterans—the veterans affected by this policy—with 'not on ground' (NOG) veterans, who were not directly affected.

The 2001 policy change coincided with a sharp acceleration in the number of recipients of DC benefits as shown in Figure 1. From 1997 to 2001 the number of DC beneficiaries grew by just 0.6 percent per year but this growth rate accelerated to 3.5 percent annually during the next seven years. Total benefits paid in the 2008 fiscal year were $31 billion. The DC enrollment growth was especially rapid among Vietnam era veterans, with the fraction receiving DC benefits increasing from 9.4 to 13.0 percent from 2001 to 2007. Estimates that use peacetime era veterans as a comparison group suggest that the fraction receiving benefits increasing by 2.7 percentage points as a result of the policy. However, this estimated enrollment impact groups

[4] See Gruber (2000) for an analysis of a reform to the federal government's disability program in all parts of Canada except for Quebec that is estimated to have reduced labor supply.

together both Vietnam era veterans who served in the Vietnam theater with their counterparts who did not. Approximately half of Vietnam era veterans did not actually serve in the Vietnam theater during the War, and thus would not be directly affected by the policy. Thus the actual increase in disability enrollment among "boots on the ground" Vietnam veterans could be approximately twice as large as that suggested by this simple comparison of Vietnam era veterans with veterans of other service eras.

This policy change provides a unique opportunity to estimate the effect of a change in program benefits on the labor supply of near elderly men. At the time of this policy change, the vast majority of Vietnam era veterans were between the ages of 47 and 61, and thus approaching the most common retirement age. Whether affected veterans responded to this change by moving up their date of retirement (extensive margin) or reducing their labor supply (intensive margin) is the subject of the current study.

Similar research has recently been conducted for the Social Security retirement benefit reductions caused by the 1983 Amendments (Mastrobuoni, 2008). The results from this research suggest that the increase in the full retirement age, and the associated reduction in retirement benefit generosity, has led many near elderly individuals to delay their date of retirement. More specifically, each two month increase in the full retirement age has on average been associated with a one month increase in the average retirement age.

Interpretation of this behavioral effect is, however, complicated by the complexity of the policy change underlying it. The 1983 Amendments not only reduced the generosity of Social Security benefits but also changed the incentives for retirees to delay claiming by affecting the actuarial adjustment beyond the age of 62. It also may have affected norms by informing people that their "full retirement age" was higher than previously. The effect of the policy also seems to

vary across individuals, with a sizeable fraction responding to the less generous benefits by applying for SSDI. And finally, because the reduction in benefit generosity is a function of year-of-birth, it is difficult to separately identify the effects of the policy from cohort effects.

In the analyses that follow, we utilize administrative data for a sample of more than 4 million U.S. Army veterans linked to longitudinal Social Security earnings data to estimate the effect of this increase in Disability Compensation benefits on the DC enrollment and labor supply of Vietnam era veterans, and to study the spillovers this has on other federal benefits programs, in particular SSDI, SSI, and OASI.

## III. Data and Analysis Sample

Prior to discussing the Disability Compensation program in detail and considering its relationship to other federal benefits programs, we describe our data sources and analysis sample. The subsequent discussion of the DC program draws extensively on the analysis sample.

### *A. The OEMA, NCHS, VA, and SSA Data Sets*

We use four sources of administrative data that to estimate the effect of the Disability Compensation program on outcomes of interest. The first data set was obtained from the U.S. Army's Office of Economic and Manpower Analysis (OEMA) and contains detailed demographic and service information for a sample of more than 4.1 million veterans, virtually all of whom left the Army between the years of 1969 and 1985.[5]

Approximately 33.3 percent of the 3.897 million veterans in the sample with a non-missing "loss year" were determined by OEMA to have served in Vietnam during the conflict there. Not surprisingly, the distribution of this variable is much more concentrated around the 1969 to 1972

[5] Approximately 4.95 percent of the sample is missing the loss year. An additional 0.90 have a loss year between 1986 and 1991 with 0.01 percent having a loss year between 1960 and 1967 and 0.61 percent a loss year of 1968.

period for veterans who had "boots on the ground" (BOG) in Vietnam, as shown in Table 1. It is worth noting that, given this distribution of the loss year, the sample will tend not to include veterans who served in the earlier part of the Vietnam Era, which according to the U.S. Department of Veterans Affairs stretched from August 5, 1964 to May 7, 1975.[6]

This OEMA data set was first linked using veterans' social security numbers to the National Center for Health Statistics Master Death File, which includes the year of death for any individuals in the sample who died in 2006 or earlier. According to this data, approximately 12.0 percent of the 4.1 million individuals in the sample were deceased by late 2006.

This data set was then linked using veterans' social security numbers to administrative data that were constructed by the VA. This third data set includes detailed information about veterans' enrollment in and benefits received from VA programs such as Disability Compensation in September of each year from 1998 to 2006. An examination of this data reveals that in 1998, the fraction of BOG veterans receiving DC benefits was 17.7 percent versus just 6.8 percent for NOG veterans. That there is a disparity is not surprising, as one would expect higher rates of service-connected disability among those who had served in the Vietnam conflict. Similarly, the average age is much greater among the BOG sample (55 versus 47 in 2000) than among their NOG counterparts. Given that rates of disability enrollment tend to increase with age, one would expect a higher rate of DC enrollment in the BOG for this reason as well.

The overall trends in DC enrollment were quite similar for the two groups from 1998 to 2000, just prior to the policy change described above. Specifically, DC enrollment increased by 0.7 percentage points among BOG veterans versus 0.3 percentage points among their NOG counterparts during this two-year period. During the subsequent six years, however, DC

[6] If a veteran served for even one month during the Vietnam Era, he would typically be classified as a Vietnam Era veteran. Thus for example someone who served from the beginning of 1961 until the end of 1964 would be classified as a Vietnam Era veteran as would an individual who began service in early 1975.

enrollment rose by 8.1 percentage points in the BOG sample versus just 1.4 percentage points in the NOG. We examine this differential increase in DC enrollment and benefits received more formally in the sections that follow.

Finally, the OEMA sample was linked to a data set constructed by the Social Security Administration that included information on earnings, OASDI benefits, and SSI benefits in each year from 1976 to 2005. For this final linkage, the SSA required a match not only on the social security number but also on the last name and date of birth of each individual. A successful match on all three variables occurred for 57.5 percent of the 4.1 million veterans in the full sample.

Confidentiality rules then required us to group individuals into cells of 5 to 9 observations so that for example no individual-level earnings could be observed. In doing this, we grouped together individuals with similar background characteristics, such as gender, year of birth, race, and year of entering the military. The construction of our cells, each of which included between 5 and 9 individuals, is described in the Appendix.

The key variables in the SSA data provided to us are summary statistics on the mean and median labor earnings, social security benefits, and SSI benefits within each cell. Thus, for example, we have data on average earnings and on the number with zero earnings in each cell in every year between 1976 and 2005.

### *B. Construction of the Analysis Sample*

To investigate the effect of the DC program on the labor supply and receipt of SSA benefits by Army veterans, we begin by constructing samples of BOG and NOG veterans that are comparable to one another. As demonstrated in Table 1 above, there are important differences between the full BOG and NOG samples. Most notably, individuals in the NOG are on average

substantially younger than those in the BOG, with average years-of-birth of 1953.1 and 1945.6, respectively. Similarly, individuals in the BOG tended to enter the military much earlier than their counterparts in the NOG, with more than 35 percent of the NOG entering the Army in 1976 or later and thus technically not Vietnam era veterans. These differences are summarized in the distributions of the year-of-birth and the start year among veterans in both the BOG and NOG samples in Tables 2 and 3.

As noted above, the SSA was unable to match all three of the required variables for 42.5 percent of the individuals in our full OEMA sample. An examination of our data reveals that the overall verification rate of 82 percent in the BOG sample is much higher than the corresponding rate of 43 percent in the NOG. Additionally, the probability of a match varies considerably across time. Detailed analysis by OEMA reveals that most of the non-match is due to missing or incomplete last names in the OEMA data, and the severity of this problem differs by year. The source of the poor quality of name information can be traced to the fact that the OEMA data were generated by the Defense Manpower Data Center (DMDC) by scanning and processing the original paper documents using optical character recognition software. This produced particularly poor results with names.

Appendix Table 1 shows the variation in the verification rate across start years, and lists these rates separately for the BOG and NOG samples. As the table shows, the verification rate for the BOG sample is substantially greater than in the NOG in every year. The verification rate in the NOG is especially low at less than 15 percent from 1970 to 1974, and we therefore exclude these start years from our analysis sample by focusing on individuals with start years of 1969 or earlier. We further exclude individuals with start years of 1965 or earlier because they are extremely rate in the data. We thus consider individuals with start years from 1966 to 1969.

Within this group, we restrict attention to individuals with years of birth between 1942 and 1951 inclusive, with these individuals accounting for more than 95 percent of those matched to SSA data with a start year from 1966 to 1969.

One possible concern with our analysis sample is that the individuals who verify in the SSA data may differ systematically from those who do not. This is easy to see for the start year restriction, which will lead us to omit veterans who joined the Army either early or late in the Vietnam era. To the extent that the effect of the expansion in the DC program's medical eligibility criteria varies by entry year, our estimates will not capture the effect for all veterans. A related concern is that the individuals in our BOG and NOG samples whom we are unable to link to SSA data may differ from those who do not. We discuss this issue in more detail in the next section.

*C. Characteristics of the BOG and NOG Samples*

The first two columns of Table 3 present summary statistics for the individuals in the BOG and NOG samples with start years between 1966 and 1969 inclusive, and birth years between 1942 and 1951 inclusive, who were successfully verified in the SSA data. As the table indicates, the two groups are similar in many ways. For example, the fraction of individuals who had died by December of 1997 was equal to 6.3 percent for both samples.[7] Similarly, the fraction of individuals in the BOG who are black at 10.3 percent is just slightly higher than the corresponding fraction in the NOG at 8.8 percent. Additionally, both the average age and the average AFQT score of the two groups are quite similar. Perhaps most importantly, the two groups are fairly similar in terms of their labor force participation and their receipt of social security benefits just prior to the policy change. For example while 21.0 percent of the BOG

[7] With few exceptions, veterans who died during their military service are not included in the OEMA data. This omission reflects the sample design and is not a feature of our data restrictions.

sample had zero earnings in 2000, the corresponding share for those in the NOG sample was 22.4 percent.

There are several important differences between the two groups, as shown in the same table. A much larger fraction of the NOG sample did not have a high school degree when first enrolled in the Army (though this may partly reflect our missing education data for a large fraction of the NOG sample). Additionally, the fraction of individuals in the BOG receiving DC benefits in both 1998 and 2000 was substantially greater than in the NOG just prior to the policy change. Finally, average earnings in the NOG sample in the year prior to the policy change were almost 22 percent higher than in the BOG. Of course, these differences may not reflect deficiencies in the sample. The higher rate of DC receipt and lower labor force earnings among BOG veterans may reflect causal effects of combat-service on subsequent outcomes.

Despite these differences, an examination of the trends in the outcome variables of interest just prior to the 2001 policy change suggests that they were very similar for the two groups. The fraction with zero earnings increased by similar amounts for both samples as did the fraction receiving SSDI benefits. This is especially important, as the key assumption of our identification strategy, which we describe in more detail below, is that the post policy trends in labor supply, SSDI receipt, and other outcome variables of interest would have been the same for the two groups had the policy change not occurred.

One concern with the analysis sample as it is currently constructed is that individuals who verify in the SSA data may systematically differ from those who do not. To address this issue, in the next two columns of the table we provide summary statistics for the BOG and NOG samples that result if we do not require SSA verification. In many cases, the differences between the two samples become much smaller with this change, and this is most notable for the educational

distribution. Additionally, for both samples the fraction of individuals receiving DC benefits is substantially smaller when we consider this broader set of individuals. And while the AFQT score does not differ much between the verified and unverified BOG samples, the same can not be said for the NOG samples. It therefore appears that the requirement that individuals verify in the SSA data affects the composition of our samples.

Partly because of this issue, OEMA is currently working the Transunion Corporation, a credit bureau, to supplement these incomplete names. When complete, OEMA has arranged with SSA to perform a revised match once this supplementation is complete. This should substantially increase our verification rate, though in the analyses that follow we focus exclusively on the verified sample.

*D. Comparison of OEMA data with Census data*

As a further benchmark for the reliability of the sample, we compare the OEMA data with similarly drawn group of males from the 2000 IPUMS Census file. Using the 5 percent Census IPUMS extract, we draw a group of all males born between 1942 and 1952, and further limit the sample to (self-reported) Vietnam-era veterans for most analyses. To facilitate direct comparisons of earnings between the Census and OEMA data, we create 'pseudo' cells in the Census data of size 5 to 9 observations, grouping on year of birth, education and race, as was done with the OEMA cells.

Table 4 provides a side-by-side comparison of the Census and OEMA samples, focusing on age, race, completed schooling, and annual earnings. Panel A summarizes these variables for the OEMA sample, while panel B summarizes the Census sample. The first column of the Census panel provides statistics for the full sample of males from the 1942 through 1952 birth cohorts. Subsequent columns are limited to Vietnam era veterans. To account for the fact that the age

distribution of the OEMA sample is not representative of all Vietnam era veterans due to the issues with data matching discussed earlier, we additionally reweight the Census sample in the final three columns to match the year-of-birth by race distribution of the OEMA sample.

The comparisons in Table 4 table provide several noteworthy results. First, the samples appear closely comparable along the dimensions of race and labor force participation rates (overall and by race). One important difference, however, is that the education distribution in the OEMA sample indicates considerably lower educational attainment than the Census sample. This pattern is expected, however, since the OEMA data reflect education at the time of military enlistment (at an average age of 20), whereas the Census data measure educational attainment in late adulthood. A second notable pattern is that we find closely comparable earnings levels among veterans in the Census data (using annual labor earnings in 1999) and in the OEMA data (using SSA labor earnings from 1999). One exception, however, is that annual earnings of black veterans are about 10 to 15 percentage points higher in the Census data than the OEMA data, while labor force participation of black veterans is higher in the OEMA than Census data. We do not at present have a preferred explanation for this discrepancy. Overall, this cursory comparison of OEMA and Census data provides some assurance that the OEMA sample is roughly representative of the target population of Vietnam era veterans. We expect that the comparability of the samples will improve further when pending SSA data match issues are resolved.

## IV. The Veterans Disability Compensation Program: Eligibility, Application and Benefits

In this section, we detail the structure of the Veterans Disability Compensation program, and drawing upon on the analysis sample described above to illustrate key points.

### *A. The application process*

The Veterans Disability Compensation program (DC) provides cash benefits and prioritized

access to Veterans Administration health facilities to disabled veterans whose conditions are plausibly caused or aggravated by their military service. To apply for DC benefits, a veteran submits an application to one of 63 regional offices of the Veterans Benefit Administration (VBA). The 'authorization unit' collects necessary information regarding the claimant's application, including military service records and medical records from both VA medical facilities and private providers. The application is then forwarded to a Rating Board, which determines for each disability claimed whether the disability is service connected and, if so, what disability rating is applicable according to the Schedule for Rating Disabilities. Unlike other federal disability programs that classify disability as an all-or-nothing state, the DC program classifies disability on a numerical scale. This scale ranges from 0 to 100 percent in 10 percent increments, depending on the type and severity of the disability, with more severe conditions receiving a higher rating. [8] If the recipient receives ratings for multiple disabilities, the recipient's Combined Disability Rating (CDR) is an increasing, concave function of the individual ratings (concavity prevents the combined rating from exceeding 100 percent).[9]

In a typical year, more than 70 percent of those applying for DC seek benefits for more than one medical condition. Veterans applying for benefits face one of three possible outcomes: outright rejection, an award for some but not all conditions, or an award for all conditions. During the 2000 fiscal year, 14 percent of applicants received awards for all conditions claimed, 48 percent received awards for some conditions, and 38 percent were rejected entirely (VBA

[8] The range of possible ratings differs among disabilities. For example, type II diabetes can have a rating of 10, 20, 40, 60, or 100 percent. Arthritis can be be assigned a rating of 10 or 20 percent. For a list of conditions and ratings see http://www.warms.vba.va.gov/bookc.html. A disability with a zero percent rating would not increase the monthly cash benefit but would entitle the veteran to priority for health care treatment through the Veterans Health Administration.

[9] If a claimant has multiple disabilities, only the claimant's 'residual ability' is considered when determining the effect of each additional disability on the CDR. For example, if a veteran has two disabilities rated at 50%, his CDR would be he equal to the sum of 50% for the first disability and 50% of his residual capacity of 50% for the second disability, all rounded to the nearest increment of 10%. Thus, two disabilities rated at 50% results in a CDR of [0.5 + (1 – 0.5) * 0.5] = 0.75, which is then rounded up to 0.80.

Annual Report, 2001).[10] In 2006, current DC beneficiaries averaged 2.97 disabilities per recipient, with the highest number of disabilities per capita among Gulf War and Vietnam Era veterans, and the lowest number among WWII veterans.

*B. Benefits determination*

Monthly benefits awarded by DC are a steeply increasing function of the veteran's CDR, as shown in Figure 2. In 2007, a 10 percent award provided a monthly payment of $117 whereas a 100 percent award provided a monthly payment of $2,527. Veterans receiving a CDR of 30 or higher and who have spouses, dependent children, or surviving parents also receive modest additional benefits.[11] In addition, the VBA also considers employment capability for veterans with severe disabilities. Veterans who have a single disability rated at 60 percent or above or a Combined Disability Rating of at least 70 percent and one disability rated at 40 percent or more can receive the Individual Unemployability (IU) designation if the VBA determines that they unable to "to secure and follow a substantially gainful occupation by reason of service-connected disability." Veterans found to be unemployable are provided cash payments at the 100 percent CDR level.

Table 5 summarizes DC cash benefits paid in fiscal year 2006. The first three columns list the count of recipients, the total dollars paid, and the average monthly benefit in each CDR category at the end of fiscal year 2006. The average annual payment to the 2.73 million DC recipients in this year was $9,400 per capita, totaling approximately $25.6 billion for the year. Veterans with ratings between 0 and 20 percent accounted for 44 percent of recipients but just 8 percent of dollars paid. Those with ratings at 70 percent or above comprised 21 percent of the

[10] These decisions are frequently appealed. Existing DC recipients can also apply for an increase in their benefit amount, either because of an increase in the severity of a rated condition or because a new health problem arises.
[11] The stated policy of the VBA that the DC benefits schedule reflects the average reduction in earnings capacity for each value of the CDR. Since benefits determination depends only on CDR and family status, it is clear that the benefit payment will exceed the earnings loss for some veterans and fail to meet the earnings loss of others.

population and received 62 percent of the benefits payments.[12]

There is considerable variation across service eras in the distribution of the combined disability rating, as shown in the right hand panel of Table 5.[13] Among Vietnam era DC recipients, 32 percent have CDRs of 70 percent or more. The corresponding share for DC recipients serving in the Gulf War is just 13 percent. Consistent with these differences in disability rating, average annual benefits differ widely by service era, from a low of $6,988 for Gulf War veterans to a high of $12,049 for veterans serving in Vietnam. DC recipients from peacetime, World War II, and the Korean War eras have average monthly benefits of $7,721, $8,831, and $9,473, respectively (VBA Annual Benefits Report, 2006).

In considering the generosity of the DC cash transfers, several features of the DC program deserve particular note. First, DC benefits are not subject to federal income tax; hence a dollar in DC income is roughly equivalent to $1.30 to $1.50 in pre-tax earned income, depending upon the Veteran's marginal tax rate. Second, like OASDI benefits, DC benefits are adjusted annually according to the Consumer Price Index. Hence, their real value is not eroded over time. Third, DC benefit awards are not generally offset by other federal transfer benefits; for example, a Veteran may receive both DC and SSDI payments without any reduction in benefits from either program, though this would not be true for SSI. Fourth, once awarded, DC benefits are rarely retracted, and hence are roughly akin to permanent indexed income. Finally, a veteran's eligibility for DC benefits is determined only by medical criteria, conditional on service-connectedness, and is therefore not work-contingent or income-contingent. Thus, a Veteran with

[12] The average monthly benefit amounts for those with ratings between 0 and 20 percent are very close to the baseline amounts because veterans with these ratings are not eligible for dependent benefits. The average amounts paid for those rated 60 percent and higher are substantially greater than the baseline amounts because many of these recipients are eligible for the 100 percent payment amount because they are receiving the Individual Unemployability benefit.

[13] DC recipients are assigned to eras based on where their most significant disability occurred. Thus a person serving in Korea and in peacetime whose disability was incurred during peacetime would be categorized as peacetime for the DC program but as a Korean War veteran in the population data.

a 100 percent CDR may participate in the labor market to any degree feasible.[14] In brief, DC provides untaxed, permanent, indexed income (and health care) to veterans while placing no restrictions on labor force participation or asset accumulation.

*C. The 2001 Agent Orange Decision, Type II Diabetes and 'Service-Connectedness'*

The requirement that a disability must be "a result of disease or injury incurred or aggravated during active military service" to be compensable generally means that it is easier for a veteran to obtain disability compensation for a tangible injury that occurs during service than for an ailment that typically develops later in life, such as cancer or heart disease. In 2006, for example, the five most prevalent service-connected disabilities were defective hearing, tinnitus, general musculoskeletal disorders, arthritis due to trauma, and scars (VBA Annual Report, 2006). Nevertheless, disabilities that typically develop post-service are not uncommon. For example, post-traumatic stress disorder and hypertensive vascular disease (high blood pressure) were the sixth and ninth most prevalent service-connected disabilities in 2006.

This requirement of a service-connectedness would generally seem to exclude type II diabetes as a compensable disability. Indeed a 2000 report by the National Academy of Sciences' Institute of Medicine argued that the most important determinants of diabetes were physical inactivity, family history, and obesity (IOM, 2000). Despite this, approximately 1.6 percent of DC recipients had diabetes as one of their covered conditions in September of 2000. While this number is non-trivial, diabetes was not among the twenty most common conditions among DC recipients at that time, nor was it one of the ten most common conditions for DC recipients from

[14] Veterans whose DC award is increased by the Individual Unemployability designation in theory lose their IU rating if their annual labor market earnings (measured by SSA earnings data) exceed a threshold amount. In 2004 and 2005, this threshold was $6,000 (GAO, 2006).

any of the five major service eras. [15]

This situation changed rapidly when the Secretary of the Veterans Affairs announced in November of 2000 that, due to Veterans' potential exposure to Agent Orange while serving in Vietnam, type II diabetes would be compensable under the DC program effective July of 2001. Critically, this policy change stipulated that diabetes would be "presumptively" service-connected among those veterans who served in Vietnam, meaning that a veteran developed type II diabetes and was covered under the policy would not have to prove service-connectedness.

The Agent Orange decision had been many years in the making. Agent Orange was one of fifteen herbicides used by the U.S. military to defoliate jungle areas that might otherwise provide cover to opposing forces. Estimates suggest that from 1962 to 1971, more than 19 million gallons of herbicides were sprayed in all four military zones of Vietnam, with the affected area equal to 8.5 percent of the country's total land area. Although the use of Agent Orange did not begin until 1965, it represented more than 80 percent of all herbicides sprayed in Vietnam (U.S. Department of Veterans Affairs, 2003).

Soon after the war ended, many Vietnam veterans voiced concerns about the possible long-term effects of exposure to the dioxins in Agent Orange and other herbicides used in Vietnam. Responding to these concerns, the VA established the Agent Orange Registry in 1978, which provided voluntary medical examinations to veterans who served in Vietnam between 1962 and 1975. Thirteen years later, the Agent Orange Act of 1991 was enacted, which charged the National Academy of Sciences' Institute of Medicine with conducting an independent review of the existing scientific literature regarding the possibility of a link between Agent Orange exposure and the prevalence of certain medical conditions.

[15] The 2006 VBA annual report only lists the top 20 conditions overall and the top four conditions within each major diagnostic category. The condition ranked number 20 overall had a 2.7 percent share.

In a series of five reports released between 1994 and 2003, the Institute of Medicine (IOM) grouped forty different medical conditions into one of four categories: (1) sufficient evidence of an association between Agent Orange and the condition; (2) limited or suggestive evidence of an association; (3) inadequate or insufficient evidence; and (4) limited or suggestive evidence of no association. Interestingly, none of the four categories required causal evidence—merely a statistical association. In the first three reports, diabetes was placed in the third category, with the IOM concluding that there was insufficient evidence to establish an association between dioxin exposure and the onset of diabetes.

But soon after the third IOM report was released in 1999, two new studies were released that provided supporting evidence of an association between dioxin exposure and diabetes (Calvert et. al., 1999; Air Force Health Study, 2000). In October of 2000, in response to a special request by the VA, the IOM evaluated the new studies in the context of previous research and concluded that there was suggestive evidence of an association between Agent Orange exposure and the onset of diabetes (IOM, 2000). This conclusion moved diabetes from category three above to category two ('limited or suggestive evidence of an association') and spurred the November 2000 decision by the Secretary of Veterans Affairs decision to make type II diabetes both compensable and presumptively service-connected.[16] Growth in total enrollment increased dramatically thereafter.

Of central importance for our analysis is that the November 2000 decision applied to veterans who served in the Vietnam theater (with 'boots on the ground') but not to other Vietnam-era

[16] No such presumption was made for veterans who served during the Vietnam era but not did not serve Vietnam, with the exception of veterans who served in Korea in either 1968 or 1969. This group was added because Agent Orange and similar herbicides were used by the U.S. military in Korea during this two-year period (VBA Annual Report, 2005).

veterans—that is, those who were not in-theatre.[17] This contrast between 'boots on ground' (BOG) and 'not on ground' (NOG) veterans underlies our empirical strategy, as we discuss in greater detail below. Although it is undoubtedly the case that BOG and NOG veterans differ along many dimensions other than simply their access to DC benefits, the point of leverage offered by the Agent Orange policy is that, almost three decades after the end of the Vietnam war and without a precipitating change in health, veterans who served with boots on the ground were unexpectedly granted presumptive eligibility for generous Disability Compensation benefits. Veterans of the same service era who did not serve with boots on the ground were not granted similar eligibility.

## V. Incentives under the DC Program, and its Potential Spillovers to other Federal Benefits Programs

The structure of benefits and incentives under the DC program is distinct from other federal disability benefits programs. An understanding of these incentives is relevant for assessing how veterans might be expected to respond to the Agent Orange policy, and how these responses might interact with other federal benefits programs.

### *A. Comparing benefits and incentives under DC and SSDI*

The main differences between the DC and SSDI programs can be grouped into four categories: definition of disability, benefits determination, replacement rates, and work incentives.

Distinct from SSDI and SSI, the DC disability definition involves eleven gradations of disability, from zero to 100 percent in units of 10 percentage points, where a rating of zero entitles veterans to prioritized access to VA healthcare. Because DC benefits determinations are

[17] Veterans who served in Korea in 1968 or 1969 were also covered by the policy. Agent Orange and similar herbicides were used by the U.S. military in Korea during this two-year period (VA, 2005).

not all-or-nothing, the initial medical hurdle to qualifying for a DC award is likely to be significantly lower than for SSI or SSDI. However, the graduated scale of DC disability ratings means that veterans may repeatedly reapply to increase their CDRs as their health conditions evolve. Veterans' CDRs—and therefore their benefits—tend to rise relatively steeply in the several years following the initial award.[18]

This pattern may be seen in panels A and B of Table 6, which summarize the evolution of CDRs and monthly benefit amounts by year of veterans receiving a DC award in years 1999 forward.[19] After initial DC enrollment, veterans' CDRs and benefit levels rise steeply. For example, veterans enrolling in DC in 1999 received an average CDR of 41 and a mean monthly benefit of $693 (in 1999 dollars). By 2006, the surviving veterans of this enrollment cohort had a mean CDR of 59 and mean monthly benefits of $1,277, which is almost a doubling of monthly cash benefits. This pattern is not unique to cohorts entering in the late 1990s. Veterans who received a first DC award in 2002 experienced an increase in their average CDR from 38 to 53 by 2005, and their mean monthly benefits rose from $619 to $1,084.

It seems likely that once a first DC award is obtained, veterans recognize that may further raise their benefits by applying for additional service-connected disabilities or by seeking higher ratings for service-connected disabilities that have become more severe. Thus, policies that induce veterans to obtain an initial DC award, even at a low CDR, may lead to substantially larger claims over the longer term.

A second key distinction between DC and SSDI is in determination of benefit levels. Whereas SSDI benefits are a progressive function of prior covered labor earnings, DC benefits

[18] In fact, we observe very few reductions in CDRs in our data, and it is possible that those few that exist reflect coding errors. Veterans face little risk of having their CDRs reduced after the initial award.

[19] Although our data code DC receipt in each year from 1998 through 2005, we can only determine what year DC was awarded if a veteran is observed *not* receiving DC in a prior year. We can thus identify DC enrollment cohorts from 1999 forward, but not for 1998.

depend only upon a veteran's CDR and his family status, that is, spouse, number of dependent children and surviving parents. Potential cash transfers under DC are sizable. In 2006, DC recipients received an overall average of $9,400 in cash transfers per capita; veterans with a CDR of 70 or above (21 percent of the DC population) received mean annual transfers of between $22,326 and $33,821.

Because these benefits are not subject to federal income tax, they should be adjusted upward for direct comparison with other forms of income. Assuming, for example, that veterans face an average federal tax rate of 30 percent on labor income (income plus payroll taxes minus deductions), an untaxed DC cash benefit equaling the average payment of $33,821 to DC enrollees with a 100 percent CDR in 2006, would be equivalent to $48,316 in pre-tax dollars.[20] This number is substantial relative to average earnings of Vietnam-era veterans. Table 7 shows that mean annual earnings in 2005 of veterans participating in the labor force were approximately $55 and $47 thousand for NOG and BOG veterans respectively. For nonwhite veterans, labor force earnings were substantially lower, approximately $35.5 thousand for both BOG and NOG veterans. Accordingly, the effective 'replacement rate' of DC benefits for labor income could readily exceed 100 percent for veterans with modest earnings and high disability ratings.

Consideration of replacement rates under DC highlights another central difference between DC and other disability insurance programs (including SSI, SSDI and most private disability insurance policies): receipt of Veterans Disability Compensation benefits depends only on a veteran's diagnoses and is not contingent upon labor force status.[21] Consequently, DC benefits may be used to replace labor income or to supplement it. Thus, to a first approximation, the DC

[20] Accounting for the in-kind value of prioritized access to VHA healthcare benefits would raise this estimate further.
[21] Veterans with significant disabilities may of course be unable to work.

program does not provide direct incentives for recipients to curtail labor force participation. Consistent with this observation, a substantial fraction of veterans receiving DC participate in the labor force. Panel A of Table 8 shows that in 2005, approximately 75 percent of veterans with a CDR of 10 or 20 had positive labor earnings. Even among veterans with high disability ratings, participation rates were significant: 20 to 28 percent of veterans with a CDR of 80 or 90 had positive labor earnings in 2005; similarly, 15 percent of Veterans in 2005 with a CDR of 100 percent had positive labor earnings.

This feature of DC—that incentives for labor force non-participation are notionally absent—stands in contrast to SSDI, which is believed to affect labor supply through a combination of two channels: an income effect, stemming from the pure effect of receipt of transfer income; and a substitution effect, owing to the fact that workers must not be working to qualify for an SSDI award, and then subsequently risk forfeiting their SSDI benefits if their labor income exceeds the Substantial Gainful Activity level.

*B. Non-work incentives in the DC program*

Though DC disability ratings depend on medical criteria exclusively, it is possible that non-work incentives implicitly enter the DC system through one or more channels. One potential channel is that veterans may believe that their disabilities will receive higher ratings—yielding larger cash payments—if they report themselves unable to work when applying to obtain or increase benefits. As far as we ascertain from discussions with VHA personnel, inability to work should not directly affect medical evaluation and rating of service-connected disabilities (i.e., the CDR). We therefore suspect this issue is minor.

A far more important channel by which the DC program may implicitly induce non-work incentives is through the Individual Unemployability (IU) benefit. This benefit awards 100

percent disability compensation to veterans of any age with a CDR of 60 or above who the VA determines are unable to work because of service-connected disabilities.[22] The IU benefit is of substantial monetary value. A 2006 General Accounting Office report found that the discounted present value of receiving an IU award in 2005 (on top of existing DC benefits) was approximately $300 to $460 thousand for veterans age 20, and was $89 to $142 thousand for veterans age 75 (GAO, 2006).

The availability of this benefit appears likely to induce at least some subset of work-capable veterans to curtail labor force participation to qualify for the benefit and, moreover, keep labor market earnings at a low level once the benefit is awarded so as not to lose eligibility. While the fact that only veterans with severe disabilities (a CDR of 60 or higher) are eligible for the IU benefit might be expected to deter malingering, this need not be the case. As shown in Table 8, veterans' CDRs tend to rise steeply in the years following DC enrollment. Recognizing this fact, veterans with even relatively modest disabilities and low CDRs may engage in strategic behavior to increase the odds of ultimately receiving a IU designation. Such behavior might include exiting employment or choosing not to seek employment following job loss.[23]

How significant is this issue in practice? Unfortunately, we are not aware of any rigorous analysis of this question, nor does the design of the DC program provide a ready means to answer it. The GAO has, however, repeatedly found that VA does not stringently or consistently screen UI claims (GAO, 1987, 2006). For example, the GAO found in 2006 (GAO, 2006, p. 2):

> "VA's criteria, guidance, and procedures for awarding IU benefits do not ensure that its IU decisions are well supported. VA regulations and guidelines lack key criteria and

[22] More precisely, eligibility for the IU benefit requires a single disability rated at 60 or above, or a combined rating of 70 or above for multiple disabilities, with at least one disability rated at 40 or higher

[23] Veterans who enrolled in DC under the Agent Orange policy and received only a diagnosis of type II diabetes would receive a CDR of 20, which is very far from the threshold required to be considered for the IU benefit. Nevertheless, these veterans might anticipate qualifying for the IU benefit at a later date, and so modify their behavior accordingly.

> guidance that are needed to determine unemployability. VA guidelines also do not give rating specialists the procedures to obtain the employment history and vocational assessments needed to support IU decisions. As a result, some VA staff told us that IU benefits have been granted to some veterans with employment potential."

This suggests that the IU benefit may invite abuse by veterans who are potentially employable but view receipt of total disability compensation as more attractive than labor force participation.

Our efforts to explore the importance of this concern with VA personnel have, unfortunately, yielded very little information to date, likely owing to the sensitivity of the topic. It is clear from our data, however, that the incidence of IU awards is high. Panel B of Table 8 shows that in 2005, 55 to 75 percent of veterans with a CDR of between 70 and 90 received the IU benefit, as did an additional 15 to 25 percent of veterans with a CDR of 60.[24] The dynamic nature of the qualification process for the IU benefit is evident in panel D of (the prior) Table 6. Among veterans enrolling in DC in 1999, 14 percent qualified for either the IU benefit or 100 percent disability, either of which entitles them to maximum benefits. Seven years later, in 2006, 46 percent of the 1999 DC enrollment cohort was either unemployable or 100 percent disabled.[25] Similarly, 13 percent of veterans qualified for maximum benefits at the time of DC enrollment in DC in 2002. By 2006, 33 percent of this cohort was receiving maximum benefits. Notably, this rise of 21 percentage points over 4 years is similar to the 24 percentage point rise experienced by the 1999 cohort during its first 4 years of DC receipt (from 14.1 to 37.7 percent). It is therefore likely that the share of 2002 entrants receiving maximum benefits continued rising after 2005, likely by another 10 to 15 percentage over two years.

Because veterans receiving the IU benefit should not in theory participate in the labor force,

[24] The IU designation is not applicable to veterans with a CDR of 100 percent since they already receive maximal benefits. In our data, a small percentage of veterans with CDRs below 60 percent appear to receive the IU benefit. This is likely due to data inaccuracies.

[25] We do not distinguish between the IU benefit and 100 percent disability since many DC recipients with 100 percent disability may have previously qualified for the IU benefit with a lower CDR.

it is of interest to examine the labor force participation rate of DC recipient with high CDRs who do not have an IU designation.[26] Panel C of Table 8 shows that labor force participation of this group is non-trivial. In 2005, 50 to 60 percent of non-IU-qualified DC recipients with CDRs of 60 through 80 had positive labor earnings. Remarkably, 35 to 40 percent of veterans with a CDR of 90 (but no IU benefit) also worked in 2005. Thus, a substantial fraction of DC recipients who have significant to severe disabilities participate in the labor force. What is unknown, of course, is what share of beneficiaries designated as unemployable would participate in the labor force were it not for the additional cash benefits and adverse work incentives that accompany this designation.

A final channel by which non-work incentives may enter the DC program is through the DC program's interactions with other federal benefits programs, SSDI in particular. Although the DC and SSDI programs use different screening criteria, it seems likely that the medical information generated by the DC award may alert some veterans that they suffer from impairments that could merit an SSDI award. Moreover, receipt of DC benefits may render the SSDI application process less financially onerous. Because SSDI applicants must remain out of the labor force while seeking benefits, and moreover must wait two years for Medicare coverage under SSDI, the expected financial hardship of applying for benefits is likely to deter many work-capable individuals from seeking an SSDI award.[27] Receipt of DC is likely to blunt this deterrent effect since it DC enrollees will receive cash benefits and VA healthcare during (and after) the SSDI application. Notably, the attractiveness of SSDI benefits is probably not diminished by receipt of

[26] In our data, about 5 to 8 percent of veterans in each year with an IU designation have positive labor force earnings. These veterans may be working below the VA's substantial gainful activity (SGA) level or may have worked part of the year prior to receiving the IU benefit. It is also likely that some veterans work in excess of the SGA. Historically, the VA has done a poor job of monitoring these violations of policy (GAO, 2006).

[27] Gruber and Kubik (2002) find that workers who would be likely to lose access to health insurance during the two year Medicare waiting period following an SSDI award are substantially deterred from applying for SSDI benefits. By implication, when veterans obtain health insurance coverage through the DC program, this deterrent effect may disappear.

DC. As noted above, cash benefits from these programs are additive rather than offsetting. And moreover, veterans may find the combination of VA and Medicare benefits more attractive than either program alone since these health benefits differ in the ailments covered, rapidity of access to treatment, size of co-pays, and coverage of prescription drugs. It is therefore plausible that for a subset of veterans, receipt of DC meaningfully increases the odds of applying for SSDI.

Notably, if DC awards do spur SSDI applications, this will likely reduce labor force participation of veterans receiving DC both during the SSDI application and after an award is made. This reduction would reflect a substitution effect stemming from the interaction between DC and SSDI. Thus, the complementarity between receipt of DC and application for SSDI effectively imparts some of the incentive effects in the SSDI program onto the DC program.

*C. Spillovers from DC to other federal programs*

While the availability of SSDI benefits may implicitly cause 'incentive spillovers' onto the DC program, programmatic spillovers in the opposite direction are likely more significant. These spillovers may operate through three channels. A first is the potential complementary between DC and SSDI receipt noted above. If present, this complementary implies that exogenous increases in DC enrollment—stemming, for example, from the Agent Orange decision—will spur additional SSDI applications and awards.[28]

A second programmatic interaction is early retirement. If the Agent Orange decision leads to a rise in labor force exit and early retirement among Vietnam era veterans, this would likely increase their rate of early claiming of OASDI retirement benefits.

Finally, VDC benefits expansions may serve to reduce enrollment in and expenditures on the means-tested SSI program. In 2006, a veteran with a 100 percent combined disability rating

[28] Logically, a rise in disability due to military activity will spur growth in both VDC and SSDI rolls. Our hypothesis is that, in addition to the direct effect of wartime injuries on SSDI rolls, VA policies that affect VDC awards will lead to further spillovers onto SSDI.

(CDR) would receive $2,393 monthly in untaxed VDC income, and additional supplements for dependent children or a spouse. Similarly, a VDC recipient with a 50 percent CDR would receive $690 per month, which is slightly higher than the maximum federal SSI benefit of $603 in 2006. DC benefits at or above this level would typically disqualify a veteran from receiving means-tested SSI benefits.[29] Even those with lower CDRs who qualify for SSI might see a reduction in their SSI benefit following a VDC award.

## VI. Empirical Framework

We now outline our strategy for estimating the effect of the policy-induced increase in Disability Compensation enrollment on labor supply and on the receipt of Social Security and Supplemental Security Income benefits. Figure 3 displays the difference between the fraction of the BOG and NOG samples receiving DC benefits in each year from 1998 to 2006. As this figure makes clear, there was a rapid increase in DC enrollment for the BOG sample following the DC policy change and an examination of the data in Table 9 reveals that there was little corresponding change for the NOG. More specifically, from 2000 to 2006, the fraction of the BOG sample receiving DC benefits increased from 16.8 to 25.9 percent versus an increase from just 7.4 to 9.1 percent for the NOG sample.

An examination of the benefit data in Table 9 also reveals that average monthly benefits increased significantly more rapidly among BOG DC recipients than among their NOG counterparts. This is at least partly because many in the BOG sample who were already receiving DC benefits at the time of the policy change qualified for an increase in their benefits because they had diabetes.

[29] "Generally, ineligibility for SSI occurs when countable income equals the federal benefit standard plus the amount of state supplementation, if any" (2004 Overview of Entitlement Programs). It is also worth noting that VDC recipients frequently experience an increase in their CDR, which could also reduce their SSI benefit.

As discussed above, the most important direct effect of the 2001 policy change was to add diabetes to the set of conditions for which service-connectedness would be presumed for BOG veterans. Data from the National Health Interview Survey indicate that the fraction of individuals with diabetes varies substantially by race, with rates among blacks substantially higher than among whites. Consistent with this, an examination of the trends in DC enrollment in Table 9 reveals striking differences by race, with DC enrollment increasing by 70 percent more among blacks from 2000 to 2006 than among whites during the same period.

To the extent that the policy-induced increase in DC enrollment affects labor supply or enrollment in other government programs, one would expect to detect a differential change for the BOG relative to the NOG following the 2001 policy change. We investigate this issue by initially estimating difference-in-differences models of the following type:

$$(1) \qquad Y_{jt} = \sum_{t=1998}^{2006}\left(a_t \times J_t\right) + \gamma \times BOG_j + \sum_{t=1999}^{2006}\left(\beta_t \times BOG_j \times J_t\right) + \sum_{t=1998}^{2006}\left(\mu_t \times X_{jt} \times J_t\right) + \varepsilon_{jt}$$

In this specification, the variable $Y_{jt}$ represents an outcome variables of interest, such as one of the measures of labor supply described above, for person *j* in year *t*, while $BOG_j$ is an indicator variable that is set equal to one if individual j is in the BOG sample and is otherwise equal to zero. The term $J_t$ is a vector of nine indicator variables, with $J_t$ equal to one in year *t* and otherwise equal to zero. We also interact these year indicators with the person's year-of-birth. This accounts for the possibility that macroeconomic conditions, for example, may induce differential labor force exits depending on a person's year of birth.

The parameters of particular interest in this equation are included in the vector $\beta_t$. The elements of this vector capture the differential change in *Y* relative to the base year (1998) for individuals in the BOG sample relative to their counterparts in the NOG sample. The key

identifying assumption of our approach is that the change in *Y* in recent years would have been the same for the BOG sample as it was for the NOG in the absence of the 2001 policy change. If this assumption is correct, then the parameters $\beta_t$ capture the policy-induced change in the outcome variable Y.

To the extent that differential trends in *Y* between the BOG and NOG groups are present even prior to the policy change, this would suggest that the identifying assumptions are suspect. We therefore estimate a companion set of models of the following type to determine whether, conditional on any preexisting trend, there was a trend shift in the outcome variable in the BOG relative to NOG sample following the policy change:

$$Y_{jt} = \sum_{t=1998}^{2006} (a_t \times J_t) + \gamma \times BOG_j + \sum_{t=1999}^{2006} (\beta_0 \times BOG_j \times (t - 1998)) + \sum_{t=2002}^{2006} (\beta_1 \times BOG_j \times (t - 2001)) + \sum_{t=1998}^{2006} (\mu_t \times X_{jt} \times J_t) + \varepsilon_{jt} \quad (2)$$

In this equation, $\beta_0$ captures the pre-existing trend in the BOG relative to the NOG just prior to the policy change while $\beta_1$ estimates any change in the BOG relative to NOG trend following the policy. Here we take 2002 to be the first post-policy year, though the results reported below are similar if we use 2001 instead. We also estimate separate versions of both equations (1) and (2) by race given the much larger increases in policy-induced DC enrollment among blacks described above.

## VII. Main Results

### *A. DC Enrollment in the BOG and NOG Samples*

The first column of Table 10 reports the results from a specification analogous to (2) in which we consider DC enrollment as our outcome variable and include all individuals in our sample. In this specification, we use data from 1998 through 2006 inclusive and include nine

person-year observations for each individual in the verified sample described above. The one exception to this occurs when we exclude a person-year observation if the person died in the current year or in any previous year. All specifications include interactions between a person's year of birth and the calendar year to account for the fact that macroeconomic conditions and other factors may not have the same effect for all ages. We cluster our standard errors at the person level to account for interdependence in the error term within a person across years.

The estimate of .0848 for $\gamma$ in this first column reveals that DC enrollment was approximately 8.5 percentage points higher among individuals in the BOG relative to their counterparts in the NOG prior to the policy change. This is approximately equal to the (unconditional) baseline differences in 1998 reported in Table 3. But the significantly positive estimate of .0042 for $\beta_0$ suggests this disparity was increasing by approximately 0.42 percentage points per year even prior to the policy change. However, the statistically significant estimate of .0096 for $\beta_1$ reveals that this difference in trends becomes much greater following the policy change. According to this estimate, DC enrollment increased by 1.38 percentage points more per year for the BOG sample than for the NOG sample after 2001, versus 0.42 percentage points more per year beforehand.

The next column of the same table reports the results from specification (1) above, in which we once again include the full sample and consider DC enrollment as our outcome variable. This equation allows us to observe on a year-by-year basis how DC enrollment evolved from 1998 through 2006 for the two groups. An examination of the values for $\beta_t$ reveals that DC enrollment started to accelerate somewhat between September of 2000 and September of 2001, which is not surprising given that the policy took effect in July of 2001. Enrollment then increased much more rapidly in the BOG than in the NOG in each subsequent year. These coefficient estimates

are depicted in Figure 3. For the values in this figure, we add the estimates of $\beta_t$ to the estimate for $\gamma$ to capture the average difference in DC enrollment between the two samples in each year.

The next two columns of Table 10 report the results from an analogous set of specifications for black veterans. An examination of the results reported there indicates that the effect of the policy change was much greater for blacks than for whites, which one would expect given the higher rates of diabetes among blacks. The final two columns report the results for whites only. The estimates for this group are quite similar to those for the full sample, which is not surprising given that 90 percent of the full sample is white. The coefficient estimates for $\beta_t$ for both blacks and whites, depicted in Figure 4, demonstrate that the increase in DC enrollment was much more pronounced among blacks.

To the extent that the policy-induced increase in DC enrollment affects labor supply or other outcome variables of interest, one would expect to observe differential changes for the BOG sample relative to the NOG sample beginning in 2001 or 2002.[30] We evaluate this hypothesis for labor supply in the next section.

*B. The Effect of DC Enrollment on Labor Supply*

The SSA administrative earnings data described above can be used to assess the effect of the DC program on labor supply. Recall that in our data, we do not observe individual-level earnings but instead earnings at the cell level, with each cell including between five and nine individuals. Cells are constructed separately for the BOG and NOG samples so that individuals within each cell would be as similar as possible on background characteristics such as year-of-birth, year of entry into the Army, and so forth.

[30] If one is considering an outcome variable such as having non-zero earnings during the year, one would not expect an effect in 2001 given that the policy took effect midway through that same year.

One of our earnings statistics is the number of veterans with zero earnings in the cell in the calendar year. We divide this by the number of living individuals in the cell to calculate the fraction of individuals in each cell who were out of the labor force in each year. We then estimate models analogous to (1) and (2) above, in which we explore whether the fraction of BOG and NOG individuals with zero earnings changed differentially following the 2001 policy change. In all models, we cluster our standard errors at the cell rather than the individual level.

Because we are using linear models to estimate differential changes at the cell level in labor force nonparticipation between the BOG and NOG samples, the results using these cell level measure will be no different from the results that would obtain if we had an individual level measure of the outcome variables of interest. Of course, this labor supply measure—having zero annual earnings—will capture only the effect of the DC program on the extensive margin, whether the individual works or not, rather than the intensive margin.

The first two columns of Table 11 report the results for equations (1) and (2) for the full sample. As the estimate for $\gamma$ in the first column reveals, labor force participation in the BOG sample was lower than in the NOG sample by approximately 2.06 percentage points in 1998. This disparity was however declining slightly in the years prior to the policy change, with a statistically significant estimate of 0.07 percentage points for $\beta_0$. This trend accelerated beginning in 2002, however, suggesting that the increase in DC enrollment that we estimated in the previous section led to significant declines in labor force participation in the BOG. In Figure 5, we display estimates for $\beta_t$. This figure underscores that while labor force participation had been declining somewhat more rapidly in the BOG relative to the NOG from 1998 to 2001, this trend picked up noticeably in the subsequent four years.

The reduced form estimate of 0.26 percentage points for $\beta_1$ in the first column can be divided

by the corresponding estimate of 0.96 percentage points for DC enrollment in Table 10 to estimate the effect of the policy-induced increase in DC enrollment on labor supply. Assuming that no other factors were changing differentially for BOG relative to NOG soldiers after 2001, this simple Wald-style estimate suggests that 27 percent of the individuals who enrolled in the DC program left the labor force.

In the next four columns of Table 11, we present two pairs of specifications, one for blacks and another for whites. We depict the estimates of $\beta_t$ in Figure 6, which reveals that there was a break in trend DC enrollment for both black and white BOG veterans relative to NOG veterans, though the apparent change is much greater for blacks than for whites. This is consistent with the much greater increase in DC enrollment among blacks, and strongly suggests that we are capturing the effect of the program rather than the effect of some other factor.

Dividing our reduced-form estimates for $\beta_1$ in each case by the corresponding estimate for $\beta_1$ in Table 10, we can estimate how the effect of the policy-induced increase in DC enrollment differed for the two groups. For blacks, we estimate that approximately 50 percent of those who enrolled in the DC program left the labor force. We estimate an effect of 24 percent for whites. This set of results suggests that the policy change described above had a larger impact on blacks than for whites for two reasons. First, a much larger fraction of blacks became eligible for DC benefits because of the expanded medical eligibility criteria. And second, a larger fraction of those made eligible for the program left the labor force once awarded benefits.

It is important to emphasize that these findings assume that in the absence of the 2001 policy change, the differences in the pre-existing trends between the BOG and NOG samples for both DC enrollment and labor supply would not have changed. Additionally, even if this assumption is correct, our estimates shed light only on the labor supply effect of the program on the

extensive margin. To the extent that the policy caused workers to reduce their number of hours rather than to leave the labor force, this first set of estimates will not capture the total effect. Subsequent work will explore this channel by considering impacts of the 2001 policy change on earnings as well as labor force participation.

*C. The Effect of DC Enrollment on OASDI and SSI Enrollment*

The findings from the preceding section suggest that the policy-induced increase in DC enrollment had a significant effect on the labor supply of BOG veterans. As much previous research has noted, one important cost of applying for disability benefits from either the SSDI or SSI program is that an individual may have to leave the labor force. Because many individuals did leave the labor force after becoming eligible for DC benefits, this would effectively lower their cost of applying for disability benefits from the SSDI or SSI programs. Similarly, individuals may be more likely to claim their retirement benefits early once they have left the labor force. This latter effect may be difficult to detect, however, as the vast majority of our sample does not reach the early retirement age of 62 before the final year of our SSA data (2005).

To examine impacts of the 2001 policy change on enrollment in OASDI and SSI, we estimated specifications analogous to (1) for the full sample and then for blacks and whites separately for SSDI, SSI, and OASI benefits. The results from these nine specifications are summarized in Table 12. The first column reports results for SSDI for the full sample. The point estimates for the $\beta_t$ coefficients are small in magnitude, though there is a clear break in trend that begins in 2001. An examination of the results for blacks and whites separately suggests that the effect is greater for blacks than whites, a contrast that is visible in Figure 7. These results provide

suggestive evidence that the policy-induced increase in DC enrollment in the BOG sample led to an increase in SSDI enrollment. The implied effect for black veterans of approximately 1.0 percentage points is economically large, though of course much smaller than the effect of the policy on DC enrollment.

The corresponding estimates for SSI are summarized in the second, fifth, and eighth column of results in the table. The point estimates provide suggestive evidence that the differential increase in DC enrollment may have reduced SSI enrollment. This would not be surprising if, for example, DC benefits made some individuals ineligible for the means-tested SSI benefits. However, an examination of the $\beta_t$ coefficients that are depicted in Figure 8 suggests that the differential SSI declines may have already been underway prior to the 2001 policy change. Thus, we view this causal interpretation as speculative.

In the third, sixth, and ninth columns, we summarize the results for OASI benefits. When interpreting these estimates, it is important to remember that only the youngest members of our sample (those born in 1942 or 1943) would reach the early retirement age in 2005 or earlier. It is therefore not surprising that the estimates for $\beta_t$ in the years prior to 2005 are very small in magnitude. However, for all three groups, the estimates for $\beta_{2005}$ are significantly negative. This suggests that the increase in DC enrollment may actually have reduced the number of veterans applying for early retirement benefits.

There are at least two possible mechanisms for such an effect. First, to the extent that the policy led to an increase in SSDI enrollment, individuals who might otherwise have claimed early retirement benefits might now be on SSDI. Second, the availability of transfer income may have relieved liquidity constraints for some veterans, allowing them to delay claiming and benefit from the actuarial adjustment in their OASI retirement benefits. It will be interesting to

probe more carefully on this result when more years of SSA data are available, as a larger fraction of our sample could potentially be affected in the timing of retirement benefits claims.

## VIII. Discussion

In this paper, we have provided evidence that the increase in DC enrollment caused by a 2001 policy change had a significant effect on the labor supply of Vietnam veterans who had boots on the ground in the Vietnam theater during the conflict there. More specifically, we find that almost thirty percent of individuals who became eligible for the DC program dropped out of the labor force. We have also uncovered evidence that the policy change had an effect on enrollment in those programs administered by the Social Security Administration. More specifically, we find that SSDI enrollment increased as a result of the policy change. The most plausible explanation for this is that, by causing some individuals to leave the labor force, the policy reduced the costs of applying for SSDI among affected veterans. We have also uncovered some suggestive evidence of effects for both SSI and OASI benefits, though these results are less clear-cut than the results for either labor supply or SSDI.

In ongoing work, we are building on these findings in a number of important ways. First, we hope to substantially expand the size of our BOG and NOG samples, as noted above. This step will allow both for greater precision in estimation and greater external validity since the samples will more closely represent the target populations. We also hope to add additional years of SSA data, which will allow us to examine the long-run labor supply effects of the policy as well as to trace out the full effect of enrollment in the VA's Disability Compensation program on the receipt of SSDI, SSI, and OASI benefits. We will also consider the effect of the policy change on additional outcome variables such as the level of earnings, which will allow us to explore whether there are effects on the intensive margin as well as the extensive margin. And finally, we

will examine more formally both the incentive and income effects of this program to better understand how it influences labor force participation and claims on other federal benefits (SSDI, SSI, and OASI) as a greater share of the Vietnam veterans affected by the policy reach SSA's early retirement age.

## IX. Appendix: Construction of Cells in the BOG and NOG Samples

To construct cells for matching individuals in the BOG and NOG samples with SSA data, we group individuals based on their values of certain background characteristics, including their gender, year of birth, race, and so forth. The variables used in forming the cells are listed below in the order in which the grouping occurred.

Before forming cells, we determined which individuals could be verified upon matching to the SSA data. A match would be verified if the social security number, date of birth, and the at least six letters of the last name could be matched in the two data sets. Cells were then constructed from the verified BOG and NOG samples so that there are between 5 and 9 individuals in the cell. Each cell consists only of individuals in the BOG or in the NOG. The number of variables used in the grouping varies across cells. This occurs because in some cases, a cell reaches a size of 5 to 9 after grouping on a relatively small number of variables. If a cell is between 10 and 18 individuals, we do not split further, but instead split the cell into two while sorting on the next matching variable to maximize similarity within the cell. If a cell has fewer than 5 individuals then we re-merge it with an adjacent cell.

The distribution of the cell size for the verified NOG sample is:

| Cell Size | Frequency | Percentage |
|---|---|---|
| 5 | 421,425 | 33.34 |
| 6 | 464,238 | 36.73 |
| 7 | 215,663 | 17.06 |
| 8 | 80,656 | 6.38 |
| 9 | 81,945 | 6.48 |
| Total | 1,263,927 | 100.00 |

The corresponding distribution for the verified BOG sample is:

| Cell Size | Frequency | Percentage |
|---|---|---|
| 5 | 446,060 | 36.06 |
| 6 | 438,858 | 35.48 |
| 7 | 200,893 | 16.24 |
| 8 | 75,944 | 6.14 |
| 9 | 75,096 | 6.07 |
| Total | 1,236,851 | 100.00 |

The list of variables used in the matching is as follows:

1. Diabetes in 1999 or 2000? (0/1)
2. Ever had service-connected diabetes? (0/1)
3. Year of service-connected diabetes upgrade (2000-2006)
4. Death year (e.g. 1955)
5. Gender (e.g. M for male or female)
6. Race (e.g. W for black, white, other)
7. Year-of-birth (e.g. 1942)
8. DC enrollment from 1998 – 2006 (e.g. 000011111)

9. Diabetes status from 1999-2006 (e.g. 00000011)
10. Unemployability indicator from 1999-2006 (e.g. 00001111)
11. First and last DC rating (e.g. 4060)
12. AFQT score quintile (e.g. 2)
13. Highest education category (e.g. 0, 1, 2, 3, 4 for hsd, hsg, smc, clg, clg-plus)
14. Start year (e.g. 1966)
15. State of residence (e.g. CA)
16. Ever had non-diabetes upgrade (0/1)
17. Year of non-diabetes upgrade (e.g. 2000-06)
18. Loss year (e.g. 72)
19. Disability pension enrollment from 1998 – 2006 (e.g. 000000111)
20. Military category (e.g. E, O, or W for enlisted, officer, or warrant)
21. Occupation type (e.g. 03)
22. Military grade (e.g. 07)
23. Marital status (e.g. M for married, widowed, etc.)
24. Component (e.g. R for active, reserve, national guard, etc.)
25. Number of dependents (0-9)

The frequency distribution for the number of variables used in the BOG and NOG cell formation is as follows (thus 38,852 in the BOG matched on variables 1 through and including 10):

| Vars used | # in NOG | # in BOG |
|---|---|---|
| 1 | 12 | 15 |
| 2 | 38 | 16 |
| 3 | 198 | 322 |
| 4 | 456 | 611 |
| 5 | 929 | 1,186 |
| 6 | 8,286 | 8,411 |
| 7 | 14,300 | 15,290 |
| 8 | 7,373 | 13,588 |
| 9 | 7,706 | 16,681 |
| 10 | 14,643 | 38,852 |
| 11 | 26,649 | 50,508 |
| 12 | 57,003 | 109,225 |
| 13 | 77,860 | 106,550 |
| 14 | 263,853 | 227,669 |
| 15 | 88,134 | 65,160 |
| 16 | 0 | 156 |
| 17 | 116,449 | 100,515 |
| 18 | 124,928 | 73,845 |
| 19 | 3,268 | 9,278 |
| 20 | 186,125 | 129,924 |
| 21 | 154,735 | 90,987 |
| 22 | 40,035 | 33,837 |
| 23 | 8,837 | 18,180 |
| 24 | 0 | 58,312 |
| 25 | 62,110 | 67,733 |
| Total | 1,263,927 | 1,236,851 |

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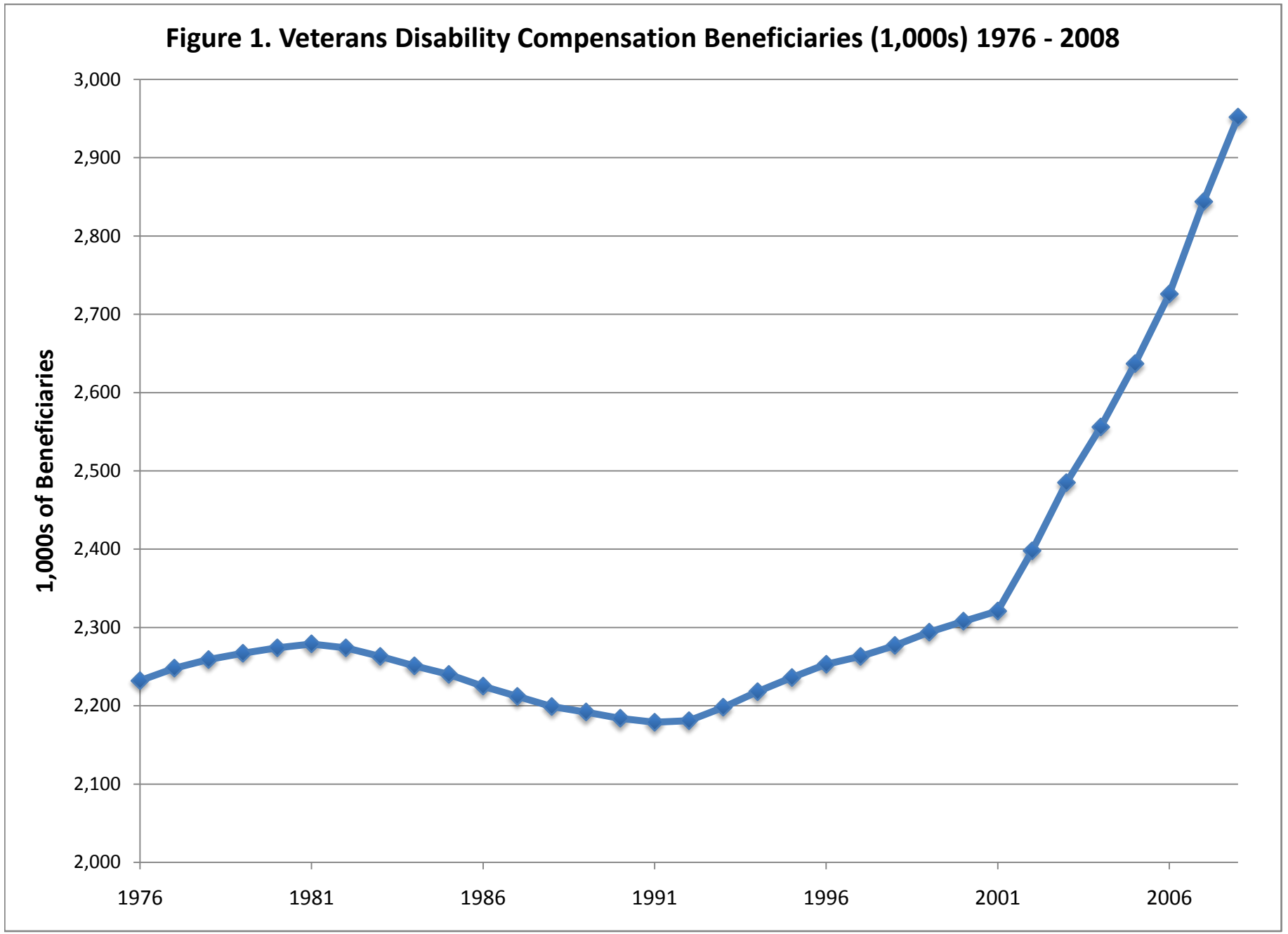


Figure 1. Veterans Disability Compensation Beneficiaries (1,000s) 1976 - 2008
1,000s of Beneficiaries
3,000
2,900
2,800
2,700
2,600
2,500
2,400
2,300
2,200
2,100
2,000
1976
1981
1986
1991
1996
2001
2006

## Figure 2. Monthly Disability Benefit Payment Amount by Combined Disability Rating: 2007 Benefits Schedule

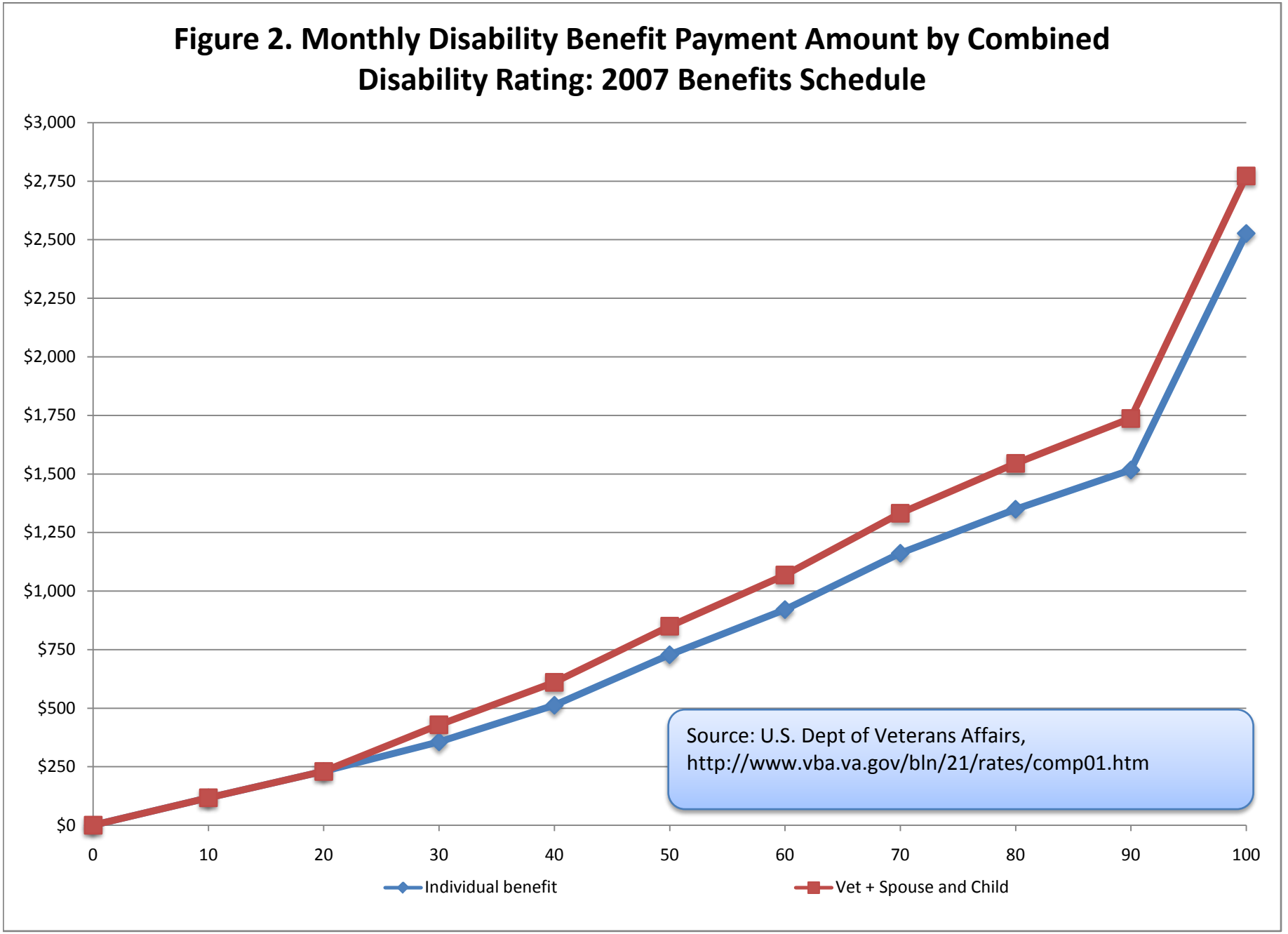
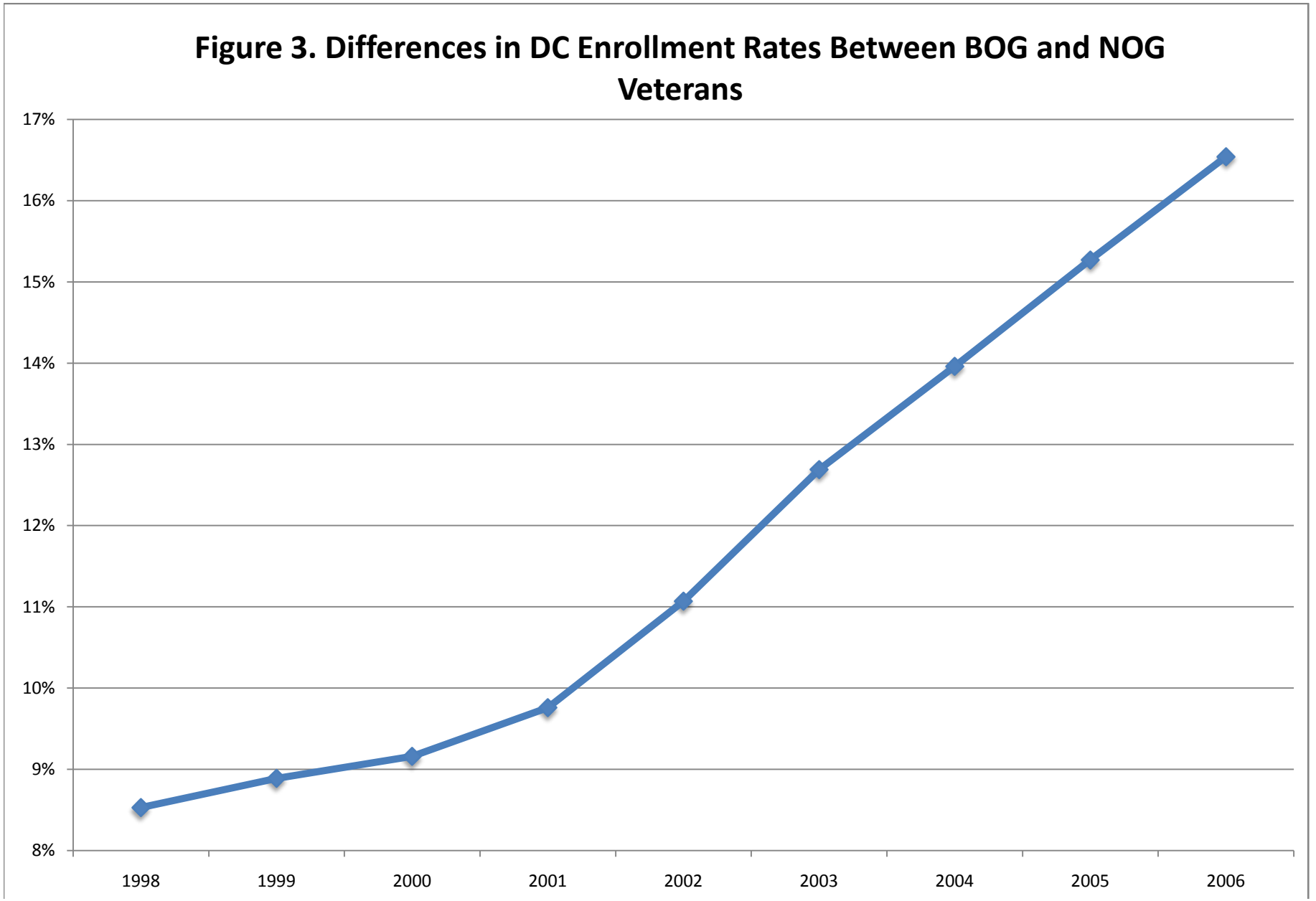
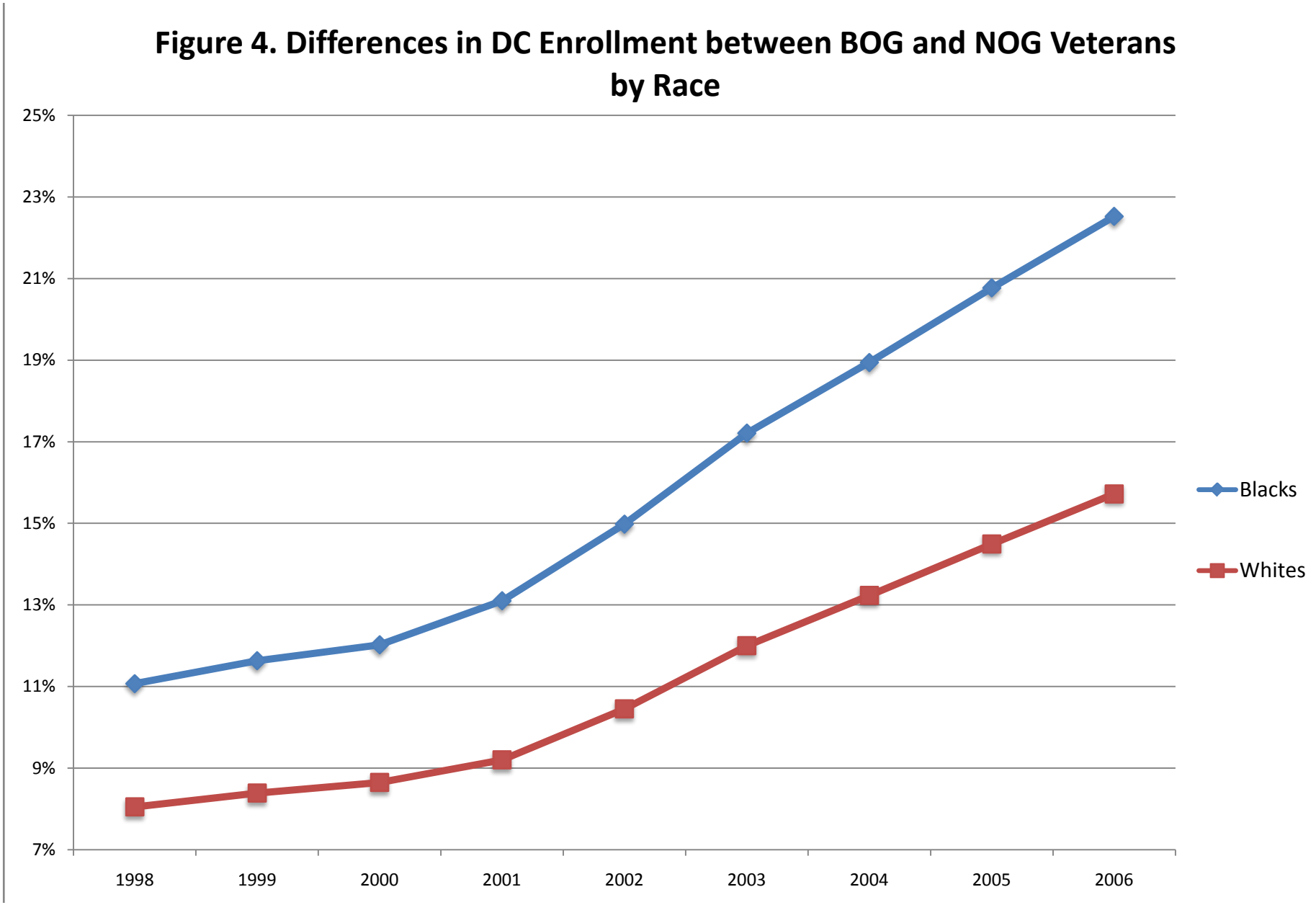


**Figure 3. Differences in DC Enrollment Rates Between BOG and NOG Veterans**



## Figure 4. Differences in DC Enrollment between BOG and NOG Veterans by Race



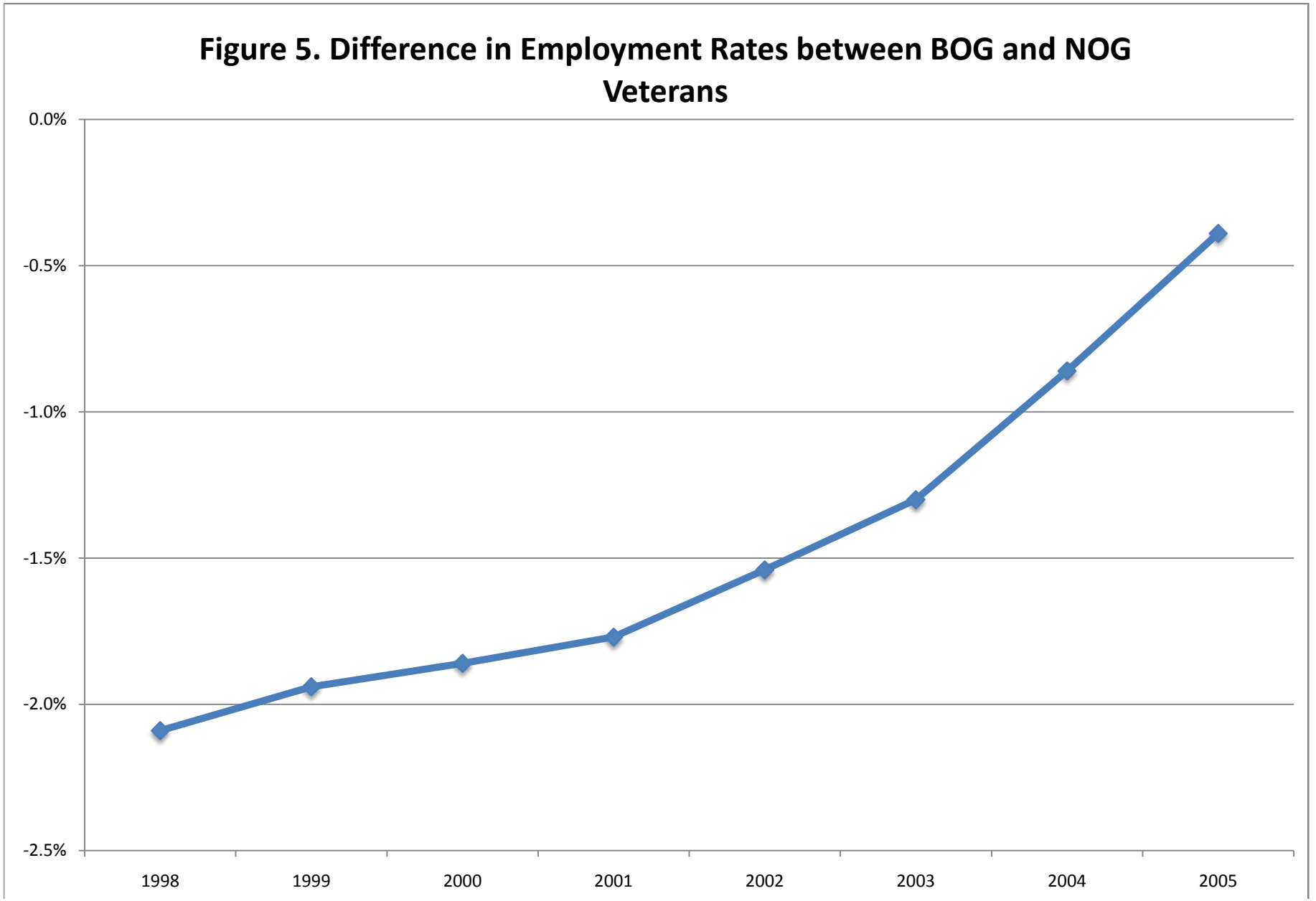


Figure 5. Difference in Employment Rates between BOG and NOG Veterans
0.0%
-0.5%
-1.0%
-1.5%
-2.0%
-2.5%
1998
1999
2000
2001
2002
2003
2004
2005

**Figure 6. Differences in Employment Rates between BOG and NOG Veterans by Race**

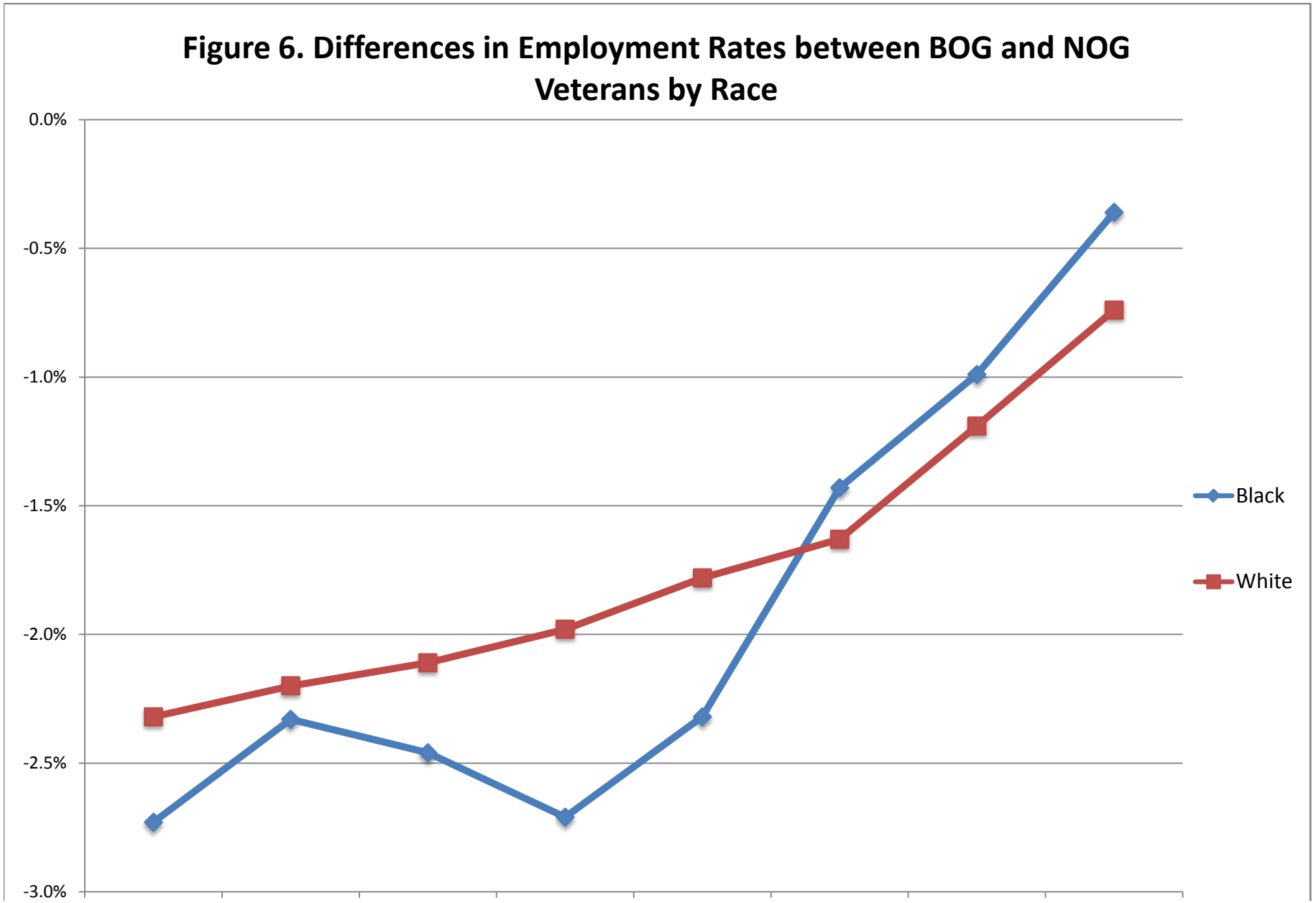
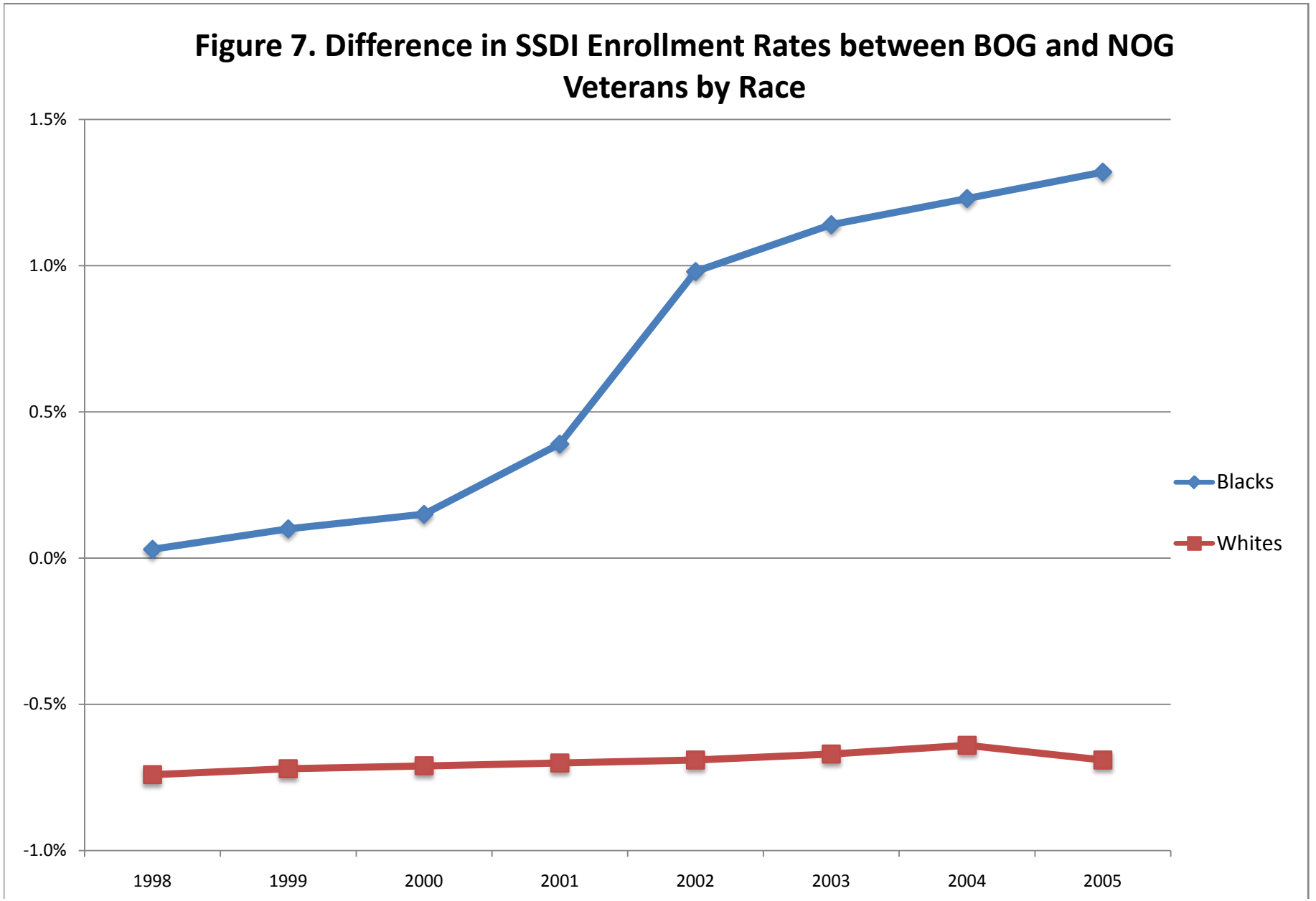


**Figure 7. Difference in SSDI Enrollment Rates between BOG and NOG Veterans by Race**



## Figure 8. Difference in SSI Enrollment Rates between BOG and NOG Veterans by Race

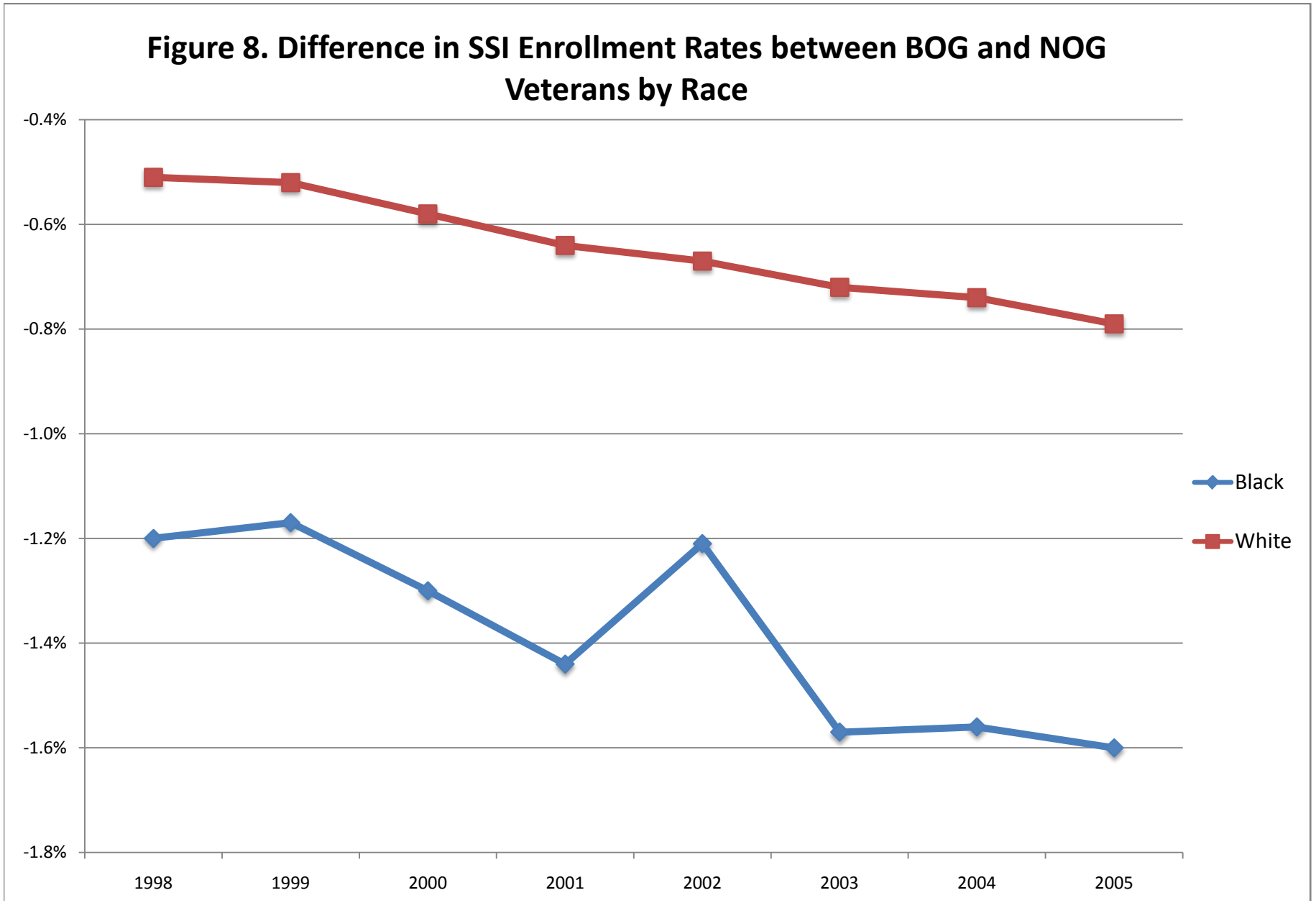


**Table 1. Distribution of Loss Year in OEMA Sample**

| | NOG Veterans | | BOG Veterans | |
|---|---|---|---|---|
| Loss Year | Observations | Percent of Sample | Observations | Percent of Sample |
| 1960 | 0 | 0.00 | 2 | 0.00 |
| 1961 | 0 | 0.00 | 2 | 0.00 |
| 1962 | 0 | 0.00 | 2 | 0.00 |
| 1963 | 0 | 0.00 | 2 | 0.00 |
| 1964 | 0 | 0.00 | 8 | 0.00 |
| 1965 | 0 | 0.00 | 25 | 0.00 |
| 1966 | 0 | 0.00 | 140 | 0.01 |
| 1967 | 0 | 0.00 | 250 | 0.02 |
| 1968 | 17,804 | 0.68 | 7,144 | 0.55 |
| 1969 | 83,975 | 3.23 | 166,738 | 12.85 |
| 1970 | 267,555 | 10.29 | 260,489 | 20.08 |
| 1971 | 242,036 | 9.31 | 275,471 | 21.24 |
| 1972 | 122,484 | 4.71 | 221,274 | 17.06 |
| 1973 | 164,096 | 6.31 | 77,685 | 5.99 |
| 1974 | 175,472 | 6.75 | 51,747 | 3.99 |
| 1975 | 163,025 | 6.27 | 32,966 | 2.54 |
| 1976 | 160,424 | 6.17 | 26,253 | 2.02 |
| 1977 | 146,559 | 5.64 | 23,591 | 1.82 |
| 1978 | 129,341 | 4.97 | 21,523 | 1.66 |
| 1979 | 141,324 | 5.44 | 19,784 | 1.53 |
| 1980 | 138,023 | 5.31 | 16,065 | 1.24 |
| 1981 | 120,203 | 4.62 | 13,027 | 1.00 |
| 1982 | 128,738 | 4.95 | 12,229 | 0.94 |
| 1983 | 142,098 | 5.47 | 11,473 | 0.88 |
| 1984 | 131,394 | 5.05 | 11,680 | 0.90 |
| 1985 | 125,495 | 4.83 | 10,945 | 0.84 |
| 1986 | 0 | 0.00 | 6,120 | 0.47 |
| 1987 | 0 | 0.00 | 5,146 | 0.40 |
| 1988 | 0 | 0.00 | 5,749 | 0.44 |
| 1989 | 0 | 0.00 | 5,261 | 0.41 |
| 1990 | 0 | 0.00 | 4,438 | 0.34 |
| 1991 | 0 | 0.00 | 3,538 | 0.27 |
| 1992 | 0 | 0.00 | 3,725 | 0.29 |
| 1993 | 0 | 0.00 | 2,596 | 0.20 |
| Total | 2,600,046 | 100.00 | 1,297,088 | 100.00 |

**Table 2. Distribution of Year of Birth and Start Year in BOG vs. NOG Samples**

A. Year-of-Birth Distribution

| Start Year | NOG # obs | NOG % of obs | BOG # obs | BOG % of obs |
|---|---|---|---|---|
| <=1924 | 15564 | 0.60 | 18239 | 1.22 |
| 1925 | 1,507 | 0.06 | 3,276 | 0.22 |
| 1926 | 1,846 | 0.07 | 4,969 | 0.33 |
| 1927 | 2,269 | 0.09 | 5,741 | 0.38 |
| 1928 | 2,821 | 0.11 | 7,959 | 0.53 |
| 1929 | 3,224 | 0.12 | 9,508 | 0.63 |
| 1930 | 3,535 | 0.14 | 11,044 | 0.74 |
| 1931 | 3,924 | 0.15 | 11,767 | 0.78 |
| 1932 | 3,218 | 0.12 | 12,995 | 0.87 |
| 1933 | 2,579 | 0.10 | 12,302 | 0.82 |
| 1934 | 2,389 | 0.09 | 12,601 | 0.84 |
| 1935 | 2,480 | 0.10 | 13,435 | 0.90 |
| 1936 | 2,669 | 0.10 | 14,041 | 0.94 |
| 1937 | 2,851 | 0.11 | 13,354 | 0.89 |
| 1938 | 3,555 | 0.14 | 14,336 | 0.96 |
| 1939 | 4,820 | 0.19 | 15,124 | 1.01 |
| 1940 | 7,546 | 0.29 | 18,093 | 1.21 |
| 1941 | 10,889 | 0.42 | 24,055 | 1.60 |
| 1942 | 19,044 | 0.73 | 34,674 | 2.31 |
| 1943 | 29,386 | 1.13 | 47,411 | 3.16 |
| 1944 | 38,911 | 1.50 | 60,487 | 4.03 |
| 1945 | 48,243 | 1.86 | 80,722 | 5.38 |
| 1946 | 72,742 | 2.80 | 132,657 | 8.84 |
| 1947 | 109,365 | 4.21 | 228,222 | 15.21 |
| 1948 | 166,631 | 6.41 | 229,645 | 15.31 |
| 1949 | 155,948 | 6.00 | 192,828 | 12.85 |
| 1950 | 143,976 | 5.54 | 135,576 | 9.04 |
| 1951 | 141,933 | 5.46 | 73,551 | 4.90 |
| 1952 | 165,629 | 6.37 | 37,457 | 2.50 |
| 1953 | 141,430 | 5.44 | 15,825 | 1.05 |
| 1954 | 157,254 | 6.05 | 5,887 | 0.39 |
| >=1955 | 1,131,868 | 43.53 | 2,449 | 0.16 |
| Total | 2,600,046 | 100.00 | 1,500,230 | 100.00 |

B. Start Year Distribution

| Birth Year | NOG # obs | NOG % of obs | BOG # obs | BOG % of obs |
|---|---|---|---|---|
| <=1944 | 6977 | 0.27 | 8240 | 0.55 |
| 1945 | 1,323 | 0.05 | 2,824 | 0.19 |
| 1946 | 2,062 | 0.08 | 3,971 | 0.26 |
| 1947 | 2,028 | 0.08 | 3,775 | 0.25 |
| 1948 | 3,761 | 0.14 | 6,024 | 0.40 |
| 1949 | 3,849 | 0.15 | 4,816 | 0.32 |
| 1950 | 5,401 | 0.21 | 8,327 | 0.56 |
| 1951 | 4,280 | 0.16 | 13,634 | 0.91 |
| 1952 | 1,927 | 0.07 | 13,195 | 0.88 |
| 1953 | 1,986 | 0.08 | 15,688 | 1.05 |
| 1954 | 1,524 | 0.06 | 13,452 | 0.90 |
| 1955 | 1,809 | 0.07 | 13,024 | 0.87 |
| 1956 | 1,779 | 0.07 | 12,205 | 0.81 |
| 1957 | 1,782 | 0.07 | 12,204 | 0.81 |
| 1958 | 2,487 | 0.10 | 15,291 | 1.02 |
| 1959 | 2,261 | 0.09 | 14,871 | 0.99 |
| 1960 | 2,349 | 0.09 | 14,302 | 0.95 |
| 1961 | 4,021 | 0.15 | 16,135 | 1.08 |
| 1962 | 4,366 | 0.17 | 15,786 | 1.05 |
| 1963 | 4,788 | 0.18 | 15,993 | 1.07 |
| 1964 | 6,384 | 0.25 | 18,496 | 1.23 |
| 1965 | 9,738 | 0.37 | 27,107 | 1.81 |
| 1966 | 38,327 | 1.47 | 89,320 | 5.95 |
| 1967 | 97,691 | 3.76 | 175,680 | 11.71 |
| 1968 | 197,425 | 7.59 | 265,203 | 17.68 |
| 1969 | 200,250 | 7.70 | 265,255 | 17.68 |
| 1970 | 163,743 | 6.30 | 150,876 | 10.06 |
| 1971 | 191,839 | 7.38 | 61,039 | 4.07 |
| 1972 | 215,119 | 8.27 | 15,215 | 1.01 |
| 1973 | 144,853 | 5.57 | 3,100 | 0.21 |
| 1974 | 187,491 | 7.21 | 1,771 | 0.12 |
| >=1975 | 1,085,148 | 41.74 | 5,459 | 0.36 |
| Missing | 1,278 | 0.05 | 197,952 | 13.19 |
| | 2,600,046 | 100.00 | 1,500,230 | 100.00 |

**Table 3. Characteristics of the BOG and NOG Samples**

| Variable | SSA Verified Sample BOG | SSA Verified Sample NOG | Full Sample BOG | Full Sample NOG |
|---|---|---|---|---|
| % Verified in SSA Data | 100.0 | 100.0 | 68.8 | 43.0 |
| % with Start Yrs 1966-69 | 100.0 | 100.0 | 100.0 | 100.0 |
| % with YOB 1942-51 | 100.0 | 100.0 | 100.0 | 100.0 |
| % Dead by 12/31/97 | 6.3 | 6.3 | 6.3 | 6.4 |
| % Dead by 12/31/06 | 11.7 | 11.3 | 11.5 | 11.3 |
| % Black | 10.3 | 8.8 | 10.5 | 10.3 |
| % Missing Educ | 2.2 | 42.6 | 1.7 | 20.9 |
| % HS Dropout | 37.9 | 49.2 | 32.9 | 32.1 |
| % HS Grad | 39.1 | 14.3 | 44.5 | 37.9 |
| % Some College | 15.1 | 21.9 | 15.1 | 18.4 |
| % College Grad | 7.2 | 10.5 | 6.7 | 9.7 |
| % More Than College | 0.8 | 4.1 | 0.7 | 2.0 |
| Average AFQT Score | 51.5 | 50.3 | 51.7 | 52.9 |
| Average Year-of-Birth | 1947.3 | 1946.6 | 1947.5 | 1947.3 |
| Average Start Year | 1967.7 | 1967.7 | 1967.9 | 1968.1 |
| Average Loss Year | 1971.4 | 1970 | 1971.1 | 1970.5 |
| % on DC in 9/1998 | 16.0 | 7.3 | 14.1 | 6.2 |
| % on DC in 9/2000 | 16.9 | 7.4 | 14.7 | 6.3 |
| % on DC in 9/2006 | 26.0 | 9.1 | 20.9 | 7.1 |
| Avg Benefit in 9/1998 | $101 | $55 | | |
| Avg Benefit in 9/2000 | $121 | $60 | | |
| Avg Benefit in 9/2006 | $255 | $85 | | |
| Avg Benefit in 9/1998 if > 0 | $629 | $752 | | |
| Avg Benefit in 9/2000 if > 0 | $721 | $805 | | |
| Avg Benefit in 9/2006 if > 0 | $984 | $927 | | |
| % with Zero Earnings in 1998 | 19.2 | 20.8 | | |
| % with Zero Earnings in 2000 | 21.0 | 22.4 | | |
| % with Zero Earnings in 2005 | 31.2 | 31.5 | | |
| Average Earnings in 1998 | 40,011 | 49,274 | | |
| Average Earnings in 2000 | 40,906 | 49,717 | | |
| Average Earnings in 2005 | 33,675 | 39,655 | | |
| % on SSDI in 1998 | 4.82 | 5.14 | | |
| % on SSDI in 2000 | 5.79 | 6.04 | | |
| % on SSDI in 2005 | 9.58 | 9.51 | | |
| % on SSI in 1998 | 0.68 | 1.13 | | |
| % on SSI in 2000 | 0.65 | 1.17 | | |
| % on SSI in 2005 | 0.73 | 1.43 | | |
| # Observations | 533,321 | 219,104 | 774,930 | 509,552 |

**Table 4. Comparison of Census and Army/SSA Demographics and Earnings Data for year 1999**

| | A. Army/SSA Matched Sample | | | B. Males Born 1942-1952: 2000 Census IPUMS | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | | | | *Age-Reweighted to Match OEMA* | | |
| | All | Whites | Blacks | All | All Vietnam | All Viet Vets | White Viet Vets | Black Viet Vets |
| Veteran status | | | | | | | | |
| Vietnam era veteran | 100.0 | 100.0 | 100.0 | 32.1 | 100.0 | 100.0 | 100.0 | 100.0 |
| Boots on ground | 70.7 | 70.3 | 75.1 | - | - | - | - | - |
| Boots not on ground | 29.3 | 29.7 | 24.9 | - | - | - | - | - |
| Age | 51.9 | 51.9 | 51.7 | 51.6 | 52.1 | 51.9 | 51.9 | 51.7 |
| | (2.1) | (2.1) | (2.1) | (3.1) | (2.8) | (2.1) | (2.1) | (2.1) |
| Race | | | | | | | | |
| White | 88.3 | 100.0 | 0.0 | 81.4 | 86.7 | 86.7 | 100.0 | 0.0 |
| Black | 10.6 | 0.0 | 100.0 | 9.7 | 8.7 | 8.7 | 0.0 | 100.0 |
| Other | 1.1 | 0.0 | 0.0 | 9.0 | 4.6 | 4.6 | 0.0 | 0.0 |
| Education | | | | | | | | |
| HS dropout | 37.6 | 37.1 | 42.1 | 15.4 | 7.5 | 7.5 | 6.7 | 11.6 |
| HS grad | 35.9 | 34.8 | 44.4 | 25.1 | 28.7 | 28.9 | 28.9 | 30.5 |
| Some college | 16.3 | 17.1 | 9.9 | 28.5 | 37.5 | 37.9 | 37.3 | 41.7 |
| College grad | 8.6 | 9.2 | 3.2 | 17.4 | 16.0 | 15.8 | 16.5 | 10.6 |
| Post-college | 1.7 | 1.8 | 0.4 | 13.8 | 10.3 | 9.9 | 10.5 | 5.6 |
| Not in labor force | 21.3 | 20.2 | 30.5 | 20.7 | 18.4 | 18.1 | 17.2 | 24.5 |
| Annual earnings | | | | | | | | |
| Mean (full sample) | 41,630 | 43,771 | 24,835 | 42,712 | 42,025 | 42,062 | 43,818 | 30,046 |
| | (59,291) | (62,332) | (21,361) | (29,460) | (24,981) | (24,612) | (25,090) | (17,168) |
| Median (full sample) | 33,555 | 35,206 | 20,349 | 35,655 | 36,555 | 36,828 | 43,823 | 26,009 |
| | (24,455) | (24,771) | (16,914) | (25,760) | (22,027) | (21,567) | (48,736) | (17,276) |
| Mean (in labor force) | 52,897 | 54,823 | 35,742 | 54,228 | 51,856 | 51,665 | 53,248 | 39,912 |
| | (68,188) | (71,321) | (25,480) | (32,807) | (27,531) | (26,866) | (27,394) | (19,212) |
| Observations | 763,792 | 673,475 | 80,531 | 936,806 | 302,104 | 302,104 | 264,608 | 23,628 |

**Table 5. Count of DC Recipients and Average Annual DC Benefit by Combined Disability Rating (CDR), July 2006: Data from VBA Annual Benefits Report 2006**

| CDR | Recipients | Payments ($ Millions) | Mean Annual Benefit | *Share with Each Rating by Service Era* Gulf | Vietnam | Korea | WW II | Peacetime |
|---|---|---|---|---|---|---|---|---|
| 0% | 14,291 | $ 12.8 | $894 | 0.1% | 0.3% | 2.5% | 1.4% | 0.4% |
| 10% | 775,346 | $ 1,048.7 | $1,353 | 27.6% | 22.0% | 30.6% | 33.2% | 37.4% |
| 20% | 417,721 | $ 1,103.4 | $2,642 | 17.9% | 13.4% | 13.0% | 12.5% | 17.8% |
| 30% | 334,931 | $ 1,507.6 | $4,501 | 15.3% | 10.7% | 12.5% | 13.2% | 11.0% |
| 40% | 259,834 | $ 1,693.1 | $6,516 | 12.5% | 9.0% | 8.2% | 8.1% | 8.0% |
| 50% | 161,568 | $ 1,478.9 | $9,153 | 7.1% | 5.9% | 5.4% | 5.8% | 4.6% |
| 60% | 184,264 | $ 2,800.8 | $15,200 | 6.9% | 7.0% | 7.6% | 7.1% | 5.6% |
| 70% | 165,257 | $ 3,689.6 | $22,326 | 4.7% | 8.5% | 5.5% | 5.3% | 4.1% |
| 80% | 113,404 | $ 2,814.4 | $24,818 | 3.3% | 5.6% | 4.1% | 4.0% | 2.7% |
| 90% | 60,546 | $ 1,640.5 | $27,096 | 1.6% | 3.1% | 2.2% | 2.3% | 1.4% |
| 100% | 238,662 | $ 7,833.0 | $32,821 | 3.0% | 14.6% | 8.5% | 7.2% | 6.9% |
| Total | 2,725,824 | $ 25,622.9 | $9,400 | 674,021 | 939,200 | 160,007 | 334,222 | 594,590 |

Data in the first three columns provides the number of DC recipients, the total amount paid, and the average monthly DC benefit by combined disability rating (CDR) in July of 2006. Data in the last five columns are from June 2006 and provide the share of DC recipients from each service era with each value of the CDR. These data were obtained from the Department of Veterans Affairs following an email request. Because the final five columns draw on a different data source, the sum of recipients by service era is not identical to the overall count given in the first column

**Table 6. Evolution of Disability Compensation Benefits by Year of First Disability Compensation Award: DC Award Cohorts 1999 - 2005**

| Year of Award | Outcome Year 1998 | 1999 | 2000 | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 |
|---|---|---|---|---|---|---|---|---|---|
| | A. Combined Disability Rating (%) | | | | | | | | |
| 1999 | . | 40.9 | 43.1 | 46.4 | 49.6 | 53.4 | 55.8 | 57.9 | 59.4 |
| 2000 | | | 43.0 | 47.5 | 51.3 | 54.6 | 56.9 | 58.9 | 60.7 |
| 2001 | | | | 40.6 | 44.7 | 49.6 | 53.4 | 56.7 | 58.8 |
| 2002 | | | | | 38.0 | 43.8 | 47.5 | 50.8 | 53.2 |
| 2003 | | | | | | 41.2 | 46.0 | 49.5 | 52.2 |
| 2004 | | | | | | | 42.3 | 46.5 | 49.0 |
| 2005 | | | | | | | | 40.8 | 43.9 |
| | | | | | | | | | 40.5 |
| | B. Monthly benefit amount ($) | | | | | | | | |
| 1999 | . | 693 | 808 | 920 | 1,007 | 1,117 | 1,178 | 1,229 | 1,277 |
| 2000 | | | 774 | 936 | 1,044 | 1,145 | 1,208 | 1,260 | 1,316 |
| 2001 | | | | 698 | 820 | 969 | 1,088 | 1,188 | 1,247 |
| 2002 | | | | | 619 | 798 | 915 | 1,009 | 1,084 |
| 2003 | | | | | | 695 | 841 | 949 | 1,032 |
| 2004 | | | | | | | 721 | 854 | 930 |
| 2005 | | | | | | | | 673 | 769 |
| | | | | | | | | | 656 |
| | D. Receiving non-employability benefit or 100% CDR (%) | | | | | | | | |
| 1999 | . | 14.1 | 20.2 | 25.9 | 31.7 | 37.7 | 41.2 | 44.0 | 45.5 |
| 2000 | | | 16.4 | 25.2 | 32.4 | 38.0 | 41.7 | 44.6 | 46.5 |
| 2001 | | | | 14.5 | 21.5 | 28.8 | 34.8 | 39.6 | 41.6 |
| 2002 | | | | | 12.7 | 20.6 | 26.2 | 30.5 | 33.3 |
| 2003 | | | | | | 15.6 | 22.2 | 27.4 | 30.2 |
| 2004 | | | | | | | 17.3 | 23.3 | 25.9 |
| 2005 | | | | | | | | 15.1 | 18.1 |
| | | | | | | | | | 13.4 |
| | C. Receiving compensation for diabetes (%) | | | | | | | | |
| 1999 | . | 2.1 | 3.0 | 4.2 | 10.3 | 13.6 | 15.0 | 16.6 | 17.5 |
| 2000 | | | 3.0 | 4.4 | 11.3 | 14.6 | 16.3 | 18.0 | 19.0 |
| 2001 | | | | 16.9 | 39.9 | 41.3 | 42.0 | 42.6 | 42.8 |
| 2002 | | | | | 60.9 | 61.6 | 61.9 | 62.0 | 62.1 |
| 2003 | | | | | | 52.1 | 52.5 | 52.9 | 53.4 |
| 2004 | | | | | | | 45.1 | 46.1 | 46.8 |
| 2005 | | | | | | | | 43.6 | 44.5 |
| | | | | | | | | | 42.4 |
| | E. Labor Force Participatipation Rates through 2005 (Pre/post DC award years separated by dividing lines) | | | | | | | | |
| 1999 | 69.8 | 62.1 | 56.0 | 52.2 | 48.6 | 45.6 | 42.9 | 40.4 | . |
| 2000 | 71.9 | 68.0 | 61.9 | 54.3 | 49.3 | 45.6 | 42.5 | 39.6 | . |
| 2001 | 75.8 | 73.1 | 70.1 | 63.2 | 55.4 | 49.9 | 46.0 | 42.4 | . |
| 2002 | 78.8 | 77.0 | 74.8 | 70.3 | 63.3 | 56.2 | 51.8 | 47.9 | . |
| 2003 | 81.2 | 79.4 | 77.8 | 75.0 | 70.0 | 61.8 | 54.3 | 49.5 | . |
| 2004 | 81.8 | 80.7 | 79.4 | 77.5 | 73.7 | 69.4 | 60.7 | 53.1 | . |
| 2005 | 83.9 | 82.7 | 81.8 | 79.8 | 77.1 | 73.2 | 68.2 | 60.2 | . |

**Table 7. Labor Force Participation Rate and Average Annual Earnings of Positive Earners: BOG and NOG Veterans, 1998 - 2005 (OEMA sample)**

| | All | | White | | Black/Other | |
|---|---|---|---|---|---|---|
| | NOG | BOG | NOG | BOG | NOG | BOG |
| | A. Percent with positive labor earnings | | | | | |
| 1998 | 79.4 | 80.9 | 80.3 | 81.9 | 71.1 | 72.4 |
| 1999 | 78.7 | 80.0 | 79.5 | 81.1 | 70.6 | 71.5 |
| 2000 | 77.8 | 79.1 | 78.6 | 80.2 | 69.4 | 70.4 |
| 2001 | 76.4 | 77.7 | 77.3 | 78.8 | 67.4 | 68.6 |
| 2002 | 74.5 | 75.6 | 75.5 | 76.8 | 65.1 | 65.9 |
| 2003 | 72.6 | 73.5 | 73.6 | 74.8 | 63.1 | 63.0 |
| 2004 | 70.7 | 71.3 | 71.8 | 72.6 | 60.7 | 60.2 |
| 2005 | 68.7 | 69.0 | 69.8 | 70.3 | 58.6 | 57.6 |
| | B. Mean annual earnings of positive earners (std devs in parentheses) | | | | | |
| 1998 | 59,361 | 48,076 | 61,774 | 49,718 | 36,376 | 34,551 |
| | (83,758) | (42,541) | (87,376) | (44,329) | (23,324) | (17,876) |
| 1999 | 60,145 | 49,111 | 62,548 | 50,757 | 37,205 | 35,523 |
| | (99,851) | (50,008) | (104,321) | (51,831) | (26,306) | (27,556) |
| 2000 | 61,072 | 50,217 | 63,545 | 51,943 | 37,363 | 35,915 |
| | (113,389) | (80,088) | (118,626) | (84,195) | (24,720) | (23,984) |
| 2001 | 59,252 | 49,517 | 61,574 | 51,183 | 36,875 | 35,675 |
| | (94,129) | (84,926) | (98,372) | (89,284) | (21,214) | (25,864) |
| 2002 | 56,687 | 48,086 | 58,785 | 49,602 | 36,387 | 35,415 |
| | (85,537) | (62,196) | (89,356) | (65,267) | (20,297) | (20,156) |
| 2003 | 55,262 | 47,158 | 57,264 | 48,582 | 35,777 | 35,155 |
| | (81,824) | (46,729) | (85,447) | (48,739) | (19,739) | (20,026) |
| 2004 | 55,478 | 47,861 | 57,514 | 49,342 | 35,611 | 35,297 |
| | (83,828) | (95,347) | (87,530) | (100,422) | (20,324) | (21,920) |
| 2005 | 55,238 | 47,145 | 57,324 | 48,629 | 34,733 | 34,466 |
| | (94,333) | (89,551) | (98,567) | (94,261) | (20,604) | (21,089) |

**Table 8. Labor Force Participation Rate, Non-Employability, and Diabetes Diagnosis by Combined Disability Rating (CDR), 1999, 2001, 2003, and 2005**

| | 1999 | | 2001 | | 2003 | | 2005 | |
|---|---|---|---|---|---|---|---|---|
| CDR | NOG | BOG | BOG | NOG | BOG | NOG | BOG | NOG |
| | | | A. Labor force participation (%) | | | | | |
| None | 79.7 | 82.6 | 77.6 | 80.8 | 73.9 | 77.7 | 70.2 | 74.3 |
| 0 | 100.0 | 47.6 | 100.0 | 46.2 | 90.0 | 39.5 | 90.0 | 34.7 |
| 10 | 84.3 | 86.4 | 82.9 | 84.7 | 78.1 | 81.8 | 74.4 | 77.7 |
| 20 | 81.8 | 84.4 | 81.4 | 83.2 | 77.5 | 79.5 | 73.8 | 75.1 |
| 30 | 71.9 | 72.2 | 71.5 | 73.7 | 67.4 | 71.4 | 63.6 | 67.7 |
| 40 | 74.1 | 74.1 | 71.2 | 73.3 | 70.7 | 71.4 | 63.7 | 68.7 |
| 50 | 65.8 | 65.8 | 65.6 | 65.0 | 62.6 | 63.6 | 62.2 | 62.0 |
| 60 | 59.0 | 57.8 | 57.0 | 57.7 | 56.8 | 57.6 | 53.2 | 58.3 |
| 70 | 46.1 | 34.9 | 38.7 | 29.5 | 35.2 | 29.7 | 32.5 | 26.6 |
| 80 | 40.8 | 29.1 | 35.7 | 25.2 | 31.2 | 23.8 | 27.5 | 22.3 |
| 90 | 33.2 | 30.2 | 31.9 | 26.2 | 27.8 | 20.9 | 23.1 | 19.6 |
| 100 | 16.6 | 10.9 | 15.8 | 11.4 | 15.6 | 13.4 | 15.4 | 14.6 |
| | | | B. Receiving non-employability benefit (%) | | | | | |
| 0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| 10 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| 20 | 0.1 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| 30 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| 40 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| 50 | 0.0 | 0.0 | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 | 0.0 |
| 60 | 18.0 | 11.8 | 20.7 | 12.6 | 23.4 | 12.5 | 24.8 | 13.3 |
| 70 | 34.8 | 43.1 | 45.7 | 54.4 | 52.8 | 59.4 | 56.7 | 62.0 |
| 80 | 45.1 | 55.8 | 50.0 | 60.2 | 58.0 | 65.3 | 63.0 | 68.1 |
| 90 | 56.3 | 64.3 | 61.7 | 64.7 | 65.1 | 69.6 | 69.6 | 73.9 |
| 100 | . | . | . | . | . | . | . | . |
| | C. LFP of DC recipients excluding veterans receiving non-employability benefit (%) | | | | | | | |
| 60 | 66.1 | 59.7 | 65.5 | 60.7 | 64.8 | 61.0 | 61.6 | 61.6 |
| 70 | 65.7 | 54.8 | 62.7 | 54.7 | 62.5 | 56.0 | 59.0 | 57.9 |
| 80 | 61.8 | 53.7 | 61.0 | 50.1 | 54.2 | 48.1 | 55.5 | 51.3 |
| 90 | 60.9 | 44.0 | 55.1 | 39.9 | 51.1 | 34.2 | 41.0 | 35.2 |

**Table 9. DC Receipt and Diabetes Compensation among BOG and NOG Veterans, 1998 - 2005 (OEMA sample)**

| | All | | White | | Black/Other | |
|---|---|---|---|---|---|---|
| | NOG | BOG | NOG | BOG | NOG | BOG |
| | | | A. Receiving DC benefits (%) | | | |
| 1998 | 7.3 | 16.0 | 6.9 | 15.2 | 10.4 | 22.0 |
| 1999 | 7.3 | 16.4 | 7.0 | 15.7 | 10.5 | 22.7 |
| 2000 | 7.4 | 16.8 | 7.1 | 16.1 | 10.7 | 23.3 |
| 2001 | 7.6 | 17.6 | 7.2 | 16.8 | 10.8 | 24.3 |
| 2002 | 7.8 | 19.1 | 7.5 | 18.2 | 11.2 | 26.5 |
| 2003 | 8.2 | 21.1 | 7.8 | 20.1 | 11.7 | 29.2 |
| 2004 | 8.5 | 22.7 | 8.1 | 21.7 | 12.2 | 31.3 |
| 2005 | 8.8 | 24.4 | 8.4 | 23.3 | 12.8 | 33.6 |
| 2006 | 9.1 | 25.9 | 8.7 | 24.8 | 13.2 | 35.6 |
| | | | A. Mean Monthly DC Payment Conditional on Receipt ($) | | | |
| 1998 | 751.6 | 628.4 | 753.4 | 625.3 | 740.9 | 635.8 |
| 1999 | 775.3 | 673.3 | 776.2 | 669.4 | 770.1 | 685.4 |
| 2000 | 804.6 | 720.6 | 803.3 | 716.1 | 813.8 | 735.4 |
| 2001 | 830.3 | 764.6 | 828.8 | 758.6 | 841.0 | 789.2 |
| 2002 | 837.2 | 792.5 | 834.7 | 784.0 | 853.3 | 831.5 |
| 2003 | 869.0 | 852.9 | 863.8 | 841.2 | 902.5 | 911.3 |
| 2004 | 888.2 | 904.6 | 881.9 | 891.2 | 928.0 | 974.9 |
| 2005 | 905.5 | 948.1 | 898.5 | 933.3 | 949.3 | 1028.0 |
| 2006 | 926.6 | 983.9 | 919.2 | 967.7 | 973.8 | 1073.0 |
| | | | B. Percent of DC Recipients Receiving Compensation for Diabetes | | | |
| 1998 | . | . | . | . | . | . |
| 1999 | 1.7 | 1.5 | 1.5 | 1.2 | 3.3 | 3.2 |
| 2000 | 1.9 | 1.6 | 1.6 | 1.3 | 3.8 | 3.5 |
| 2001 | 2.3 | 3.2 | 2.0 | 2.8 | 4.4 | 5.9 |
| 2002 | 5.6 | 13.6 | 5.1 | 12.7 | 8.6 | 19.1 |
| 2003 | 7.9 | 19.9 | 7.3 | 18.8 | 11.7 | 26.6 |
| 2004 | 9.6 | 23.2 | 8.8 | 22.0 | 14.1 | 30.3 |
| 2005 | 11.1 | 26.0 | 10.3 | 24.7 | 15.8 | 33.3 |
| 2006 | 12.5 | 28.4 | 11.7 | 27.2 | 17.1 | 35.6 |

**Table 10. DC Enrollment in the BOG versus NOG Samples from 1998-2006**

| | A. All | | B. Black | | C. White | |
|---|---|---|---|---|---|---|
| BOG | 0.0848 | 0.0853 | 0.1095 | 0.1107 | 0.0800 | 0.0805 |
| | (.0008) | (.0008) | (.0031) | (.0031) | (.0008) | (.0008) |
| BOG * (YR-1998) | 0.0042 | | 0.0071 | | 0.0039 | |
| | (.0001) | | (.0005) | | (.0001) | |
| BOG * (YR-2001) * (YR>=2002) | 0.0096 | | 0.0122 | | 0.0093 | |
| | (.0002) | | (.0007) | | (.0002) | |
| BOG * YR99 | | 0.0036 | | 0.0056 | | 0.0034 |
| | | (.0002) | | (.0007) | | (.0002) |
| BOG * YR00 | | 0.0063 | | 0.0095 | | 0.0060 |
| | | (.0002) | | (.0010) | | (.0002) |
| BOG * YR01 | | 0.0123 | | 0.0203 | | 0.0115 |
| | | (.0003) | | (.0012) | | (.0003) |
| BOG * YR02 | | 0.0254 | | 0.0391 | | 0.0240 |
| | | (.0004) | | (.0016) | | (.0004) |
| BOG * YR03 | | 0.0416 | | 0.0614 | | 0.0395 |
| | | (.0005) | | (.0019) | | (.0005) |
| BOG * YR04 | | 0.0543 | | 0.0787 | | 0.0518 |
| | | (.0005) | | (.0021) | | (.0005) |
| BOG * YR05 | | 0.0674 | | 0.0970 | | 0.0644 |
| | | (.0006) | | (.0023) | | (.0006) |
| BOG * YR06 | | 0.0801 | | 0.1145 | | 0.0767 |
| | | (.0006) | | (.0025) | | (.0006) |
| CONSTANT | 0.0818 | 0.0818 | 0.1172 | 0.1172 | 0.0786 | 0.0786 |
| | (.0006) | (.0006) | (.0025) | (.0025) | (.0006) | (.0006) |
| # OBS | 6,163,060 | 6,163,060 | 576,196 | 576,196 | 5,523,330 | 5,523,330 |
| YOB * Yr Fes? | Yes | Yes | Yes | Yes | Yes | Yes |
| Yrs Included | 1998-2006 | 1998-2006 | 1998-2006 | 1998-2006 | 1998-2006 | 1998-2006 |

**Table 11. Fraction with Zero Earnings in the BOG versus NOG Samples from 1998-2005**

| | A. All | | B. Black | | C. White | |
|---|---|---|---|---|---|---|
| BOG | -0.0206 | -0.0209 | -0.0252 | -0.0273 | -0.0230 | -0.0232 |
| | (.0013) | (.0013) | (.0046) | (.0047) | (.0013) | (.0013) |
| BOG * (YR-1998) | 0.0007 | | -0.0005 | | 0.0008 | |
| | (.0003) | | (.0011) | | (.0003) | |
| BOG * (YR-2001) | 0.0026 | | 0.0061 | | 0.0022 | |
| * (YR>=2002) | (.0004) | | (.0016) | | (.0005) | |
| BOG * YR99 | | 0.0015 | | 0.0040 | | 0.0012 |
| | | (.0006) | | (.0023) | | (.0006) |
| BOG * YR00 | | 0.0023 | | 0.0027 | | 0.0021 |
| | | (.0008) | | (.0028) | | (.0008) |
| BOG * YR01 | | 0.0032 | | 0.0002 | | 0.0034 |
| | | (.0009) | | (.0031) | | (.0009) |
| BOG * YR02 | | 0.0055 | | 0.0041 | | 0.0054 |
| | | (.0010) | | (.0035) | | (.0010) |
| BOG * YR03 | | 0.0079 | | 0.0130 | | 0.0069 |
| | | (.0010) | | (.0038) | | (.0011) |
| BOG * YR04 | | 0.0123 | | 0.0174 | | 0.0113 |
| | | (.0011) | | (.0041) | | (.0012) |
| BOG * YR05 | | 0.0170 | | 0.0237 | | 0.0158 |
| | | (.0012) | | (.0043) | | (.0013) |
| CONSTANT | 0.2540 | 0.2540 | 0.3618 | 0.3618 | 0.2447 | 0.2447 |
| | (.0010) | (.0010) | (.0039) | (.0039) | (.0011) | (.0011) |
| # OBS | 5,481,677 | 5,481,677 | 512,913 | 512,913 | 4,912,213 | 4,912,213 |
| YOB * Yr Fes? | Yes | Yes | Yes | Yes | Yes | Yes |
| Yrs Included | 1998-2005 | 1998-2005 | 1998-2005 | 1998-2005 | 1998-2005 | 1998-2005 |

**Table 12. Fraction on SSDI or SSI in the BOG versus NOG Samples from 1998-2005**

| | A. All | | | B. Black | | | C. White | | |
|---|---|---|---|---|---|---|---|---|---|
| | SSDI | SSI | OASI | SSDI | SSI | OASI | SSDI | SSI | OASI |
| BOG | -0.0058 | -0.0054 | -0.00010 | 0.0003 | -0.0120 | -0.00034 | -0.0074 | -0.0051 | -0.00010 |
| | (.0007) | (.0003) | (.0001) | (.0030) | (.0014) | (.00023) | (.0008) | (.0003) | (.00004) |
| BOG * YR99 | 0.0003 | -0.0001 | 0.00003 | 0.0007 | 0.0003 | 0.00004 | 0.0002 | -0.0001 | 0.00002 |
| | (.0003) | (.0002) | (.00002) | (.0011) | (.0010) | (.00010) | (.0003) | (.0002) | (.00002) |
| BOG * YR00 | 0.0004 | -0.0008 | 0.00006 | 0.0012 | -0.0010 | 0.00002 | 0.0003 | -0.0007 | 0.00006 |
| | (.0004) | (.0002) | (.00003) | (.0015) | (.0012) | (.00014) | (.0004) | (.0002) | (.00003) |
| BOG * YR01 | 0.0009 | -0.0014 | 0.00005 | 0.0036 | -0.0024 | -0.00013 | 0.0004 | -0.0013 | 0.00006 |
| | (.0005) | (.0003) | (.00003) | (.0018) | (.0013) | (.00017) | (.0005) | (.0003) | (.00003) |
| BOG * YR02 | 0.0015 | -0.0015 | 0.00006 | 0.0095 | -0.0001 | 0.00011 | 0.0005 | -0.0016 | 0.00004 |
| | (.0005) | (.0003) | (.00004) | (.0022) | (.0014) | (.00021) | (.0006) | (.0003) | (.00004) |
| BOG * YR03 | 0.0019 | -0.0023 | 0.00028 | 0.0111 | -0.0037 | 0.00069 | 0.0007 | -0.0021 | 0.00016 |
| | (.0006) | (.0003) | (.00010) | (.0025) | (.0016) | (.00033) | (.0006) | (.0003) | (.00010) |
| BOG * YR04 | 0.0023 | -0.0025 | 0.00025 | 0.0120 | -0.0036 | -0.00006 | 0.0010 | -0.0023 | 0.00015 |
| | (.0007) | (.0003) | (.00021) | (.0028) | (.0016) | (.00061) | (.0007) | (.0003) | (.00022) |
| BOG * YR05 | 0.0020 | -0.0030 | -0.00223 | 0.0129 | -0.0040 | -0.00057 | 0.0005 | -0.0028 | -0.00260 |
| | (.0008) | (.0003) | (.00038) | (.0030) | (.0016) | (.00110) | (.0008) | (.0003) | (.00040) |
| CONSTANT | 0.0727 | 0.0134 | 0.00391 | 0.1097 | 0.0314 | 0.00468 | 0.0698 | 0.0118 | 0.00388 |
| | (.0007) | (.0002) | (.00006) | (.0026) | (.0011) | (.00025) | (.0007) | (.0002) | (.00007) |
| # OBS | 5,481,677 | 5,481,677 | 5,481,677 | 512,913 | 512,913 | 512,913 | 4,912,213 | 4,912,213 | 4,912,213 |
| YOB * Yr Fes? | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Yrs Included | 1998-2005 | 1998-2005 | 1998-2005 | 1998-2005 | 1998-2005 | 1998-2005 | 1998-2005 | 1998-2005 | 1998-2005 |

**Appendix Table 1. Verification Rate in the NOG versus BOG Samples**

| | NOG Sample | | BOG Sample | |
|---|---|---|---|---|
| Start Year | % Verified | # Verified | % Verified | # Verified |
| <=1944 | 71.3 | 6,977 | 91.6 | 8,240 |
| 1945 | 63.1 | 1,323 | 92.0 | 2,824 |
| 1946 | 67.1 | 2,062 | 93.5 | 3,971 |
| 1947 | 64.8 | 2,028 | 91.7 | 3,775 |
| 1948 | 62.2 | 3,761 | 90.4 | 6,024 |
| 1949 | 65.3 | 3,849 | 87.9 | 4,816 |
| 1950 | 44.6 | 5,401 | 83.9 | 8,327 |
| 1951 | 43.4 | 4,280 | 84.5 | 13,634 |
| 1952 | 44.6 | 1,927 | 95.0 | 13,195 |
| 1953 | 34.2 | 1,986 | 96.7 | 15,688 |
| 1954 | 38.0 | 1,524 | 97.5 | 13,452 |
| 1955 | 37.4 | 1,809 | 97.7 | 13,024 |
| 1956 | 41.1 | 1,779 | 96.9 | 12,205 |
| 1957 | 51.4 | 1,782 | 97.4 | 12,204 |
| 1958 | 51.5 | 2,487 | 97.1 | 15,291 |
| 1959 | 63.9 | 2,261 | 96.5 | 14,871 |
| 1960 | 77.6 | 2,349 | 96.8 | 14,302 |
| 1961 | 75.5 | 4,021 | 95.7 | 16,135 |
| 1962 | 69.2 | 4,366 | 93.9 | 15,786 |
| 1963 | 67.1 | 4,788 | 93.7 | 15,993 |
| 1964 | 63.3 | 6,384 | 93.5 | 18,496 |
| 1965 | 62.6 | 9,738 | 92.1 | 27,107 |
| 1966 | 74.5 | 38,327 | 94.3 | 89,320 |
| 1967 | 64.4 | 97,691 | 81.1 | 175,680 |
| 1968 | 55.2 | 197,425 | 68.9 | 265,203 |
| 1969 | 17.3 | 200,250 | 53.9 | 265,255 |
| 1970 | 11.7 | 163,743 | 96.2 | 150,876 |
| 1971 | 8.1 | 191,839 | 97.3 | 61,039 |
| 1972 | 8.9 | 215,119 | 97.7 | 15,215 |
| 1973 | 12.7 | 144,853 | 95.7 | 3,100 |
| 1974 | 12.0 | 187,491 | 93.4 | 1,771 |
| >=1975 | 69.2 | 1,085,148 | 95.5 | 5,459 |
| Missing | 18.2 | 1,278 | 98.0 | 197,952 |
| Total | 43.4 | 2,600,046 | 81.9 | 1,500,230 |