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The Impact of Disability Benefits on Labor Supply: Evidence for the VA's Disability Compensation Program

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Abstract

We analyze the labor market effects of the U.S. Department of Veterans Affairs' Disability Compensation (DC) program. The largely unstudied DC program currently provides income and health insurance to approximately four million veterans of military service who have service-connected disabilities. 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 effects of DC eligibility by contrasting the outcomes of BOG and NOG veterans. Our results indicate that the policy-induced increase in DC enrollment reduced labor force participation by 18 percentage points among BOG veterans who enrolled in the DC program as a result of the policy change. We also find evidence of program spillovers, with DC recipients significantly more likely to qualify for Social Security Disability Insurance benefits.

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Introduction

This paper investigates the effect of the Department of Veterans Affairs' (VA's) Disability Compensation program on the labor supply of military veterans. We focus on a major legislative change that took effect in 2001, which generated a plausibly exogenous increase in the generosity of disability benefits for one group of Vietnam Era veterans but not another.[1] Specifically, in July of 2001, the VA expanded the medical eligibility criteria for Vietnam veterans who served in the Vietnam theatre (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. Adopting the terminology used by the military, we distinguish among 'boots on the ground' (BOG) Vietnam era veterans—the veterans directly affected by this policy—with 'not on ground' (NOG) veterans, who were not.

The 2001 policy change coincided with a sharp acceleration in the number of veterans receiving DC benefits as shown in Figure 1A. Some of this overall increase was attributable to a much higher rate of DC enrollment among veterans serving in the 1990s and 2000s than among their counterparts from earlier service eras.[2] But much of it was driven by the rise in DC enrollment among Vietnam era veterans. As shown in Figure 2, the fraction of Vietnam era veterans receiving DC benefits had been trending up gradually prior to the 2001 policy change so that 9.3 percent of Vietnam-era veterans received DC benefits in that year. But there was a

[1] 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.

[2] Veterans from the Gulf War and Global War on Terror are 2-3 times more likely than veterans from WWII or the Korean War era to receive DC benefits. Shifts in the composition of veterans (as those from older eras die and the recent era join the ranks) have contributed to a substantial increase in total DC enrollment.

significant break in that trend after 2001 so that by 2013 more than 18 percent of Vietnam era veterans were receiving DC benefits. No similar changes in rates of DC enrollment occurred for veterans from other service eras.

The policy-induced increase in DC enrollment provides a unique opportunity to estimate the effect of disability benefits on the labor supply of near elderly veterans, virtually all of whom are men. To do this, we utilize administrative data for a sample of more than 4 million U.S. Army veterans to compare the evolution of labor market outcomes among BOG veterans to other Vietnam era veterans who did not serve in the Vietnam theatre during the conflict there. One advantage of our approach is that – by using other Vietnam era veterans as our comparison group - we account for the possibility that veterans would have retired sooner (or later) than non-veterans for reasons unrelated to the DC program. And given the large amount of pre-2001 data that we have, we can control for possible differential trends between BOG and NOG veterans.

Our results demonstrate that prior to 2001 – DC enrollment was increasing gradually among both BOG and NOG veterans. Enrollment growth was somewhat higher among the BOG than among the NOG sample though trends for both groups were relatively stable. However, after 2001, the rate of DC enrollment grew much more rapidly among BOG veterans, as shown in Figure 3. By 2006, almost 1-in-4 BOG veterans in our analysis sample were receiving DC benefits versus a rate of just 1-in-12 among veterans in our NOG sample. This break in trend was primarily driven by a sharp increase in the number of diabetes awards to BOG veterans as shown in Figures 4 and 5. Interestingly, DC enrollment growth among the BOG shows essentially no break in trend when one excludes DC recipients with a diabetes diagnosis, as shown in Figure 6.

We use this policy-induced change in enrollment to estimate the effect of the DC program on the labor supply of near-elderly Vietnam veterans. Our results demonstrate that labor force

participation (defined as having strictly positive earnings for the year in our administrative data) declines sharply among BOG relative to NOG veterans soon after the 2001 policy change. The results are similar for younger and older Vietnam veterans in our analysis sample, suggesting it reflects an effect of the policy rather than the effect of health differences or some other factor. For every 100 individuals newly receiving DC benefits as a result of the policy change, we estimate that 18 drop out of the labor force. Part of this effect likely reflects an effect on individuals who were already receiving DC in 2001, as additional conditions can lead to increases in benefits given that benefits are an increasing function of a veteran's combined disability rating (CDR). We also uncover evidence of spillovers to other federal programs. Most notably, we estimate a significant increase in SSDI enrollment among BOG veterans after 2001.

Taken together, our results demonstrate that the DC program led to a substantial reduction in labor supply among Vietnam veterans. These effects likely reflect a combination of income and substitution effects. For the vast majority of DC recipients, there are no explicit disincentives to work. However, for nearly 10 percent of BOG veterans who were receiving DC by the end of our study period, benefits are increased to the maximum amount because the recipient is considered to be unable to work. Thus the effects that we estimate are unlikely to be driven solely by income effects.

Our results take on additional significance when one considers the growth in enrollment in the VA's Disability Compensation program shown in Figure 1A and annual program expenditures that now exceed $50 billion. Today's veterans are substantially more likely than their counterparts from earlier eras to receive DC benefits. Consistent with that, veterans who served since 2001 are more than twice as likely today as Vietnam era veterans were to receive benefits soon after their service.

The paper proceeds as follows. We begin in Section I by detailing the construction of our data, which are used in Section II to describe the operation and growth of the Veterans Disability Compensation program. Section III discusses the financial and labor force participation incentives created by the program and considers potential interactions between DC and other federal benefits programs including Social Security Disability Insurance (SSDI), Supplemental Security Income (SSI), and Social Security retirement benefits (OASDI). The analysis of the Agent Orange decision begins in Sections IV, which assesses the impact of the policy change on enrollment in DC benefits, while section V assesses the impacts that this policy had on the employment and earnings of Vietnam-era veterans. Section VI concludes.

I. Data and Analysis Sample

To estimate the effect of the Disability Compensation program on veterans' labor supply and receipt of Social Security benefits, we draw on four administrative data sources assembled for this research. 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 million veterans. This sample represents a near census of veterans who left the Army between 1968 and 1985. OEMA constructed the dataset from two Defense Manpower Data Center (DMDC) files. The first DMDC file contained information on essentially every person who left the Army between 1968 and 1985 (designated as the service member's "loss year"). OEMA then merged this "loss year" file with DMDC's Vietnam file, which identified the vast majority of veterans who served in the Vietnam theatre and had a loss year of 1968 or later.[3]

[3] Thus, the file does not include the comparatively small number of Army service members who died during service. U.S. government archives record 38,224 Army service members who were killed in action in Vietnam, relative to roughly 1.5 million Army soldiers who served in theatre.

Approximately 36 percent of the sample served in Vietnam, Cambodia, or Laos during the Vietnam War era, which according to the U.S. Department of Veterans Affairs stretched from August 1964 to May 1975. We refer to this group of veterans as those who had "boots on the ground" (BOG) in Vietnam and who would therefore be directly affected by the 2001 expansion of the DC program's medical eligibility criteria. Their counterparts did not have boots on the ground (NOG) during the Vietnam War era.[4]

Table 1 lists the distribution of the "loss year" (the year that an individual in our sample left the U.S. Army) separately for each group in our sample. Because our sample only includes veterans with loss years between 1968 and 1985, our sample will tend not to include veterans who served in the earlier part of the Vietnam Era.[5] Table 2 provides the distribution for the year-of-birth and the start year (the first year of service in the Army) for both samples. More than 35 percent of the NOG sample had a start year of 1976 or later and thus did not serve during the Vietnam War era.

The OEMA data set was linked using veterans' social security numbers, last names, and dates of birth to the National Center for Health Statistics (NCHS) Master Death File, which includes the year of death for any individuals in the sample who died in 2006 or earlier. According to the NCHS data, approximately 11 percent of the 4.1 million individuals in the sample were deceased by late 2006. The OEMA sample was also linked 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 through 2006.

[4] Veterans who were in the "loss year" file and the Vietnam file are in the BOG sample while veterans who were in the "loss year" file but not the Vietnam file are part of the NOG sample. The Vietnam file also had loss information, which explains why some BOG veterans have loss years before 1968 and after 1985.

[5] As Table 1 shows, a small fraction of our BOG sample has a loss year before 1968 or after 1985. These individuals appeared only in the Vietnam file and do not end up in our final analysis sample below.

Finally, the OEMA sample was linked to a data set constructed by the Social Security Administration (SSA) that included information on earnings and Social Security benefits in each year from 1976 through 2007. 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 93.1 percent of the 4.1 million veterans in the full sample. Appendix Table 1 shows the variation in the match rate across start years, and lists these rates separately for the BOG and NOG samples. As the table shows, the overall match rates exceeded 90 percent for both samples.

Confidentiality rules required SSA to group individuals into cells of 5 to 9 observations so that no individual-level earnings could be observed. In doing this, we grouped together individuals with similar background characteristics, such as gender, race, BOG / NOG, and year of birth. 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 for each year on the cell-specific mean and median of labor earnings and the fraction receiving Social Security Disability Insurance and other SSA benefits. We also have data on the number with zero earnings in each cell in every year between 1976 and 2007.

A. Construction of the Analysis Sample

To investigate the effect of the VA's Disability Compensation program on the labor supply and receipt of Social Security benefits among U.S. Army veterans, we begin by constructing comparable samples of BOG and NOG veterans. As is visible in Tables 1 and 2, 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, and thus tended to enter and

leave the military much later. For example, more than one-third of the NOG sample entered the military in 1976 or later and thus are not Vietnam era veterans.

An examination of Table 2 reveals that the number of BOG veterans in our sample is largest in the 1966 through 1971 start years. Veterans who began their service after 1971 tended not to serve in the Vietnam theater while those entering before 1966 are much less likely to be included in our sample (because their loss year would often have been outside of our sample range). We therefore restrict to veterans (both BOG and NOG) with a start year between 1966 and 1971 inclusive, which reduces our sample from 4.085 million veterans to 1.887 million veterans. We then further restrict attention to individuals born between 1946 and 1951 inclusive, with 1.478 million veterans remaining. Of this group, the Social Security Administration was unable to match to earnings data for approximately 7.3 percent, which results in a sample of 1.371 million individuals. And finally, we drop an additional 1.8 percent of the sample with a missing loss year or with missing race information. Our final sample includes 1.346 million veterans of the U.S. Army who began their service between 1966 and 1971 and were born between 1946 and 1951.

B. Characteristics of the BOG and NOG Samples

The first two columns of Table 3 provide summary statistics for veterans in the BOG and NOG samples. As the table indicates, the two groups are similar in many ways. The fraction nonwhite is approximately equal in the two samples (11.2 and 11.6 percent, respectively) as is the fraction with positive earnings in 1998 (84.4 and 85.2 percent). Among those with a non-missing AFQT, the average scores are also relatively close (52.1 and 53.4). And by construction, the average year-of-birth and start year is comparable in the two groups.

There are some clear differences between the BOG and NOG samples as well. Most notably,

BOG veterans are more than twice as likely to be receiving DC benefits in 2000 (13.4 versus 6.3 percent) just prior to the 2001 policy change described above. This presumably at least partially reflects the greater toll that military service took on those who served in the Vietnam theatre.[6] Additionally members of the NOG sample are more likely to be missing data on education and less likely to be missing AFQT score data.

Despite these differences, an examination of the trends in the outcome variables of interest prior to the 2001 policy change suggests that they were similar for the two groups. The fraction with zero earnings increased by similar, though not identical, amounts for both samples from 1998 to 2000 (1.5 and 1.1 pct points for BOG and NOG samples, respectively) as did the fraction receiving SSDI benefits (1.2 and 0.9 pct points for BOG and NOG samples, respectively). To the extent that there are differential trends on any outcome variable of interest that preceded the policy change, we will control for these trends in our empirical analyses.

One concern with the analysis sample as currently constructed is that individuals who verify in the SSA data may systematically differ from those who do not. To explore this issue, we provide summary statistics in the next two columns of Table 3 for the BOG and NOG samples that result if we do not restrict to the SSA verified sample. The patterns there are fairly similar, with the largest difference for the fraction of veterans in each group who have died by 1997. The larger difference in mortality by 1997 for the SSA-verified sample is largely a function of our matching methodology, which relies on accurate name information. Due to a high rate of garbled name data in the NOG data, we contracted with TransUnion, a credit information provider, to obtain names for those with incomplete information. Unfortunately, TransUnion could did not have name data for most sample members who were deceased as of 1997. Since a larger fraction

[6] Additionally BOG veterans are more likely to have died by 1997 (4.8 versus 2.9 percent). But the rates are much more similar (6.2 and 6.0 percent) if one also considers those who did not match in the SSA data as well, as shown in the final two columns of this same table.

of NOG soldiers (including those who were deceased) had garbled name data, we obtain a lower rate of SSA records matching for deceased soldiers in the NOG sample. While the rates are quite different in the SSA-verified samples (4.8 and 2.9 percent for BOG and NOG, respectively), they are almost identical (6.2 and 6.0 percent) when we do not condition on SSA verification. It is also noteworthy that the fraction of veterans who die between 1997 and 2006 in the BOG and NOG samples is very similar whether we condition on SSA verification or not.

C. Comparison of OEMA data with Census data

As a further benchmark for the reliability of the sample, we compare the OEMA data with a 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 1946 and 1951, and further limit the sample to (self-reported) Vietnam-era veterans. The Census data do not allow us to distinguish among veterans according to their branch of military service. To the extent that Army veterans are different from their counterparts serving in the Navy, Air Force, Marines, or Coast Guard, we would expect some differences between the Census and OEMA samples.

Table 4 provides a side-by-side comparison of these samples, focusing on age, race, schooling, annual earnings, and share with non-zero earnings. The samples appear similar along the dimensions of race and labor force participation rates. For example, the fraction of our sample with non-zero earnings is 84.1 percent and for the Census sample is 82.2 percent while the comparable values for percent nonwhite are 11.2 percent and 13.3 percent, respectively. 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 difference is that average earnings in our sample are about 10 percent lower than in the Census sample. This difference and others that are apparent in the table could reflect differences between Army veterans and veterans from other branches of the military. It is also possible that SSA earnings do not capture all earnings sources, including data from self-employment and in non-covered work. Overall, our comparison of OEMA and Census data provides some assurance that the OEMA sample is representative of the target population of Vietnam era veterans, measured in terms of age, race, labor force participation and earnings.

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

In this section, we detail the structure of the Veterans Disability Compensation program, and draw upon the analysis sample described above to illustrate key points. The Department of Veterans Affairs' Disability Compensation (DC) program pays cash benefits and provides prioritized access to Veterans Administration health facilities to military veterans with medical conditions that are service-connected, meaning that they are caused or aggravated by their military service. Since the ratification of the U.S. Constitution in 1789, the federal government has provided cash benefits to disabled veterans. During the Civil War, these benefits were revised to be an increasing function of disability severity. Veterans' benefits were administered by multiple agencies until the summer of 1930, when they were consolidated under a new federal agency called the Veterans' Administration (VA).[7]

The VA oversaw a substantial increase in Disability Compensation (DC) enrollment during and in the years immediately following World War II, with 0.4 million DC recipients in 1940 rising to 2.0 million by 1950. As shown in Figure 1A, the number of DC recipients remained

[7] This was changed to the Department of Veterans Affairs in 1989.

relatively stable during the next fifty years.[8] Figure 1B reveals that the fraction of veterans receiving DC benefits declined from 10.5 percent in 1950 (the first year of the Korea War era) to 8 percent by 1964 (the first year of the Vietnam War era), though during the next 35 years remained between 7.5 percent and 9.0 percent.

There was, however, a sharp break in trend for both total enrollment and enrollment as a share of all veterans in 2001. The number of DC recipients increased by approximately 70 percent from 2.3 million in 2001 to 3.9 million by 2014. Because the number of veterans was declining during this period, the increase in the share of veterans enrolled in DC was even more striking, more than doubling from 8.9 percent in 2001 to 18.0 percent by 2014.

Average monthly benefits for DC recipients during the 2013 fiscal year were $1,105, which is quite similar to the average of $1,146 for disabled workers receiving SSDI benefits. Total DC expenditures in that same year stood at $49.1 billion versus $20.8 billion in 2001 (in 2013 dollars). The much larger increase in program expenditures than in program enrollment (136 percent versus 70 percent) reflects the fact that the average CDR of DC recipients steadily increased during this period. Despite a 16 percent decline in the number of veterans from 2001 to 2013, real annual expenditures on the DC program per living veteran, inclusive of both DC beneficiaries and non-beneficiaries, increased by 180 percent (from $798 to $2,234).[9]

For the vast majority of DC recipients, the program does not explicitly reduce the incentive to work. But as we detail below, subtle incentives in the DC program suggest that caution is warranted in concluding that it generates exclusively income and not incentive effects.

[8] As World War I and World War II veterans left the DC rolls (almost always because of death), they were replaced in approximately equal numbers by veterans from subsequent service eras.

[9] In 2013, total DC benefits payments were $49.15 billion and the estimated veteran population was 22.149 million (U.S. Veterans Benefits Administration, 2013). In 2001, total DC benefit payments were $20.8 billion in 2013 dollars and the estimated veteran population was 26.063 million (U.S. Veterans Benefits Administration, 2001).

A. The application process

To apply for DC benefits, a veteran submits an application to one of 56 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—including SSDI and SSI—that classify disability using a categorical, all-or-nothing determination, the DC program rates disability on a discrete scale with eleven gradations. The 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.[10] If the recipient receives ratings for multiple disabilities, the recipient's Combined Disability Rating (CDR) is an increasing, concave function of the individual ratings, where concavity prevents the combined rating from exceeding 100 percent.[11]

During the 2000 fiscal year, more than 70 percent of those applying for DC sought 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 that same year, 14 percent of applicants received awards for all conditions

[10] 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 0 percent rating would not increase the monthly cash benefit but would entitle the veteran to priority for health care through the Veterans Health Administration.

[11] 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.

claimed, 48 percent received awards for some conditions, and 38 percent were rejected entirely (VBA, 2001).[12] 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. In 2007, a 10 percent award provided a monthly payment of $117 whereas a 100 percent award provided a monthly payment of $2,527.[13] Veterans receiving a CDR of 30 or higher and who have spouses, dependent children, or surviving parents also receive modest additional benefits.[14] 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 even if their CDR is less than 100 percent.

Table 5 summarizes DC cash benefits paid in fiscal year 2006 (the final year for which we have individual-level DC data in the analyses below). The first three columns list the count of recipients, the total dollars paid, and the average monthly benefit in each CDR category at the

[12] 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.
[13] The payment schedule in 2007 for veterans without dependents, spouse or surviving parents was (in order of increasing CDR from 0% to 100% in 10% increments): $0, $117, $230, $356, $512, $728, $921, $1,161, $1,349, $1,517, and $2,527.
[14] 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.

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 population and received 62 percent of the benefits payments.[15]

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.[16] 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, 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

[15] 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.

[16] 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.

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. Indeed, unlike federal SSDI benefits, DC benefits do not terminate when a recipient reaches retirement age—even for recipients receiving the Individual Unemployability (IU) benefit. Finally, with the exception of the six percent of DC recipients who have an IU rating, 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.[17]

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

[17] 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).

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 any of the five major service eras.[18]

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 that developed type II diabetes and was covered under the policy and 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

[18] 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.

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.

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. 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

veterans—that is, those who were not in-theatre.[19] 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 in addition to 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 financially significant Disability Compensation benefits. Veterans of the same service era who did not serve with boots on the ground were not granted similar eligibility.

III. Incentives under the DC Program and Spillovers to other Federal Programs

Distinct from other federal disability benefits programs, the graduated scale of DC disability ratings creates a complex set of incentives. One is that veterans face an incentive to repeatedly reapply to increase their CDRs—and therefore their benefits—as their health conditions evolve. Veterans' CDRs tend to increase accordingly in the several years following the initial award.[20] This pattern is evident 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.[21]After initial DC enrollment, veterans' CDRs and benefit levels rise steeply. Veterans enrolling in DC in 1999 received an average CDR of 40 and a mean monthly benefit of $898 (in 2013 dollars). By 2006, the surviving veterans of this enrollment cohort had a mean CDR of 58

[19] 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).
[20] 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.
[21] Although our data codes DC receipt in each year from 1998 through 2006, 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.

and mean monthly benefits of $1,652, 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 2006, and their mean monthly benefits rose from $801 to $1,429.

A. Non-work incentives

Because benefit levels tend to rise once veterans are enrolled in DC, 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. This 'escalator' aspect of the evolution of DC benefits may also serve to discourage labor force participation. Though disability ratings for DC recipients notionally depend exclusively on medical criteria rather than employment status, veterans may nevertheless perceive that their disabilities will receive higher ratings—yielding larger cash payments—if they are not employed when applying to obtain or increase benefits.

The potential disincentive of DC enrollment on labor force participation is likely amplified by an additional feature of the program, the Individual Unemployability benefit. As noted above, the IU 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 due to service-connected disabilities.[22] The IU benefit is of substantial monetary value. A 2006 General Accounting Office report found that the present discounted 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

[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

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 all but the most disabled veterans from qualifying, the data displayed in Table 6 indicate that very high CDRs are not uncommon, even for veterans that initially enter with low or moderate CDRs. For example, among veterans awarded DC benefits in 1999, only 15 percent qualified for either the IU benefit or 100 percent disability (panel C). Seven years later, in 2006, three times that number (45 percent) of the 1999 DC enrollment cohort was either receiving the IU benefit or was 100 percent disabled.[23] As is visible in the table, subsequent cohorts exhibit a comparable rate of progression towards IU status or 100 percent disability. In light of 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.[24]

B. Interactions with other federal benefits programs

One additional channel through 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 apply different disability 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. Receipt of DC benefits may render the SSDI application process less financially onerous as well, since SSDI applicants must wait at least five

[23] 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.

[24] 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 for consideration for the IU benefit. Nevertheless, these veterans might anticipate qualifying for the IU benefit at a later date, and so modify their behavior accordingly.

months (and typically much longer) to qualify for SSDI benefits following disability onset, during which time they may not participate gainfully in the labor force.[25] The attractiveness of SSDI benefits is probably not diminished by receipt of DC since cash benefits from the two programs are additive rather than offsetting.[26] 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 could effectively impart some of the incentive effects in the SSDI program onto the DC program. While the availability of SSDI benefits may implicitly cause 'incentive spillovers' onto the DC program, programmatic spillovers in the opposite direction may be even more important. These spillovers may operate through three distinct 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.[27]

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, DC benefits

[25] 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. Receipt of DC is likely to blunt this deterrent effect since DC enrollees will receive cash benefits and VA healthcare during (and after) the SSDI application.
[26] The combination of VA health benefits from DC and Medicare benefits from SSDI may also be more attractive than either program alone since VA and Medicare differ in ailments covered, rapidity of access to treatment, size of co-pays, and coverage of prescription drugs.
[27] 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 DC awards will lead to further spillovers onto SSDI.

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 (CDR) would receive $2,393 monthly in untaxed DC income, and additional supplements for dependent children or a spouse. Similarly, a DC 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.[28] Even those with lower CDRs who qualify for SSI might see a reduction in their SSI benefit following a DC award.

IV. The Impact of the Agent Orange Decision on DC Enrollment

We now analyze the consequences of the 2001 Agent Orange decision on the benefits receipt and labor force participation of Vietnam-era veterans, using the contrast between BOG and NOG veterans as a source of quasi-experimental variation. Figure 3 plots the fraction of BOG and NOG sample members receiving DC benefits in September of each year from 1998 through 2006. As this figure makes clear, there was a rapid relative increase in DC enrollment for the BOG sample following the 2001 policy change: the fraction of the BOG sample receiving DC benefits increased from 14.3 to 23.0 percent versus an increase from just 6.5 to 7.6 percent for the NOG sample.. The data summarized in Table 7 also indicate that prior to the Agent Orange change, DC enrollment was rising somewhat more rapidly among BOG than NOG veterans. But while the trend for NOG veterans remained relatively steady throughout the 1998 through 2006 period, this trend accelerated substantially after 2000 among BOG veterans, from 0.4 to 1.5

[28] "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.

percentage points annually.[29]

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 fact, an examination of the trends in DC enrollment in Table 7 reveals substantial differences by race in the BOG sample, with DC enrollment increasing about 50 percent more among nonwhites than among whites from 2000 to 2006 (12.1 versus 8.3 percentage points).

Of course, these raw differences in DC enrollment trends between BOG and NOG veterans may reflect differences in veteran characteristics as well as policy changes that may differentially affect their DC enrollment. To account for these factors, we estimate a set of OLS models that regress DC enrollment on a full set of controls for veterans' year-of-birth, race, and AFQT score quintile. For consistency with our later analyses of labor market outcomes, we group veterans into the same cells used for those measures. Each cell includes data for between 5 and 9 veterans and we weight each cell-year by the number of observations when estimating specifications of the following type for the 1998 to 2006 period:[30]

$$(1) \qquad Y_{jt} = \alpha_t + \gamma_0 \times \mathrm{BOG}_j + \sum_{1999}^{2006} \gamma_t \times \mathrm{BOG}_j + X'_{jt}\beta_t + \varepsilon_{jt}$$

In this specification, the variable Y_{jt} represents the fraction of cell j enrolled in the DC program in September of year t, while BOG_j is an indicator variable that is set equal to one if veterans in

[29] The policy change took effect in July of 2001 and we measure DC enrollment in September. Thus while our 2001 data is arguably more "pre" than "post", it is likely that the policy change did contribute to DC enrollment growth between 2000 and 2001.

[30] A veteran who dies in year *t* is dropped from the analysis sample in year *t* and in all subsequent years. Because year-of-death is one of the variables that we use when constructing cells, typically either all members of a cell die in year *t* or none die in that year. The number of surviving cells falls by approximately 4.2 percent from 1998 to 2006.

cell j are in the BOG sample and is otherwise equal to zero (cells include either all BOG or all NOG veterans). The term α_t is a vector of nine indicator variables for each year considered, and X_{jt} is a vector set of 14 variables corresponding to the possible values of year-of-birth, AFQT quintile, and race.[31] These interactions account for differential levels or growth rates in DC enrollment by age, race, or AFQT level during the 1998 through 2006 period. The coefficient γ_0 corresponds to the (conditional) baseline DC enrollment gap between BOG and NOG veterans in in the base year of 1998, while the coefficient vector γ_t estimates the *difference* in this gap in each subsequent year 1999 through 2006 relative to the 1998 level.

The statistically significant estimate of 0.0696 for γ_0 in the first column implies a 7.0 percentage point difference in DC enrollment between the BOG and NOG samples in the baseline year (1998) after controlling for race, year-of-birth, and AFQT quintile. This is quite similar to the unconditional estimated difference of 7.2 percentage points in that same year in Table 7. The next eight rows of the first column display the estimates for γ_t in each year from 1999 through 2006. The statistically significant estimates of .0033 and .0059 for γ_{1999} and γ_{2000} imply that DC enrollment was increasing more rapidly (by 0.3 percentage points annually) for the BOG sample relative to its NOG counterparts prior to the 2001 policy change. Beginning in 2001, these coefficients increase much more rapidly, by about 1.2 percentage points per year, and reach a cumulative differential of 8.0 percentage points by September of 2006.

The next several columns of Table 8A investigate the extent to which the policy-induced increase in DC enrollment varied by year-of-birth, race, and AFQT. Column 2 through 4 split

[31] In our sample there are 6 possible values of YOB (1946 through 1951), 2 possible values of race (white and non-white), and 6 possible values of AFQT quintiles (we group those with a missing AFQT into a sixth category). Given there are nine years of data used in these specifications, we are including 126 interactions. Veterans within each cell have the same year-of-birth and race (by construction) and the vast majority also have the same AFQT quintile. Data for each veteran is included in the specification in each of the 9 years unless they die at some point between 1998 and 2006, in which case they are dropped from the sample. Year-of-death is one of the variables used to construct the cells so this typically results in the entire cell being dropped.

veterans into three cohorts based on year-of-birth: 1946-47, 1948-49, and 1950-51. The point estimates in those columns suggest a slightly larger effect for BOG veterans born from 1948-49, with an 8.0 percentage point increase from 2000 to 2006 compared with 7.3 and 6.3 percentage points, respectively, for the older (1946-47) and younger (1950-51) groups. The next two columns reveal a substantially larger increase for non-white BOG veterans and for BOG veterans with an AFQT score in the bottom two quintiles.[32] We return to this heterogeneity when summarizing differences in the estimated labor supply effects below.

The sharp break in trend for DC enrollment among BOG relative to NOG veterans that is apparent in the year-by-year estimates in Table 8A motivates a more parameterized version of equation (1) where we replace the full set of year-by-BOG interactions with two linear time trends, the first a pre-2001 trend, the second a post-2001 trend *change*, estimated relative to the pre-2001 trend. Specifically, we fit the following model:

$$(2) \qquad Y_{jt} = \alpha_t' + \gamma_0' \times \mathrm{BOG}_j + X_{jt}'\beta_t' + \delta_0 \times \mathrm{BOG}_j \times (t - 2001)$$
$$+\delta_1 \times \mathrm{BOG}_j \times (t - 2001) \times 1[t > 2001] + \varepsilon_{jt}'$$

In this equation, δ_0 captures the pre-existing trend in BOG relative to NOG DC participation just prior to the policy change while δ_1 estimates any change in the BOG relative to NOG trend following the policy. We define 2002 to be the first post-policy year in this specification given that most of the time from September 2000 to September 2001 occurred before the policy took effect in July 2001. For δ_1 to capture the causal effect of the Agent Orange policy change on DC enrollments, we must be willing to assume that the pre-existing trend in the difference in DC enrollment between the BOG and NOG samples would have continued after 2001.

[32] Veterans with a missing AFQT score are omitted from these two specifications.

In this equation, δ_0 captures the pre-existing trend in BOG relative to NOG DC participation just prior to the policy change while δ_1 estimates any change in the BOG relative to NOG trend following the policy. We define 2002 to be the first post-policy year in this specification given that most of the time from September 2000 to September 2001 occurred before the policy took effect in July 2001. For δ_1 to capture the causal effect of the Agent Orange policy change on DC enrollments, we must be willing to assume that the pre-existing trend in the difference in DC enrollment between the BOG and NOG samples would have continued after 2001.

The estimates summarized in Table 8B are quite consistent with those in Table 8A and reveal a significant trend increase in DC enrollment among BOG relative to NOG veterans following the 2001 policy change. The acceleration is largest among BOG veterans who are non-white, have relatively low AFQT scores, and were born from 1946 through 1949. The pre-existing trend in each case is slightly larger and the break in trend is slightly smaller than is suggested by the year-by-year estimates because we treat 2001 as a pure pre-policy year.

Figures 4, 5 and 6 investigate whether the trend break in DC enrollment among BOG relative to NOG veterans is accounted for in substantial part by an increase in diabetes awards. Figures 4 and 5 document a sharp increase in the likelihood that BOG veterans receive compensation for diabetes, either among new and incumbent DC recipients (Figure 4) or among new entrants (Figure 5). Between 2001 and 2006, the fraction of DC enrollees receiving compensation for diabetes among BOG veterans rose from 2.4 to 28.1 percent, an eleven-fold increase (panel D of Table 7). Among NOG veterans, this fraction rose from 1.6 to 6.4 percent, a three-fold increase.[33] Figure 6 shows that there was relatively little change in the fraction of veterans

[33] If diabetes was presumptively service-connected only for Vietnam era veterans who saw service in theatre, why did diabetes compensation also rise among NOG veterans? There are two likely explanations. One is that our data only imperfectly classify BOG and NOG veterans—some veterans who are categorized as NOG may in fact have served in theatre and hence presumptively qualified for service-connectedness for their type II diabetes. A second

receiving DC *without* compensation for diabetes. Thus, a large share of the growth in DC enrollment in this time was proximately accounted for by a sharp rise in DC receipt among BOG veterans receiving compensation for diabetes.

To the extent that the policy-induced increases in DC enrollment also affect labor supply and other outcome variables of interest, one would expect to observe differential change in these outcomes among the BOG relative to the NOG populations beginning in 2001 or soon thereafter.[34] We evaluate this possibility next.

V. The Effect of DC Enrollment on Labor Supply and Social Security Income

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 race and year-of-birth. Additional details on the construction of our cells are provided in the Appendix.

A. The Effects of DC Enrollment on Labor Force Participation

The first labor market outcome we examine is fraction of living veterans with zero labor earnings in each calendar year. We calculate this statistic by dividing the number of veterans with zero earnings in the cell in the calendar year by the number of living individuals in the cell. This yields the fraction of individuals in the cell who were—by this definition—out of the labor

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explanation is that administration of DC benefits appears to be somewhat discretionary. Even prior to the Agent Orange decision in 2000, 1.4 percent of BOG and 0.7 percent of NOG DC recipients were receiving compensation for service-connected diabetes (panel C of Table 7). This source of slippage does not invalidate our identification strategy provided that the post-2001 trend break in DC receipt in BOG versus NOG populations is induced by the Agent Orange Policy change. We view this assumption as plausible.

[34] 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.

force in each year. Using this outcome variable, we estimate models analogous to (1) and (2) to explore whether the fraction of BOG veterans in the labor force declined differentially relative to NOG veterans following the 2001 policy change. Of course, this outcome variable will capture only the effect of the DC program on the extensive margin of labor supply (whether the individual works) and not on intensive margin of hours or earnings per hour. [35]

Tables 9A and 9B summarize the results from equations (1) and (2) for our SSA-verified sample. Reflecting the longer sample window for the SSA outcome data, these specifications use a somewhat larger outcome window (1996 through 2007) than before, with two additional "pre" years and one additional "post" year. As the estimate for γ_0 in the first column of Table 9A reveals, labor force participation in the BOG population was slightly lower (by about 0.3 percentage points) than in the NOG population in the baseline year of 1996. This participation gap was growing in the early part of our sample period, with the estimate for γ_{2001} indicating an increase from 0.3 percentage points in 1996 to 1.1 percentage points as of 2001.

As the pattern of post-2001 coefficients reveals, this difference grew more rapidly during the next several years. Labor force participation declined by 0.32 percentage points more rapidly per year among BOG relative to NOG veterans after 2001, relative to a differential decline of just 0.14 percentage points annually prior to 2001. In net, the BOG labor force participation rate fell by an additional 2.0 percentage points relative to the NOG labor force participation rates between 2001 and 2007, the final year of our study period.

Coupled with the results from Tables 8A and 8B, these findings are consistent with the hypothesis that the differential increase in DC enrollment among BOG veterans induced a

[35] Because we are using linear models for cell level means, the properties of OLS imply that the cell level estimates will be no different from those that we would obtain if cells were instead disaggregated to individual level rows. One exception to this general rule is that 10 percent of the cells in our data have more than one AFQT quintile represented within the cell. In these cases, we assign the cell to the AFQT quintile nearest to the cell's mean AFQT quintile, implying that the micro-level and cell-level regressions will differ slightly in these cases.

differential reduction in their labor supply. One threat to this interpretation, however, is that BOG veterans may have dropped out of the labor force at an increasing rate after 2001 for reasons unrelated to the policy-induced increase in DC enrollment. For example, BOG veterans may on average be in worse health as a result of the greater demands placed on them during their military service, and thus their chosen date of retirement may be earlier as well.

To investigate this possibility, the next three columns of Table 9a divide the sample into three groups, those born in 1946-47, 1948-49, and 1950-51. These groups averaged 54.5, 52.5, and 50.5 years old, respectively, at the time of the July 2001 policy change. To the extent that the LFP patterns described above are driven by differences in health rather than by the 2001 policy change, one would expect to detect a much earlier break in LFP trends for the 1946-47 cohorts than for the 1948-49 or 1950-51 cohorts.

An examination of the results presented in columns (2) and (3) in Table 9A reveals an almost identical pattern of labor force exit among the 1946-47 and the 1948-49 groups. More specifically, the rate of increase in the year-specific estimates grows from about 0.16 percentage points per year from 1996 through 2001 to more than twice that annual amount from 2002 through 2007. If differences in underlying health rather than expansions in DC enrollment were driving the patterns that we observe in column (1), one would expect the trend break to occur two years later for the 1948-49 cohort than for their 1946-47 counterparts. The differences in LFP between the BOG and NOG for the 1950-51 cohort is somewhat smaller, though this is consistent with a lower rate of DC enrollment shown in Tables 8A and 8B. The results in Table 9B are broadly consistent with this pattern: for all three groups the LFP decline among BOG relative to NOG veterans accelerated significantly soon after the 2001 policy change.

We found above that the policy-induced increase in DC enrollment was substantially greater

among nonwhite BOG veterans and among BOG veterans with AFQT scores below the 45th percentile than among NOG veterans with these same characteristics. Consistent with this pattern, Tables 9A and 9B show that differential declines in labor force participation were larger among these groups than among white and high-AFQT veterans. Though these differences in the post-2001 acceleration of labor force decline among demographic groups are modest, they are qualitatively consistent with the patterns of post-2001 DC uptake: the acceleration in labor force withdrawal is largest for nonwhites and low-AFQT BOG veterans, and smallest for high-AFQT and white BOG veterans.

The point estimate for the break in trend in BOG relative to NOG LFP participation after 2001 in the first column of Table 9B is approximately one-fifth as large as the corresponding estimate for DC enrollment in the first column of Table 8B. To the extent that the differential decline in labor force participation is entirely driven by those newly enrolling in the DC program, this suggests that DC enrollment reduces labor force participation rates by approximately 20 percentage points. However, part of this effect may also reflect the policy-induced increases in monthly benefits among existing DC recipients.

B. The Effects of DC Enrollment on Earnings

The estimates above capture changes in labor force participation along the extensive margin only but do not capture changes in earnings among those working. It is plausible that the policy-induced increase in benefits receipt following the Agent Orange decision may also have affected annual earnings among those who remained in the labor force (most likely through a reduction in hours, though out data do not allow us to distinguish between changes in hours worked and earnings per hour). To explore this possibility, we estimate a companion set of specifications in

which the outcome variable is the log of mean cell earnings. As shown in Tables 10A and 10B, earnings were on average several percentage points lower among BOG than NOG veterans in our baseline year, and this gap was increasing modestly through 2001. This differential earnings trend increases sharply after the 2001 policy change, however, and the pattern is once again quite similar across the three sets of birth cohorts. Additionally, the heterogeneity by race and by AFQT category is similar to that for labor force participation, with larger gaps emerging for nonwhites and lower-AFQT veterans.

These estimates for log earnings necessarily condition on cell earnings being non-zero, and this may bias inference regarding overall earnings effects, particularly in light of our finding that LFP declined more rapidly among BOG than NOG veterans after 2001. To (imperfectly) address this limitation of the earnings estimates, Tables 11a and 11b estimate an analogous set of specifications in which the outcome variable is the log of one plus average earnings within the cell-year. As shown in Tables 11A and 11B, the results from these specifications suggest an even larger earnings contrast after 2001.

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

The findings so far suggest that policy-induced increases in DC enrollment may have significantly reduced labor force participation of BOG veterans along both the extensive and intensive margins. These changes in labor force participation may in turn potentially have had spillover effects onto other federal benefits programs, most significantly SSDI. As much previous research has noted, a significant cost that workers face when applying for SSDI or SSI disability benefits is that they may not participate gainfully in the labor force while awaiting a benefits determination. This requirement may deter applicants with some residual work capacity

from applying because for benefits because they must forfeit both potential earnings and the option value of continued labor force participation in pursuit of an uncertain benefit.

Given that many veterans *did* leave the labor force after becoming eligible for DC benefits, this change in labor force status may lower their cost of applying for disability benefits from the SSDI or SSI programs. We would therefore anticipate that the Agent Orange policy might induce additional applications (and ultimately awards) to these federal disability programs.

To examine impacts of the 2001 policy change on enrollment in SSDI and SSI, we estimated specifications analogous to (1) and (2) for the full sample and then separately for subsamples defined by birth cohort, race, and AFQT scores. The results from these specifications are summarized in Tables 12A and 12B (for SSDI) and in Tables 13A and 13B (for SSI). The first column of each table reports results for the full SSA-verified sample.

The results in the first pair of tables demonstrate that SSDI was trending up differentially for BOG relative to NOG veterans prior to the 2001 policy change and that this trend accelerated after 2001. The magnitude of the estimated effect on SSDI is approximately half as large as the corresponding estimate for labor force participation. Results are similar for the 1946-47 and 1948-49 cohorts though slightly smaller for those born in 1950-51. Interestingly the estimated trend break is slightly smaller for nonwhite BOG veterans than for white BOG veterans though the point estimates are imprecise. Taken together, these results for SSDI suggest that part of the labor supply effect of the 2001 policy change may be driven by SSDI enrollment as well.

The pattern of estimates for SSI—the means-tested counterpart of SSDI—goes in exactly the opposite direction. More specifically, SSI enrollment was growing more rapidly among NOG than among BOG veterans prior to 2001 and this trend accelerated after 2001. The likely explanation for this is that the increase in transfer income from the DC program made some

current or potential future SSI recipients ineligible for the program. However, the magnitude of the estimated effect for SSI is only one-tenth as large as the corresponding estimate for SSDI.

VI. Discussion

In this paper, we have provided evidence that the increase in policy-induced increase in enrollment in the VA's Disability Compensation program had a significant effect on the labor supply of Vietnam veterans who had boots on the ground in the Vietnam theatre during the conflict there. More specifically, we estimate that up to twenty percent of individuals who became eligible for the DC program dropped out of the labor force. Analyses that explore the effect on average earnings support the conclusion that DC enrollment substantially lowered labor supply among Vietnam-era veterans who served in the Vietnam theatre during the conflict there.

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 substantially among BOG veterans 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. In contrast, enrollment in the means-tested SSI program actually declined as a result of the rise in enrollment, though the estimated effects are an order of magnitude smaller than for SSDI.

Taken together, our results suggest that the VA's Disability Compensation program has substantially lowered labor force participation and increased SSDI receipt among near-elderly veterans of the Vietnam era. Whether this policy improved the health and economic well-being of this group represents an important topic for future research. And given the rapid increase in DC enrollment among younger veterans, more research that explores the labor supply effects for this group is clearly warranted as well.

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, race, year of birth, 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 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 29 individuals, we do not split further, but instead split the cell into the maximum number of cells with size 5 or more after sorting on the next matching variable to maximize similarity within the cells. If a cell has fewer than 5 individuals then we re-merge it with an adjacent cell. If a cell with fewer than 5 individuals is merged with an adjacent cell to form a new cell with more than 9 individuals, the merged cell is split in two.

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

In Cell of Size	Frequency	Percentage
5	1,388,440	58.86
6	657,696	27.88
7	171,269	7.26
8	70,048	2.97
9	71,271	3.02
Total	2,358,724	100.00

The corresponding distribution for the verified BOG sample is:

In Cell of Size	Frequency	Percentage
5	942,415	65.19
6	328,338	22.71
7	92,785	6.42
8	41,120	2.84
9	40,878	2.83
Total	1,445,536	100.00

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

1. Gender (male or female)
2. Race (white or nonwhite)
3. Death year (e.g. 1985)
4. Year-of-birth (e.g. 1946)
5. Start year (e.g. 1966)
6. Education at entry (e.g. 0, 1, 2, 3, 4 for hsd, hsg, smc, clg, clg-plus)

7. AFQT score quintile (e.g. 2)
8. Loss year (e.g. 1972)
9. Region of residence (e.g. Northeast)

The frequency distribution for the number of variables used in the BOG and NOG cell formation is as follows (thus 206,780 in the BOG matched on variables 1 through and including 8):

Number of Vars	# in NOG	# in BOG
1	0	0
2	89	61
3	3,391	4,149
4	38,087	30,716
5	86,671	62,807
6	80,909	65,140
7	174,281	162,909
8	397,667	206,780
9	1,577,629	912,974
Total	2,358,724	1,445,536

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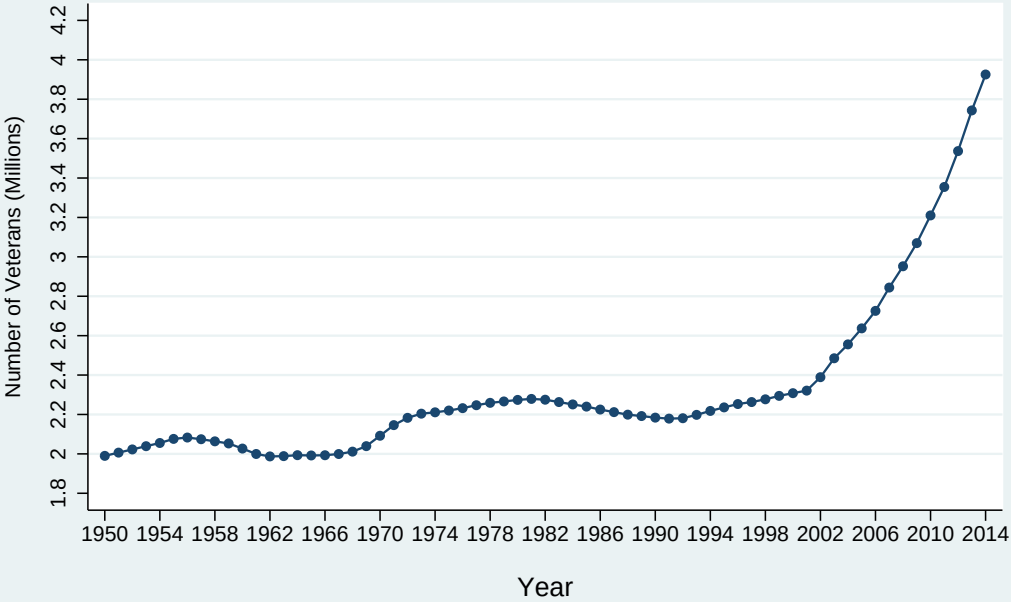
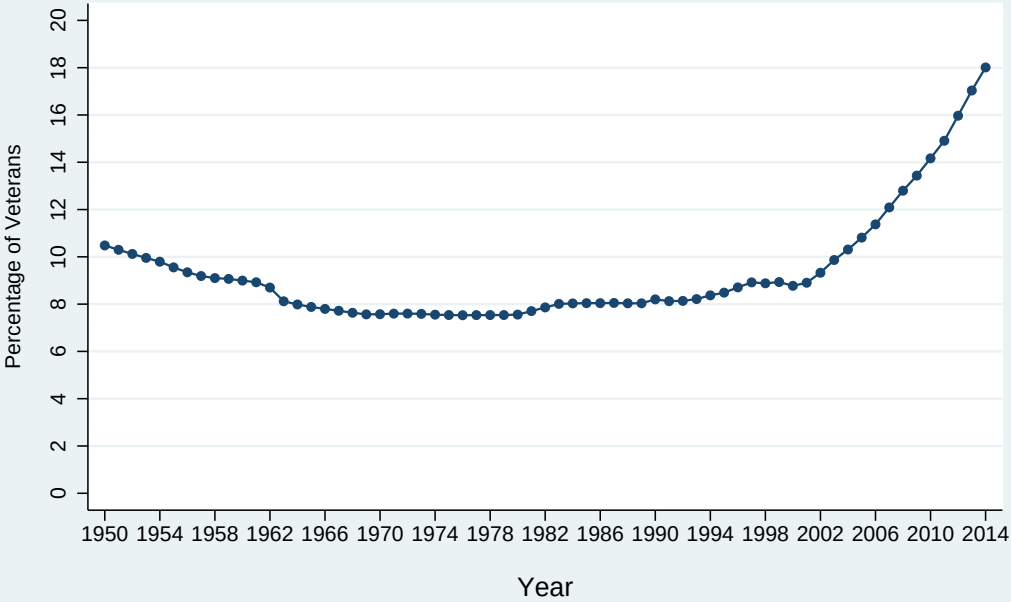
Figure 1A. Total Disability Compensation Enrollment: 1950-2014



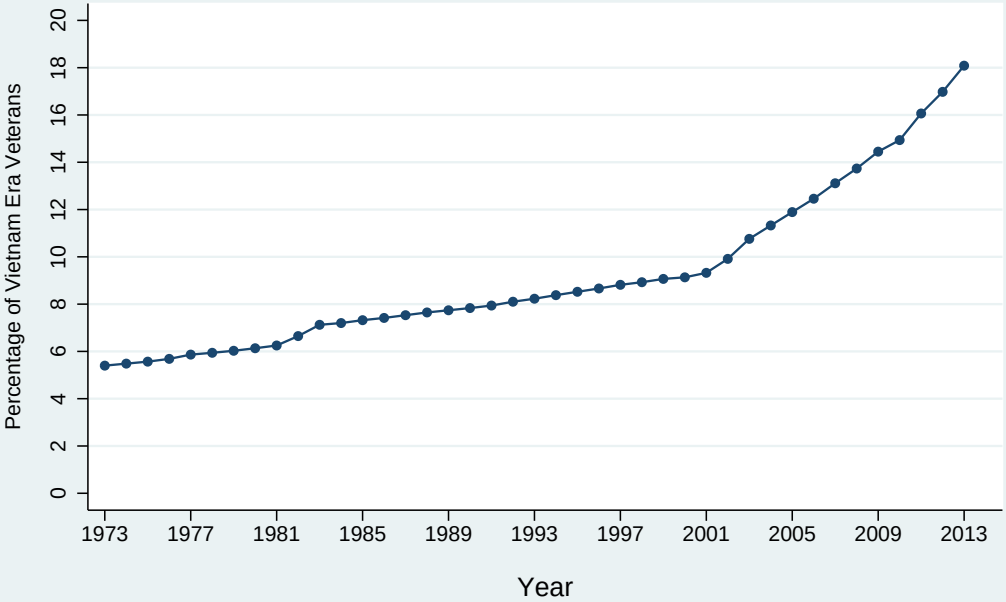
Sources: SSA Annual Statistical Supplement (1950-1998), VBA Annual Benefits Reports (1999-2013), VA Statistics at a Glance (2014)

Figure 1B. DC Enrollment as a Percentage of Veterans: 1950-2014



Sources: SSA Annual Statistical Supplement (1950-1998), VBA Annual Benefits Reports (1999-2013), VA Statistics at a Glance (2014), Veteran Population Projection Model (2011)

Figure 2. Percentage of Vietnam Era Veterans Receiving DC: 1973-2013



Sources: SSA Annual Statistical Supplement (1950-1998), VBA Annual Benefits Reports (1999-2013), VA Statistics at a Glance (2014), Veteran Population Projection Model (2011)

Figure 3. Percentage of BOG/NOG Veterans Enrolled in DC

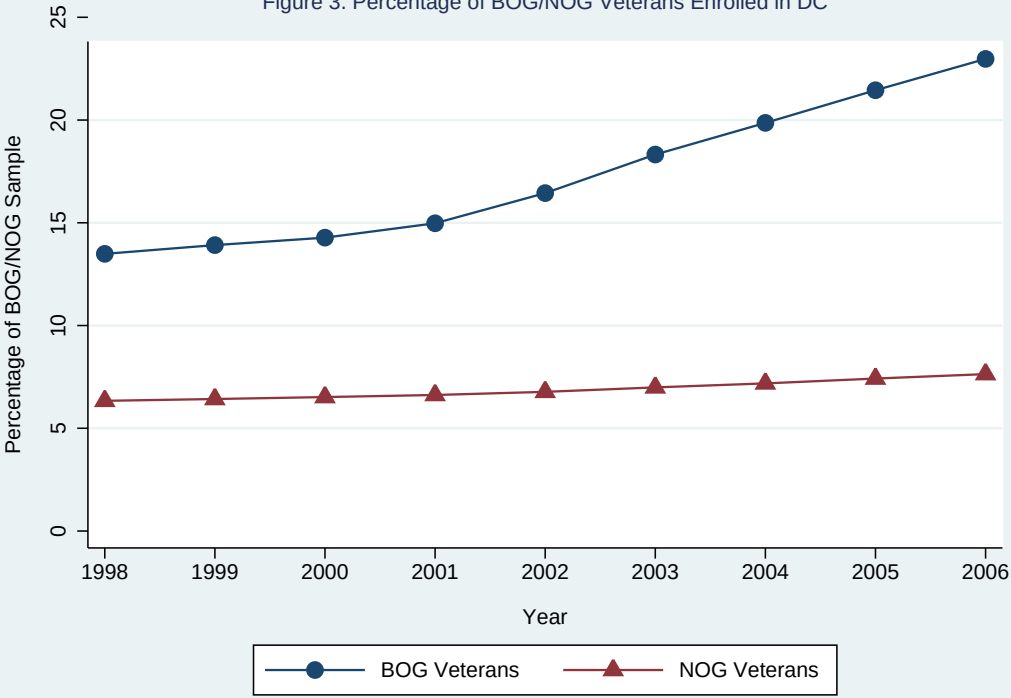


Figure 4. Percentage of BOG/NOG Veterans with Diabetes Award

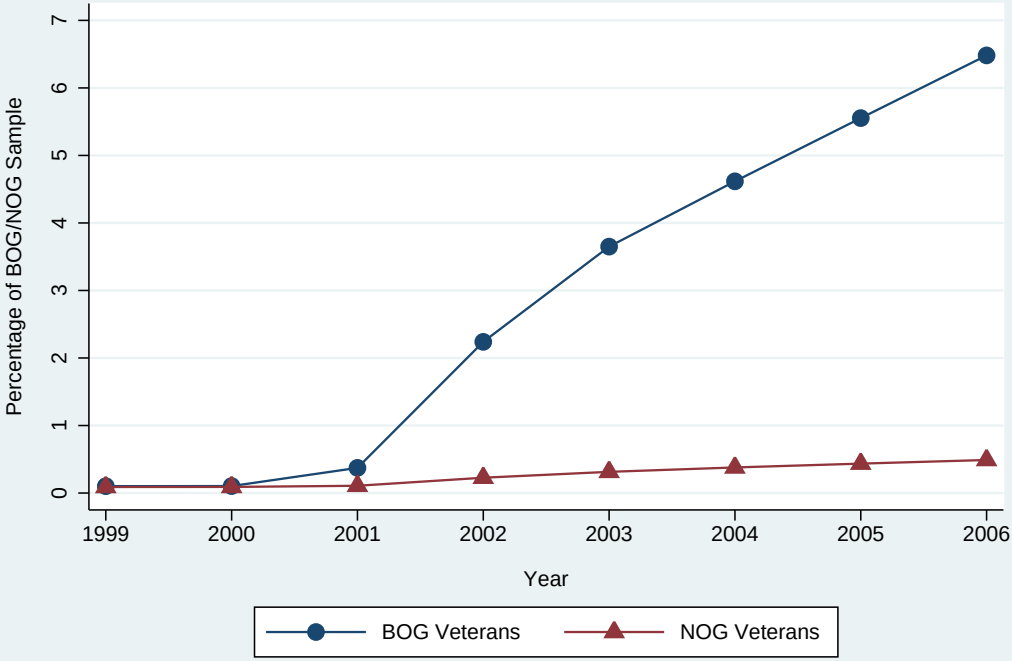


Figure 5. Percentage of BOG/NOG Veterans who Enter DC with Diabetes Award

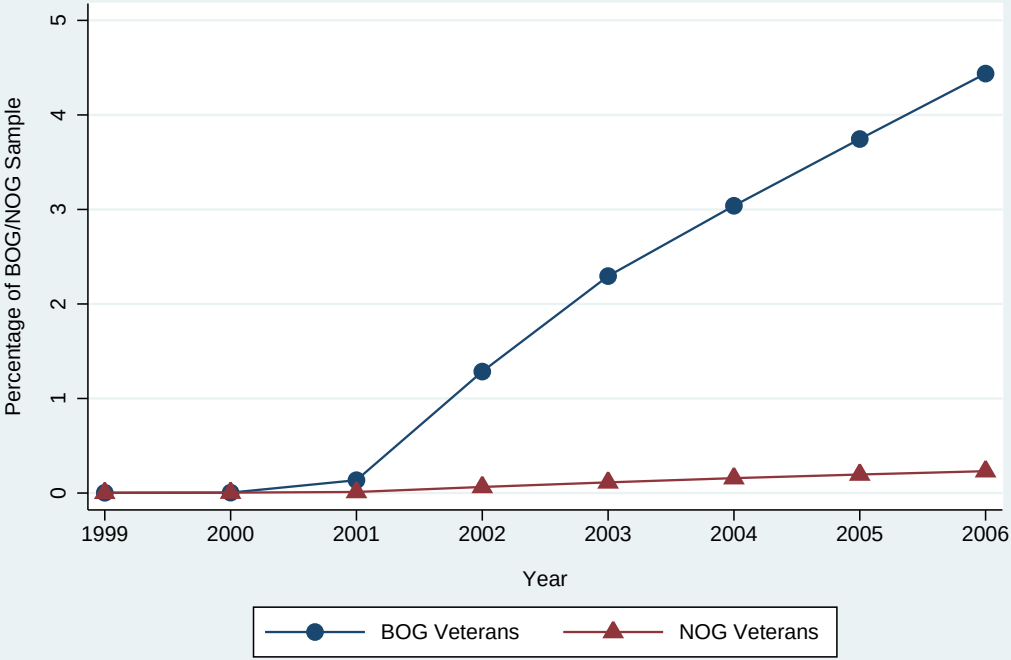


Figure 6. Percentage of BOG/NOG Veterans Enrolled in DC Without Diabetes Award

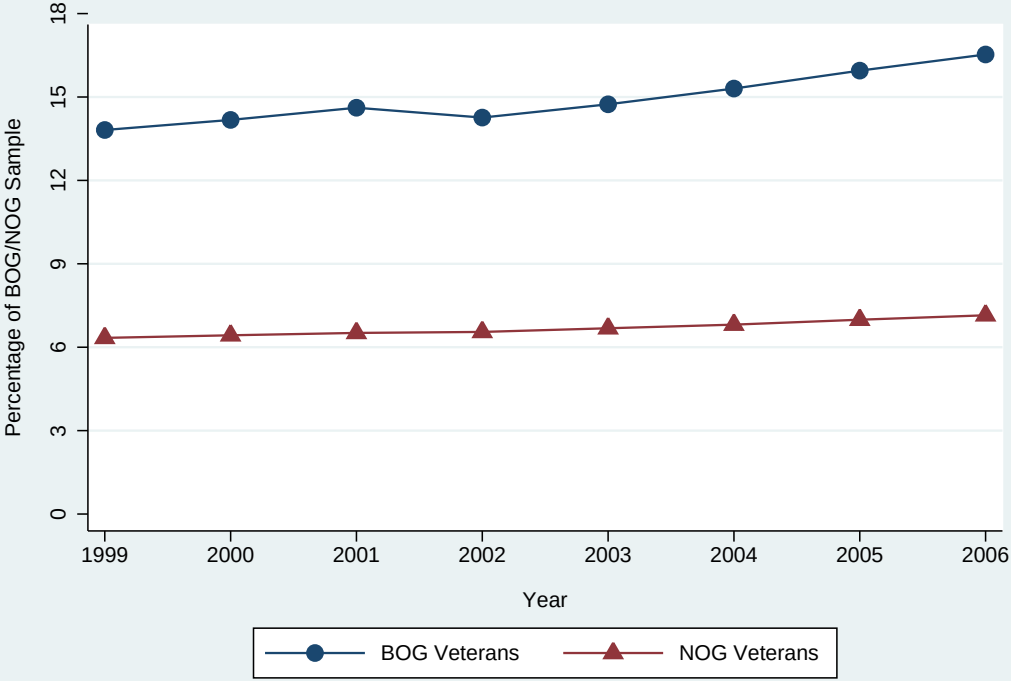


Figure 7. DC Progression. New DC Entrants (w/ diabetes) in 2002, N=8772

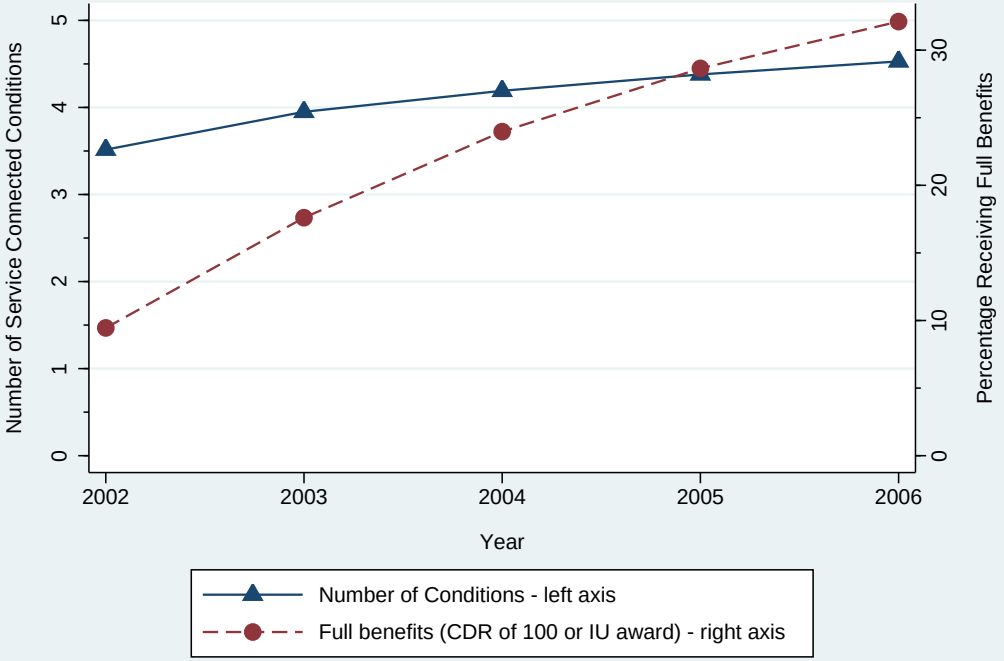
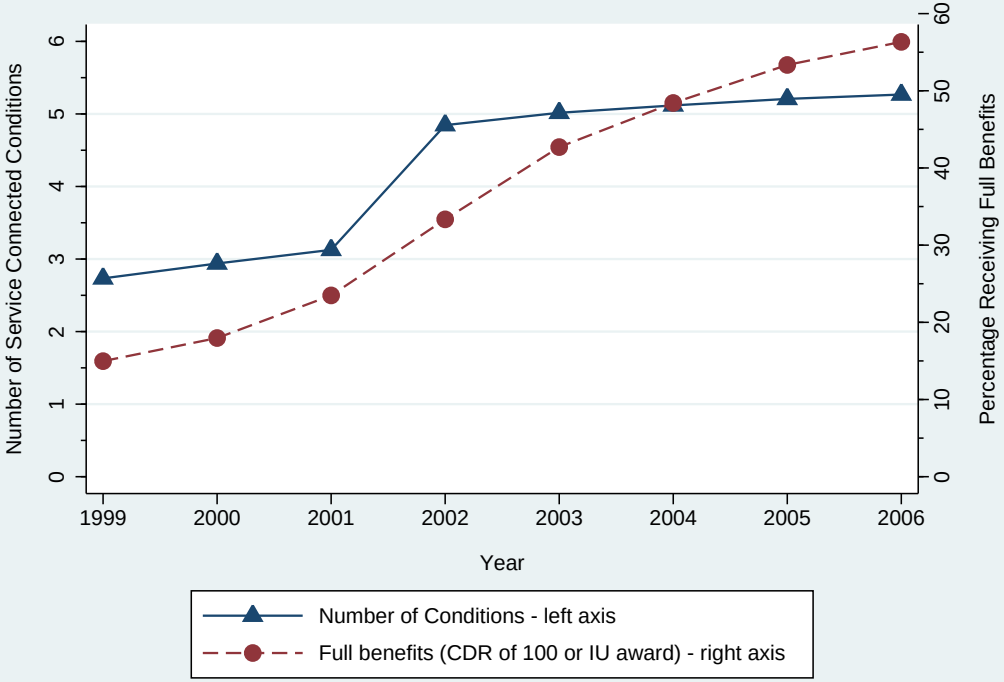
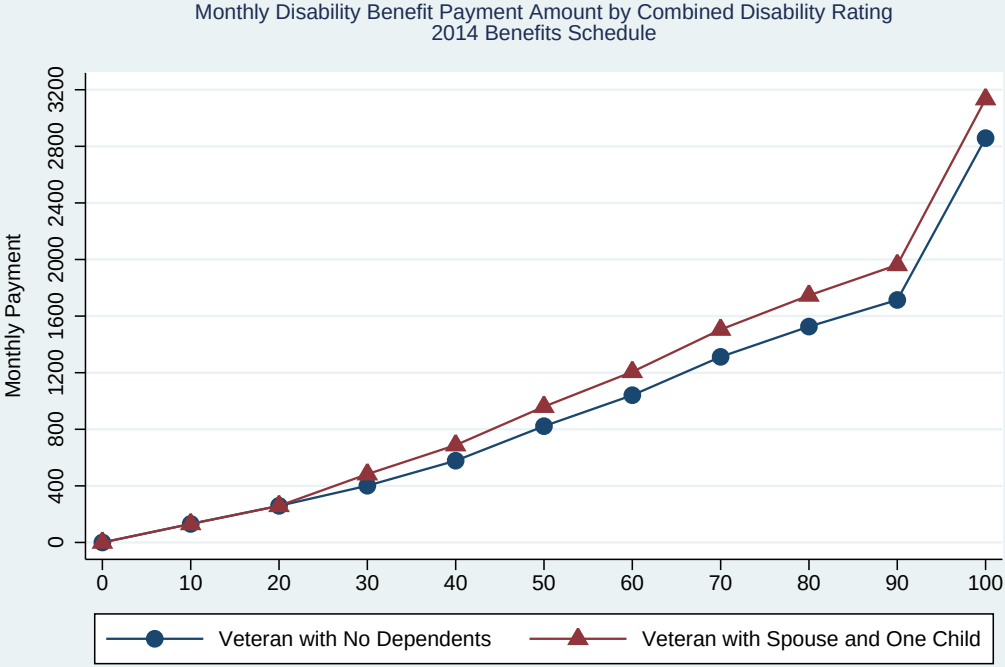


Figure 8. DC Progression. Veterans with DC Promotion (for diabetes) in 2002, N=6646





Source: Department of Veterans Affairs, http://benefits.va.gov/COMPENSATION/resources_comp01.asp

Table 1: Distribution of Loss Year in OEMA Sample

	NOG Veterans		BOG Veterans	
Loss Year	Observations	Percent of NOG Sample	Observations	Percent of BOG Sample
1960	-	0.00	1	0.00
1961	-	0.00	2	0.00
1962	-	0.00	2	0.00
1963	-	0.00	2	0.00
1964	-	0.00	8	0.00
1965	-	0.00	25	0.00
1966	-	0.00	140	0.01
1967	-	0.00	250	0.02
1968	17,787	0.69	7,128	0.48
1969	83,222	3.21	165,959	11.14
1970	265,968	10.25	258,909	17.37
1971	241,772	9.32	273,725	18.37
1972	122,400	4.72	219,993	14.76
1973	164,026	6.32	77,013	5.17
1974	175,414	6.76	51,296	3.44
1975	162,983	6.28	32,696	2.19
1976	160,387	6.18	26,037	1.75
1977	146,516	5.65	23,410	1.57
1978	129,296	4.98	21,334	1.43
1979	141,220	5.44	19,612	1.32
1980	137,754	5.31	15,946	1.07
1981	119,916	4.62	12,921	0.87
1982	128,458	4.95	12,153	0.82
1983	141,806	5.46	11,393	0.76
1984	131,090	5.05	11,597	0.78
1985	125,179	4.82	10,857	0.73
1986	-	0.00	6,052	0.41
1987	-	0.00	5,089	0.34
1988	-	0.00	5,684	0.38
1989	-	0.00	5,212	0.35
1990	-	0.00	4,392	0.29
1991	-	0.00	3,501	0.23
1992	-	0.00	3,671	0.25
1993	-	0.00	2,563	0.17
Missing	-	0.00	201,786	13.54
Total	2,595,194	100.00	1,490,359	100.00

NOTES: This table reports the distribution of loss years within the NOG and BOG groups of the original OEMA sample. A loss year is the year a veteran exited the Army.

Table 2: Distribution of Year of Birth and Start Year in OEMA Sample

A. Year of Birth Distribution

Birth Year	NOG Veterans Observations	NOG Veterans Percent of NOG Sample	BOG Veterans Observations	BOG Veterans Percent of BOG Sample
≤1924	15,539	0.60	18,138	1.22
1925	1,503	0.06	3,258	0.22
1926	1,841	0.07	4,939	0.33
1927	2,265	0.09	5,704	0.38
1928	2,819	0.11	7,923	0.53
1929	3,223	0.12	9,452	0.63
1930	3,530	0.14	10,971	0.74
1931	3,915	0.15	11,696	0.78
1932	3,215	0.12	12,918	0.87
1933	2,577	0.10	12,233	0.82
1934	2,382	0.09	12,514	0.84
1935	2,478	0.10	13,333	0.89
1936	2,665	0.10	13,933	0.93
1937	2,840	0.11	13,250	0.89
1938	3,548	0.14	14,212	0.95
1939	4,813	0.19	15,024	1.01
1940	7,522	0.29	17,947	1.20
1941	10,865	0.42	23,911	1.60
1942	18,988	0.73	34,477	2.31
1943	29,298	1.13	47,114	3.16
1944	38,786	1.49	60,134	4.03
1945	48,100	1.85	80,247	5.38
1946	72,463	2.79	131,973	8.86
1947	108,739	4.19	227,042	15.23
1948	165,733	6.39	227,941	15.29
1949	155,513	5.99	191,467	12.85
1950	143,759	5.54	134,445	9.02
1951	141,803	5.46	72,982	4.90
1952	165,508	6.38	37,175	2.49
1953	141,310	5.45	15,720	1.05
1954	157,134	6.05	5,849	0.39
1955	162,502	6.26	919	0.06
≥1956	968,018	37.30	1,518	0.10
Total	2,595,194	100.00	1,490,359	100.00

A. Start Year Distribution

Start Year	NOG Veterans Observations	NOG Veterans Percent of NOG Sample	BOG Veterans Observations	BOG Veterans Percent of BOG Sample
≤1944	6,970	0.27	8,209	0.55
1945	1,321	0.05	2,808	0.19
1946	2,057	0.08	3,943	0.26
1947	2,027	0.08	3,754	0.25
1948	3,752	0.14	5,988	0.40
1949	3,846	0.15	4,794	0.32
1950	5,400	0.21	8,286	0.56
1951	4,275	0.16	13,569	0.91
1952	1,925	0.07	13,125	0.88
1953	1,983	0.08	15,593	1.05
1954	1,522	0.06	13,370	0.90
1955	1,807	0.07	12,950	0.87
1956	1,776	0.07	12,132	0.81
1957	1,779	0.07	12,107	0.81
1958	2,485	0.10	15,181	1.02
1959	2,254	0.09	14,769	0.99
1960	2,339	0.09	14,192	0.95
1961	4,018	0.15	16,002	1.07
1962	4,359	0.17	15,689	1.05
1963	4,778	0.18	15,895	1.07
1964	6,362	0.25	18,357	1.23
1965	9,675	0.37	26,859	1.80
1966	38,014	1.46	88,681	5.95
1967	96,939	3.74	174,641	11.72
1968	196,281	7.56	263,378	17.67
1969	199,973	7.71	263,700	17.69
1970	163,525	6.30	149,738	10.05
1971	191,736	7.39	60,564	4.06
1972	215,043	8.29	15,087	1.01
1973	144,792	5.58	3,053	0.20
1974	187,413	7.22	1,753	0.12
1975	162,643	6.27	1,802	0.12
≥1976	920,849	35.48	3,624	0.24
Missing	1,276	0.05	196,766	13.20
Total	2,595,194	100.00	1,490,359	100.00

NOTES: This table reports the distribution of start years within the NOG and BOG groups of the original OEMA sample. A start year is the year a veteran entered the Army.

Table 3: Characteristics of the BOG and NOG Samples

	A. SSA Verified Sample		B. Full Sample	
Loss Year	BOG	NOG	BOG	NOG
% Verified in SSA Data	100.0	100.0	95.7	89.0
% with Start Yrs 1966-71	100.0	100.0	100.0	100.0
% with YOB 1946-1951	100.0	100.0	100.0	100.0
% Deceased by 1997	4.8	2.9	6.2	6.0
% Deceased by 2006	9.9	7.8	11.5	11.0
% Nonwhite	11.2	11.6	11.6	12.5
% Missing Education	1.1	15.3	1.1	14.3
% HS Dropout	31.5	23.2	31.4	24.3
% HS Grad	48.0	37.1	48.3	37.9
% Some College	14.5	14.4	14.3	14.2
% College Grad	4.8	9.2	4.7	8.6
% More than College	0.2	0.8	0.2	0.7
% Missing AFQT Score	26.2	19.1	25.7	17.7
Average AFQT Score	52.1	53.4	51.7	52.6
Average Year of Birth	1948.4	1948.6	1948.4	1948.7
Average Start Year	1968.4	1969.0	1968.4	1969.0
Average Loss Year	1971.1	1971.4	1971.1	1971.4
% on DC in 1998	12.8	6.1	12.6	5.8
% on DC in 2000	13.4	6.3	13.2	5.9
% on DC in 2006	20.8	7.1	20.3	6.7
Mean Monthly Benefit in 1998	97	48	97	48
Mean Monthly Benefit in 1998	117	54	117	53
Mean Monthly Benefit in 1998	234	74	231	72
Mean Monthly Benefit in 1998 if >0	677	652	681	661
Mean Monthly Benefit in 2000 if >0	766	694	768	702
Mean Monthly Benefit in 2006 if >0	984	779	985	782
% Positive Earnings in 1998	84.4	85.2		
% Positive Earnings in 2000	82.9	84.1		
% Positive Earnings in 2006	71.4	74.5		
Ln(Cell Mean Earnings) in 1998	10.67	10.70		
Ln(Cell Mean Earnings) in 2000	10.66	10.69		
Ln(Cell Mean Earnings) in 2006	10.37	10.44		
% on SSDI in 1998	5.6	4.6		
% on SSDI in 2000	6.8	5.5		
% on SSDI in 2006	12.6	9.7		
% on SSI in 1998	0.7	1.0		
% on SSI in 2000	0.7	1.0		
% on SSI in 2006	0.7	1.3		
Observations	762,716	583,105	805,667	658,104

NOTES: This table reports summary statistics for veterans in the BOG and NOG samples described in Section III.A. Panel A reports summary statistics for veterans who verified with the Social Security Administration and Panel B reports summary statistics for all veterans.

Table 4: Comparison of Census and Army/SSA Demographics and Earnings Data for Year 1999

	A. Army/SSA Verified Sample (1999)			B. Vietnam Veterans born 1946-1951: 2000 Census		
	All	Whites	Nonwhites	All	Whites	Nonwhites
Age in 1999	50.5	50.5	50.4	50.7	50.7	50.5
	(1.5)	(1.5)	(1.4)	(1.6)	(1.6)	(1.6)
Race						
White	88.7	100.0	0.0	86.7	100.0	0.0
Nonwhite	11.2	0.0	100.0	13.3	0.0	100.0
Education						
HS Dropout	27.3	26.9	30.7	5.2	4.7	8.3
HS Grad	43.6	42.9	49.3	39.3	39.1	41.0
Some College	14.7	15.3	10.3	29.7	29.1	33.4
College Grad	6.9	7.5	2.4	15.9	16.6	11.3
More than College	0.4	0.5	0.1	9.9	10.5	6.0
Positive Earnings	84.1	85.0	76.5	82.2	83.1	76.3
Annual Earnings ($1999)						
Mean (full sample)	37,559	39,028	26,029	41,828	43,559	30,539
	(54,966)	(57,858)	(17,616)	(46,646)	(47,997)	(34,568)
Mean (if >0)	43,666	44,983	33,310	50,889	52,423	40,002
	(57,479)	(60,505)	(19,458)	(46,755)	(48,040)	(34,449)
Median (full sample)	32,564	33,701	24,274	35,000	35,200	26,000
Observations	1,286,647	1,141,231	143,719	203,781	178,601	25,180

NOTES: This table reports summary statistics for veterans in the SSA verified sample and veterans in the 2000 census. Panel A maintains the same sample restrictions reported in Section III.A and further restricts to veterans who were still alive as of 1999. All statistics in panel A are from 1999. Panel B reports summay statistics for veterans in the 5 percent 2000 Census IPUMS extract who were born between 1946 and 1951.

Table 5: Percentage of DC Recipients and Average Annual Benefit by Combined Disability Rating and Service Era in July 2006

CDR	Recipients	Payments (2006 $ Millions)	Annual Benefit	*Share with Each Rating by Service Era* Gulf	Vietnam	Korea	WW II	Peacetime
0%	14,291	$13	$891	0.1%	0.3%	2.4%	1.4%	0.4%
10%	775,346	$1,049	$1,353	27.3%	21.8%	30.6%	33.0%	37.3%
20%	417,721	$1,103	$2,642	17.8%	13.3%	13.0%	12.5%	17.8%
30%	334,931	$1,508	$4,501	15.3%	10.6%	12.4%	13.1%	11.0%
40%	259,834	$1,693	$6,516	12.5%	9.0%	8.2%	8.1%	8.0%
50%	161,568	$1,479	$9,153	7.2%	5.9%	5.4%	5.8%	4.7%
60%	184,264	$2,801	$15,200	7.0%	7.0%	7.6%	7.1%	5.7%
70%	165,257	$3,690	$22,326	4.8%	8.5%	5.5%	5.4%	4.2%
80%	113,404	$2,814	$24,818	3.4%	5.7%	4.1%	4.1%	2.7%
90%	60,546	$1,641	$27,096	1.7%	3.1%	2.3%	2.3%	1.4%
100%	238,662	$7,833	$32,821	3.0%	14.7%	8.5%	7.2%	7.0%
Total	2,725,824	$25,623	$9,400	694,813	947,598	159,804	328,044	595,565

Source: U.S. Department of Veterans Affairs

Table 6: Evolution of Disability Compensation Benefits by Year of First Disability Compensation Award: DC Award Cohorts 1999-2006

Year of Enrollment	Outcome Year 1999	2000	2001	2002	2003	2004	2005	2006
	A. Mean Combined Disability Rating							
1999	40.3	42.7	46.3	49.4	53.1	55.3	56.9	58.1
2000		41.8	46.6	50.5	54.1	56.4	58.4	60.0
2001			39.7	43.9	48.9	52.4	55.7	57.5
2002				37.6	43.7	47.8	51.1	53.4
2003					40.3	45.3	49.0	51.6
2004						41.5	46.0	48.5
2005							40.0	43.4
2006								38.6
	B. Mean Monthly DC Payment ($2013)							
1999	898	1,048	1,199	1,311	1,444	1,529	1,590	1,652
2000		976	1,191	1,341	1,479	1,577	1,650	1,725
2001			892	1,050	1,249	1,393	1,527	1,610
2002				801	1,036	1,197	1,323	1,429
2003					887	1,084	1,228	1,342
2004						926	1,102	1,210
2005							861	993
2006								799
	C. Receiving IU Award or 100 CDR (%)							
1999	15.4	20.9	26.5	31.3	36.7	40.5	43.2	44.7
2000		17.6	25.3	31.3	37.0	41.5	44.6	46.5
2001			15.4	20.8	28.1	33.6	38.7	40.8
2002				12.3	20.1	25.9	30.4	33.3
2003					15.1	21.9	27.0	30.0
2004						16.9	22.6	25.6
2005							14.4	17.7
2006								11.5
	D. Receiving Compensation for Diabetes (%)							
1999	0.8	1.1	2.5	7.9	11.0	12.4	13.8	14.8
2000		0.5	2.3	9.4	12.1	13.4	14.6	15.8
2001			15.3	37.6	38.6	39.4	39.9	40.4
2002				58.2	58.8	59.3	59.5	59.7
2003					47.7	48.2	48.8	49.2
2004						41.4	42.4	43.2
2005							38.6	39.6
2006								39.7

NOTES: This table reports the progression of Disability Compensation (DC) benefits for veterans in the sample described in Section III.A. Each row within each panel corresponds to a DC enrollment cohort. Each column corresponds to a particular year.

Table 7: DC Receipt and Diabetes Compensation among BOG and NOG Veterans, 1998-2006

	All		White		Nonwhite	
	NOG	BOG	NOG	BOG	NOG	BOG
	A. Receiving DC (%)					
1998	6.3	13.5	6.0	13.0	9.1	17.1
1999	6.4	13.9	6.1	13.4	9.2	17.7
2000	6.5	14.3	6.2	13.8	9.3	18.2
2001	6.6	15.0	6.3	14.4	9.5	19.3
2002	6.8	16.4	6.4	15.8	9.7	21.4
2003	7.0	18.3	6.6	17.6	10.0	24.0
2004	7.2	19.9	6.8	19.1	10.3	26.1
2005	7.4	21.5	7.0	20.6	10.7	28.3
2006	7.6	23.0	7.2	22.1	11.0	30.3
	B. Mean Monthly DC Payment Conditional on Receipt ($2013)					
1998	832	878	832	873	833	898
1999	863	940	860	934	874	974
2000	895	1,007	891	999	918	1,046
2001	924	1,064	918	1,054	949	1,113
2002	935	1,093	928	1,081	969	1,154
2003	972	1,166	961	1,153	1,022	1,239
2004	999	1,231	987	1,215	1,055	1,324
2005	1,020	1,285	1,007	1,267	1,082	1,387
2006	1,045	1,328	1,032	1,308	1,111	1,444
	C. Percent of DC Recipients Receiving Compensation for Diabetes (%)					
1998						
1999	1.4	0.7	1.2	0.6	2.2	1.3
2000	1.4	0.7	1.2	0.6	2.2	1.3
2001	1.6	2.4	1.4	2.2	2.5	3.9
2002	3.3	13.3	3.0	12.4	4.8	18.4
2003	4.4	19.6	4.0	18.4	6.3	26.2
2004	5.2	22.9	4.8	21.8	7.2	30.1
2005	5.8	25.6	5.4	24.4	7.8	32.9
2006	6.4	28.1	6.0	26.9	8.4	35.3

NOTES: Panel A of this table reports the percentage of veterans in the BOG and NOG samples who were enrolled in the DC program between 1998 and 2006. Panel B reports the mean monthly disability payments (in 2013 dollars) for veterans enrolled in the DC program between 1998 and 2006. Panel C reports the percenage of DC enrolled veterans who receive compensation for diabetes. 1998 is not included in Panel C because our data only indicates specific conditions for 1999 through 2006.

Table 8A: DC Receipt in the BOG versus NOG samples from 1998-2006.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	6.96***	6.71***	7.48***	6.36***	7.81***	6.85***	8.43***	5.97***
	(0.06)	(0.12)	(0.09)	(0.11)	(0.21)	(0.07)	(0.12)	(0.09)
BOG * (YR99)	0.33***	0.32*	0.34***	0.31**	0.48	0.31***	0.39**	0.26**
	(0.09)	(0.18)	(0.13)	(0.16)	(0.29)	(0.09)	(0.16)	(0.12)
BOG * (YR00)	0.59***	0.53***	0.62***	0.60***	0.84***	0.56***	0.73***	0.47***
	(0.09)	(0.18)	(0.13)	(0.16)	(0.29)	(0.09)	(0.16)	(0.12)
BOG * (YR01)	1.18***	1.10***	1.24***	1.14***	1.71***	1.11***	1.50***	0.93***
	(0.09)	(0.18)	(0.13)	(0.16)	(0.29)	(0.09)	(0.16)	(0.12)
BOG * (YR02)	2.46***	2.40***	2.62***	2.24***	3.53***	2.32***	3.10***	1.94***
	(0.09)	(0.18)	(0.13)	(0.16)	(0.29)	(0.09)	(0.17)	(0.12)
BOG * (YR03)	4.07***	4.01***	4.36***	3.63***	5.79***	3.85***	5.12***	3.22***
	(0.09)	(0.18)	(0.13)	(0.16)	(0.30)	(0.09)	(0.17)	(0.12)
BOG * (YR04)	5.39***	5.31***	5.76***	4.81***	7.49***	5.12***	6.74***	4.28***
	(0.09)	(0.18)	(0.13)	(0.16)	(0.30)	(0.09)	(0.17)	(0.12)
BOG * (YR05)	6.71***	6.58***	7.20***	5.97***	9.22***	6.38***	8.36***	5.36***
	(0.09)	(0.18)	(0.13)	(0.16)	(0.30)	(0.09)	(0.17)	(0.12)
BOG * (YR06)	7.98***	7.84***	8.62***	6.99***	10.90***	7.60***	9.85***	6.43***
	(0.09)	(0.18)	(0.14)	(0.16)	(0.30)	(0.09)	(0.17)	(0.12)
# OBS (Cell)x(Year)	2,141,602	576,645	985,162	579,795	232,245	1,909,357	660,671	988,695

Notes: This table reports estimates of specification (1) where the dependent variable is the percentage of each cell enrolled in the Disability Compensation program in September of a specific year. Each cell has one observation for each year between 1998 and 2006. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 8B: DC Enrollment in the BOG versus NOG samples from 1998-2006.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	8.105***	7.794***	8.670***	7.470***	9.531***	7.914***	9.902***	6.848***
	(0.041)	(0.082)	(0.062)	(0.074)	(0.137)	(0.043)	(0.077)	(0.057)
BOG * (YR-2001)	0.399***	0.380***	0.418***	0.388***	0.608***	0.372***	0.522***	0.307***
	(0.024)	(0.048)	(0.037)	(0.044)	(0.081)	(0.025)	(0.045)	(0.034)
BOG * (YR-2001) * (YR>=2002)	0.990***	0.996***	1.085***	0.813***	1.269***	0.954***	1.190***	0.813***
	(0.035)	(0.068)	(0.052)	(0.062)	(0.115)	(0.036)	(0.064)	(0.048)
# OBS (Cell)x(Year)	2,141,602	576,645	985,162	579,795	232,245	1,909,357	660,671	988,695

Notes: This table reports estimates of specification (2) where the dependent variable is the percentage of each cell enrolled in the Disability Compensation program in September of a specific year. Each cell has one observation for each year between 1998 and 2006. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 9A: Labor Force Participation in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	-0.33***	-0.47***	0.54***	-1.82***	-2.33***	-0.07	-0.83***	-0.93***
	(0.08)	(0.14)	(0.11)	(0.15)	(0.26)	(0.08)	(0.15)	(0.10)
BOG * (YR97)	-0.16	-0.21	-0.22	0.00	-0.45	-0.12	-0.23	-0.09
	(0.11)	(0.21)	(0.16)	(0.21)	(0.36)	(0.11)	(0.21)	(0.14)
BOG * (YR98)	-0.31***	-0.33	-0.33**	-0.27	-0.74**	-0.26**	-0.40*	-0.22
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.14)
BOG * (YR99)	-0.45***	-0.38*	-0.49***	-0.45**	-0.90**	-0.39***	-0.65***	-0.32**
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.14)
BOG * (YR00)	-0.77***	-0.76***	-0.78***	-0.76***	-1.05***	-0.73***	-1.04***	-0.53***
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.14)
BOG * (YR01)	-0.81***	-0.82***	-0.83***	-0.76***	-1.04***	-0.78***	-0.92***	-0.61***
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.14)
BOG * (YR02)	-1.01***	-1.07***	-1.08***	-0.84***	-1.38***	-0.96***	-1.04***	-0.75***
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.14)
BOG * (YR03)	-1.33***	-1.52***	-1.44***	-0.99***	-2.04***	-1.24***	-1.51***	-1.01***
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.14)
BOG * (YR04)	-1.73***	-1.91***	-1.85***	-1.38***	-2.18***	-1.68***	-1.87***	-1.32***
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.14)
BOG * (YR05)	-2.14***	-2.25***	-2.25***	-1.86***	-2.84***	-2.05***	-2.41***	-1.63***
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.14)
BOG * (YR06)	-2.42***	-2.45***	-2.68***	-1.97***	-3.07***	-2.33***	-2.71***	-1.82***
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.14)
BOG * (YR07)	-2.82***	-2.92***	-3.00***	-2.45***	-3.63***	-2.72***	-3.18***	-2.21***
	(0.11)	(0.21)	(0.16)	(0.21)	(0.37)	(0.11)	(0.21)	(0.15)
# OBS (Cell)x(Year)	2,861,891	770,487	1,316,544	774,860	310,544	2,551,347	883,525	1,321,162

Notes: This table reports estimates of specification (1) where the dependent variable is the percentage of each cell with positive annual earnings. Each cell has one observation for each year between 1996 and 2007. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 9B: Labor Force Participation in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	-1.099***	-1.280***	-0.263***	-2.503***	-3.475***	-0.791***	-1.728***	-1.482***
	(0.044)	(0.084)	(0.065)	(0.086)	(0.149)	(0.046)	(0.086)	(0.058)
BOG * (YR-2001)	-0.151***	-0.160***	-0.152***	-0.141***	-0.187***	-0.146***	-0.165***	-0.110***
	(0.016)	(0.030)	(0.023)	(0.031)	(0.054)	(0.016)	(0.031)	(0.021)
BOG * (YR-2001) * (YR>=2002)	-0.181***	-0.186***	-0.212***	-0.131***	-0.215**	-0.178***	-0.198***	-0.154***
	(0.026)	(0.049)	(0.038)	(0.050)	(0.088)	(0.027)	(0.050)	(0.034)
# OBS (Cell)x(Year)	2,861,891	770,487	1,316,544	774,860	310,544	2,551,347	883,525	1,321,162

Notes: This table reports estimates of specification (2) where the dependent variable is the percentage of each cell with positive annual earnings. Each cell has one observation for each year between 1996 and 2007. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 10A: 100 x Ln(Cell Mean Earnings) in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	-2.49***	-9.49***	0.32	-1.04**	-4.46***	-2.20***	-0.04	-3.97***
	(0.25)	(0.53)	(0.36)	(0.46)	(0.87)	(0.26)	(0.46)	(0.32)
BOG * (YR97)	-0.40	-0.49	-0.25	-0.55	-1.21	-0.29	-0.27	-0.22
	(0.36)	(0.75)	(0.51)	(0.65)	(1.24)	(0.37)	(0.65)	(0.46)
BOG * (YR98)	-0.68*	-0.98	-0.59	-0.52	-0.96	-0.64*	-0.50	-0.52
	(0.36)	(0.75)	(0.51)	(0.65)	(1.24)	(0.37)	(0.65)	(0.46)
BOG * (YR99)	-0.77**	-0.92	-0.74	-0.60	-1.47	-0.66*	-0.80	-0.74
	(0.36)	(0.75)	(0.51)	(0.65)	(1.24)	(0.37)	(0.65)	(0.46)
BOG * (YR00)	-1.41***	-1.60**	-1.38***	-1.19*	-1.77	-1.35***	-1.56**	-0.85*
	(0.36)	(0.75)	(0.51)	(0.65)	(1.24)	(0.37)	(0.65)	(0.46)
BOG * (YR01)	-1.53***	-1.70**	-1.60***	-1.16*	-2.29*	-1.42***	-1.63**	-0.90*
	(0.36)	(0.75)	(0.51)	(0.66)	(1.25)	(0.37)	(0.66)	(0.46)
BOG * (YR02)	-1.71***	-1.83**	-1.87***	-1.19*	-2.93**	-1.54***	-1.80***	-0.96**
	(0.36)	(0.75)	(0.51)	(0.66)	(1.25)	(0.37)	(0.66)	(0.46)
BOG * (YR03)	-2.12***	-2.86***	-2.08***	-1.38**	-3.78***	-1.89***	-2.37***	-1.23***
	(0.36)	(0.75)	(0.51)	(0.66)	(1.25)	(0.37)	(0.66)	(0.46)
BOG * (YR04)	-3.07***	-3.62***	-3.06***	-2.37***	-4.45***	-2.87***	-2.93***	-1.91***
	(0.36)	(0.75)	(0.51)	(0.66)	(1.25)	(0.37)	(0.66)	(0.46)
BOG * (YR05)	-3.93***	-4.31***	-4.22***	-2.91***	-6.43***	-3.58***	-3.95***	-2.25***
	(0.36)	(0.75)	(0.51)	(0.66)	(1.26)	(0.37)	(0.66)	(0.46)
BOG * (YR06)	-4.75***	-4.98***	-5.17***	-3.62***	-7.24***	-4.40***	-5.04***	-2.80***
	(0.36)	(0.76)	(0.51)	(0.66)	(1.26)	(0.37)	(0.66)	(0.46)
BOG * (YR07)	-5.81***	-6.80***	-6.10***	-4.14***	-9.46***	-5.32***	-5.95***	-3.64***
	(0.36)	(0.76)	(0.51)	(0.66)	(1.27)	(0.37)	(0.67)	(0.47)
# OBS (Cell)x(Year)	2,856,418	769,144	1,314,146	773,128	308,917	2,547,501	880,542	1,320,368

Notes: This table reports estimates of specification (1) where the dependent variable is 100 times the natural logarithm of a cell's mean annual earnings. Each cell has one observation for each year between 1996 and 2007. Cells with 0 mean annual earnings are excluded from the regressions. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 10B: 100 x Ln(Cell Mean Earnings) in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	-3.710***	-10.942***	-0.898***	-1.962***	-6.341***	-3.327***	-1.352***	-4.714***
	(0.145)	(0.305)	(0.207)	(0.267)	(0.508)	(0.150)	(0.267)	(0.187)
BOG * (YR-2001)	-0.214***	-0.244**	-0.230***	-0.139	-0.286	-0.202***	-0.254***	-0.115*
	(0.052)	(0.109)	(0.074)	(0.096)	(0.182)	(0.054)	(0.096)	(0.067)
BOG * (YR-2001) *	-0.486***	-0.533***	-0.527***	-0.370**	-0.846***	-0.442***	-0.453***	-0.307***
(YR>=2002)	(0.085)	(0.178)	(0.121)	(0.156)	(0.298)	(0.088)	(0.157)	(0.110)
# OBS (Cell)x(Year)	2,856,418	769,144	1,314,146	773,128	308,917	2,547,501	880,542	1,320,368

Notes: This table reports estimates of specification (2) where the dependent variable is 100 times the natural logarithm of a cell's mean annual earnings. Each cell has one observation for each year between 1996 and 2007. Cells with 0 mean annual earnings are excluded from the regressions. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 11A: 100 x Ln(1+Cell Mean Earnings) in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	-2.23***	-9.94***	0.77*	-0.45	-4.32***	-1.93***	0.68	-3.83***
	(0.31)	(0.62)	(0.44)	(0.59)	(1.22)	(0.31)	(0.62)	(0.36)
BOG * (YR97)	-0.20	-0.22	-0.32	-0.01	-0.96	-0.10	-0.28	-0.14
	(0.44)	(0.88)	(0.62)	(0.84)	(1.73)	(0.44)	(0.88)	(0.50)
BOG * (YR98)	-0.65	-0.80	-0.76	-0.30	-1.35	-0.55	-0.54	-0.61
	(0.44)	(0.88)	(0.62)	(0.84)	(1.73)	(0.44)	(0.88)	(0.51)
BOG * (YR99)	-0.85*	-0.51	-1.08*	-0.73	-2.50	-0.63	-1.37	-0.89*
	(0.44)	(0.88)	(0.62)	(0.84)	(1.73)	(0.44)	(0.88)	(0.51)
BOG * (YR00)	-1.35***	-1.22	-1.57**	-1.03	-3.19*	-1.10**	-1.83**	-1.01**
	(0.44)	(0.89)	(0.62)	(0.84)	(1.74)	(0.44)	(0.88)	(0.51)
BOG * (YR01)	-1.48***	-1.34	-1.61***	-1.30	-2.84	-1.29***	-1.63*	-1.01**
	(0.44)	(0.89)	(0.62)	(0.84)	(1.74)	(0.44)	(0.88)	(0.51)
BOG * (YR02)	-1.67***	-1.79**	-1.88***	-1.12	-4.44**	-1.29***	-1.91**	-1.14**
	(0.44)	(0.89)	(0.63)	(0.84)	(1.74)	(0.44)	(0.88)	(0.51)
BOG * (YR03)	-2.44***	-3.05***	-2.48***	-1.71**	-7.68***	-1.74***	-3.45***	-1.46***
	(0.44)	(0.89)	(0.63)	(0.84)	(1.75)	(0.44)	(0.89)	(0.51)
BOG * (YR04)	-3.18***	-4.10***	-3.27***	-2.02**	-7.84***	-2.54***	-4.05***	-1.86***
	(0.44)	(0.89)	(0.63)	(0.84)	(1.75)	(0.44)	(0.89)	(0.51)
BOG * (YR05)	-4.25***	-5.08***	-4.22***	-3.43***	-10.46***	-3.41***	-5.49***	-2.53***
	(0.44)	(0.89)	(0.63)	(0.85)	(1.75)	(0.44)	(0.89)	(0.51)
BOG * (YR06)	-5.23***	-6.25***	-5.45***	-3.83***	-11.50***	-4.39***	-7.04***	-3.12***
	(0.44)	(0.89)	(0.63)	(0.85)	(1.76)	(0.45)	(0.89)	(0.51)
BOG * (YR07)	-6.91***	-8.57***	-6.71***	-5.60***	-16.00***	-5.70***	-8.81***	-4.49***
	(0.44)	(0.90)	(0.63)	(0.85)	(1.76)	(0.45)	(0.90)	(0.51)
# OBS (Cell)x(Year)	2,861,891	770,487	1,316,544	774,860	310,544	2,551,347	883,525	1,321,162

Notes: This table reports estimates of specification (1) where the dependent variable is 100 times the natural logarithm of 1 plus a cell's mean annual earnings. Each cell has one observation for each year between 1996 and 2007. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 11B: 100 x Ln(1+Cell Mean Earnings) in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	-3.340***	-10.921***	-0.556**	-1.228***	-7.369***	-2.776***	-0.700*	-4.648***
	(0.178)	(0.360)	(0.254)	(0.342)	(0.708)	(0.180)	(0.360)	(0.206)
BOG * (YR-2001)	-0.198***	-0.168	-0.230**	-0.150	-0.539**	-0.151**	-0.243*	-0.128*
	(0.064)	(0.129)	(0.091)	(0.123)	(0.254)	(0.064)	(0.129)	(0.074)
BOG * (YR-2001) *	-0.651***	-0.958***	-0.573***	-0.516***	-1.380***	-0.556***	-0.877***	-0.368***
(YR>=2002)	(0.104)	(0.211)	(0.149)	(0.200)	(0.415)	(0.105)	(0.211)	(0.121)
# OBS (Cell)x(Year)	2,861,891	770,487	1,316,544	774,860	310,544	2,551,347	883,525	1,321,162

Notes: This table reports estimates of specification (2) where the dependent variable is 100 times the natural logarithm of 1 plus a cell's mean annual earnings. Each cell has one observation for each year between 1996 and 2007. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 12A: SSDI Receipt in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	0.77***	0.80***	0.63***	1.03***	1.17***	0.71***	1.16***	0.83***
	(0.05)	(0.09)	(0.07)	(0.10)	(0.17)	(0.05)	(0.10)	(0.06)
BOG * (YR97)	0.11	0.13	0.12	0.06	0.21	0.10	0.11	0.10
	(0.07)	(0.13)	(0.11)	(0.14)	(0.24)	(0.07)	(0.15)	(0.09)
BOG * (YR98)	0.22***	0.25*	0.22**	0.17	0.37	0.20***	0.23	0.19**
	(0.07)	(0.13)	(0.11)	(0.14)	(0.24)	(0.07)	(0.15)	(0.09)
BOG * (YR99)	0.35***	0.37***	0.33***	0.34**	0.56**	0.32***	0.42***	0.27***
	(0.07)	(0.13)	(0.11)	(0.14)	(0.24)	(0.07)	(0.15)	(0.09)
BOG * (YR00)	0.49***	0.55***	0.48***	0.45***	0.67***	0.47***	0.55***	0.41***
	(0.07)	(0.13)	(0.11)	(0.14)	(0.24)	(0.07)	(0.15)	(0.09)
BOG * (YR01)	0.64***	0.76***	0.63***	0.53***	0.86***	0.61***	0.60***	0.56***
	(0.07)	(0.13)	(0.11)	(0.14)	(0.24)	(0.07)	(0.15)	(0.09)
BOG * (YR02)	0.86***	1.03***	0.84***	0.75***	1.23***	0.81***	0.93***	0.71***
	(0.07)	(0.13)	(0.11)	(0.14)	(0.24)	(0.07)	(0.15)	(0.09)
BOG * (YR03)	1.16***	1.44***	1.08***	1.01***	1.52***	1.11***	1.23***	0.96***
	(0.07)	(0.13)	(0.11)	(0.14)	(0.25)	(0.07)	(0.15)	(0.09)
BOG * (YR04)	1.44***	1.72***	1.42***	1.19***	1.83***	1.38***	1.55***	1.19***
	(0.07)	(0.13)	(0.11)	(0.14)	(0.25)	(0.07)	(0.15)	(0.09)
BOG * (YR05)	1.71***	2.07***	1.70***	1.39***	1.99***	1.67***	1.80***	1.45***
	(0.07)	(0.13)	(0.11)	(0.14)	(0.25)	(0.07)	(0.15)	(0.09)
BOG * (YR06)	1.93***	2.32***	1.96***	1.51***	2.26***	1.88***	2.03***	1.61***
	(0.07)	(0.13)	(0.11)	(0.14)	(0.25)	(0.07)	(0.15)	(0.09)
BOG * (YR07)	2.03***	2.40***	2.09***	1.59***	2.31***	1.99***	2.08***	1.69***
	(0.07)	(0.13)	(0.11)	(0.14)	(0.25)	(0.07)	(0.15)	(0.09)
# OBS (Cell)x(Year)	2,861,891	770,487	1,316,544	774,860	310,544	2,551,347	883,525	1,321,162

Notes: This table reports estimates of specification (1) where the dependent variable is the percentage of each cell enrolled in SSDI. Each cell has one observation for each year between 1996 and 2007. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 12B: SSDI Receipt in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	1.410***	1.564***	1.236***	1.616***	2.112***	1.316***	1.845***	1.370***
	(0.029)	(0.053)	(0.043)	(0.056)	(0.099)	(0.030)	(0.061)	(0.036)
BOG * (YR-2001)	0.134***	0.161***	0.123***	0.125***	0.189***	0.126***	0.141***	0.112***
	(0.010)	(0.019)	(0.015)	(0.020)	(0.036)	(0.011)	(0.022)	(0.013)
BOG * (YR-2001) * (YR>=2002)	0.116***	0.139***	0.137***	0.059*	0.066	0.122***	0.117***	0.095***
	(0.017)	(0.031)	(0.025)	(0.033)	(0.058)	(0.018)	(0.035)	(0.021)
# OBS (Cell)x(Year)	2,861,891	770,487	1,316,544	774,860	310,544	2,551,347	883,525	1,321,162

Notes: This table reports estimates of specification (2) where the dependent variable is the percentage of each cell enrolled in SSDI. Each cell has one observation for each year between 1996 and 2007. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 13A: SSI Receipt in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	-0.26***	-0.19***	-0.31***	-0.20***	-0.39***	-0.24***	-0.34***	-0.08***
	(0.02)	(0.03)	(0.02)	(0.04)	(0.08)	(0.02)	(0.04)	(0.02)
BOG * (YR97)	0.00	-0.02	0.01	0.01	0.06	-0.01	-0.02	0.02
	(0.02)	(0.04)	(0.03)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR98)	-0.02	-0.04	-0.01	-0.01	0.04	-0.02	-0.01	-0.02
	(0.02)	(0.04)	(0.03)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR99)	-0.03	-0.04	-0.03	-0.03	0.03	-0.04*	-0.06	-0.02
	(0.02)	(0.04)	(0.03)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR00)	-0.07***	-0.07*	-0.07**	-0.06	-0.03	-0.07***	-0.11**	-0.04
	(0.02)	(0.04)	(0.03)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR01)	-0.13***	-0.10**	-0.12***	-0.17***	-0.15	-0.12***	-0.22***	-0.06**
	(0.02)	(0.04)	(0.03)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR02)	-0.14***	-0.16***	-0.10***	-0.17***	-0.10	-0.14***	-0.23***	-0.08***
	(0.02)	(0.04)	(0.04)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR03)	-0.19***	-0.19***	-0.16***	-0.25***	-0.25**	-0.19***	-0.33***	-0.11***
	(0.02)	(0.04)	(0.04)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR04)	-0.23***	-0.19***	-0.22***	-0.28***	-0.34***	-0.22***	-0.41***	-0.11***
	(0.02)	(0.04)	(0.04)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR05)	-0.25***	-0.20***	-0.25***	-0.31***	-0.33***	-0.24***	-0.43***	-0.13***
	(0.02)	(0.04)	(0.04)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR06)	-0.32***	-0.23***	-0.32***	-0.41***	-0.53***	-0.30***	-0.55***	-0.18***
	(0.02)	(0.04)	(0.04)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
BOG * (YR07)	-0.32***	-0.20***	-0.33***	-0.41***	-0.51***	-0.29***	-0.63***	-0.16***
	(0.02)	(0.04)	(0.04)	(0.05)	(0.11)	(0.02)	(0.06)	(0.03)
# OBS (Cell)x(Year)	2,861,891	770,487	1,316,544	774,860	310,544	2,551,347	883,525	1,321,162

Notes: This table reports estimates of specification (1) where the dependent variable is the percentage of each cell enrolled in SSI. Each cell has one observation for each year between 1996 and 2007. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 13B: SSI Receipt in the BOG versus NOG samples from 1996-2007.

	All	YOB: 46-47	YOB: 48-49	YOB: 50-51	Nonwhites	Whites	AFQT<45	AFQT≥45
BOG	-0.361***	-0.314***	-0.397***	-0.323***	-0.458***	-0.348***	-0.509***	-0.137***
	(0.010)	(0.016)	(0.014)	(0.022)	(0.045)	(0.010)	(0.023)	(0.011)
BOG * (YR-2001)	-0.025***	-0.025***	-0.021***	-0.033***	-0.026	-0.025***	-0.039***	-0.015***
	(0.004)	(0.006)	(0.005)	(0.008)	(0.016)	(0.003)	(0.008)	(0.004)
BOG * (YR-2001) *	-0.014**	0.006	-0.021**	-0.018	-0.053**	-0.009	-0.037***	-0.004
(YR>=2002)	(0.006)	(0.010)	(0.008)	(0.013)	(0.026)	(0.006)	(0.013)	(0.006)
# OBS (Cell)x(Year)	2,861,891	770,487	1,316,544	774,860	310,544	2,551,347	883,525	1,321,162

Notes: This table reports estimates of specification (2) where the dependent variable is the percentage of each cell enrolled in SSI. Each cell has one observation for each year between 1996 and 2007. All regressions include year fixed effects and fixed effects for each combationation of (AFQT-quintile)x(year), (year of birth)x(year), and (race)x(year), where race is defined as white or nonwhite. Column 1 includes all veterans in our sample and columns 2-8 restrict the sample to veterans in the specified group. Columns 1-6 group veterans with a missing AFQT score into a sixth AFQT category while columns 7 and 8 exclude veterans with missing AFQT scores. All regressions weight each observation according to the number of veterans in the cell. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Appendix Table 1: SSA Verification Rate by Start Year

	NOG Veterans			BOG Veterans		
Start Year	Obs	1st SSA Match Rate	2nd SSA Match Rate	Obs	1st SSA Match Rate	2nd SSA Match Rate
≤1944	6,970	71.3	86.5	8,209	91.6	95.8
1945	1,321	63.1	85.3	2,808	92.0	96.4
1946	2,057	67.0	88.2	3,943	93.4	97.2
1947	2,027	64.8	87.3	3,754	91.7	96.4
1948	3,752	62.1	87.8	5,988	90.4	96.3
1949	3,846	65.3	88.6	4,794	87.8	95.8
1950	5,400	44.6	82.2	8,286	83.8	95.0
1951	4,275	43.4	82.4	13,569	84.4	95.3
1952	1,925	44.6	82.8	13,125	95.0	98.0
1953	1,983	34.1	81.5	15,593	96.7	98.6
1954	1,522	37.9	82.4	13,370	97.5	98.9
1955	1,807	37.4	84.3	12,950	97.6	99.0
1956	1,776	41.0	84.8	12,132	96.9	98.8
1957	1,779	51.4	88.1	12,107	97.4	99.0
1958	2,485	51.5	88.4	15,181	97.1	98.9
1959	2,254	63.8	91.0	14,769	96.5	98.9
1960	2,339	77.5	93.5	14,192	96.8	99.0
1961	4,018	75.4	93.6	16,002	95.7	98.8
1962	4,359	69.2	91.4	15,689	93.9	98.5
1963	4,778	67.1	91.4	15,895	93.7	98.4
1964	6,362	63.3	91.5	18,357	93.5	98.5
1965	9,675	62.4	91.6	26,859	92.1	98.2
1966	38,014	74.3	94.4	88,681	94.2	98.8
1967	96,939	64.3	93.3	174,641	81.0	97.2
1968	196,281	55.0	92.3	263,378	68.8	95.5
1969	199,973	17.3	87.0	263,700	53.8	93.1
1970	163,525	11.6	87.1	149,738	96.2	98.8
1971	191,736	8.1	86.6	60,564	97.3	99.4
1972	215,043	8.9	86.2	15,087	97.7	99.5
1973	144,792	12.7	85.6	3,053	95.7	98.8
1974	187,413	11.9	85.8	1,753	93.4	99.1
1975	162,643	17.3	88.2	1,802	95.3	98.6
≥1976	920,849	78.4	96.3	3,624	95.7	99.1
Missing	1,276	18.2	81.7	196,766	98.0	99.4
Total	2,595,194	43.3	90.9	1,490,359	81.9	97.0