The Effect of Disability Insurance on Health Investment

Evidence from the Veterans Benefits Administration's Disability Compensation Program

Perry Singleton

ABSTRACT

I examine whether individuals respond to monetary incentives to detect latent medical conditions. The effect is identified by a policy that deemed diabetes associated with herbicide exposure a compensable disability under the Veterans Benefits Administration's Disability Compensation program. Since a diagnosis is a requisite for benefit eligibility, and nearly one-third of diabetics remain undiagnosed, the advent of disability insurance may have encouraged the detection of diabetes among the previously undiagnosed population. Evidence from the National Health Interview Survey suggests that the policy increased the prevalence of diagnosed diabetes by 3.1 percentage points among veterans.

I. Introduction

The early detection and treatment of certain medical conditions, which substantially reduces the risk of morbidity and mortality, is an integral component of public health policy. For example, Bunker, Frazier, and Mosteller (1994) conclude that screening for and treatment of hypertension increased life expectancy by five to six months, and periodic testing for cervical cancer could increase life expectancy by ten to fifteen years. Based on current medical evidence, the U. S. Preventive Services Task Force (2008) recommends proactive screening for numerous medical conditions and diseases, including hypertension and cervical cancer. These

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recommendations are based in part on whether the benefits of early detection and treatment outweigh the costs of screening.

Despite the potential health benefits of early detection, the direct and indirect costs of medical screening may discourage many individuals from seeking medical consultation. Indeed, health policies designed to increase medical screening rates generally reduce the direct costs borne by the individual. But even when the cost of medical consultation is negligible, many health diseases and conditions remain undiagnosed. For example, the cost of determining one's HIV status is modest relative to the potential health benefits: HIV testing is inexpensive, reliable, timely, and confidential, and the early detection of HIV can add years to life expectancy (Branson et al. 2006). Yet one-quarter of individuals infected with HIV remain undiagnosed in 2003 (Glynn and Rhodes 2005). If the detection and treatment of latent medical conditions is a prescribed policy goal, then understanding the motives for medical screening has important policy implications.

In this study, I consider whether individuals respond to monetary incentives to determine their diabetes mellitus (commonly known as Type 2 Diabetes, hereafter referred to as diabetes) status by exploiting a policy change to the Veterans Benefits Administration's (VBA) Disability Compensation (DC) program. The DC program compensates veterans for disabilities either acquired or aggravated during military service. Prior to the policy change, many Vietnam veterans causally associated their diabetes with Agent Orange exposure, an herbicide used extensively during the Vietnam War. But veterans seeking compensation for diabetes faced two difficulties with establishing service-connectedness. First, there was little to no medical evidence linking Agent Orange exposure to the onset or aggravation of diabetes. And second, applicants would have to provide documentation that they were exposed to Agent Orange during military service. Therefore, many veterans seeking DC benefits for diabetes were presumably denied.

This changed in November 2000 when the Department of Veterans Affairs deemed diabetes mellitus a presumptively service-connected disability. In accordance with U. S. Code Title 38, the policy change (hereafter referred to as T38), eliminated the two difficulties with establishing service-connectedness. First, the National Academy of Sciences, commissioned by the Department of Veterans Affairs, concluded there was limited or suggestive evidence of an association between exposure to dioxin, a chemical contained in Agent Orange, and the onset of diabetes. This substantiated the claim that diabetes was indeed service-connected. And second, the VBA would presume exposure to dioxin based on dates and location of service, so Vietnam veterans would not have to prove direct exposure. Because of the extensive use of Agent Orange in Vietnam, presumably all diabetic Vietnam veterans became medically eligible for DC benefits after the policy was implemented, which occurred in July 2001.

The change to DC eligibility criteria had a measurable impact on DC rolls and expenditures. Duggan, Rosenheck, and Singleton (2010) conclude that approximately 8 percent of Vietnam veterans alive in 2006 were either newly eligible for or enjoyed an increase in DC benefits, increasing DC expenditures by an estimated \$2.85 billion in fiscal year 2007 and total expenditures in net present value by \$50 billion. The effect of the policy on DC rolls was so evident that by the end of fiscal year 2006, 23.5 percent of the 947,598 Vietnam Era beneficiaries were receiving compensation for diabetes, compared to (at most) 6.2 percent of the 749,554 thousand Vietnam Era beneficiaries just five years earlier.[1]

Because a diabetes diagnosis is a requisite for DC benefit eligibility, the change to the DC program's medical eligibility criteria may have encouraged the detection of diabetes among the previously undiagnosed population. This is a plausible behavioral response since nearly one-third of adult diabetics in the United States remained undiagnosed (Cowie et al. 2003). If disability insurance positively impacts the incidence of diabetes diagnosis, then the prevalence of diagnosed diabetes (hereafter, "diagnosed" is implied unless otherwise noted) should rise in tandem with the increase in DC rolls and expenditures. If disability insurance has no detection effects, there should be no differential increase in diabetes prevalence among those affected by the policy.

Numerous studies examine the effect of disability insurance on labor supply - Autor and Duggan (2003); Autor and Duggan (2007); Black, Daniel, and Sanders (2002); Borsch-Supan (2000); Bound (1989); Chen and Van der Klaauw (2007); Duggan, Rosenheck, and Singleton (2010); Duggan, Singleton, and Song (2007); Gruber (2000); and Parsons (1980) - yet few studies examine detection effects of disability insurance. As with labor supply, detection effects are difficult to identify since benefit rules are generally applied universally, precluding analysis with separate treatment and comparison groups. In this study, however, the policy only affected Vietnam veterans, affording nonveterans as a comparison group.

But even when an adequate comparison group is available, the advent of disability insurance may change de facto medical standards for diagnosis. Kubik (1999) examines legislative changes to Supplemental Security Income and concludes that SSI benefits encouraged the detection and treatment of mental health conditions among children. And Cullen (2003) finds that an increase in supplemental revenue to schools to accommodate students with disabilities increases the percent of students defined as disabled. However, in both cases, the rise in prevalence may reflect imprecise medical standards rather than increased detection. In this study, I consider health measures that are arguably less subjective: the detection and treatment of diabetes.

One study by Thornton (forthcoming) experimentally examines whether monetary incentives encourage health investment. In her study, participants were offered a cash incentive to learn their HIV status sometime after they had been tested. Thornton finds that even small monetary incentives have a measurable effect on whether individuals returned to learn their HIV status. But it is difficult to extrapolate her findings to T38 because the study was conducted in Malawi, a developing country where monetary incentives may be more effective.

This study also contributes to the discussion of the causal pathways responsible for the socioeconomic status and health gradient; in particular, differential discount

1. There were 46,395 total compensated diabetes cases on the rolls at the end of fiscal year 2001 and 215,430 compensated cases among Vietnam Era beneficiaries at the end of fiscal year 2006. There were 749,554 and 947,598 Vietnam Era beneficiaries receiving disability compensation at the end of fiscal years 2001 and 2006, respectively (Veterans Benefits Administration Reports, Selected Years). The figures cited are calculated assuming that all cases in 2001 were among Vietnam Era veterans, a conservative assumption, and that each beneficiary can have at most one diabetes case.

rates. The differential discount rate hypothesis states that patient individuals value future returns relative to immediate returns more so than less patient individuals; so patient individuals may invest more in human capital, including both education and health (Fuchs 1982). Time preferences, however, are difficult to quantify for empirical analysis. In this study, I circumvent this difficulty by examining a policy that effectively shifted the returns of health investment from the future (marginal declines in mortality and morbidity later) to the present (disability benefits effective immediately).

To estimate the impact of T38 on the prevalence of diabetes among the veteran population, I use data from the National Health Interview Survey. Survey years 1997 through 2006 are chosen to adequately span before and after T38 was announced and implemented. In the empirical analysis, the treated population of interest is male veterans born in 1944 through 1950. Nearly all Vietnam veterans are male, and a significant proportion of male veterans born during these years served in or around Vietnam.

Using nonveteran males as a comparison group, I find that T38 increased the prevalence of diabetes by 3.1 percentage points among veterans. However, only half of veterans in the NHIS sample actually served in or around Vietnam, so the policy may have increased diabetes prevalence by as much as 6.1 percentage points among those affected by the policy. The results are not likely to reflect differential exposure to Agent Orange because, according to a publication by the Institute of Medicine, the increased risk of diabetes onset from herbicide exposure is ‘‘small’’. Auxiliary analysis suggests that the increase in diabetes prevalence is driven by new diagnoses and not false-positive survey responses.

If the detection of latent medical conditions is a prescribed policy goal, the results presented here have a direct policy implication: provide monetary incentives to encourage medical screening and treatment. The efficiency of such a policy depends on the extent to which the benefits of early detection and treatment offset the additional medical costs and DC expenditures. Based on a simplified calculation, I estimate that the value of additional time lived offsets approximately 9 percent of the total costs at both the aggregate and individual levels.

II. VBA Disability Compensation

A. Program Rules

DC benefits are awarded to veterans with disabilities deemed to be service-connected, defined as a disease or injury either aggravated or acquired during active military service (which includes wartime and peacetime service). At the initial application level, applications are vetted through a three-member Rating Board composed of one medical and two nonmedical personnel. The Board evaluates the disability and its service-relatedness by reviewing the timing and nature of the medical condition and the applicant’s service record. An applicant may appeal the initial decision to the Board of Veterans Appeals; subsequent appeals are made to the U.S. Court of Appeals for Veterans Claims, the U.S. Court of Appeals for the Federal Circuit, and finally the Supreme Court of the U.S.

DC benefit amounts are based on the severity of the disability or disabilities, quantified by the combined disability rating (CDR). To determine the CDR, disabilities and their severity are first identified using the VBA's Schedule for Rating Disabilities, which defines and quantifies each disability using a percentage scale ranging from 0 percent to 100 percent (totally disabled) by increments of 10 percent. If a veteran exhibits multiple service-connected disabilities, the individual ratings are aggregated using the combined ratings table. The total disability is then rounded to the nearest 10 percent to yield the CDR. The benefit amount associated with a particular CDR is statutorily defined as the average reduction in earnings capacity resulting from the disability.[2] The benefit amount increases nonlinearly with the CDR: in 2001, the monthly payment to a single veteran with no dependents and a CDR of 10, 50, and 100 percent was $103, $625, and $2,163, respectively.[3] As a compensation program, disability benefits are not offset by other earned or unearned income.

Compensation for multiple and partial disabilities is a defining feature of the DC program – Social Security Disability Insurance and Supplemental Security Income do not provide benefits for partial disabilities - and is quite common among DC beneficiaries.[4] According to the combined ratings table, the impact of an additional disability on the total disability is based on residual functional capacity, so the loss in earnings resulting from each additional disability are not considered additive. For example, if a veteran has two separate disabilities rated at 70 percent and 50 percent, only 30 percent of his or her ability is subject to the second 50 percent disability. The overall disability would therefore be 85 percent, which is subsequently rounded to 90 percent. In this manner, the combined ratings table precludes a CDR greater than 100.

B. U.S. Code Title 38: Agent Orange and Diabetes

From 1965 to 1971, U.S. military forces sprayed approximately 18 million gallons of Agent Orange in all four military zones of Vietnam. Shortly after the War ended, veterans become increasingly concerned that exposure to dioxin, a chemical contained in Agent Orange, may pose adverse health consequences. In 1978, the Veterans Health Administration established the Agent Orange Registry to monitor the health of veterans who were likely exposed to herbicides; however, no conclusive evidence existed at that time which linked Agent Orange exposure to the onset of any determinable medical condition. Therefore, most veterans who sought DC benefits for conditions associated with Agent Orange exposure were presumably denied.

2. "The Secretary shall adopt and apply a schedule of ratings of reductions in earning capacity resulting from specific injuries or combination of injuries… based, as far as practicable, upon the average impairments of earning capacity resulting from such injuries in civil occupations (U.S. Code, Title 38, Part II, Chapter 11, Subchapter VI, Section 1155)."

3. Dependency benefits are payable to beneficiaries who have a combined disability rating that is at least 30 percent.

4. At the end of fiscal year 2005, there were 7.68 million compensated disabilities and 2.64 million DC beneficiaries, averaging 2.9 disabilities per beneficiary. While SSDI does not explicitly compensate for partial disabilities, an SSDI applicant can satisfy the severity requirement of the general disability standard maintained by SSA -'inability to engage in any substantial gainful activity' – with either a single impairment or a combination of impairments.

Diabetes was perhaps the most common medical condition anecdotally associated with dioxin exposure. However, in 1994, a scientific review committee comprised of Institute of Medicine (IOM) researchers concluded that there was inadequate or insufficient evidence of an association between dioxin exposure and the onset of diabetes (IOM, 1994).[5] The IOM reversed its decision in 1999, based in part on new research conducted by the National Institute for Occupational Safety and Health (Calvert et al. 1999) and the U. S. Air Force (Air Force Health Study 2000), concluding that there was limited or suggestive evidence of an association between dioxin exposure and diabetes. Its new conclusion, which reversed the conclusion of three preceding committees, was published in a report titled, ''Veterans and Agent Orange: Herbicide/Dioxin Exposure and Type 2 Diabetes,'' released in October 2000 (IOM 2000).

One month after this publication's release, in November 2000, the Acting Secretary of the Department of Veterans Affairs announced that diabetes would be deemed a presumptively service-connected disability for Vietnam veterans (Department of Veterans Affairs News Release 2000).[6] This substantiated the claim that diabetes was indeed service-connected. Moreover, ''presumption'' meant that Vietnam Era veterans did not have to prove direct exposure to Agent Orange; instead, the VBA would presume exposure based on period and location of service. Because of the extensive use of Agent Orange in Vietnam, presumably all diabetic Vietnam veterans became medically eligible for DC benefits.

Compensation by the DC program for diabetes could be substantial. Because the change in benefits due to diabetes is determined by residual functional capacity, the impact of T38 on a beneficiary's disability payment depends on his or her initial disability rating. Table 1 reports the initial benefit amount and the change in monthly benefits resulting from an appended diabetes case, illustrating the varied, and considerable, incentive for Vietnam Era veterans to seek compensation for diabetes after T38's implementation. A veteran whose diabetes was rated at 10 percent (requiring a restricted diet only) and was not previously receiving DC benefits (an initial rating of 0 percent) would receive $1,236 in benefits annually after T38. The change in benefits could be as large as $25,956 annually, resulting from a 0 percent initial disability rating and a 100 percent diabetes rating.

The increased generosity of DC benefits had a measurable impact on DC enrollment and expenditures. According to Duggan, Rosenheck, and Singleton (2010), 6.0 percent of Vietnam veterans became newly eligible for DC benefits, and another 1.7 percent received an increase in preexisting DC benefits, increasing DC expenditures by approximately $50 billion in net present value. The increase in DC rolls and expenditures - and the abundance of VBA sponsored pamphlets and websites to disseminate information regarding Agent Orange and DC benefits – suggests that veterans were well informed of the policy change.

5. Subsequent IOM reports, Update 1996 and Update 1998, upheld this initial position.

6. Currently, thirteen medical conditions are presumptively service-connected due to Agent Orange exposure, including Hodgkin's disease, chronic lymphocytic leukemia, prostate cancer, and respiratory cancers. Certain Vietnam Era veterans who served in the Korean War would also become eligible for DC benefits, but their exposure and consequent service-connectedness would not be presumed.

Table 1
Initial Monthly Benefit and Change after T38 by Diabetes Rating, 2002

Initial rating (percent)	0	10	20	30	40	50	60	70	80	90	100
Initial benefit (dollars)	0	103	199	306	439	625	790	995	1155	1299	2163
Diabetes rating (percent)[a]	Change in Monthly Benefit (dollars)										
10	103	96	107	133	186	165	0	0	0	0	0
20	199	203	240	133	186	165	205	160	0	0	0
40	439	522	426	484	351	370	365	160	144	0	0
60	790	687	796	689	716	530	365	304	144	864	0
100	2,163	2,060	1,964	1,857	1,724	1,538	1,373	1,168	1,008	864	0

Source: Author's Tabulations
Note: All figures are in 2002 dollars.
a. Diabetes ratings are defined as follows: 0 percent, manageable by restricted diet only; 20 percent, rating of 10 percent and requiring insulin or hypoglycemic agent; 40 percent, rating of 20 percent and regulation of activities; 60 percent, activities with episodes of ketoacidosis or hypoglycemic reactions requiring one or two hospitalizations per year or twice a month visits to a diabetic care provider, plus complications that would not be compensable if separately evaluated; 100 percent: requiring more than one daily injection of insulin, restricted diet, and regulation of activities (avoidance of strenuous occupational and recreation activities) with episodes of ketoacidosis or hypoglycemic reactions requiring at least three hospitalizations per year or weekly visits to a diabetic care provider, plus either progressive loss of weight and strength or complications that would be compensable if separately evaluated.

III. Data

A. Data Specifications

In this study, I consider whether disability insurance encourages the detection of latent medical conditions, identified by the advent of DC benefits for diabetes. If monetary incentives do increase the incidence of detection, then the prevalence of diabetes should rise in tandem with the growth in disability rolls. The National Health Interview Survey (NHIS), conducted annually by the National Center for Health Statistics, is well suited for this study. The NHIS is a standard data source for tracking trends in health behavior and illness, including diabetes prevalence, in the noninstitutionalized U.S. population. In contrast to the Behavioral Risk Factor Surveillance System, another commonly used health survey, the NHIS is conducted nationally and is considerably more standard across both U. S. states and time. Additionally, the sample sizes of the NHIS surveys are quite large relative to the National Health and Nutrition Examination Survey, another common source for health statistics.

I use annual NHIS data from 1997 through 2006 and restrict them to the population of interest. The survey years were chosen to adequately span before and after T38 was announced and implemented, which occurred in November 2000 and July 2001, respectively. Within each interviewed household, only one adult is asked extensive questions about his or her health status, including explicit questions about diabetes. Only these adults, contained in the Sample Adult file, are used for the analysis. I consider the impact of the policy on male veterans; due to the low frequency in the data, female veterans are dropped from the sample. As a comparison group, I consider all nonveteran males who were not directly affected by the policy. I drop observations that contain missing or unreported values for variables pertinent to the study: education, marital status, military service, health insurance status, and self-reported diabetes diagnosis.

A limitation of the NHIS, as well as the other health surveys, is that respondents are only asked whether they are veterans; era or location of service is not reported. Because the presumption clause of T38 only applied to Vietnam veterans who served in or around Vietnam, it is difficult to precisely identify veterans affected by T38 in these data.

To identify those most likely to have been affected by the policy, I use the Veteran Supplement of the Current Population Survey, available biannually, which contains information on period of service as well as location. I restrict the Current Population Survey to males; and to increase the sample size, I pool supplements from years 1997, 1999, and 2001. In Figure 1, I separately plot the percentages of veterans, Vietnam Era veterans, and Vietnam veterans who served in or around conflict areas by year of birth.[7] The graph illustrates a downward trend in the proportion of veterans from older to younger birth cohorts, but there is a noticeable spike in the proportion of veterans between birth years 1944 and 1950. The

7. The Veterans Supplement does not contain year of birth; it is approximated by subtracting age reported in the survey from the survey year. Vietnam veteran refers to veterans who served near Vietnam, Laos, or Cambodia by land, air, or water.

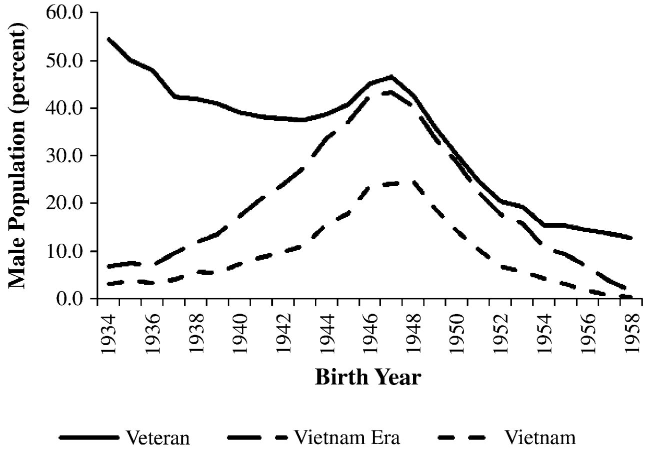

Figure 1
Veteran Status by Year of Birth, Males in the CPS 1997, 1999, and 2001

increased demand for manpower during the Vietnam War explains nearly the entire spike, indicated by Vietnam Era veteran line. However, not all Vietnam Era veterans actually served in Vietnam. According to the Vietnam veteran line, approximately half of these Vietnam Era veterans served in or around Vietnam. Thus, I restrict the NHIS data to veteran and nonveteran males born between 1944 and 1950, keeping in mind that T38 applied to only half of the veterans in this sample.

B. Data Summary

Table 2 contains standard demographic information from the NHIS analysis sample by veteran status. By construction, veterans and nonveterans are similar in age. The rate of marriage, which may provide protective health effects, is also similar between veterans and nonveterans. A notable difference between the two groups is educational attainment. More specifically, the distribution of educational attainment among nonveteran males has denser tails compared to veteran males: 18.0 percent of nonveteran males have less than a high school diploma and 35.6 percent have a college degree or more, compared to 6.6 percent and 27.4 percent among veteran males. The racial composition is similar between veterans and nonveterans, with veterans slightly less likely to be a racial minority.

Veterans and nonveterans also differ by insurance rates: 7.5 percent of veterans report no health insurance, compared to 12.3 percent of nonveterans. This disparity is not due to differences in rates of insurance through an employer, which is reported by approximately 80 percent of both veterans and nonveterans. The gap in uninsured rates partially reflects health insurance through the Veterans Health Administration (VHA), reported by 13.0 percent of veterans, though 4.1 percent report both VHA and employer-provided health insurance.

Table 2
Demographic Characteristics by Veteran Status

Veteran Status	Veterans	Nonveterans
Observations	5,621	9,022
Age	54.1	53.5
	(0.05)	(0.04)
Married	74.7	74.5
	(0.58)	(0.46)
Race		
White	89.2	82.1
	(0.41)	(0.40)
Black	7.8	9.8
	(0.36)	(0.31)
Other	3.0	8.1
	(0.23)	(0.29)
Education		
Less than high school	6.6	18.0
	(0.33)	(0.40)
High school	30.5	24.5
	(0.61)	(0.45)
Some college	35.5	21.9
	(0.64)	(0.44)
College and beyond	27.4	35.6
	(0.59)	(0.50)
Insurance status		
Private	79.9	79.5
	(0.53)	(0.43)
VHA	13.0	0.4
	(0.45)	(0.07)
None	7.5	12.3
	(0.35)	(0.35)

Source: Author's tabulation from pooled NHIS data, 1997 through 2006.
Note: The data are restricted to males born in or between 1944 and 1950 that contain no missing values for the listed variables. With the exception of age, which is measured in years, all figures are percentages. Adult Sample weights were used. Standard errors are in parentheses.

C. Diabetes Prevalence and Difference-in-Differences Estimates

Keeping in mind the differences in demographic characteristics between veterans and nonveterans, I construct preliminary evidence of the impact of T38 on diabetes prevalence by plotting the prevalence of diabetes by veteran status and time (Figure 2, Panel A, estimates and standard errors are reported in the Appendix Table). I initially define diabetics as those who report either diabetes or borderline diabetes, since it is

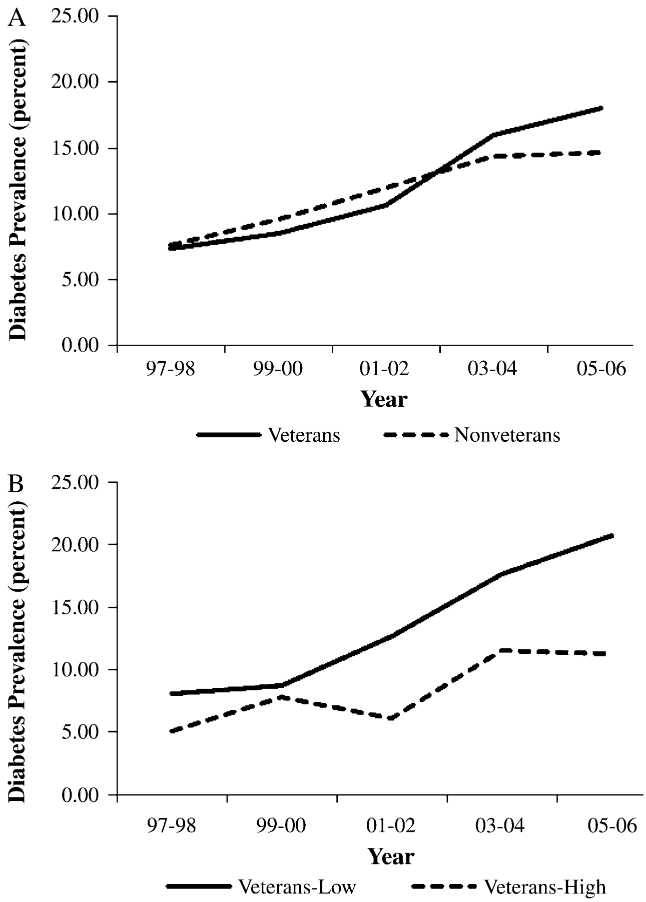

Figure 2
(A) Diabetes Prevalence by Veteran Status (B) Diabetes Prevalence by Education Attainment among Veterans, Males in the NHIS 1997 through 2006

unclear a priori at what stage of diabetes onset the marginal diagnosis most likely occurs.[8] To reduce sampling noise, estimates are calculated within two-year increments.

According to the figure, the series of prevalence estimates before T38 appear similar in level and slope, but shortly after T38 was implemented, diabetes prevalence

8. The American Diabetes Association defines diabetes as a fasting glucose level greater than 125 mg/dL. In the NHIS, diabetes status was determined by a question contained in the Sample Adult questionnaire: "Ever been told you have diabetes?" Respondents may also report having been diagnosed with borderline diabetes. Borderline diabetes, which has since been renamed pre-diabetes by the American Diabetes Association, is defined as a fasting glucose level at or between 100 and 125 mg/dL. Gestational diabetes, which occurs among women during pregnancy, is asked separately from diabetes.

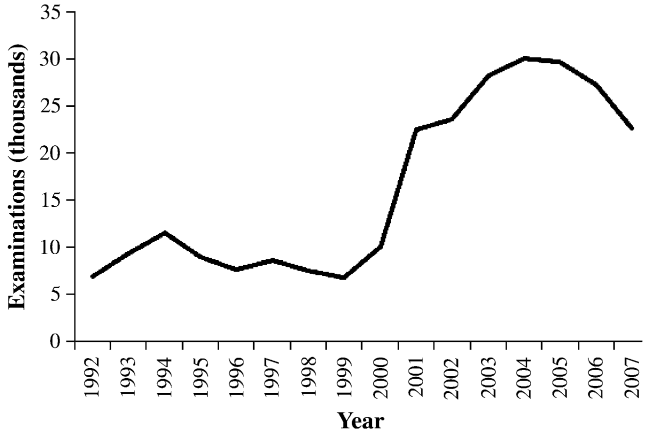

Figure 3
Agent Orange Registry Examinations, 1992 through 2007

increased differentially among veterans relative to nonveterans. Assuming the prevalence of diabetes would have increased similarly among both groups in the absence of T38, the figure suggests that much of the rise in diabetes among veterans is attributable to the policy.

Although the figure is suggestive, it is important to address the apparent time lag between T38's announcement and implementation and the differential rise in diabetes among veterans: the periods 1999/2000 and 2001/2002 more accurately represent periods just before and after the policy, though diabetes prevalence increased the most from 2001/2002 to 2003/2004. The delayed rise in diabetes would be problematic if knowledge of and response to the policy were immediate. But if there is a reasonable delay between the policy's implementation and diabetes detection, the policy may not have a measurable effect on the prevalence of diabetes until well after it became effective.

The number of examinations conducted through the Agent Orange Registry, which specifically screens for diabetes, sheds some light on this issue. In Figure 3, I plot the number of Registry examinations conducted by year from 1992 to 2007.[9] The figure indicates that there were generally less than 10,000 examinations conducted annually in years prior to the policy. But from 2001 to 2007 – the year when T38 was implemented and the last full year of data, respectively – the number of examinations exceeded 20,000, and peaked at approximately 30,000 in 2004 and 2005. Although the increase in examinations occurred discretely in 2001, the heightened number of examinations persists well after the policy was implemented. Thus, a reasonable lag between the policy's implementation and a measureable effect on diabetes prevalence is expected.

I use the standard difference-in-differences estimator to quantify the impact of T38 on diabetes prevalence. Because of the delayed response to T38, I omit survey years

9. Agent Orange Registry examination figures are unpublished and were obtained directly from the Environmental Agents Service, DVA Central Office.

2001 and 2002 from the NHIS sample and consider years 1997 to 2000 as the pre-reform period and years 2003 to 2006 as the post-reform period. Between these two periods, the prevalence of diabetes increased from 7.9 percent to 17.0 percent among veterans and from 8.6 percent to 14.5 percent among nonveterans. The difference-in-differences estimate is therefore 3.2 percentage points (standard error: 1.2), which is interpreted as the effect of the policy on the prevalence of diabetes among veterans by 2003 through 2006.

In addition to year of birth, I consider other instruments in the Veteran Supplement to further identify veterans who served in or around Vietnam. To be a reasonable and feasible instrument, the policy should have no impact on the instrument, and the variable must be available in both the Veteran Supplement and the NHIS. Conditional on veteran status, educational attainment is the only reasonable and observable characteristic correlated with Vietnam veteran status. According to the Veteran Supplement, 43.2 percent of male veterans with a college degree (and born in or between 1944 and 1950) served in or around Vietnam, compared to 52.6 percent of veterans without a college degree. Therefore, if less educated, veteran males are more likely to have served in Vietnam, then the rise in diabetes should be greater among them relative to their more educated, veteran counterparts.

To determine if this is indeed the case, I plot the prevalence of diabetes among veterans by educational attainment between 1997 and 2006 in Figure 2, Panel B. Although the prevalence trends are noisier due to smaller samples, the figure suggests that much of the rise in diabetes among veterans is driven by the less educated.

Although the aggregate trends suggest that T38 increased the prevalence of diabetes among veterans, and that the increase is concentrated among those more likely to have served in Vietnam, the results remain inconclusive. The most notable concern is that individuals born in 1944 through 1950 are between the ages of 46 and 62 during the analysis period, ages at which diabetes prevalence is on the rise. Therefore, if there are unobservable factors that generate a heightened prevalence of diagnosed diabetes at these ages – due to different rates of diabetes onset or detection or both - that are correlated with veteran status or their socioeconomic characteristics, then it would be misleading to attribute the entire rise in diabetes among veterans to T38.

The first concern is whether unobservable factors unique to veterans differentially affect the prevalence of diabetes as they age. To explore this possibility, I use NHIS data from 1997 through 2001 – before the apparent effect of the policy on diabetes prevalence – to estimate the cross-sectional prevalence of diabetes by veteran status and age. The diabetes-by-age profiles are plotted in Figure 4. As indicated, the profiles of veterans and nonveterans are similar in level and slope, suggesting that the differential rise among veterans in Figure 2, Panel A is not due to factors unique to veterans. However, not all veterans in Figure 4 served during the Vietnam Era (particularly at older ages, see Figure 1), so there may still be factors unique to Vietnam Era veterans that differentially affect the prevalence of diabetes during the analysis period.

The most obvious factor unique to Vietnam veterans is dioxin exposure: if dioxin is indeed a causal factor for diabetes onset, then Vietnam veterans may exhibit a differential rise in diabetes diagnosis as they age. But this alternative mechanism seems unlikely according to a follow-up report by the National Academy of Sciences: “The increased risk, if any, posed by herbicide or TCDD exposure appears to be small…

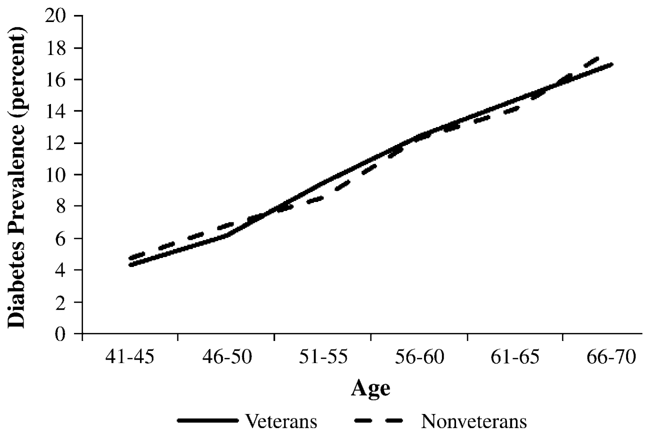

Figure 4
Diabetes Prevalence by Veterans Status and Age, Males in the NHIS 1997 through 2001

the known predicators of diabetes risk – family history, physical inactivity, and obesity – continue to greatly outweigh any suggested increased risk posed by wartime exposure to herbicides (IOM 2002)." Thus, differential exposure to Agent Orange should not drive the empirical results.

Another concern is that Vietnam Era veterans and nonveterans face different costs associated with medical screening. To be sure, Vietnam veterans were eligible for free diabetes screenings through the Agent Orange Registry, whereas 11.4 percent of nonveterans were uninsured (Table 1); so even if the prevalence of undiagnosed diabetes increased similarly between the two groups, it may be more readily detected among veterans as they age. But this alternative explanation cannot account for the precipitous increase in Agent Orange Registry examinations illustrated in Figure 3.

A final concern is that the cohort of veterans and nonveterans in the analysis sample vary across observable socioeconomic and demographic dimensions. If the rate of diabetes onset or detection varies along these dimensions by age, then the differential rise among veterans may not be entirely due to T38. In the next section, I control for these differences using a regression framework.

IV. Regression Framework

A. Empirical Specification

The aggregate trends suggest that T38 increased the rate of self-reported diabetes among the veteran population. However, veterans and nonveterans systematically differ along observable characteristics; so if the incidence of diabetes differs along these dimensions as they age, the differential increase in diabetes diagnoses among veterans may be spurious.

A regression framework addresses this issue by estimating the effect of T38 while simultaneously controlling for observable characteristics. The preferred, linear probability specification is given by:

$$(1)\quad P(D_{it} = 1|.) = \alpha + \beta Vet_i * Post_t + \sum_{\tau=1}^{7} \delta_\tau Year_t + \gamma Vet_i + \theta X_{it},$$

where $P(D_{it} = 1|.)$ is the conditional probability that individual i in time t reports having been diagnosed with diabetes. The variable Vet_i is an indicator of veteran status, equaling one if the respondent is a veteran and zero otherwise. The variable $Post_t$ is an indicator for after T38. As before, survey years 2001 and 2002 are omitted, so the post-reform term equals zero for years 1997 through 2000 and one for years 2003 through 2006. Variation in the interaction of these two variables identifies β, the differential change in the prevalence of diagnosed diabetes among veterans. The individual-year fixed effects control for average changes in diabetes prevalence over time. The veteran fixed effect captures the time-invariant difference in diabetes prevalence between veterans and nonveterans. And X_{it} controls for a vector of individual characteristics; race (black only and other race relative to white only), education (high school, some college, and college and beyond relative to no high school degree), and individual-age fixed effects.

The coefficient on the interaction term β represents the relative change in diabetes prevalence among veterans after T38 was implemented. To causally associate β with the advent of DC benefits for diabetes, it must be assumed that veterans and nonveterans would have exhibited similar changes in diabetes prevalence over time in the absence of T38, measured by the year fixed effects. Since veterans systematically differ along socioeconomic and demographic dimensions, which may interact with age over time, this identification assumption may not be valid. To plausibly control for the effects of socioeconomic and demographic factors on diabetes prevalence over time, I include full interaction terms of race and educational attainment with individual-age fixed effects.

B. Regression Results

The baseline estimates from the NHIS sample are presented in Panel A of Table 3. Initially, I exclude race and education from the specification and estimate a model of diabetes diagnosis with the post-T38 term interacted with the veteran indicator, the veteran indicator singly, and the individual-age and year fixed effects. The estimate of β, presented in Column A1, implies that T38 increased the prevalence of diabetes among veterans by 3.0 percentage points after the policy was implemented. This is similar to the difference-in-differences estimate of 3.2 percentage points calculated in the previous section. Only half of veterans in this sample actually served in or around Vietnam, so T38 reasonably increased the prevalence of diabetes among Vietnam veterans by six percentage points. The results also indicate that veterans are slightly less likely to be diagnosed with diabetes relative to nonveterans, but the estimate is not statistically significant. The estimates on the individual-age and year fixed effects (not shown) indicate that diabetes prevalence increases with both age and time.

Table 3
Linear Probability Estimates of Diabetes Diagnosis by Educational Attainment

	A. All Education Groups			B. No College Degree		
	(1)	(2)	(3)	(1)	(2)	(3)
Vet*Post	3.00	3.01	3.05	3.66	3.79	4.16
	(1.49)*	(1.48)*	(1.48)*	(1.86)*	(1.86)*	(1.88)*
Vet	−0.95	−0.53	−0.47	−1.85	−0.85	−0.91
	(0.82)	(0.84)	(0.84)	(1.01)	(1.04)	(1.04)
Black		6.68	−3.25		5.84	−4.03
		(1.34)**	(3.01)		(1.51)**	(3.07)
Other race		7.14	9.61		6.66	−1.48
		(1.67)**	(10.42)		(2.11)**	(1.14)
High school		−2.56	8.28		−2.57	7.11
		(1.23)*	(5.24)		(1.24)*	(5.00)
Some college		−3.34	0.05		−3.27	−0.32
		(1.25)**	(1.16)		(1.25)**	(0.67)
College and beyond		−7.12	4.09			
		(1.14)**	(3.23)			
N	11,625	11,625	11,625	8,081	8,081	8,081
R-squared	0.02	0.03	0.04	0.03	0.03	0.04

Note: Estimates from NHIS sample described in Table 2, except years 2001 and 2002 are excluded. Individual year and age fixed effects are included in all specifications, but the estimates are not presented for brevity. In the third column of both panels, full interactions between race and education with individual age fixed effects are also included. Estimates are factored by 100 and are interpreted as percentage point changes. Adult Sample weights were used. Robust standard errors are in parentheses. ** and * indicates significance at the 1 percent and 5 percent level of confidence.

I add covariates to the specification in Column A1 in two steps. First, in Column A2, I include the race and education fixed effects to the specification equation. The estimate of β is 3.0 percentage points, which is similar to the estimate in Column A1. And second, in the final column of Panel A, I include full interaction terms between race and educational attainment, separately, with individual-age fixed effects, allowing the effects of race and education to vary by age. To control for the interaction between race and age, the white, black, and other race fixed effects are fully interacted with the individual-age fixed effects (ages 46 through 62). The omitted term is the interaction between the white and age 46 indicators. Education is similarly interacted with age, and the omitted term is the interaction between the less than high school and the age 46 indicators. The race and education coefficients that are reported in Table 3 are interpreted as the effects of race and education at age 46. As indicated in Column A3, the estimated impact of T38 is 3.1 percentage points when interactions of race and education with age are included. Given the prevailing assumption, the policy increased diabetes by 6.1 percentage points among those affected by the policy.

I consider three alternative specifications of the data in comparison to Panel A of Table 3. First, because Vietnam veterans are less likely to have a college degree, I estimate the same specifications excluding those with college degrees. The estimates are presented in Panel B of Table 3. Across all specifications, the estimated effect of T38 on the prevalence of diagnosed diabetes is greater than those in Panel A. According to Column B3, diabetes increased by 4.2 percentage points among veterans relative to nonveterans.

Second, I redefine the outcome variable as diabetes only; so in contrast to the previous specification, the outcome variable equals zero for those who report borderline diabetes. Using the same specification equation and sample as Column A3 of Table 3, the estimate of β (not shown) is 2.5 percentage points and statistically significant at the 10 percent level of confidence. In comparison to Column A3 of Table 3, the estimate suggests that borderline diabetes diagnoses account for about 20 percent of the overall rise in diabetes prevalence.

And third, I define the comparison group as nonveteran females who, like nonveteran males, were not directly affected by the reform. (There are not enough veteran females in the data to compare veteran females to nonveteran females.) Again, using the same specification equation as Column A3 of Table 3, the estimate of β (not shown) is 4.2 percentage points and statistically significant at the 1 percent level of confidence. Taken together, the substantive results in Panel A of Table 3 appear robust to several specifications of the data.

C. Mechanism and Other Margins of Response

The increase in DC enrollment and expenditures and the simultaneous rise in diabetes prevalence among veterans suggest that monetary incentives encourage the detection of latent medical conditions. The results do not appear to reflect demographic factors associated with veterans status, and the discrete increase in Agent Orange Registry examinations supports the contention that the rise in diabetes diagnosis results from the detection of previously undiagnosed diabetes.

To provide further evidence for the suggested mechanism, I estimate the probability of being diagnosed with diabetes during the past five years of the survey year using the same sample above.[10] A five year lag was chosen because the last year of the NHIS sample is 2006 and the policy was implemented in 2001. Controlling for race, education, and individual-age fixed effects, as well as full interactions between race and education with age, the difference-in-differences estimate of recently diagnosed diabetes is 2.4 percentage points (Column 1 of Table 4). The estimate suggests that much of this rise in diagnosed diabetes is a result of recent diagnoses.

If veterans are more likely to report being diabetic without being formerly diagnosed (false-positives), then the detection effects of disability insurance may be overstated. I consider three outcomes to address this concern. First, I estimate the probability of treatment for diabetes, defined as taking insulin or diabetes pills, which is arguably less influenced by false-positive responses than a diagnosis singly. The estimated effect, in Column 2 of Table 4, is 2.2 percentage points and statistically

10. The number of years since diabetes diagnosis was approximated by subtracting the reported age at the time of diagnosis from the reported age at the time of the survey.

Table 4
Linear Probability Estimates of Auxiliary Outcome Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome Variable	Recent	Treatment	Hypertension	Cancer	Fair/Poor	Overweight	Obese
Mean (percent)	5.22	9.39	35.85	6.37	14.75	74.43	28.32
Vet*Post	2.37	2.23	2.35	0.93	−0.15	−0.1	0.85
	(1.05)*	(1.33)	(2.15)	(1.11)	(1.48)	(1.93)	(2.07)
Vet	−0.77	−1.24	1.81	0.41	−0.11	1.04	1.92
	(0.57)	(0.72)	(1.39)	(0.65)	(0.89)	(1.32)	(1.35)
Black	−2.66	−1.86	3.7	−1.13	−5.52	−5.55	3.19
	(2.59)	(2.71)	(10.98)	(0.77)	(5.50)	(10.99)	(10.56)
Other race	−1.06	10.56	−4.02	−4.23	28.57	−20.36	−5.01
	(0.68)	(10.50)	(9.38)	(2.83)	(14.45)*	(13.46)	(9.66)
High school	4.87	6.31	−13.24	−0.62	−13.38	−2.54	5.41
	(4.61)	(4.83)	(14.27)	(0.47)	(12.79)	(13.88)	(10.68)
Some college	−0.17	0.43	−25.35	0.8	−16.08	11.76	−3.5
	(0.48)	(1.2)	(12.79)*	(1.21)	(12.24)	(12.32)	(9.29)
College and beyond	0.21	2.5	−11.49	7.25	−21.99	−10.97	10.76
	(1.00)	(2.95)	(14.08)	(4.97)	(12.08)	(13.93)	(11.82)
N	10,975	11,624	11,608	11,615	11,614	11,453	11,453
R-squared	0.02	0.04	0.05	0.02	0.1	0.03	0.03

Note: Estimates from NHIS sample described in Table 2, except years 2001 and 2002 are excluded. The sample size is reduced due to missing values for the outcome variable. Individual year and age fixed effects and full interactions between race and education with individual age fixed effects are included, but the estimates are not presented for brevity. Estimates are factored by 100 and are interpreted as percentage point changes. Adult Sample weights were used. Robust standard errors are in parentheses. ** and * indicates significance at the 1 percent and 5 percent level of confidence.

significant at the 10 percent level of confidence. In comparison to the estimated increase in diagnosed diabetes (3.1 percentage points), the estimate implies that 71 percent of newly diagnosed diabetics receive treatment, which exceeds the 38 to 44 percent of treated diabetes cases among all diabetics in the United States (Dewitt and Hirsch 2003). Second, I estimate the probability of having been diagnosed with hypertension — a condition that may be coincidentally diagnosed with diabetes but, unrelated to T38, lacks an incentive to be falsely reported. The estimate, in Column 3 of Table 4, indicates that the prevalence of diagnosed hypertensives increased by 2.4 percentage points, though the estimate is statistically insignificant. And third, I estimate the effect of the policy on the prevalence of cancer. In addition to diabetes, Agent Orange is associated with the onset of certain cancers, though some types of cancer were deemed presumptively service-connected before T38 was implemented. Thus, false-positive responses for cancer may the affect prevalence of cancer, but the rate should not change differentially among veterans when T38 was implemented. The estimate, presented in Column 4 of Table 4, suggests no measurable effect of T38 on the prevalence of cancer. Taken together, the results suggest that the observed rise in diabetes reflects the detection of undiagnosed diabetes rather than false-positive survey responses.

I next consider the effect of the policy on two outcomes that do not necessarily require a medical diagnosis. The first outcome is self-reported health status, which may be positively or negatively affected by the early detection and treatment of diabetes. On one hand, the detection and treatment of diabetes may ameliorate symptoms that were previously unaddressed, which may improve self-assessed health. On the other hand, if the condition was unexpected, it may worsen self-reported health status (conversely, if the condition was expected and the diabetes test is negative, self-assessed health may improve). The estimate, presented in Column 5, indicates that there was no measurable change in self-reported health, on average, among veterans after the policy was implemented.

The second outcome is body mass. Similar to self-reported health status, the effect of the policy on body mass is ambiguous. On one hand, moral hazard behavior may increase body mass since diet and exercise are two of the three known causes of diabetes – the third being family background. On the other hand, individuals who learn that their fasting glucose level is above normal may try to curb the progression of diabetes by losing weight. I consider separately the likelihood of being overweight and obese, defined as having a body mass index in excess of 25 and 30, respectively.[11] The estimates, presented in Columns 6 and 7, suggest that the policy is not associated with a differential change in body mass among veterans.

V. Policy Implications

The results presented here suggest that monetary incentives encourage the detection of latent medical conditions. In fact, based on simplifying assumptions, T38 encouraged the detection of approximately 40 percent of diabetes cases

11. Body mass index is weight, in kilograms, divided by height, in meters, squared.

among Vietnam Era veterans−80 percent of diabetes cases among Vietnam veterans – that would have otherwise been undiagnosed.[12] If identifying conditions among undiagnosed populations is a prescribed policy goal, then the apparent response of veterans to T38 has a direct policy implication: provide monetary incentives to encourage medical screening. The efficiency of such a policy, of course, depends on the extent to which the benefits of early detection offset the costs.

One benefit of detecting and treating diabetes early is the value of additional time lived. Using two sources, I approximate this value for an individual who is diagnosed five years earlier at age 55 – close to the average age of veterans in the sample – compared to a clinical diagnosis at age 60. Assuming diabetes onset occurred at age 50, Hoerger et al. (2004) estimate that detecting and treating diabetes at age 55 compared a clinical diagnosis at age 60 increases life expectancy by 0.18 years (from 18.96 to 19.14 years). Murphy and Topel (2006) appraise the value of the life-year at age 74, the age when this marginal increase in life expectancy occurs, at approximately $200,000. To calculate the present value of additional time lived, I factor $200,000 by 0.18 to determine the current value of additional time lived, and discount this product 19 years at an annual rate of 3 percent, yielding $20,530 in present value.

To approximate the aggregate value of additional time lived due to T38, I factor the individual value of additional time lived by the estimated number of newly diagnosed diabetes cases as a result of the policy. To determine the number of new diagnoses, I factor the estimated effect of the T38 on the prevalence of diabetes among Vietnam Era veterans (3.1 percent) and the estimated 7.3 million Vietnam Era veterans in 2006, yielding 226,207 newly diagnosed diabetes cases.[13] The welfare gain per newly diagnosed diabetic is $20,530, so the total welfare gain among veterans is approximately $4.6 billion.

There are two costs associated with T38 considered in this exercise: the increase in DC benefits and the net change in medical expenditures resulting from the early detection and treatment of diabetes. First, Duggan, Rosenheck, and Singleton (2010) conclude that the policy increased total DC expenditures by $50 billion in net present value. And second, Hoerger et al. (2006) estimate that the net change in medical expenditures due to an early diabetes diagnosis – accounting for the cost of screening, diagnostic tests, treatment, interventions and complications – is $8,909 per person. Factored by the number of newly diagnosed cases due to T38, the total net change in medical expenditures is approximately $2.0 billion. The sum of these two costs, increased DC benefits and medical expenditures, total $52.0 billion.

12. The estimate is based on two calculations. First, to estimate the prevalence of diagnosed diabetes among Vietnam Era veteran in the absence of the policy, I assume that the prevalence rates would have been similar among Vietnam veterans and other Vietnam Era veterans in 2006 had the policy not been implemented. The rate of diagnosed diabetes in 2005/2006 was 18.1 percent (Appendix 1), so if 3.1 percent of these cases are newly diagnosed, the estimated effect of the policy, the prevalence would have been 15.0 percent. Second, assuming one-third of diabetes cases would have been undiagnosed in the absence of the policy – the average rate in the United States—the prevalence of diagnosed and undiagnosed diabetes was 22.5 percent in 2006. Thus, 40 percent (3.1/(22.5-15.0)) of undiagnosed diabetes cases had been detected as a result of T38.

13. According to VetPop2007, a veteran population model maintained by the VA, there were 7,297,000 veterans who served in Vietnam who did not serve in Korea, WWII, or after August 1990.

A cost to benefit comparison suggests that the value of additional time lived accounts for approximately 9 percent of total costs. However, this figure, which compares aggregate costs to aggregate benefits, may be different than the cost to benefit ratio at the individual level for three reasons: the increase in DC expenditures is not entirely attributable to newly compensated diabetes cases (Duggan, Rosenheck, and Singleton 2010), some newly compensated diabetes cases were already diagnosed, and not all newly diagnosed diabetics are compensated.[14] To approximate the increase in DC expenditures for a newly diagnosed and compensated diabetic, I factor the average annual increase in DC benefits among all beneficiaries affected by T38, which is $11,000 according to Duggan, Rosenheck, and Singleton (2010), with the expected number of years remaining, which is 19.14 years. This cost, combined with the increase in medical expenditures of $8,909, yields a total cost of $219,449. Relating the individual benefit to costs suggests that the benefit accounts for about 9 percent of total costs at the individual level as well.

The simplified calculation suggests that the value of additional time lived can account for some of the costs associated with the early detection and treatment of diabetes, but there are important components of this calculation that are omitted. First, the benefits may be understated if early detection delays the onset of debilitating conditions, or overstated if there are adverse psychological costs due to testing and treatment. However, the likelihood that a veteran reports fair or poor health did not change differentially relative to nonveterans after T38, so either these effects have yet to be realized or the net effect is negligible. Second, the additional timed lived may need to be adjusted for life-quality; but as Murphy and Topel (2006) point out, according to the U.S. Environmental Protection Agency Advisory Board (2000), there are no published studies suggesting that individuals with chronic conditions are willing to pay less to increase longevity than those without chronic conditions. And finally, the costs do not reflect the continued expansion of DC rolls and expenditures in the years to come. To be sure, the discrete change in DC roll growth as a result of the policy persists well after T38 was implemented.

VI. Discussion and Conclusion

Economic theory predicts that the availability of disability insurance may encourage health disinvestment by inducing moral hazard behavior. The null hypothesis of moral hazard effects, therefore, is that the prevalence of the insured disability does not increase once disability benefits become available. However, if benefits encourage the detection of latent conditions, which also predicts a rise in the prevalence of the insured disability, then the empirical test of detection effects and moral hazard effects are indistinguishable.

This study provides suggestive evidence that disability insurance does increase the incidence of diagnosis. More specifically, the advent of DC benefits to Vietnam veterans for diabetes increased the prevalence of diagnosed diabetes among the Vietnam veteran population by 6.1 percentage points, an increase driven by newly diagnosed

14. The estimated number of new diagnoses exceeds the 169,000 new diabetes cases compensated by the DC program between 2001 and 2006 (see note 1).

conditions and not false-positive responses. There is little evidence to suggest that rates of overweight and obesity increased after T38 was implemented, casting doubt on the degree to which social disability insurance induces moral hazard behavior in this context.

The analysis also contributes to the discussion of the causal pathways responsible for the socioeconomic status and health gradient. First, if patient individuals are more likely to invest in health and education, as the argument goes, then we may expect a greater impact of T38 on the prevalence of diabetes among the less educated. This hypothesis is supported in the data – the rate of diabetes increased considerably more among the less educated – though it is difficult to disentangle this effect from the fact that Vietnam Era veterans without a college degree are slightly more likely to have served in Vietnam. And second, free diabetes screenings have been available to Vietnam veterans through the Agent Orange registry since 1978, yet a considerable proportion of diabetic veterans chose not to be diagnosed. Thus, in the context of this study, it is difficult to argue that differential access to medical screening plays a large role in the rate of undiagnosed diabetes across socioeconomic strata. Developing a more nuanced understanding of the detection and treatment decision, and how this decision contributes to the socioeconomic and health gradient, is an important area of future research.

Appendix 1
Prevalence of Diabetes and Diabetes Treatment

	A. Veterans			B. Nonveterans		
	Diabetes	Diabetes & Borderline	Treatment	Diabetes	Diabetes & Borderline	Treatment
Period						
1997-1998	6.09	7.30	5.34	6.57	7.62	5.48
	(0.69)	(0.75)	(0.65)	(0.54)	(0.58)	(0.50)
1999-2000	7.21	8.52	5.61	8.88	9.60	8.05
	(0.76)	(0.82)	(0.68)	(0.66)	(0.68)	(0.63)
2001-2002	9.46	10.60	8.11	11.15	11.95	9.76
	(0.88)	(0.92)	(0.82)	(0.72)	(0.74)	(0.68)
2003-2004	14.02	15.98	12.68	13.51	14.39	12.03
	(1.03)	(1.09)	(1.00)	(0.84)	(0.86)	(0.80)
2005-2006	15.99	18.05	13.96	13.12	14.64	12.18
	(1.15)	(1.21)	(1.09)	(0.88)	(0.92)	(0.85)

Note: Estimates are derived from pooled the NHIS sample described in Table 2. The analysis sample is identical to the one considered in Table 2. Adult Sample weights were used. Standard errors are in parentheses.

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